THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON,

FROM THEIR COMMENCEMENT, IN 1665, *TO THE YEAR* 1800;

Abridged,

WITH NOTES AND BIOGRAPHIC ILLUSTRATIONS,

BY

CHARLES HUTTON, LL.D. F.R.S.
GEORGE SHAW, M.D. F.R.S. F.L.S.
RICHARD PEARSON, M.D. F.S.A.

VOL. IX.

FROM 1744 TO 1749.

LONDON:
PRINTED BY AND FOR C. AND R. BALDWIN, NEW BRIDGE-STREET, BLACKFRIARS.
1809.

CONTENTS OF VOLUME NINTH.

	Page
E. R. SEEHL, Volatile Acid of Sulphur	1
Geo. Aylett, of a Spina Bifida	5
C. Warwick, Injections on tapping for ascites	6
Dr. Ste. Hales, on the same subject	8
Dr. John Fothergill, Origin of Amber	9
Biographical Notice of Dr. John Fothergill	ibid
J. B. Sec. de Montesquieu, Figured Stones	12
Biograph. Notice of J. B. S. de Montesquieu	ibid
H. Baker, on the Eye-sucker	15
Peter Collinson, on Shells and Soal-fish	ibid
J. F. Camillis, Disorder of F. Bolognini	16
Dr. Jurin, on the Action of Springs	18
Albert Haller, Tumour of the Ovarium	29
Jos. Miller, Catalogue of 50 Plants, &c.	30
H. Baker, to take Impressions of Coins	ibid
Dr. Fothergill, on the Manna Persicum	31
Alb. Haller, on the Cyanus Foliis Radic	ibid
Ro. Pickering, on Mushrooms	ibid
——— Diary of the Weather	34
Wm. Watson, Poison of boiled Hemlock	38
Biographical Notice of Dr. Wm. Watson	ibid
C. L. Gersten, Eclipses and Occultations	40
Chr. Warwick, Injections on Tapping	ibid
Dr. Bevis, on the Planet Mercury	41
H. W. Taube, Rupture of the Navel	ibid
Wm. Watson, on Mushrooms and Fungi	ibid
Tho. Preston, on the Island of Shetland	44
Dr. Bevis, Occultation of Jupiter observed	45
Abr. Demoivre, Annuities on Lives	ibid
H. Baker, on a Fiery Meteor	46
J. Ward, on Weidler on Numeral Characters	ibid
Rev. J. Betts, Observation of a late Comet	47
Alb. Haller, Scirrhosity of the Cerebellum	49
Dr. J. Mitchell, Different Colours of People	50
N. Peters, on a Boy shot through the Lungs	67
Dr. J. Bartram, Muscles and Oyster-banks	70
Chr. Hunter, Ancient Roman Inscriptions	71
Gowan Knight, Magnetical Experiments	ibid
J. H. Winkler, on his Book of Electricity	74
Abr. Trembley, Fresh-water Polypi	75
Dr. Ja. Parsons, on Vegetable Seeds	80
Jos. Miller, Catalogue of 50 Plants	82
Ro. Pickering, Manure of Fossil Shells	ibid
Wm. Arderon, Shuttle-spire in a Bladder	83
Dr. Tho. Hope, Remarkable Cure of an Eye	ibid
J. Ward, on a Roman Inscription	86
Dr. Huxham, a Ureter grown up, also a beautiful Stalactites, and a Calculus	87
Dr. J. Wall, on Musk in Convuls. Disord.	89
Andr. Reid, the Tonquinese Medicine	ibid
Wm. Watson, Description of a new Plant	93
S. C. Hollmann, Sudden Freezing, Electric Fire; and Micrometer to the Microscope	ibid
Tho. Dawkes, on a Gigantic Boy	95
B. Langrish, Receivers in Distillation	96
Dr. Cha. Paters, Bite of a Mad Dog	ibid
John Martyn, New Species of Fungus	99
Rog. Gale, Vegetation of Old Melon Seeds; and a Fossil of a Man	100
Wm. Watson, Stone in Horse's Stomach	101
Fr. Wollaston, a Snake swall. by a Porcupine	102
Dr. Bruni, on the Bologna Bottles	102
Tho. Wright, on two Ancient Camps	103
Dr. Fothergill, Recovering a Dead Man, by inflating the Lungs	ibid
Rev. L. Richmond, on a moving Moss	106
J. Ward, on two Dates in Arabian Figures	107
Rev. Dr. Miles, Firing Phosph. by Electric.	ibid
Cl. Amyand, Fracture of the Os Humeri	ibid
J. S. Winthorp, Bones of a Fœtus per Anum	108
Dr. Chr. Jernegan, Cystis in the Liver	ibid
J. H. Winkler, Observations on Electricity	109
Ja. Stirling, to blow Fire by Fall of Water	ibid
Dr. Lining, Statical Experiments	110
H. Baker, large Fossil Elephant Tooth	ibid
Dr. S. Myddleton, Extra-uterine Conception	112
Rev. G. Wheler, Rotation of Glass Tubes	114
J. Ward, Remains of Antiquity	118
G. Knight, Variations in Magnetic Poles	122
J. Bartram, Curious Wasps' Nests	123
Dr. Mortimer, a Boy with a Canine Appetite	124
Cl. Amyand, on an Iliac Passion	ibid
Col. Wm. Cook, Warming Rooms by Hot-steam, and on Ship's Bot. eaten by Worms	125
Ambrose Burer, on Osteocolla	126
Dr. Cookson, on the Boy with a Can. Appe.	ibid
Fr. Gmelin, on Ipecacuanha	ibid
Dr. Le Cat, on Cutting for the Stone	127
M. De Bozes, Extra. Electrical Phenomena	ibid
Jos. Miller, Catalogue of 50 Plants	128
Dr. Jurin, Force of moving Bodies	ibid
Rev. Dr. Miles, Lum. Emanat. & on Electr.	136
Paul Rolli, on Bianchini's Treatise on the Death of a Lady of Cesena	138
J. Hilliard, on a Person taking Fire	144
On the same subject	ibid
Dr. J. Parsons, a Quadruped from Bengal	145
——— Minute Crystal Stones	147
J. Ellicot, Specific Gravity of Diamonds	ibid
Dr. Mortimer, Natural Heat of Animals	148
Wm. Watson, Electrical Experiments	151
Dr. S. Hales, Expelling Stones of the Bladd.	159
M. Allamand, Unannealed Glass Vessels	161
Rev. Dr. Miles, making Cyder and Perry	165
H. Baker, Ancient Flint Building	167
Rev. Geo. Costard, on a Fiery Meteor	168
Biographical Notice of the Rev. G. Costard	ibid
Benj. Cooke, Mixtures of Blossoms	169
Mr. Arderon, Sinking of the Ground	ibid
Ja. Simon, Bones per Anum, and on Fossils	170
Dr. Mortimer, Distemper of the Cows	171
D. P. Layard, Fracture of the Os Ilium	173
D. E. Baker, the Ancient City Industria	174
Dr. Mortimer, Distemper of the Cows	177
Wm. Arderon, on the Weaver's Larum	180
Mar. Folkes, Petrified Human Bones	181
W. Arderon, a Water-wheel, and Jes. Bark	182
Dr. Mortimer, Distemper of the Cows	184
Dr. Fothergill, Rupture of the Diaphragm	187
Wm. Arderon, Fish in Glass Jars, &c.	189
Rev. Dr. Miles, Sealing-wax and Brimstone in Electrical Experiments	191

	Page
Wm. Watson, Electrical Experiments	195
Sir Hans Sloane, a Curious Sea-plant......	198
Rev. Dr. Miles, Electrical Experiments....	ibid
Mr. Trembley, Quicksilv. luminous in a tube	199
John Hill, Seeding of Mosses	200
Biographical Notice of Mr. John Hill	ibid
Ja. Sherwood, Paste Eels Viviparous......	202
Peter Collinson, on the Cancer Major	203
Dr. Parsons, on Musk as a Medicine......	206
Rev. Dr. Miles, on Electrical Fire........	207
———— Electricity of Water ..	213
Wm. Arderon, improved Hygroscope	214
Anonymous, Strength of Electric. Effluvia	215
Tho. Knowlton, the Ancient Delgovicia, and two very large Men..............	216
Dr. Jurin, Dynamical Principles..........	217
Tho. Knowlton, Large Deer's Horns......	225
Ja. Ferguson, Phenomena of Venus......	226
Biographical Notice of Mr. James Ferguson	ibid
Wm. Cock, Machine for Sounding the Sea	228
J. Breintal, Bite of a Rattle-snake	229
R. Badcock, Farina Fœcundans of Plants..	230
Rev. Dr. Miles, Electrical Observations ..	232
P. H. Zollmann, on A. L. Moro, on Petrifac.	233
Mr. Badcock, Farina of the Passion Flower	234
Wm. Arderon, on the Weather-cord......	235
Cha. Hamilton, Clepsydra or Water Clock	236
Dr. Le Cat, the High Operation in the Stone	238
Wm. Arderon, Deal-wood Hygrometer....	242
R. Badcock, Farina Fœcundans of Yew ..	243
Wm. Arderon, a Bristle in a Man's Foot..	244
Cl. Amyand, on the Spina Ventosa	245
J. H. Winkler, Effects of Electricity on him	251
Jos. Miller, Catalogue of 50 Plants	ibid
Geo. Howell, a Large Stone by the Urethra	252
Rob. Southwell, Extraordinary Echos.....	253
J. Durant, a Coal Mine on Fire; also on the Blue Well, and Subterraneous Cavern ..	254
Dr. Parsons, on Burning the Cows dead of the Distemper......................	255
W. Watson, poison of the Oenanthe aquatica	256
Ben. Robins, Electricity of Glass disturbs the Mariner's Compass and Nice Balances	262
T. Needham, Electrical Exper. at Paris....	263
Ja. Hodgson, Path of a Comet Obs. at Pekin	267
J. C. Pepusch, Music of the Ancients	268
Biographical Notice of Dr. Pepusch	ibid
Wm. Arderon, Cliffs on the Norfolk Coast	272
Dr. P. Templeman, Polypus at the Heart, and a Tumour of the Uterus	274
Dr. Monnier, Communicat. of Electricity ..	275
Dr. Bailey, Stones in Intestines of Horses..	278
Ja. Simon, Lough-neagh Petrifactions	282
Bp. Berkeley, on the same Subject	288
Biographical Notice of Bp. Berkeley......	288
P. Collinson, Libella or Ephemera May-fly	290
Wm. Fidge, Stone from a Dog's Bladder..	292
Sam. Glass, Dropsy from wanting a Kidney	ibid
Dr. J. Taylor, Ancient Inscription	295
Rev. Dr. Miles, on Gould's Acct. of Ants	298
M. Folkes, Esq. on a Passage in Pliny....	303
Rev. Mr. Mason, on Spelter, and Melting Iron with Pit-coal, and a Burning Well..	305
J. Browning, Effect of Electric. on Veget ..	306
C. L. Gersten, Transit of Mercury.......	307
Wm. Watson, Observ. on Dr. le Monnier on Electricity........................	308
Wm. Arderon, Perpendicular Ascent of Eels	311
E. M. Da Costa, on Belemnites.........	ibid
Dr. Brocklesby, on the Indian Poison	316
Biographical Notice of Dr. Rd. Brocklesby	ibid
Rd. Dunthorne, on the Moon's Motion...	318
Leonard Euler, Discoveries of the Russians on the N. E. Coast of Asia............	320
Biographical Notice of L. Euler..........	ibid
Wm. Arderon, on the Prickleback, &c....	322
————, on the Gossamer.........	324
Dr. Mortimer, on the Turquoise Stone....	ibid
Henry Baker, on a Curious Echinites.....	326
Mark Catesby, on Birds of Passage.......	327
Rev. Mr. Creed, Machine to Write Music	332
Dr. Gronovius, on the Mustela Fossilis....	335
Dr. Bevis, Occultation of Cor Leonis.....	336
P. Collinson, Uncommon Gleam of Light	337
Ben. Cook, New Flannel Sparkling......	ibid
John Hill, on Windsor Loam...........	ibid
Rob. Lucas, Use of Alicant Soap and Lime-water in the Stone..................	340
C. Lucas, Calculous concretions in a kidney	ibid
Wm. Arderon, Formation of Pebbles......	341
Arth. Dobbs, Distance between Asia and America..........................	ibid
G. Costard, Chinese Chron. and Astronomy	343
Turb. Needham, on Buffon's Burning Mirror	344
Marquis Nicolini, on the same..........	ibid
Winkler, Pirorganon, or Electrical Machine	345
Rob. Dingley, Gems or Precious Stones...	ibid
C. L. Gersten, New Mural Quadrant.....	347
Alb. Haller, on Morbid Anatomy........	348
Da. Fr. Baker, Water Efts casting their Skin	349
J. Ferguson, Improv. of the Celestial Globe	351
Edw. Davis, a Child's Bones Displaced ...	ibid
Dr. J. Burton, on the Ancient Delgovitia..	352
Fr. Drake, on the same................	354
Jos. Amos, on a Plica Polonica..........	356
Henry Baker, Clay Moulds of Roman Coins	ibid
W. Jones, Equations of Goniometrical lines	357
Biographical Notice of Wm. Jones, Esq...	ibid
Mr. Hoare, Antique Pictures at Herculaneum	363
G. Stovin, Body of a Woman and an Antique Shoe found in a Morass.............	364
H. Baker, Grubs Destroying the Grass....	367
Dr. Wall, Use of Bark in the Small Pox..	369
Biographical Notice of Dr. John Wall.....	ibid
Dr. Doddridge, of a person, having no Ear for Music, Singing well in a Delirium..	370
Jos. Miller, Catalogue of 50 Plants, &c. ..	ibid
Mortimer, on Catesby's Carolina......	370, 469
Dr. Cameron, Death of Dr. Green, by a Fracture of the Os Pubis..............	ibid
Dr. Miles, Difference of Cold and Thermometers Within Doors and Without.....	372

Page
Dr. Middleton, a Child 16 years in the Abdomen.... 373
Dr. Parsons, a Woman Speaking without a Tongue 375
Abr. Trembley, Small Water Polypi...... 377
M. Folkes, Esq Knight's Magnet. Expers. 390
Dr. Mortimer, a New Metalline Thermom. 397
Dr. S. Hales, Remarks on the same...... 407
Wm. Watson, on Electricity continued 408, 410
Dr. Bradley, a Motion of the Fixed Stars.. 417
Dr. Parsons, Shell-fish in a Stone........ 438
Wm. Watson, Velocity of Electricity..... 440
F. Drake, Fœtus discharged near the Navel 456
Dr. Rd Pococke, on the Giants' Causeway 457
Biographical Notice of Dr. Rd. Pococke. .. ibid
Mau. Johnson, a Metalline Thermometer.. 459
Dr. Mounsey, a Fœtus 13 Years in the Fallopian Tube.................... 460
Biographical Notice of Dr. J. Mounsey. .. ibid
Tho. Simpson, Motion of Projectiles...... 464
Biographical Notice of Mr. Tho. Simpson.. ibid
Rev Mr. Squire, Dumbness Cured by a Frightful Dream........................ 465
Wm. Arderon, on the Hearing of Fishes.. ibid
Charles Bonnet, Planting Seeds in Moss... 468
Biographical Notice of M. Charles Bonnet ibid
Rev. Tho. Birch, Inscrip. on a Roman Altar 470
H. Baker, on Russian Quab and Crabs' Eyes ibid
J. T. Klein, Account of the Marmot..... 473
J. Ellicot, Laws of Electricity............ 475
J. Ward, on a Roman Tessera.......... 484
Dr. Mortimer, a Divine with 2 Tongues... ibid
Dr. Brocklesby, on the Hearing of Fishes.. 485
——————, Poisonous Root in Gentian 488
Wm. Arderon, Caverns in Chalk Hills.... 490
Dr. Fothergill, Obs. and Exper. in Siberia.. 491
J. H. Winkler, Medical Electricity....... 494
H. Baker, on the same Subject.......... 497
Dr. Ste. Hales, on Checking Fire........ 498
George Graham, Magnetic Variation...... 499
Rev. Ph. Shelton, on the Cornel Caterpillar 500
Dr. Mounsey, Everlasting Fire in Persia. .. 503
Abr. Trembley, on Caterpillars.......... 504
M. de Reaumur, Preserving Dead Birds, &c. 506
Biographical Notice of Mons. de Reaumur ibid
Dr. Lyttelton, a Beautiful Nautilites...... 510
Biographical Notice of the Rev. Dr. Lyttelton ibid
Rob. Roche, a Coat Fired by Electricity... 512
Dr. Huxham, Child born with a Tumour, &c. ibid
Tho. Simpson, Fluents of Multinomials. .. 513
Dr. J. Lining, Weather in Carolina, &c... 514
Rev. J. Clark, Bills of Mortality......... 516
Sam. Jeake, Elements of a Short Hand.... ibid
Wm. Watson, of Brownrigg on Making Salt 518
Ja. Hodgson, Eclipses of Jup. Satellites.... 527
Dr. Miles, on a Storm of Thunder....... 528
John Byrom, on a Short Hand...... 530, 534
B. Cooke, Sparkling of Flannel and Hair in the Dark......................... 532
Rev. J. Forster, Earthquake at Taunton. .. 533
Dr. Stukely, Roman Inscription at Bath. .. ibid
Rev. Ste. Hales, on Electrical Experiments 533
Dr. Tho. Aery, Wound in the Cornea, &c. 535
Dr. Rd. Davies, Tables of Specific Gravities 536
W. Watson, Velocity of Electricity...... 553
Le Cat, Double Fetus of Calves 555
Dr. Dodridge, Monstrous Lamb, &c. 557
Biographical Notice of Dr. Dodridge...... ibid
Mons. Buffon, on his Burning Glass...... 558
Biographical Notice of Count Buffon..... ibid
Dr. Miles, Essay on Quantity........... 559
Rd. Hassel, on a Wounded Eye.......... 566
Dr. Bevis, on a Solar and a Lunar Eclipse.. 567
A. N. Grischow, Lunar Circle, &c....... ibid
Dr. Parsons, Preternatural Birth......... 568
Dr. Mitchel, Making of Potash.......... 572
Dr. Garcin, the Cyprus of the Ancients... 583
Dr. Stukely, on an Ancient Shrine........ 590
Mr. Short, an Eclipse of the Sun......... 591
Biographical Notice of Mons. le Monnier.. ibid
D. E. Baker, Two Extraordinary Belemnites 597
B. Cooke, Mixed Breed of Apples. 599
J. Ward, Town of Silchester, & ancient date ibid
Turb. Needham, Generation, &c. of Animal and Vegetable Substances.......... 604
J. de Castro Sarmento, Astron. Observ.... 615
Dr. Miles, Thermometers and Weather... 616
C. N. le Cat, Glasses to Preserve Preparations of Anatomy 618
J. de Castro Sarmento, Astron. Observ.... 619
Ant. de Ulloa, Solar and Lunar Eclipses... 620
Biographical Notice of Antonio de Ulloa... ibid
G. Smethurst, a New Shwan-Pan........ 622
John Mudge, Cutting for the Stone...... 625
Biographical Notice of Dr. John Mudge... ibid
On the Locusts in 1747 and 1748........ 629
H. Baker, Ammonitæ or Cornua Ammonis 632
Rev. Mr. Murdock, a Stomach Cartilaginous 633
Apothecaries' Catalogue of 50 Plants...... 635
Rob. Clarke, Calculus on the Glans Penis ibid
L. Garcin, Salvadora Plant.............. ibid
Sam. Reynardson, Weights and Measures.. 637
Le Cat, Dry Gangrenes, and New Forceps 643
N. Struyck, Parabolic Orbits of Comets.. 648
Dr. J. J. Huber, a Body without a Gall-bladder, and on a Gibbosity of the Sternum . 649
Dr. le Cat, Lithotomy on Women....... 650
S. C. Hollmann, Barometers and Weather 651
Cap. Waddell, Effe. of Lightn. on the Need. 652
G. Knight, the Needle struck by Lightning 653
Sir H. Sloane, a Serpent Stone, and the Rhinoceros Bezoar, and Double-horned Rhinoceros......................... 655
Dr. Parsons, on the Rana Piscatrix........ 658
Benj. Robins, Ascent of Rockets........ 660
Wm. Watson, Black Vomit of S. America 661
Condamine, Hour by the Southern Cross.. 664
Biographical Notice of C. M. la Condamine ibid
E. da Costa, on two Beautiful Echinites .. 665
M. Mackenzie, the Tides in the Orkneys .. 667
Wm. Watson, on Tradescant's Garden.... 668
Biographical Notice of John Tradescant .. ibid

	Page
Rd. Dunthorne, Moon's Acceleration	669
Biograph. Notice of R. Dunthorne	ibid
Alb. Haller, Morbid Struct. of Dead Bodies	675
N. le Cat, Lacrymæ Batavicæ, glass drops	ibid
G. M. Bose, Electricity of Glass, &c......	681
P. Daval, on an extraordinary Rainbow ..	682
W. Arderon, Roman Camp at Castor, and a Halo	ibid
L. Euler, the Earth's approach to the Sun..	684
B. Cooke, Mixture of Apple Farina, also on Mayze, and a Child born with Jaundice..	685
Dr. Miles, Agreement of Thermomet. &c.	686
F. Drake, Basrelief of Mithras	687
Dr. Mounsey, Russ. Castor, Baths, Salt Mi.	688
Dr. Mortimer, Small-pox 2 days after Birth	692
W. Watson, Small-p. on the Fetus in Utero	ibid
W. Milner, a Boy's Feet, turned in when born, turned out by sitting cross-legged..	695
Ja. Short, on his Equatorial Telescope....	ibid
Dr. Bevis, Eclipse of the Moon..........	698
Tho. Barker, Extraordinary Meteor	ibid
Wm. Borlace, Spars and Cornish Diamonds	699
John Bartram, Great Black Wasp........	ibid
Wm. Elstobb, a Lunar Eclipse	ibid
Ja. Hodgson, Eclipses of Jupiter's Satellites	ibid
Mrs. Senex, on Senex's large Globes......	700

THE CONTENTS CLASSED UNDER GENERAL HEADS.

Class I. MATHEMATICS.

1. *Arithmetic, Annuities, Political Arithmetic.*

	Page
Annuities on Lives, Demoivre	45
The Numeral Characters, J. Ward	46
Dates in Arabian Figures, J. Ward	107
Bills of Mortality, J. Clark	516
New Shwan-pan, G. Smethurst..........	522
Weights and Measures, S. Reynardson....	637

2. *Algebra, Analysis, Fluxions.*

Fluents of Multinomials, T. Simpson 513

3. *Geometry, Surveying, &c.*

Equations of Goniometrical Lines, Wm. Jones 357

Class II. MECHANICAL PHILOSOPHY.

1. *Dynamics.*

Action of Springs, Dr. Jurin	18
Force of Momentum, Dr. Jurin	128
Dynamical Principles, Dr. Jurin	217
Essay on Quantity, Dr. Miles	559

2. *Statics.*

Statical Experiments, Dr. Lining 110

3. *Astronomy, Chronology, Navigation.*

Eclipses and Occultations, Gersten	40
The Planet Mercury, Dr. Bevis	41
Occultation of Jupiter, Dr. Bevis	45
Of a late Comet, J. Betts	47
Phenomena of Venus, James Ferguson	226
A Comet observed at Pekin, Ja. Hodgson	267
Transit of Mercury, Gersten	307
On the Moon's Motion, R. Dunthorne	318
Occultation of Cor Leonis, Dr. Bevis	336
Chinese Chronol. and Astronomy, G. Costard	243
New Mural Quadrant, Gersten	347
The Celestial Globe, James Ferguson	351
Motion of the fixed Stars, Dr. Bradley	417
Eclipses of Jupiter's Satellites, Ja. Hodgson	527
A solar and a lunar Eclipse, Dr. Bevis	567, 698
Lunar Circle, &c. Grischow	ibid
A Solar Eclipse, Mr. Short	591
Astronom. Observ. De Castro Sarm.	615, 619
Solar and Lunar Eclipses, De Ulloa	620
Parabolic Orbits of Comets, Struyck	648
Hour by the Southern Cross, Condamine ..	664
Tides in the Orkneys, Mackenzie	667
The Moon's Acceleration, Dunthorne	669
Earth's Approach to the Sun, Euler	684
Equatorial Telescope, James Short	695
A Lunar Eclipse, Wm. Elstobb..........	699
Eclipses of Jupiter's Satel. Hodgson	ibid
Senex's large Globes, Mrs. Senex	700

4. *Projectiles, Gunnery.*

Motion of Projectiles, T. Simpson	464
Ascent of Rockets, B. Robins	660

5. *Mechanics.*

To blow Fire by the Fall of Water, Stirling	109
Rotation of Glass Tubes, G. Wheeler	114

	Page		Page
The Weaver's Larum, Wm. Arderon	180	Clepsydra or Water-clock, Cha. Hamilton	236
A Water wheel, Wm. Arderon	182	Machine to write Music, Mr. Creed	332
Machine for sounding the Sea, Wm. Cock	228		

6. *Hydrostatics.*

Tables of Specific Gravities, Dr. Davies .. 536

7. *Pneumatics.*

Improved Hygroscope, Wm. Arderon .... 214 Deal-wood Hygrometer, Wm. Arderon .. 242

8. *Acoustics, Music.*

Extraordinary Echoes, R. Southwell	253	Machine to write Music, Mr. Creed	332
Music of the Ancients, Pepusch	268		

9. *Optics.*

Micrometer to the Microscope, Hollmann	93	Buffon's Burning Mirror, Nicolini	344
Buffon's Burning Mirror, Needham	344	Burning Mirror, Buffon	558

10. *Electricity, Magnetism, Pyrotechny, Thermometry, &c.*

Magnetical Experiments, G. Knight	71	Buffon's burning Mirror, T. Needham	344
On Electricity, J. H. Winkler	74	————————, Nicolini	ibid
Electric Fire, S. C. Hollmann	93	Pirorganon, or Electrical Machine, Winkler	345
Firing Phosphorus by Electricity, Dr. Miles	107	Thermometers within and without, Miles ..	372
Observations on Electricity, Winkler ...	109	Knight's Magnet. Experiments, M. Folkes	390
Variations in Magnetic Poles, G. Knight ..	122	Metalline Thermometer, Dr. Mortimer ..	397
Warming Rooms by hot Steam, Wm. Cook	125	Remarks on the same, Dr. S. Hales	407
Extraord. Electrical Phenomena, De Bozes	127	Electricity continued, Wm. Watson ..	408, 410
Electricity and Lum. Emanations, Dr. Miles	136	Velocity of Electricity, Wm. Watson	440, 553
Electrical Experiments, Wm. Watson	151, 195	Metalline Thermometer, M. Johnson	459
————————, Dr. Miles	191, 198	Laws of Electricity, J. Elliot	475
Quicksilver luminous in a Tube, Trembley	199	Medical Electricity, J. H. Winkler	494
Electrical Fire, Dr. Miles	207	————————, Henry Baker	497
Electricity of Water, Dr. Miles	213	On checking Fire, Dr. Stephen Hales	498
Strength of Electrical Effluvia, Anonymous	215	Magnetic Variation, Geo. Graham	499
Electrical Observations, Dr. Miles	232	A Coat fired by Electricity, R. Roche	512
Effects of Electricity, J. H. Winkler	251	Sparkling of Hair and Flannel, B. Cooke ..	532
Electricity of Glass disturbs the Mariner's Compass and nice Balances	262	Electrical Experiments, Dr. Stephen Hales	534
Electrical Experiments at Paris, T. Needham	263	Burning Mirror, Buffon................	558
Communication of Electricity, Monnier ..	275	Thermometers and Weather, Dr. Miles ..	616
Effect of Elect. on Vegetables, Browning ..	306	Effect of Lightning on the Needle, Waddel	652
On Monnier's Electricity, Wm. Watson ..	308	On the same Needle, G. Knight	653
Uncommon Gleam of Light, Collinson	337	Electricity of Glass, &c. G. M. Bose	681
		Agreement of Thermometers, &c. Miles ..	686

Class III. NATURAL HISTORY.

1. *Zoology.*

The Eye-sucker, Henry Baker	15	Of the Marmot, J. T. Klein	473
Fresh water Polypi, Abr. Trembley	75	The Cornel Caterpillar, Ph. Skelton	500
Elephants' Teeth, Henry Baker.........	110	On Caterpillars, Abr. Trembley	504
Curious Wasps' Nests, J. Bartram	123	Locusts in 1747 and 1748	629
Quadruped from Bengal, Dr. Parsons	145	Double-horned Rhinoceros, Sir H. Sloane	655
On Gould's Account of Ants, Dr. Miles ..	298	The Rana Piscatrix, Dr. Parsons	658
The Prickleback, &c. Wm. Arderon	322	The great Black Wasp, J. Bartram	699
Small Water Polypi, Abr. Trembley	377		

2. *Botany.*

Catalogue of 50 Plants, Jos. Miller	30	Catalogue of 50 Plants, Jos. Miller	128
Cyanus Foliis Radicalibus, Haller	31	Curious Sea-plant, Sir Hans Sloane	198
On Mushrooms, Robert Pickering	ibid	Catalogue of 50 Plants, Jos. Miller	251
Catalogue of 50 Plants, Jos. Miller	82	————————, Jos. Miller	370
On a new Plant, Wm. Watson	93	————————, Apothecaries	635
New Species of Fungus, J. Martyn	99	Salvadora Plant, L. Garcin	ibid
Old Melon Seeds, Roger Gale	100	Tradescant's Garden, Wm. Watson	668

4

3. *Mineralogy.*

	Page		Page
Origin of Amber, Dr. Fothergill	9	Curious Echinites, Henry Baker	326
Figured Stones, Montesquieu	12	Mustela Fossilis Gronovius	335
Fossil of a Man, Roger Gale	100	Formation of Pebbles, Wm. Arderon	341
On Osteocolla, Ambr. Burer	126	Gems or precious Stones, Dingley	345
Minute Crystal Stones, Dr. Parsons	147	Body of a Woman and an Antique Shoe, found in a Moss, G. Stovin	364
On Fossils, James Simon	170	Shellfish in a Stone, Dr Parsons	438
Petrified Human Bones, Martin Folkes	181	Giants' Causeway, Dr Richard Pococke	457
Monro's Petrifactions, Zollman	233	Russian Qua[illegible] and Crabs' Eyes, H. Baker	470
Mine on Fire, and a Cavern, J. Durant	254	Beautiful Nautilites, Dr. Lyttelton	510
Cliffs on the Norfolk Coast, Arderon	272	Extraordinary Belemnites, D. E. Baker	597
Lough-Neagh Petrifactions, Ja. Simon	282	Ammonitæ, or Cornua Ammonis, H. Baker	632
——— Bishop Berkeley	288	Beautiful Echinites, E. Da Costa	665
Spelter, Melting Iron with Pit-coal, Mason	305	Baths, Salt-mines, &c. Dr. Mounsey	688
On Belemnites, E. M. Da Costa	311	Spars and Cornish Diamonds, Borlace	699
The Turquoise Stone, Dr. Mortimer	324		

4. *Geography, and Topography.*

Shetland Isle, Thomas Preston	44	Ancient Delgovitia, Dr. J. Burton	352
Ancient City Industria, D. E. Baker	174	———, Fr. Drake	354
Ancient Delgovitia, T. Knowlton	216	Catesby's Hist. of Carolina, Mortimer	370, 469
Lough-Neagh, James Simon	282	Observ. in Siberia, Dr. Fothergill	491
——— Bp. Berkeley	288	Town of Silchester, John Ward	509
N. E. Coast of Asia, L. Euler	320	Tides in the Orkneys, M. Mackenzie	667
Distance between Asia and America, Dobbs	341	Roman Camp at Castor, Wm. Arderon	682

Class IV. Chemical Philosophy.

1. *Chemistry.*

Volatile Acid of Sulphur, E. R. Seehl	1	Unannealed Glass Vessels, M. Allamand	161
Receivers in Distillations, Dr. Peters	96	Making Cyder and Perry, Dr. Miles	165
Bologna Bottles, Dr. Bruni	102		

2. *Meteorology.*

Diary of the Weather, R. Pickering	34	Weather and Thermometers, Dr. Miles	616
A Fiery Meteor, Henry Baker	46	Weather and Barometers, Hollmann	651
Sudden Freezing, S. C. Hollmann	93	Effects of Lightning on the Needle, Waddell	652
A Fiery Meteor, Geo. Costard	168	———, G. Knight	653
The Weather-cord, Wm. Arderon	235	Extraordinary Rainbow, P. Daval	682
Uncommon Gleam of Light, Collinson	337	On a Halo, Wm. Arderon	682
Weather in Carolina, Dr. Lining	514	Extraordinary Meteor, Tho. Barker	698
Storm of Thunder, Dr. Miles	528		

3. *Geology.*

A Moving Moss, L. Richmond	106	Windsor Loam, John Hill	337
Sinking of the Ground, Wm. Arderon	169	Caverns in Chalk-cliffs, Wm. Arderon	490
Cliffs on Norfolk Coast, Wm. Arderon	272	Earthquake at Taunton, J. Forster	533

Class V. Physiology.

1. *Anatomy.*

A Spina Bifida, Geo. Aylett	5	To preserve Preparations of Anat. Le Cat.	618
Fracture of the Os Humeri, Amyand	107	A Boy without a Gall-bladder, and a Gibbosity of the Sternum, Dr. Huber	649
Morbid Anatomy, Albert Haller	348	Morbid Structure of Dead Bodies, Haller	675
Speaking without a Tongue, Dr. Parsons	375		

2. *Physiology of Animals.*

Shells and Soal-fish, Peter Collinson	15	Fresh water Polypi, Abr. Trembley	75
Different Colours of People, Dr. Mitchell	50	Manure of Fossil Shells, R. Pickering	82
Muscles and Oyster-banks, Dr. Bartram	70	A gigantic Boy, Tho. Dawkes	95

	Page		Page
Snake swallowed by a Porcupine, Wollaston	102	On Gossamer, Wm. Arderon	324
Recovering a dead Body by inflating the Lungs, Dr. Fothergill	103	Birds of Passage, Mark Catesby	3.7
Extra-uterine Conception, Dr. Myddleton	112	Water Effts cast their Skin, Da. Er. Baker	349
Curious Wasps' Nests, Dr. Bartram	123	Grubs destroying the Grass, H. Baker	367
A Canine Appetite, Dr. Mortimer	124	A Person, having no Ear for Music, sung well in a Delirium, Dr. Dodridge	370
Worms in Ships' Bottoms, Wm Cook	125	Small Water Polypi, Abr. Trembley	377
The Canine Appetite, Dr. Cookson	126	Dumbness cured by a Frightful Dream, Squire	465
Death of a Lady of Cesena, P. Rolli	138	A Person with 2 Tongues, Dr. Mortimer	484
A Person taking Fire, Hilliard	144	Hearing of Fishes, Dr. Brocklesby	485
————, Anonymous	ibid	Double Fetus of Calves, Le Cat	555
Natural Heat of Animals, Mortimer	148	Monstrous Lamb, Dr. Doddridge	557
Fish in Glass Jars, Wm. Arderon	189	Preternatural Birth, Dr. Parsons	568
Paste Eels viviparous, James Sherwood	202	Generation, &c. of animal Substances, Needham	604
The Cancer Major, P. Collinson	203	Cartilaginous Stomach, Murdock	633
Two large Men, Thomas Knowlton	216	Child born with the Jaundice, B. Cooke	685
Large Deers' Horns, Tho. Knowlton	225	Small-pox 2 days after Birth, Mortimer	692
Bite of a Rattle-snake, J. Breintal	229	Small-pox on the Fetus in Utero, Milner	692
Libella or Ephemera May-fly, Collinson	290		
Perpendicular Ascent of Eels, Arderon	311		

3. *Physiology of Plants.*

Poison of boiled Hemlock, Wm. Watson	38	Farina of the Passion Flower, R. Badcock	234
Mushrooms and Fungi, Wm. Watson	41	Farina Fœcundans of Yew, R. Badcock	243
Vegetable Seeds, Dr. Parsons	80	Poison of the Oenanthe Aquat. W. Watson	256
Manure of Fossil Shells, R. Pickering	82	Planting Seeds in Moss, C. Bonnet	468
Vegetation of old Melon Seeds, R. Gale	100	Mixed Breed of Apples, B. Cooke	599
Mixtures of Blossoms, B. Cook	169	Generation, &c. of Veget. Subst. Needham	604
Seeding of Mosses, John Hill	200	Mixture of Apple Farina and Mayze, Cooke	685
Farina Fœcundans, R. Badcock	230		

4. *Medicine.*

Disorder of F. Bolognini, J. F. Camillis	16	The Indian Poison, Dr. Brocklesby	316
The Manna Persicum, Dr. Fothergill	31	Soap and Lime-water in the Stone, R. Lucas	340
Musk in Convulsive Disorders, Dr. Wall	89	Bark in the Small-pox, Dr. Wall	369
Tonquinese Medicine, Andr. Reid	ibid	Dumbness cured by a Fright, Squire	465
Bite of a Mad Dog, Dr. Cha. Peters	96	Russian Quab and Crabs' Eyes, H. Baker	470
On Ipecacuanha, Fr. Gmelin	126	Poisonous Root in Gentian, Brocklesby	488
Distemper in the Cows, Mortimer 171, 177,	184	Medical Electricity, J. H. Winkler	494
Jesuits' Bark, Wm. Arderon	182	———— Henry Baker	497
Musk as a Medicine, Dr. Parsons	206	Cyprus of the Ancients, Dr. Garcin	583
Bite of a Rattle-snake, J. Brintal	229	Black Vomit of S. America, Wm. Watson	661
Burning Cows dead of Distemper, Parsons	255	Russia Castor, &c. Dr. Mounsey	688

5. *Surgery.*

A Spina Bifida, Geo. Aylett	5	Bones per Anum, Ja. Simon	170
Injections on Tapping, Chr. Warwick	5, 40	Fracture of the Os Ilium, D. P. Layard	173
———— Dr. Ste. Hales	8	Rupture of the Diaphragm, Fothergill	187
Tumour of the Ovarium, Alb. Haller	29	High Operation in the Stone, Le Cat	238
Rupture of the Navel, H. W. Taube	41	Bristle in a Man's Foot, Wm. Arderon	244
Scirrhosity of the Cerebellum, Haller	49	The Spina Ventosa, Cl. Amyand	245
A Boy shot through the Lungs, N. Peters	67	Large Stone by the Urethra, Geo. Howell	252
Shuttle Spire in a Bladder, Arderon	83	Polypus at the Heart, and Tumour in the Uterus, Dr. Templeman	274
Remarkable Cure of an Eye, Dr. Hope	ibid	Stones in Horse's Intestines, Dr. Bailey	278
Ureter grown up, and a Calculus, Huxham	87	Stone in a Dog's Bladder, Wm. Fidge	292
Stone in a Horse's Stomach, Wm. Watson	101	Dropsy from wanting a Kidney, Glass	ibid
Recovering a dead body by inflating the lungs	103	Soap and Lime-water in the Stone, Lucas	340
Bones of a Fetus per Anum, Winthorp	108	Calc. Concr. in a Kidney, C. Lucas	ibid
Cystis in the Liver, Dr. Jernegan	ibid	A Child's Bones displaced, Edw. Davis	351
The Iliac Passion, Cl. Amyand	124	A Plica Polonica, Jos. Amos	356
Cutting for the Stone, Le Cat	127	Fracture of the Os Pubis, Dr. Cameron	370
Stones of the Bladder, Dr. Ste. Hales	159		

	Page		Page
A Child 16 Years in the Abdomen, Myddleton	373	Cutting for the Stone, John Mudge......	625
Fetus discharged near the Navel, Drake ..	456	Calculus on the Glans Penis, R. Clarke....	635
Fetus 13 Years in the Fallop. Tube, Mounsey	460	Dry Gangrenes, and new Forceps, Le Cat	643
Child born with a Tumour, &c. Huxham..	512	Lithotomy on Women, Le Cat..........	650
Wound in the Cornea, &c. Dr. Aery......	535	A Boy's Feet turned inwards when born, turned out by sitting cross-legged, Milner	695
A wounded Eye, Rd. Hassel............	566		

Class VI. The Arts.

1. *Mechanical.*

Elements of a Short-hand, Sam. Jeake.... 516
On a Short-hand, John Byrom.......530, 534

2. *Chemical.*

Preserving Dead Birds, &c. Reaumur 506
On making Salt, Wm. Watson.......... 518
Making Potash, Dr. Mitchel............ 572
Lacrymæ Batavicæ, or Glass Drops, Le Cat 675

3. *Fine.*

To take Impressions of Coins, H. Baker.. 30
Pictures at Herculaneum, Mr. Hoare 363

4. *Antiquities.*

The Numeral Characters, J. Ward	46	Ancient Delgovicia, Dr. Burton	352
Roman Inscriptions, Chr. Hunter........	71	——— Fr. Drake	354
A Roman Inscription, J. Ward..........	86	Clay Moulds of Roman Coins, H. Baker..	356
Two Ancient Camps, Tho. Wright	103	Antiques at Herculaneum, Mr. Hoare	363
Dates in Arabian Figures, J. Ward	107	Inscription on a Roman Altar, T. Birch ..	470
Remains of Antiquity, J. Ward..........	118	A Roman Tessera, John Ward	484
Ancient Flint Building, H. Baker........	167	Roman Inscription at Bath, Dr. Stukely ..	534
Ancient City Industria, D. E. Baker......	174	An Ancient Shrine, Dr. Stukely	590
Ancient Delgovicia, T. Knowlton........	216	Silchester, and Ancient Date, J. Ward....	599
Ancient Inscription, Dr. Taylor..........	295	Roman Camp at Caster, Wm. Arderon....	682
A Passage in Pliny, M. Folkes..........	303	Basrelief of Mithras, Fr. Drake	687

Class VII. Biography; *or, Account of Authors.*

Berkeley, Bp....	288	Dunthorne, Rd..	669	Montesquieu ...	120	Simpson, Tho. ..	464
Brocklesby, Dr..	316	Euler, Leonard..	320	Mounsey, Dr. Ja.	460	Tradescant, John	668
Bonnet, Cha....	468	Fothergill, Dr...	9	Monnier.......	591	Watson, Dr. W.	38
Buffon	558	Ferguson, Ja....	226	Mudge, Dr. John	625	Wall, Dr. John..	369
Costard, Geo....	168	Hill, Dr. John ..	200	Pepusch, Dr....	268	Ulloa, Antonio ..	620
Condamine	664	Jones, Wm.....	357	Pococke, Dr. Rd.	457		
Doddridge, Dr...	557	Lyttleton, Dr. C.	510	Reaumur......	506		

REFERENCES TO THE PLATES IN VOLUME IX.

Plate I, Fig. I, 5; II, III, 15; IV to VII, 19; VIII, 20; IX, 22; X, 25; XI, 27; XII to XVII, 37.
.... II, .. I, 76; II, III, 78; IV, 87; VI, 93; VII, 96; VIII, 100; IX, 109, X, XI, 123; XII, 147.
.... III, .. I to IX, 118.
.... IV, .. I, 181; II, 183; III, 214; IV, V, VI, 198; VII, VIII, IX, 201; X, XI, 202; XII to XIV, 231; XV, XVI, 232.
.... V, .. I, 228; II to VI, 235; VII, VIII, 236; IX, X, 237.
.... VI, .. I, II, 242; III, IV, 343; V to X, 244; XI, 261; XII, 262.
.... VII, .. I, 262; II, III, IV, 290; V, 326; VI, VII, VIII, 333.
.... VIII, .. I, 335; II, III, 351; IV, V, VI, 367; VII to XIII, 389.
.... IX, .. I, 403; II, 404; III, 405; IV, 459; V, 511; VI, 567; VII, 568; VIII, 571.
.... X, .. I, 571; II, III, IV, 598; V to IX, 615; X, XI, 618.
.... XI, .. I, II, III, 628; IV, 645; V, VI, VII, 646; VIII to X, 647; XI to XIV, 650.
.... XII, .. I, II, 657; III, 658; IV to VII, 660; VIII, 695; IX, 699.

THE

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY OF LONDON;

ABRIDGED.

An easy Method of procuring the Volatile Acid of Sulphur. By Ephraim Rinhold Seehl. N° 472, p. 1. *Vol. XLIII.*

MR. S. here describes two modes of obtaining what he terms volatile acid of sulphur, which is the sulphurous acid of later chemical writers.

His 1st method is as follows: Take 1 lb. of the flowers of brimstone, and 5 lb. of dry fixed alkaline salt; grind them together, and put the mixture into an iron pot; add gradually, a little water, so as first to dissolve the fixed alkali; then gradually dispose the whole to boil, the better to dissolve the sulphur: when these have boiled for ¼ of an hour, add more water by degrees; and, when the sulphur appears to be dissolved, filtre the solution; evaporate it to perfect dryness in an iron pot, till it almost begins to melt; then take out the dry powder when cool; put it into a tubulated retort; which being placed in a sand-heat, and a receiver luted on, pour in at the tube, by degrees, 2 lb. of rectified oil of vitriol; and immediately secure the tube with a stopple of chalk, and luting: then give a gradual fire for some hours, till all the volatile spirit of sulphur is come over; after which, let the fire go out; take off the receiver, and carefully pour the liquor into a glass phial, to be stopped with a glass stopper. The volatile spirit, thus procured, will be about 12 oz. in weight, and appear tolerably limpid, smell extremely quick, pungent, and gassy or sulphureous, almost like the gas sulphuris, prove strongly acid to the taste, and in all other experiments; so that it may be used in the way of a general acid; being perhaps the best, in all respects, that is hitherto known, except the following.

Process 2.—Take 1 lb. of the flowers of brimstone, 4½ lb. of fixed alkaline salt; grind and mix them well together; put the powder into an iron pot set over the fire; add a little water by degrees, to dissolve the salt; then boil gently for a ¼ of an hour; add more water, and afterwards 3 lb. of strong quick-lime; let all boil together for a while: when the solution is complete, filtre the lixivium, and evaporate to a dry powder, as in the first process; put this powder into a tubulated retort; and pour on gradually 1¼ lb. of rectified oil of vitriol; proceed to distil as before: thus you will obtain 8 oz. of a more strong, more acid, and more volatile spirit, than the former, and of a yellowish colour.

Observations.—1. The proportions of the several ingredients here set down, Mr. S. found, by repeated trials, to be the best. 5 lb. of alkaline salt are thus absolutely necessary to dissolve 1 lb. of sulphur; though, when quick-lime is used, as here specified, 4½ lb. of fixed salt are sufficient; or even 4 lb. if the quick-lime be very good and strong: so much does the lime strengthen the lixivium, or enable it to dissolve the sulphur.

2. These 2 processes differ somewhat considerably, as to the quantity and quality, both of the spirit and caput mortuum they afford. The spirit made with lime is less in quantity, but specifically heavier, and yet more volatile, than the other: and the caput mortuum with lime is much whiter, purer, and fitter for making the tartarus vitriolatus, than that made without lime.

3. By mixing 1 lb. of fixed alkali with the sulphur at first, boiling them a little, and filtring the liquor, then adding 2 lb. more of the salt along with 2 lb. of lime, he found that the sulphur sooner dissolved, than if he put the whole quantity in at once; and thus, after the 2d filtration, he put in the rest of the salt and lime, till all the sulphur is dissolved; as finding this the readier way to perfect the solution.

4. In the distillation, a little of the sulphur will sometimes sublime into the neck of the retort; and this seems owing either to making the fire too fierce at the beginning, or using the oil of vitriol too weak: but such a sublimation of the sulphur is no further detriment to the operation.

5. When lime is used in this process, a considerable proportion of fixed alkaline salt may be saved, the spirit will be rendered stronger, and the caput mortuum cleaner and whiter, so as to make an excellent tartar of vitriol, by solution, filtration, and crystallization. But it must be observed, that the produce of this tartar of vitriol, when prepared, is not near so large as when no lime has been used in it; and accordingly he found, that the dry powder, remaining after the solution and evaporation of the sal alkali and sulphur alone, weighed as much as they did originally: whereas, when lime has been used, the remaining powder has weighed half a pound less than the original weight of the sulphur and fixed salt; which seemed a curious phenomenon; and might lead to farther discoveries of the relation between lime and fixed alkaline salt, &c.

6. The advantages of this method, in respect of M. Homberg's, are, (1.) That

it gives a much larger quantity of the acid of sulphur. (2.) That it gives a very volatile acid; whereas his is fixed, so as scarcely to differ from oil of vitriol. (3.) That it is obtained in a much easier and cheaper manner. (4.) That this spirit has probably much greater medicinal virtues. (5.) That it is a much more powerful menstruum; especially with regard to metals, and particularly their crystallization. (6.) That the caput mortuum is a medicine of great use; and may defray the expence of the whole operation; being perhaps the best way of making the tartarus vitriolatus perfectly pure and neutral for medicinal purposes; its expected virtues greatly depending on its being clean and neutral.

7. His method has also several advantages over Dr. Stahl's; though his indeed affords a volatile acid. But then, (1.) Stahl's method burns the sulphur, and consequently destroys its texture, and throws off part of the spirit or gas; whereas this gently dissolves the sulphur, and only divides it, so as to leave the acid afterwards separable by a stronger or more ponderous acid; and no-way consumes or destroys the inflammable part, as burning does. (2.) This method is more neat or elegant than his, and affords a larger produce, at a cheaper rate, and in greater perfection, both as a medicine, and as a menstruum; leaving also the tartarus vitriolatus cleaner, and fitter for use as a medicine.

8. Persons but little versed in chemical philosophy, and its operations, might be apt to suspect, that this spirit is not a pure spirit, or acid of sulphur; but mixed with the oil of vitriol, here used as the medium to separate the spirit from the sulphur and fixed alkali: but the Society very well knows it to be a universal law, that a heavier or stronger acid, used in a suitable proportion, constantly in these cases separates a weaker, and leaves it free to rise by itself in distillation, as it remarkably does in the present operation; where all the oil of vitriol employed unites with the fixed alkali, so as to make the true tartarus vitriolatus, and leaves the lighter spirit quite detached and free to rise, and come over the helm in distillation. So that this volatile spirit and the fixed oil of vitriol are by no means the same thing; nor should the one be used for the other, especially in physic.

9. But though the oil of vitriol be allowed to differ from the volatile acid of sulphur, some may imagine that there is no difference between this volatile acid and the volatile spirit of vitriol, as it comes over in the rectification of oil of vitriol; or between our spirit and the gas sulphuris, which is extremely pungent and volatile: but whoever attentively examines and compares the volatile spirit of vitriol, or the gas sulphuris, with our spirit, will soon be convinced of a great difference; though indeed they agree in the point of gassy volatility: for the volatile spirit of vitriol is only an impure phlegm of vitriol, containing very little acid, and is chiefly impregnated with the wild fumes of the vitriol; so as, by standing a while, to quit the liquor, and leave it nauseous, vapid, and gross:

whereas the volatile spirit of sulphur long preserves its volatility, the purer gas being here lodged in a pure acid liquor, less dense and gross than oil of vitriol; so that when, by being long unstopped, this acid spirit loses of its volatility, as it will do, yet it never loses of its acidity; and even then appears to be the most pure and perfect mineral acid we can any ways procure. And as to the gas sulphuris, when made in perfection; this is no more than the fumes of burning brimstone caught and detained in water: so that this preparation, wanting the acid, cannot be compared in that respect with this spirit, which has it in perfection.

10. What the medicinal virtues and uses of this volatile acid of sulphur may be, Mr. S. submits to the Society, and the learned physicians, to whom it belongs; he only begs leave to observe, that if what is found in numerous learned physic-books be just, there are hopes that it may prove a noble medicine in many kinds of fevers, the small-pox, and even in plagues. In some of these books it is siad, that malignant fevers are owing to a superabundance of volatile alkaline salts in the body; and if that be the case, one might hope to neutralize or destroy such a superabundancy of volatile alkaline salts, by the prudent use of this fine volatile acid; which is capable of being mixed with water, juleps, and most sorts of drinks.

11. Mr. S. likewise finds, that the origin of all pestilences and plagues has been assigned to the following causes, viz. (1.) The carcases of men, horses, or cattle, killed or slain, and putrefying above ground by heat and moisture, and thus infecting the air by their noxious, volatile, urinous alkaline salts, that copiously issue from them in such a putrefying state. (2.) Dead fish, thrown out of the sea, and putrefying on the shore; or swarms of dead insects, bred in fens and marshes, drowned in the ocean, and thrown on shore by the tides, and left to putrefy in hot moist climates. (3.) Woollen goods, silks, and apparel, packed up or worn by infected persons, or those that attended the sick, or that came from infected places. (4.) Unwholesome diet, or corrupted putrefying meats, abounding with too subtilized, or too rarefied, volatile, urinous salts. (5.) Mineral, arsenical, and poisonous damps, vapours, exhalations, &c. arising from volcanos, mines, grottos, by means of subterraneous heats and fermentations.

12. It were easy, by natural reasoning on these causes assigned of the plague, to show that this distemper consists in a kind of putrefactive state of the body, when the salts are volatilized, unsheathed, and let loose to tear and wound the solids, after destroying the texture; and consequently that the volatile acid, here shown to be easily procurable, is a natural remedy in such cases.*

* These speculations of Mr. Seehl's concerning the antipestilential properties of acids, have been verified, to a certain extent, by recent experiments. But from these experiments, it appears, that the said acids prove better suited to such purposes in proportion as they are more oxygenized. Hence the sulphuric acid is more antipestilential than the sulphurous acid, or this author's volatile acid of sulphur.

An Observation of a Spina bifida, commonly so termed. By Mr. George Aylett, Surgeon, Windsor. N° 472, p. 10.

There appeared, covering the lower part of the loins of a lusty infant just born, a large incysted tumour, that seemed capable of containing a pint of water, whose contents had escaped in the birth from a small perforation in the middle of the cyst; from whence, on pressure, issued out a bloody serum. Flannels, wrung out of a hot, discutient, and restringent fomentation with spirits, were twice a day applied, to prevent its mortifying; to which the upper part seemed greatly tending.

The first 4 days there appeared no visible alteration in the child's health: she sucked well; was as hearty and strong as most at that age are; no paralysis in the extremities, but a daily discharge from the perforation of near 2 oz. of the same bloody serum which at first issued out. The nurse had observed that during all this time it had not made one drop of water. The 5th day the child was convulsed; which increasing, she died in the night following. On the division of the cyst, next day, there appeared a thin membranous substance, lining it internally; and might be an expansion of the membrane which invelopes the medulla spinalis. A number of small blood-vessels appeared about the perforation of the bone; and underneath a small portion of the medulla of a very thin consistence. There was no opportunity of making a further examination, through the mother's importunities: but the lumbar vertebræ and os sacrum were taken out, as appears in the annexed figure.

Plate 1, fig. 1, by C. M. Here AB shows the vertebræ of the loins; BC the os sacrum; CD the ossa coccygis; EF the spinal processes of the vertebræ of the loins; which spines are here discontinued, and an opening formed, FGHI, quite into the canal of the vertebræ; so that the medulla spinalis was entirely laid bare without any bony covering. This opening has been mistaken for a parting of the spinal processes into 2 rows; or as if at F they had divided into 2 branches; the 2 edges F and G feeling through the integuments like a bifurcation of the spine, and so have given rise to the notion of a spina bifida; which case C. M. doubts whether it ever exists: for a perfect spina bifida must suppose the very canal and medulla spinalis to divide into 2 branches, the bodies of the vertebræ to become near twice as wide as usual, and the spinal processes to divaricate into 2 rows or ridges of spines.

Dr. Rutty, late Secr. R. S. has communicated a case like this. See Phil. Trans. N° 366, or page 487, vol. vi. of these Abridgments.

An Improvement on the Practice of Tapping; by which that Operation, instead of a Relief for Symptoms, becomes an absolute Cure for an Ascites. By Christopher Warrick, of Truro, Surgeon. N° 472, p. 12.

In 1742, among a great many hydropics that fell under Mr. W.'s care that year, he was called to the assistance of one Jane Roman, near 50 years age, and confined to her bed, under that species of dropsy called ascites, owing its rise, some years before, to the severity of a lingering intermittent fever. The most remarkable of her complaints were, loss of appetite, difficult breathing, unquenchable thirst, suppression of urine, and a short, importunate asthmatic cough, joined to that essential symptom of the disease, a large quantity of extravasated waters in the cavity of the abdomen, distending it to an enormous size, and perceptibly fluctuating. Her more inferior parts were likewise swoln to an uncommon magnitude, with livid spots and vesications in divers places. Under these circumstances, and already satiated with tedious courses of ineffectual medicines, Mr. W. drew from her (Sept. 20th) 36 pints of a greenish transparent lymph, by a paracentesis made after the usual manner; by which her complaints vanished, and she was soon re-established. With some part of the extracted lymph, which he had conveyed to his own house, on his return thither, he made the following observations:

Obs. 1.—Being as warm as it came from the abdomen, with one pint of it he mixed the like quantity of fresh Bristol water; and immediately a slight coagulum ensued.—*Obs.* 2. Mixing equal parts of warmed lymph and cohore claret together, the same phenomenon appeared; the coagulum subsided, and the mixture became milky.—*Obs.* 3. Being mixed with Pyrmont water, it manifested little or no change, only went turbid.—*Obs.* 4. He mixed a decoction of the cortex with the like quantity of warm lymph, and it dropped a branny sediment.—*Obs.* 5. Lymph per se, boiled, became gelatinous; but being mixed with a strong solution of terra foliata tartari, it soon resumed its former fluidity.—*Obs.* 6. Bringing the above mixture to a state of boiling, the phenomenon of coagulation appeared more eminently in each of them; especially that with claret. Eaton's styptic, tormentil-roots, pomegranate-peels, and almost every restringent, more or less afforded the same appearances of coagulation.

Notwithstanding the disappearance of the symptoms, and the favourable prospect that ensued the evacuation of the waters, the relief which she had was only of a short duration: for, Sept. 30, An inundation again alarmed her, and obliged her to remove the bandage, for fear of suffocation. Hence, to the latter end of October, she re-filled incredibly; and notwithstanding any method used to prevent it, within 40 days after the paracentesis, there was again collected, in the abdomen, and depending parts, a quantity of lymph, equal to, if not greater than, that which had but just before been extracted. All her former complaints, especially the dyspnœa, likewise returned, and oppressed her more violently than ever. —Oct. 29. The waters being ready to break their confines, and the pain and distention insupportable under them, she again desired his assistance to relieve her. He had by this time drawn some conclusions from the above observations on lymph and restringents, and flattered himself that some of them, especially those of the warmest kind, applied immediately to the parts affected, (the ruptured lymphatics) must, according to their known mode of operation, close up their mouths, and prevent a further effusion of their contents, and consequently a return of the disease.

In order then to obtain this desirable end, Mr. W. resolved to try their efficacy, by way of injection, on the emptied cavity: and for this purpose the claret and Bristol water seemed to claim the superiority of esteem; not only as they produced the strongest coagulum with lymph, but also in being the safest, and least liable to create any uneasy sensations on the viscera.

Mr. W.'s apparatus was, a large trois-quarts, made on purpose, and dipped in oil; an injector, capable of containing 2 or 3 pints, adapted to it; and 3 or 4 gallons of blood-warm injection, composed of equal parts of cohore claret, and fresh Bristol water; besides compress, bandage, &c. as is usual on these occasions. It was conducted pretty nearly thus: being seated on her bed-side, and proper assistants attending her, he plunged the trois-quart into the abdomen, about 5 or 6 inches below, and as much on the left side of the umbilicus; and thereby soon discharged upwards of 20 pints of such clear briny lymph as before; which quantity did not exceed $\frac{1}{4}$ of the whole, though as much as her strength could well bear: the claret and Bristol water being then in readiness, he began to replenish the empty cavity with them; but he had scarcely injected 10 or 12 pints of it, before a syncope, a very material obstruction, made some advances, and was like to baffle his design. Here he perceived the great expedition necessary in conducting this experiment; that symptom being more or less violent, as he happened to be dextrous, or remiss; and was, for the most part, the only one of consequence that attended it. Quickening therefore his hand as fast as he was able, and an assistant stopping the mouth of the cannula with his finger, to prevent a return, he soon brought her up to her former magnitude, and had the pleasure of seeing the above symptom suspended. He had then time to ask her, what kind of sensation this new piece of practice excited within the cavity? and whether or not she thought herself capable of undergoing it a second time? She answered him in the affirmative; and said, it seemed as it were entering her stomach. Notwithstanding he had reason to believe his intentions already answered, as much as in bringing those restringents in contact with the parts affected, yet as there was a great quantity of lymph left behind in the cavity undischarged, which, on account of the syncope, he could not well prevent, he imagined their action, and full efficacy, might thereby be in some degree interrupted. Every thing therefore being in a favourable way, he repeated the mixture for a 2d injection, the claret being in a double proportion of the water, to render it the more efficacious for that purpose; drew off the whole contents of the abdomen to as much as would flow through the cannula; repeated injections as before; and once more, without the least interruption, replenished her with them.

This total discharge, however, made a great alteration in the face of affairs; and her being full, and under distention, now altered not the case, as it did at first. A pungent pain possessed her breast, frequently darting through all the viscera; her breathing became extremely difficult; her pulse faltered; the syncope returned; and she became speechless. Under these circumstances it was high time to conclude his design; and therefore, having emptied the cavity, as well as the violence of them would permit it, he withdrew the cannula, applied proper compress and bandage, and finished the operation after the usual manner; which was the more agreeable, as it ended with the perfect recovery of her senses.

The day following, Oct. 30, on repeating his visit, which he did for divers others successively, he found things under a favourable disposition; a gentle diaphoresis, from a liberal use of cardiac medicines, having totally removed the syncope, dyspnœa, and all other complaints.

Nov. 12, He renewed the bandage, and set her on foot again.

The 24th he saw her again for the same purpose; found the swelling in the extremities going off, her heart in a chearful disposition, her appetite strong, and no symptom of a relapse, as she formerly had long before that time approaching.

Dec. 1. The last visit he made her, she told him, that there was no further occasion for his assistance. Finding every thing therefore in a favourable way, her appetite well, her urine in due quantity, her breathing clear, and the extreme parts of their natural size, he left her in pursuit of that health which she soon acquired, and continued to enjoy.

A Method of conveying Liquors into the Abdomen during the Operation of Tapping; proposed by the Rev. Stephen Hales, D. D. and F. R. S. on Occasion of the preceding Paper. N° 472, p. 20.

It occurred to Dr. H. on hearing read the case of the woman in Cornwall, that, if, on further trial, that, or any other liquor, should be found effectual to the purpose, it might be more commodiously injected thus: viz. By having 2 trochars fixed at the same time, one on each side of the belly; one of them having a communication with a vessel full of the medicinal liquor by means of a small leathern pipe: this liquor might flow into the abdomen, as fast as the dropsical lympha passed off through the other trochar; by which the dropsical lympha might be conveyed off to what degree it shall be thought proper; and that without any danger of a syncope from inanition; because the abdomen would, through the whole operation, continue distended with liquor, in such a degree as should be found proper; by raising or lowering the vessel with the medicinal liquor in it.

It is probable, that if the surface of the medicinal liquor be about a foot higher than the abdomen, it may be sufficient for the purpose. It were easy to find the force with which the abdomen is distended by the dropsical lympha, by seeing to what height it arose in a glass tube fixed to the trochar; which tube being taken away, it might probably be sufficient to have the medicinal liquor flow in from a less perpendicular height, than that to which the dropsical lympha arose in the glass tube.

An Extract from the Essay on the Origin of Amber. By John Fothergill, M.D.* N° 472, p. 21.

Dr. F. remarks that after all that has been written on the subject of amber, its origin is yet in a great measure unknown. Several ingenious men have searched into this affair on the spot where the amber is principally gathered. They have related their observations with great candour; they have given the conclusions

* The following particulars concerning the life and writings of Dr. John Fothergill are for the most part extracted from the biographical memoir prefixed to Dr. Lettsom's elegant edition of his friend's works. The concluding reflexions are by the annotator.

Dr. John Fothergill was born at Carr-End, Yorkshire, in 1712. He was of the society of Quakers. After he had gone through his grammatical education, he was apprenticed with an apothecary, and when the term of his apprenticeship was expired, he went to study physic at Edinburgh, where he took his degree of M.D. in 1736. He removed to London soon afterwards, and from thence made an excursion to the Continent, returning to the metropolis to settle as physician in 1740. While he was gradually getting into practice, the appearance of an alarming epidemic in the metropolis and neighbourhood afforded him an opportunity of exercising his talent for observation. This was the ulcerous sore throat, of which he published (1748) an account describing with much accuracy its symptoms and progress, and pointing out a new and more successful method of treatment. He condemned the free use of the lancet, and of purging and other debilitating medicines in this disorder, and enforced the propriety of administering cordial remedies and the Peruvian bark. This publication brought him into great repute, and his practice with his emoluments rapidly increased. He acknowledges that Dr. Letherland had been of opinion that the ulcerous sore throat was not an inflammatory disease, but one that required a warm regimen. His biographer, however, by no means admits what some have insinuated, that Dr. F. was wholly indebted to Dr. Letherland for his insight into the real nature of this epidemic, and for the improved method of treatment which he laid down. Some years afterwards another epidemic occurred which engaged his attention. This was the influenza of 1775. During that and the following year, his receipts were far greater than they had ever been before.

Of the large fortune which he had now acquired, he expended a part in the purchase of a retreat at Upton in Essex; where he planted a botanic garden, which in the course of a few years was stored with the rarest exotics from all parts of the world. He had afterwards another botanic garden attached to his seat, called Lea-Hall, in Cheshire, whither he used to retire every year for about 2 months in the summer. Another part of his fortune he expended in forming a museum, containing a great variety of specimens in every department of natural history, but particularly distinguished for a choice collection of shells and corals, for beautiful drawings on vellum, &c.

Dr. F. was a member of the R. S. and was associated to some other learned institutions. He died in 1780. His collected works were edited by Dr. Lettsom in 1784, forming a large 4to vol. adorned with a number of elegant plates.

Without possessing talents of a very brilliant or superior kind, Dr. F. attained the highest distinction in his profession. He attained it, very deservedly, by habits of industry and punctuality, and by they drew from the facts they discovered; yet without satisfying us entirely about many particulars.

But, as a knowledge of the nature of things can only be acquired from the things themselves, Dr. F. has carefully collected every material fact he could meet with, from those who were best acquainted with the natural history of this subject, and whose industry and accuracy in observing, and good faith in relating their observations, have been generally esteemed unexceptionable. Of these he only mentions Wigandus, Hartman, and Sendelius; the last who has written professedly on this subject.

The evidence which these gentlemen afford, he has endeavoured to throw together, in the most natural order he could, without respect to any hypothesis: but as this enumeration of facts admits of no abridgment, his papers would take up too much room in these memoirs: therefore he only refers to the essay itself. On this foundation of facts is built a discussion of the following problems:

1. Whether amber is not strictly a marine production; or is reduced by some quality of the sea-water into the condition we find it in? Or,

2. Whether it is not to be considered only as a bituminous body, generated in the bowels of the earth? Or, lastly,

3. Whether it is not, in its origin, a vegetable production, a resin; but changed into its present form by a mineral acid?

It will only be necessary, in this place, to mention, that after having shown the difficulty of maintaining the first two, he has undertaken to support the last of these opinions.

He endeavours to make it appear, that amber was originally a vegetable resin; the product perhaps of the fir or pine kind; by considering the appearance of the substance itself: and that though it has some distinguishing properties, yet it has many others, which are common to an indurated resin. Its aspect, its texture, its form, are arguments for this. The bodies which it is known to inclose, are urged as proofs, that this inclusion could not happen in the sea, nor in the earth, but on its surface; as the included objects are mostly animals, mostly volatiles too; very few reptiles, except such as are often found aloft in trees, as ants, spiders, &c. and scarcely ever any aquatics, are found in amber. And, he believes he may challenge all the cabinets of the curious to produce one instance of a marine body having been found naturally inclosed in amber. That there are several fictitious ones, is granted.

That this resin, with the trees which afforded it, were buried in the earth by the

diligent and close observation of the symptoms of diseases and the changes produced in them by medicine. Something too must be attributed to that cautious but placid demeanour, for which those who belong to the society of Quakers are remarkable, and which (to judge from this and other examples) is calculated to procure attention and respect to the medical character. To these qualifications were added a truly philanthropic disposition, and an unusual degree of active benevolence.

deluge, or by some such violent renversement, and there constitute the proper veins of amber, he also endeavours to make appear, from the same evidence of facts. The substance of which these veins consist has several genuine characteristics of wood still remaining. The texture of this substance is often an undoubted proof of what it has been; being fibrous, and when dried swims in water, and burns like other wood. The amber is not disposed in these veins in one continued stratum; but lumps of it are irregularly disseminated through the whole of what he calls the woody mass.

A difficulty, which naturally offers itself in this place, is attempted to be removed:—What proof have we, that this, which is called wood, is not mere fossil wood, the product of creating power, exerted in the place where it is now found? It is answered, that as there are undoubted proofs, that many substances now occur, where they were not originally framed, we are under no greater difficulty in accounting for the change of place in one than the other. It is known, that the exuviæ of fishes are sometimes found on the tops of the highest mountains. The bones of large animals are met with at prodigious depths, where nature never formed, nor art conveyed them. Whole woods are found under ground. The cause that effected these, was capable of the other.

Yet, allowing these allegations to be just, by what causes is this change produced? It is urged, that time is one of the causes; and that the rest is completed by the acid of the earth, a vitriolic mineral acid. It is proved, from the facts abovementioned, that such an acid is present wherever amber occurs in its proper matrix: that it is sometimes found in the amber itself, in its genuine appearance; that the acid of the salt of amber appears, from experiments, to be vitriolic.* That common turpentine, a known vegetable resin, affords, by proper management with a vitriolic acid, a considerable portion of the same chemical principles that amber does; that those pieces of amber, which have been found soft and imperfect, are nearly related to a vegetable resin: in short, it is endeavoured to be proved, that we have the ingredients of amber in our power, and that nothing is wanting but a successful application of them to each other; at least to procure the medicinal preparations of amber at an easy expence. Time and repeated trials may perhaps ripen this beginning, in some person's hands, into a happy useful imitation of this valuable substance.

This account is concluded with an inquiry into the medical virtues of amber, and some of its principal preparations. It is observed, that a substance of so firm a texture, as scarcely to yield to any common menstruum, is not likely to produce any considerable effects on the human body; and that indeed there are very few genuine instances recorded of any: that busy imagination might probably at first

* This is erroneous. The acid salt of amber possesses chemical properties distinct from those of the vitriolic acid, and by subsequent chemists has been proved to be a sui generis acid, which has accordingly been denominated succinic acid.

introduce it, prejudice support it, and engage men of parts and authority to recommend it to their inattentive successors.

Dr. F. finishes this abstract with remarking, that were some of the leisure moments of men of great abilities and experience, devoted to inform the world of the inefficacy of such methods and medicines as they have proved to be so, physic would be reduced into narrower bounds; they would merit the thanks of every one in the profession; and posterity at least would commend their endeavours.

Remarks on Stones of a regular Figure found near Bagneres in Gascony: with other Observations. By Mons. Secondat de Montesquieu, of the Academy of Sciences of Bourdeaux.* N° 472, p. 26.

Though the spring called La Fontaine du Salût is at a good distance from the town of Bagneres, it is as much frequented as any in that country; and besides its admirable effects in curing a great number of distempers, it likewise offers to the eyes of the lovers of natural history a very remarkable singularity.

In the first bath, through which the largest of the two branches of the spring flows, there are found small stones, of the colour of iron rust, and of a regular figure; being either parallelopipedes with oblique angles, of which the sides are unequal; or small solid bodies with 6 sides, only differing from cubes or dice in this, that the surfaces are not perfectly perpendicular to each other, but a little inclined; as also commonly longer than they are broad, and broader than they are high.

The largest yet seen were but 11 lines in length, $9\frac{1}{2}$ in breadth, and 6 in height: they are mostly a great deal smaller. Mr. S. had a very odd one, being a parcel, of a hundred in one lump. There are some on which are observed shining striæ, that seem to be of a metallic substance.

Mr. S. happening to walk in the road newly made between Bagneres and the Fontaine de Salût, he perceived that, in digging the ditch on the side of the road, the workmen had laid open a rock of a sort of imperfect slate, but softer, and of a lighter colour, than slate commonly is. The rock itself is composed of layers or beds lying almost parallel one over the other: the substance of the slate seems to be a composition of fibres or strings, placed on the sides of each other, and equally inclined to their beds or layers; hence, on breaking them with a hammer, the pieces sometimes are pretty like the figure of a regular parallelopiped with oblique angles.

On a narrower examination of this sort of slate, he found a great number of

* Jean Baptiste de Secondat de Montesquieu, counsellor of the parliament of Bourdeaux, &c. was the son of President Montesquieu, the celebrated author of the Esprit des Loix. He died 1796, in his 79th year. The following works were published by him: Observations sur les Eaux Minerales des Pyrenées, 1750; Considerations sur la Marine de France, 1756; Considerations sur le Commerce de la Grande Bretagne, 1760.

parallelopiped stones, like those above, only smaller: also, after having broken to pieces several little bits of slate, certain black spots were observed; which, by the help of a microscope, he found to be real figured stones.

Besides every one of these stones, as long as it remains in the rock, is always found between two bundles or clusters of transparent fibres, of which generally one is placed on the one, and the other on the opposite side. These bundles are larger in great stones: those which seem, to the naked eye, to be but small black spots, are yet accompanied by their bundles.

The stone of the mountain of Barege, on which the asbestos grows, breaks also constantly into fragments of the same figure.

Having seen several productions of nature, in which we discover, that the above figure so remarkably prevails, he was nevertheless surprised, when he found the same figure in the sediment of the water of the Fontaine de Salût. He had let a considerable quantity of the water of the mineral spring evaporate: there remained a shining dust, in which he could distinguish nothing. He then looked at it through a microscope; and, among several crystals of a less regular figure, he found many which were quite regular and well-shaped, with 6 faces, and oblique angles.

The waters of this spring contain no iron, as it is commonly believed. When you put the tincture of galls in it, it grows neither black nor red: this mixture only turns it a little, and makes it look whitish, after having stood some hours.

When these waters are evaporated by a mild and equal heat, the small crystals are found swimming on the surface; where they join, and form a film on the water; some of which stick also to the sides and the bottoms of the vessel. Those crystals which are formed first, are insipid; but those which are produced towards the end of the evaporation, are indeed of the same shape, but of a tart and saltish taste. There remains yet a little of this matter, which cannot be reduced to very regular crystals: it is of a very sharp and pungent savour, but has nothing of the prevailing character of acid or alkali; at least it makes no sensible impression on blue paper.

Mr. S. has also found at Bagneres, a particular aquatic plant, which he had seen, for the first time, in the great basin of the boiling spring at Dax: it bears neither fruit nor flower, as far as appears; its substance is entirely composed of small bladders full of air; its surface is like net-work or canvas; it grows only in the hottest mineral springs.

It is well known, that the greatest degree of heat in common water, is that which it acquires by boiling; that is, if water be put on the fire, it grows by degrees hotter and hotter, till it quite boils; but after that, though there be ever so much fire added, and it stand ever so long on it, it will never grow hotter than it was on the first instant, when it began to boil. Hence the degree of heat of boiling water is considered as fixed and invariable.

Fahrenheit is the first who has remarked the contrary. He observed that the heat of boiling water was greater when the air was heavy, that is, when the mercury stood higher in the barometer; and, on the contrary, the heat was less when the air was lighter. M. Le Monnier the younger, has put Fahrenheit's discovery past all doubt, and has very much improved it.

On the 6th of October 1739, being provided with a barometer, and a mercurial thermometer of M. Delisle, he climbed up to the highest top of the Canigou, a mountain in Roussillon, esteemed the highest among the Pyrenees: there he found his barometer to stand at 20 inches $2\frac{1}{4}$ lines; while at Perpignan it stood at 28 inches 2 lines. The difference between the heat of the water which he boiled there, and that which he boild at Perpignan, was 15 degrees of his thermometer.

The same thermometer being surrounded with snow, the mercury fell down to the same degree as pounded ice had made it do at Paris. Hence he concludes, that the heaviness of the air has a sensible influence on boiling water; but that it in no way alters the term of congelation.

This same experiment Mr. S. repeated on the top of the Pic du Midy; thinking that so singular a fact ought to be observed more than once. He carried two barometers to the highest top of the Pic du Midy on the 9th of last July; the mercury rose in one of the barometers to 20 inches 2 lines; and in the other, to 20 inches $1\frac{1}{4}$ line. He surrounded his thermometer with snow, and the mercury fell exactly to the same degree as the snow had made it fall to at Bagneres. Afterwards he plunged it into boiling water; on which the mercury rose to 165° of his graduation: so that the difference between the heat of boiling water on Pic du Midy, and that at Bagneres, consisted of 15°; of which there were 180 between the marks of congelation and boiling water.

At his return to Bourdeaux, he observed that he had marked the term of boiling water at Bagneres less high by $3\frac{1}{4}$, than at the term of boiling water at Bourdeaux, taken at the time when the barometer was at 28 inches 2 or 3 lines: therefore having anew graduated the thermometer, the 165th, the degree of the former graduation, fell now on the 162d; so that the complete difference between the term of boiling water on the top of the Pic du Midy, and that of the same at Bourdeaux, the barometer being at 20 inches 3 lines, amounts to 18 degrees on the thermometer of Fahrenheit.

Now the conformity between the observation made by M. Le Monnier, and this repetition of the same observation, can hardly be greater; seeing the heights of the barometers are almost the same; and the 15 degrees of difference, found by M. Le Monnier on De Lisle's thermometer, amount precisely to 18 degrees on the thermometer of Fahrenheit.

A new discovered Sea-Insect, called the Eye-sucker. By Mr. Henry Baker.* N° 472, p. 35.

Mr. B. was lately presented with a couple of small sea-insects, by a gentleman, who said they were found fixed by the snout to the eyes of sprats; that they are often observed sticking there, and may consequently be supposed to suck their nourishment from thence.

Fig. 2, pl. 1, represents the animal as seen by the naked eye, and fig. 3 as magnified by the microscope. The length of this little creature, from end to end, is near 3 inches, of which the head is about one quarter-part. Its body is somewhat thicker than a hog's bristle, and of a pleasant green colour. A gut seems running through it, and terminates at the anus. The head is light-brown, twice the thickness of the body, and of an oblong figure, tapering towards the snout. It has a pair of fine small black eyes, and a couple of holes, at some distance forwards, which probably are its nostrils.

But the most remarkable part of the head is its proboscis or snout; which is nearly half its length, and does not end in a point, but spreads at its extremity with a considerable aperture. This snout appears of a horny substance, and has, on every side, several large knobs or protuberances; by which, when once insinuated into the fish's eye, it must necessarily be fixed there, so as not easily to be removed. But this the figure will more expressively demonstrate.

Observations on the Hardness of Shells, and on the Food of the Soal-fish. By Mr. Peter Collinson. F.R.S. N° 472, p. 37.

Mr. C. observed at the ruins of the abbey of St. Edmund's-bury, which is built of a kind of stone composed of grit or sand, an infinite number of very minute shells, which appeared to be a species of smooth shining cockle; in several parts of the building, much exposed to the air, the sand was mouldered away, but these small shells remained entire, and their polish not in the least decayed.

It may be alleged, that a petrifying juice, the same that had united the particles of the sand together, had likewise hardened these, and rendered them more durable than nature had formed them. But, as we have many instances of shells retaining their natural polish and firmness, where no such allegation can justly be made (for instance, the shells found in chalk-pits, loams, and several other places, where no such juices are hitherto proved to exist); yet of so tender shells, long exposed to the weather, and still remaining uncorrupted, the above instance is the most singular, be the cause of their duration what it may.

* This animal has sometimes been supposed to be a mutilated specimen of the *pennatula filosa.* Linn.

In September last, having bought some soal-fish before they were skinned, he observed their bellies were prominent and hard, as if they were full of large rows, but, instead of that, their guts were filled with shell-fish, a species of pectunculæ. Before these shells were taken out of the transparent guts of the fish, the whole had very much the appearance of strings of beads, or necklaces; the interstices between the shells occasioning this resemblance. On taking the shells out, some of them were almost entirely dissolved, others partly so, but many were whole and entire.

It is well known, that shell-fish are the food of several species of fish. The sea-porcupine, and a kind of ray, subsist chiefly on them; but these are wonderfully provided with an apparatus for reducing them into a state more fit for digestion; their upper and under jaws are hard enough to break or grind almost the strongest shells to the condition of pulp.

But the soal-fish has nothing of this kind: it feeds on shell-fish, but digests them not by attrition; for neither its mouth, nor any of the viscera, are framed for this purpose; but as it would appear from the preceding account, by a proper menstruum, which is prepared in the body of this animal.

Thus we see, that shells which can resist the teeth of time, the inclemency of seasons, and lie, without apparent decay, for unknown ages, in the bowels of the earth, are reduced, in all probability, in a little while, almost into a state of fluidity, by the juices of a small tender animal.

Is it the juices of the whole animal, or is it the consequence of such a kind of nourishment, that renders this fish so delicious?

An Account of the Disorder of which Father Jos. Bolognini, Abbot of SS Boniface, &c. &c. died. Extracted by C. M. Secretary to the Royal Society, from a Letter received from John Francis de Camillis, M. D. &c. N° 472, p. 40. *From the Latin.*

Father Bolognini, who was 50 years old, of a corpulent and sanguineous habit, and addicted to free living, had frequently been attacked with an intermittent fever during the autumnal season. These attacks were removed by the use of the Peruvian bark. He was afterwards troubled with the heart-burn, which he was wont to relieve by taking morning and evening after his chocolate, a draught of water, sometimes cold, sometimes warm, according to the season of the year. Every spring he was accustomed to take some opening medicines, and to be blooded.

In the spring of 1742 a lowness of spirits came upon him, with want of sleep, &c. And in July following he had a red miliary rash, and petichiæ accompanied with itching, (miliari rubra, et prurienti purpura aspersus est) but without fever; for the cure of which he drank freely of cooling emulsions and lemonade. On the 26th of July when the rash was still upon him, after heating himself with walking, he became suddenly chilled. In the evening of that day he was seized with a shivering, followed by vomiting and some degree of feverish heat. The next day he was rather better; but on the 3d day the fever returned with vomiting and head-ach. The rash had disappeared, and a vomiting of a chocolate coloured matter, with bilious evacuations by stool, &c. showed an aggravation of the disorder. The next day the pulse was small and frequent, the patient complained of pain about the left orifice of the stomach, and the vomiting was more urgent. In the morning of the 5th day the stools were discovered to be bloody, and in the course of the day blood was also discharged by vomiting, and in large quantities both ways, black and coagulated, though what had come away by stool in the morning was florid. A consultation was held, at which it was determined to have recourse to v. s. the Peruvian bark, (which had been given before) and certain astringent and cordial medicines. After v. s. the patient became manifestly weaker; delirium and a train of alarming symptoms succeeded, and he died on the 6th day from the first attack, and the 3d day from his taking to his bed. The quantity of blood discharged amounted in the whole to nearly 8 lb.

Eighteen hours after death, the body was opened. The lungs were found to be of a very dark colour, and in a diseased state. The heart appeared flaccid, and the foramen ovale was found open. The convex part of the diaphragm was inflamed, and its concave part, where it is in contact with the stomach, appeared black, and its vessels were turgid.

In the abdomen, the liver appeared nearly of its natural colour in its convex part, but in the concave part, where it is contiguous to the stomach, it was of a dark and black colour. On cutting into its substance, it was found to be internally of a pale colour, and almost destitute of blood. The substance of the spleen was found black and quite corrupted. The kidneys, mesentery, and other parts, were almost destitute of blood. The whole lining of the œsophagus was beset with coagulated blood. The stomach was distended and inflamed. It contained a large quantity of watery fluid, mixed with black fetid blood. The blood-vessels of the stomach were turgid with blood. At the bottom of the stomach, about 4 fingers breadth from the pylorus, they were astonished to find a large round tumour, which weighed nearly 5 oz. In the upper part of this tumour there was a round hole, about ¼ an inch in diameter.* Externally this tumour was of a dark red colour; it was of a sarcomatous substance, and of a cancerous nature. The whole tract of the intestinal canal was externally red, and the small intestines were in a gangrenous state. From the pylorus along the whole tract of the duodenum there was found a great quantity of dark coloured coagu-

* The aforesaid orifice was found to lead into sinuses, which contained some ichor.

lated blood. There was also found a quantity of blood, partly florid, partly dark coloured and coagulated, in a portion of the jejunum, as well as in the ileum, colon, and rectum, in which last the blood was in a very putrid state.

On the Action of Springs. By James Jurin, M. D, F. R. S. N° 472, p. 46.

The theory of springs is not only of use in the more curious parts of mechanics, as the structure of watches, &c. but may give light to many operations of nature, there being few substances but what are endued with some degree of elasticity; and particularly the bodies of animals, and even of vegetables, being known to consist, in a great measure, if not wholly, of organs strongly elastic.

For which reason it is not to be wondered, that this theory has engaged the thoughts of several eminent mathematicians of the last and present age; as Dr. Hook, Mr. John Bernouilli, M. Camus, &c.

But, as all hitherto done on this subject goes no further, than to compare the effects of different springs with each other, in one case only, where they are supposed to be bent to the same degree, and that without showing how the effect of any of them may be reduced to, or compared with, that of any other natural cause, the general proposition following may merit attention, both on account of its simplicity, and of its comprehending all possible cases of a body acting on a spring, or a spring on a body, where no other power intervenes; and also of its reducing the effect to that most known and simple one, the effect of gravity on falling bodies.

1. By a spring is meant a body of any shape perfectly elastic.—2. By the natural situation of a spring, is meant the situation it will rest in, when not disturbed by any external force.—3. By the length of a spring, is meant the greatest length, through which it can be forced inwards. This would be the whole length, were the spring considered as a mathematical line; but in a material spring is the difference between the whole length when the spring is in its natural situation, and the length or space it takes up when wholly compressed or closed.—4. By the strength of a spring, is meant the least force or weight, which, when the spring is wholly compressed or closed, will restrain it from unbending itself.—5. By the space through which a spring is bent, is meant that space or length through which one end of the spring is removed from its natural situation.—6. By the force of a spring bent or partly closed, is meant the least force or weight, which, when the spring is bent through any space less than its whole length, will confine it to the state it is then in, without suffering it to unbend any farther.

Principle.—Ut tensio, sic vis: that is, if a spring be forced or bent inwards, or drawn outwards, or anywise removed from its natural situation, its resistance is proportional to the space by which it is removed from that situation.

Thus, if the spring CL, fig. 4, pl. 1, resting with the end L against any immoveable support, but otherwise lying in its natural situation, and at full liberty, shall, by any force p, be pressed inwards, or from C towards L, through the space of 1 inch, and can be there detained by that force p, the resistance of the spring, and the force p, exactly counterbalancing each other; then the force $2p$ will bend the spring through the space of 2 inches, $3p$ through 3 inches, $4p$ through 4 inches, &c. the space cl, fig. 5, through which the spring is bent, or by which the end c is removed from its natural situation, being always proportional to the force which will bend it so far, and will detain it so bent.

And if one end L be fastened to an immoveable support, fig. 6, and the other end c be drawn outwards to l, and be there detained from returning back by any force p, the space cl, through which it is so drawn outwards, will be always proportional to the force p, which is able to detain it in that situation. The same principle holds in all cases, where the spring is of any form whatever, and is, in any manner whatever, forcibly removed from its natural situation.

Here it may be noticed, that the elastic force of the air is a power of a different nature, and governed by different laws, from that of a spring. For supposing the line LC, fig. 4, to represent a cylindrical volume of air, which, by compression, is reduced to Ll, fig. 5, or, by dilatation, is extended to Ll, fig. 6, its elastic force will be reciprocally as Ll, fig. 5 and 6; whereas the force or resistance of a spring will be directly as cl.

Dr. J. now proceeds to his general proposition, and its corollaries; in which he remarks, that if he happen at any time to express himself with less accuracy, as in making weights, times, velocities, &c. to become promiscuously the subjects of geometrical or arithmetical operations, he desires, once for all, to be understood, not as speaking of those qualities themselves, but of lines, or numbers, proportional to them.

Theorem.—If a spring of the strength P, and the length CL, fig. 7, lying at full liberty on a horizontal plane, rest with one end L against an immoveable support; and a body of the weight M, moving with the velocity V, in the direction of the axis of the spring, strike directly on the other end C, and thereby force the spring inwards, or bend it through any space CB; and a middle proportional CG, be taken between the line $\text{CL} \times \frac{\text{M}}{\text{P}}$, and $2a$, a being the height to which a heavy body would ascend in vacuo with the velocity V; and, on the radius $\text{R} = \text{CG}$, be erected the quadrant of a circle GFA, it will be,

1. When the spring is bent through any right sine of that quadrant, as CB, the velocity v of the body M, is, to the original velocity V, as the co-sine to the radius: that is, $v = \text{V} \times \frac{\text{BF}}{\text{R}}$.

2. The time t of bending the spring through the same sine CB, is to T the

time of a heavy body's ascending in vacuo with the velocity V, as the corresponding arch to $2a$: that is $t = T \times \frac{GF}{2a}$.

Demonstr.—1. While the spring is bending through the space CB, let the space, through which it is at any time bent, be called x, τ the time of bending it through the space x, and υ the velocity of the body at the time τ; and let CL $= L$, CB $= l$. Then, if p be the force, with which the spring, when bent through the space x, resists the motion of the body; by Dr. Hook's principle, $L : x :: P : p = \frac{Px}{L}$.

And since, in the case of forces that act uniformly, the quantities of motion generated are proportional to the generating forces, and the times jointly, if $M\dot{\upsilon}$ be the nascent quantity of motion taken from the body by the resistance $\frac{Px}{L}$ in the nascent time $\dot{\tau}$, $MV : -M\dot{\upsilon} :: MT :: \frac{Px\dot{\tau}}{L}$, or $-\dot{\upsilon} = \frac{VPx\dot{\tau}}{MLT}$.

Also, since, in the same case of forces acting uniformly, the spaces are proportional to the velocities, and the times jointly, $\dot{x} : 2a :: \upsilon\dot{\tau} : VT$, or $\dot{\tau} = \frac{TV\dot{x}}{2a\upsilon}$.

Therefore, $-\dot{\upsilon} = \frac{VPx}{MLT} \times \frac{TV\dot{x}}{2a\upsilon}$, or $2\upsilon\dot{\upsilon} = -\frac{V^2Px\dot{x}}{MLa}$; and the fluents of these two quantities are υ^2 and $-\frac{V^2Px^2}{2MLa}$. But the former of these was V^2, when x, and consequently the latter, was nothing; therefore,

$\upsilon^2 - V^2 = -\frac{V^2Px^2}{2MLa}$, or $\upsilon^2 = V^2 - \frac{V^2Px^2}{2MLa}$.

But, by the construction, $\frac{2MLa}{P} = R^2$; therefore, $\upsilon^2 = V^2 - \frac{V^2x^2}{R^2}$, or $\upsilon^2 = V^2 \times \frac{R^2 - x^2}{R^2}$; and, when x becomes equal to l, and υ to v, $v^2 = V^2 \times \frac{R^2 - l^2}{R^2}$; and, by the property of the circle, $R^2 - l^2$ being equal to

BF^2, $v^2 = V^2 \times \frac{BF^2}{R^2}$, or $v = V \times \frac{BF}{R}$. Q. E. D. 1°.

2. We have above, $\dot{\tau} = \frac{TV\dot{x}}{2a\upsilon}$; and $\upsilon^2 = V^2 \times \frac{R^2 - x^2}{R^2}$; or $\upsilon = V \times \frac{\sqrt{R^2 - x^2}}{R}$: therefore $\dot{\tau} = \frac{TV\dot{x}}{2a} \times \frac{R}{V \times \sqrt{R^2 - x^2}}$, or $\dot{\tau} = \frac{T}{2a} \times \frac{R\dot{x}}{\sqrt{R^2 - x^2}}$.

Now let CD, fig. 8, be equal to x; and draw the co-sine DE, the radius CE, and the perpendicular $ed = \dot{x}$; then will the triangle DEC be similar to the nascent triangle deE; and consequently DE $: de ::$ CB $: e$E $=$ $\frac{CE \times de}{DE} = \frac{R\dot{x}}{\sqrt{R^2 - x^2}}$. Therefore, $\dot{\tau} = \frac{T}{2a} \times e$E, and $\tau = T \times \frac{GE}{2a}$. And when a becomes equal to CB, and τ to t, the arch GE becomes equal to the arch GF: therefore $t = T \times \frac{GF}{2a}$. Q. E. D. 2°.

Under this theorem are comprehended the 3 following cases:

In case 1, the spring is bent through its whole length, or is entirely compressed and closed, before the moving force of the body is consumed, and its

3

motion ceases. In case 2, the moving force of the body is consumed, and its motion ceases before the spring is bent through its whole length, or wholly closed. In case 3, the moving force of the body is consumed, and its motion ceases at the instant that the spring is bent through its whole length, and is entirely closed.

For this reason, and in order to make the following corollaries of more ready use, Dr. J. distributes them into 3 classes; the first of which are as general as the theorem itself, extending to all the 3 cases, but are more particularly useful in case 1. The 2d class of corollaries extends to both the 2d and 3d case; but are more particularly useful in case 2. The 3d class extends only to case 3, and by that means are much more simple than either of the former.

CLASS 1.—*General corollaries, but of more particular use in case* 1; *wherein the spring is wholly closed, before the motion of the body ceases.*

Corol. 1.—When the spring is bent through any right sine CB, fig. 7, the loss of velocity is to the original velocity, as the versed sine to the radius, or $\text{V} - v = \text{V} \times \frac{\text{G}g}{\text{R}}$. For, since $v = \text{V} \times \frac{\text{BF}}{\text{R}}$, $\text{V} - v = \text{V} - \text{V} \times \frac{\text{BF}}{\text{R}} = \text{V} \times \frac{\text{R} - \text{BF}}{\text{R}} = \text{V} \times \frac{\text{G}g}{\text{R}}$.

Corol. 2.—When the spring is bent through any right sine CB, the diminution of the square of the velocity is to the square of the original velocity, as the square of that right sine to the square of the radius, or $\text{V}^2 - v^2 = \text{V}^2 \times \frac{\text{CB}^2}{\text{R}^2}$. For, since $v = \text{V} \times \frac{\text{BF}}{\text{R}}$, $v^2 = \text{V}^2 \times \frac{\text{BF}^2}{\text{R}^2}$, and $\text{V}^2 - v^2 = \text{V}^2 - \text{V}^2 \times \frac{\text{BF}^2}{\text{R}^2} = \text{V}^2 \times \frac{\text{R}^2 - \text{BF}^2}{\text{R}^2} = \text{V}^2 \times \frac{\text{CB}^2}{\text{R}^2}$.

Corol. 3.—When the spring is bent through any space l, v the velocity of the body is equal to $\text{V} \times \sqrt{\frac{2\text{ML}a - \text{P}l^2}{2\text{ML}a}}$, or to $\text{V} \times \sqrt{\frac{2\text{M}a - pl}{2\text{M}a}}$; and is proportional to $\sqrt{\frac{2\text{ML}a - \text{P}l^2}{\text{ML}}}$, or to $\sqrt{\frac{2\text{M}a - pl}{\text{M}}}$.

For, since $v^2 = \text{V}^2 \times \frac{\text{BF}^2}{\text{R}^2} = \text{V}^2 \times \frac{\text{R}^2 - l^2}{\text{R}^2}$; if for R^2 we substitute its value $\frac{2\text{ML}a}{\text{P}}$, we have $v^2 = \text{V}^2 \times \frac{2\text{ML}a - \text{P}l^2}{2\text{ML}a}$, or $v = \text{V} \times \sqrt{\frac{2\text{ML}a - \text{P}l^2}{2\text{ML}a}}$: and as, by Dr. Hook's principle, $\text{L} : l :: \text{P} : p$, or $\text{P}l = p\text{L}$, $v = \text{V} \times \sqrt{\frac{2\text{ML}a - p\text{L}l}{2\text{ML}a}}$, or $v = \text{V} \times \sqrt{\frac{2\text{M}a - pl}{2\text{M}a}}$. But $\frac{\text{V}}{\sqrt{a}}$, by Galileo's doctrine, is a constant quantity; and therefore v is proportional to $\sqrt{\frac{2\text{ML}a - \text{P}l^2}{\text{ML}}}$, or to $\sqrt{\frac{2\text{M}a - pl}{\text{M}}}$.

Corol. 4.—The time t of bending the spring through any space l, is proportional to the arch GF divided by $\sqrt{a}$; l being the right sine of the arch, and $\text{R} = \sqrt{\frac{2\text{ML}a}{\text{P}}}$, being the radius. For, by the theorem, $t = \text{T} \times \frac{\text{GA}}{2a}$; and $\frac{\text{T}}{\sqrt{a}}$ is a constant quantity.

Corol. 5.—The diminution of the product of the weight of the body into the square of the velocity, or, to use the expression of some late writers, the diminution of the vis viva, that is, $MV^2 - Mv^2$, by bending a spring through any space l, is always equal to $\frac{c^2Pl^2}{2LA}$, or to $\frac{c^2pl}{2A}$; where A is the height from which a heavy body will fall in vacuo in a second of time, and c is the celerity gained by that fall. For, by corol. 2, $V^2 - v^2 = V^2 \times \frac{CB^2}{R^2} = \frac{V^2l^2}{R^2}$; and R^2, by the construction, being equal to $\frac{2MLa}{P}$, $V^2 - v^2 = V^2 l^2 \times \frac{P}{2MLa}$. But, by Galileo's theory, $\frac{V^2}{a} = \frac{c^2}{A}$; therefore $V^2 - v^2 = \frac{c^2Pl^2}{2MLA}$, and $MV^2 - Mv^2 = \frac{c^2Pl^2}{2LA} = \frac{c^2pl}{2A}$.

Corol. 6.—The diminution of the vis viva, by bending a spring through any space l, is always proportional to $\frac{Pl^2}{L}$, or to pl: and if either the spring be given, or $\frac{P}{L}$ be given in different springs, the loss of the vis viva will be as l^2, or as p^2. For, by corol. 5, $MV^2 - Mv^2 = \frac{c^2Pl^2}{2LA} = \frac{c^2pl}{2A}$; and $\frac{c^2}{A}$ being a constant quantity, $MV^2 - Mv^2$ is as $\frac{Pl^2}{L} = pl$: and if $\frac{P}{L}$ be given, $MV^2 - Mv^2$ will be as l^2; or as $l^2 \times \frac{P^2}{L^2}$; or as $l^2 \times \frac{p^2}{l^2}$; or as p^2.

Corol. 7.—The loss of the vis viva, by bending a spring through its whole length, or by wholly closing it, is equal to $\frac{c^2PL}{2A}$, and is proportional to PL: and if PL be given, the loss of the vis viva is always the same. This is evident from corol. 5 and 6, for l is now equal to L.

CLASS 2.—*Corollaries of more particular use in Case 2; wherein the motion of the body ceases before the spring is wholly closed.*

Corol. 8.—If the motion of the body cease when the spring is bent through any space l, the initial velocity V is equal to $cl\sqrt{\frac{P}{2MLA}}$, or to $c\sqrt{\frac{pl}{2MA}}$. For, by corol. 5, $V^2 - v^2 = \frac{c^2Pl^2}{2MLA} = \frac{c^2pl}{2MA}$. And here, the motion of the body ceasing, $v^2 = 0$. Therefore $V^2 = \frac{c^2Pl^2}{2MLA} = \frac{c^2pl}{2MA}$; or $V = cl\sqrt{\frac{P}{2MLA}} = c\sqrt{\frac{pl}{2MA}}$.

Corol. 10.—If the motion of the body cease when the spring is bent through any space l, the time t, of bending it, is equal to 1″ of time, multiplied by $\frac{m}{2}\sqrt{\frac{ML}{2PA}}$, or to $1'' \times \frac{m}{2}\sqrt{\frac{Ml}{2pA}}$, where m is to 1, as the circumference of a circle to the diameter. For, by the theorem,

$t = T \times \frac{GF}{2a}$; and, by Galileo's theory, $\frac{T}{\sqrt{a}} = \frac{1''}{\sqrt{A}}$. Therefore $t = \frac{1''}{\sqrt{a}} \times \frac{GF}{2\sqrt{a}}$.

Also, by the theorem $v^2 = V^2 \times \frac{R^2 - l^2}{R^2}$; and therefore v^2 being now equal to 0, $R^2 = l^2$, and, fig. 9, l becomes the radius of the circle; and l being also equal to the right sine of the arch GF, that arch becomes a quadrant, and is equal to $\frac{2l \times m}{4}$. Therefore $t = \frac{1''}{\sqrt{A}} \times \frac{2lm}{4 \times 2\sqrt{a}}$, or $t = 1'' \times \frac{lm}{4\sqrt{A} \times \sqrt{a}}$.

But l being equal to $R = \sqrt{\frac{2MLa}{P}}$, $\frac{l}{\sqrt{a}} = \sqrt{\frac{2ML}{P}}$; therefore $t = 1'' \times \frac{m}{4\sqrt{A}} \times \sqrt{\frac{2ML}{P}}$; or $t = 1'' \times \frac{m}{2}\sqrt{\frac{ML}{2PA}} = 1'' \times \frac{m}{2}\sqrt{\frac{Ml}{2pA}}$.

Corol. 11. In the same case, the time of bending the spring is proportional to $\sqrt{\frac{ML}{P}}$, or to $\sqrt{\frac{Ml}{p}}$; and if $\frac{L}{P}$ be given, t will be as $\sqrt{M}$; and if both $\frac{L}{P}$ and M be given, t will always be the same, whatever be the original velocity, or through whatever space the spring be bent.

Corol. 12. If the motion of the body cease when the spring is bent through any space l, the product of the initial velocity, and the time of bending the spring, or Vt, is equal to $1'' \times \frac{mcl}{4A}$; and is proportional to l, the space through which the spring is bent.

For, by corol. 8, $V = cl\sqrt{\frac{P}{2MLA}}$, and, by corol. 9, $t = 1'' \times \frac{m}{2}\sqrt{\frac{ML}{2PA}}$; therefore $Vt = 1'' \times \frac{mcl}{4A}\sqrt{\frac{MLP}{MLP}} = 1'' \times \frac{mcl}{4A}$; and, as $1''$, m, G, and A, are given quantities, Vt is as l.

Hence, any two of the three quantities, V, t, and l, being given, the other is readily determined.

Corol. 13. In the same case, the initial quantity of motion, or MV, is equal to $cl\sqrt{\frac{PM}{2LA}}$, or to $c\sqrt{\frac{plM}{2A}}$.

For, by corol. 8, $V = cl\sqrt{\frac{P}{2MLA}} = c\sqrt{\frac{pl}{2MA}}$; therefore $MV = cl\sqrt{\frac{PM}{2LA}} = c\sqrt{\frac{plM}{2A}}$.

Corol. 14. In the same case, MV is proportional to $l\sqrt{\frac{PM}{L}}$, or to $\sqrt{plM}$, or to $\frac{Plt}{L}$, or to pt: and if $\frac{P}{L}$ be given, MV is as $l\sqrt{M}$, or as lt.

For, in the preceding corol. $\frac{c}{\sqrt{A}}$ is a given quantity; and by corol. 11, t is as $\sqrt{\frac{ML}{P}} = \sqrt{\frac{Ml}{p}}$.

Corol. 15. If the quantity of motion MV bend a spring of the strength P, and length L, through the space l, and be wholly consumed thereby, no different quantity of motion equal to the former, as $nM \times \frac{V}{n}$, will bend the same spring through the same space, and be wholly consumed thereby.

For, by the preceding corol. if the spring be bent through the space l, and each of these quantities of motion be consumed thereby; $l\sqrt{M} : l\sqrt{nM} :: MV : nM \times \frac{V}{n}$. But $MV = nM \times \frac{V}{n}$; and therefore $l\sqrt{M} = l\sqrt{nM}$, or $1 = n$, and $M = nM$, and $V = \frac{V}{n}$. Therefore the quantity of motion $nM \times \frac{V}{n}$ is not equal to MV, but is composed of an equal mass, and an equal velocity.

Corol. 16. But a quantity of motion less than MV, in any given ratio, may bend the same spring through the same space l, and the time of bending it will be less in the same given ratio.

For, let 1 to n be the given ratio; and let the less quantity of motion be $\frac{M}{nn} \times nV$; which is to MV, as 1 to n. Then by corol. 14, the spring being given, $l\sqrt{M} : l\sqrt{\frac{M}{nn}} :: MV : \frac{M}{nn} \times nV = \frac{MV}{\sqrt{M}} \times l\sqrt{\frac{M}{nn}} = \frac{MV}{n}$. Therefore the quantity of motion $\frac{M}{nn} \times nV$, being equal to $\frac{MV}{n}$, will bend the spring through the same space l.

Also, by the same corollary, MV is as lt; and l being given, the quantity of motion is as t: therefore the time of bending the spring will be less in the same ratio, as the quantity of motion is less.

Corol. 17. A quantity of motion greater than MV, in any ratio given, may be consumed in bending the spring through the same space; and the time of bending it will be greater in the same given ratio.

This appears after the same manner as the preceding, by making n a fractional number, instead of a whole one.

Corol. 18. If the motion of the body cease, when the spring is bent through any space l, the initial vis viva, or MV^2, is equal to $\frac{c^2pl^2}{2LA}$, or to $\frac{c^2Pl}{2A}$: and $2aM = \frac{Pl^2}{L} = pl$.

For, by corol. 8, $V = cl\sqrt{\frac{P}{2MLA}} = c\sqrt{\frac{pl}{2MA}}$, or $V^2 = \frac{c^2l^2P}{2MLA} = \frac{c^2pl}{2MA}$: Therefore $MV^2 = \frac{c^2Pl^2}{2LA} = \frac{c^2pl}{2A} = \frac{V^2Pl^2}{2La} = \frac{V^2pl}{2a}$.

Corol. 19. In the same case, the initial vis viva is proportional to $\frac{Pl^2}{L} = pl$; and if $\frac{P}{L}$ be given, the vis viva is as l^2, or as p^2.

For in the preceding corol. $\frac{c^2}{A}$ being a given quantity, the vis viva is as $\frac{Pl^2}{L} = pl$; and if $\frac{P}{L}$ be given, it will be as l^2, or as p^2; as p and l increase in the same proportion.

Corol. 20. If the vis viva, MV^2, bend a spring through the space l, and be totally consumed thereby; any other vis viva, equal to the former, as $nnM \times \frac{V^2}{nn}$, will bend the same spring through the same space, and be totally consumed thereby.

For, the spring being the same, $\frac{P}{L}$ is given; and therefore by corol. 19, the vis viva which will be consumed in bending the spring through the space l, is as l^2.

Corol. 21. But the time in which the same spring will be bent through the same space, by the vis viva $nnM \times \frac{V^2}{nn}$, will be to the time in which it is so bent by the vis viva $M \times V^2$, as n to 1; n being any whole or fractional number.

For, by corol. 11, since $\frac{L}{P}$ is given, the time is as VM.

CLASS 3.—*Corollaries in Case 3: wherein the motion of the Body ceases, at the Instant that the Spring is wholly closed.*

Corol. 22. If the motion of the body cease, when the spring is bent through its whole length, or is wholly closed, the initial velocity V is equal to $C\sqrt{\frac{PL}{2MA}}$.

For, by corol. 8, $V = C\sqrt{\frac{pl}{2MA}}$; and l being now equal to L, (fig. 10) p becomes equal to P; and therefore $V = C\sqrt{\frac{PL}{2MA}}$.

Corol. 23. In the same case, the initial velocity V is proportional to $\sqrt{\frac{PL}{M}}$. For $\frac{C}{\sqrt{A}}$, in the preceding corollary, is a given quantity.

Corol. 24. In the same case, if PL be given, either in the same, or in different springs, the initial velocity V is reciprocally as $\sqrt{M}$. This is plain from the preceding corollary.

Corol. 25. If the motion of the body cease, when the spring is wholly closed, the product of the initial velocity, and the time spent in closing the spring, or Vt, is equal to $1'' \times \frac{mCL}{4A}$; and is proportional to L, the length of the spring.

For, by corol. 22, $V = C\sqrt{\frac{PL}{2MA}}$; and, by corol. 10, $t = 1'' \times \frac{m}{2}\sqrt{\frac{ML}{2PA}}$: therefore $Vt = 1'' \times \frac{mCL}{4A}$; and $1''$, m, and $\frac{C}{A}$, being given quantities, Vt is as L.

Corol. 26. In the same case, the initial quantity of motion, or MV, is equal to $C\sqrt{\frac{PLM}{2A}}$. For, by corol. 23, $V = C\sqrt{\frac{PL}{2MA}}$.

Corol. 27. In the same case, MV is proportional to $\sqrt{PLM}$, or to Pt: and if PL be given, either in the same, or different springs, MV is as $\sqrt{M}$.

This appears partly from the preceding corollary, where $\frac{C}{\sqrt{A}}$ is a given quantity; and consequently MV is as $\sqrt{PLM}$; and PL being given, MV is as $\sqrt{M}$: and partly from corol. 11; where t is as $\sqrt{\frac{ML}{P}}$, and consequently Pt is as $\sqrt{PLM}$.

Corol. 28. In the same case, if $\frac{P}{L}$ be given, either in the same, or in different springs, the initial quantity of motion is as the length of the spring into the time of bending it. For, by corol. 27, MV is as Pt; and if P be as L, MV is as Lt.

Corol. 29. If the quantity of motion MV bend a spring through its whole length, and be consumed thereby, no other quantity of motion equal to the former, as $nM \times \frac{V}{n}$, will close the same spring, and be wholly consumed thereby.

This is proved in the same manner as corol. 15, putting only L for *l*.

Corol. 30. But a quantity of motion less or greater than MV, in any given ratio, may close the same spring, and be wholly consumed in closing it: and the time spent in closing the spring will be respectively less or greater, in the same given ratio. This is easily proved from corol. 16.

Corol. 31. If the motion of the body cease, when the spring is wholly closed, the initial vis viva, or MV^2, is equal to $\frac{c^2PL}{2A}$: and $2aM = PL$.

For, by corol. 22, $V = c\sqrt{\frac{PL}{2MA}}$, or $V^2 = \frac{c^2PL}{2MA}$, or $MV^2 = \frac{c^2PL}{2A} = \frac{V^2PL}{2a}$.

Corol. 32. In the same case, the initial vis viva is as the rectangle under the strength and length of the spring.

For, by the preceding corol. $MV^2 = \frac{c^2PL}{2A}$, and $\frac{c^2}{A}$ is a given quantity; therefore MV^2 is as PL.

Corol. 33. In the same case, if $\frac{P}{L}$ be given, the initial vis viva is as P^2, or as L^2. This is evident from the preceding corollary.

Corol. 34. If the vis viva MV^2 bend a spring through its whole length, and be consumed in closing it, any other vis viva equal to the former, as $nnM \times \frac{V^2}{nn}$ will close the same spring, and be consumed thereby. This is evident from corol. 32.

Corol. 35. But the time of closing the spring by the vis viva $nnM \times \frac{V^2}{nn}$, will be to the time of closing it by the vis viva MV^2, as *n* to 1.

For, by corol. 11, since the spring is given, the time is as $\sqrt{M}$.

Corol. 36. If the vis viva MV^2 be wholly consumed in closing a spring of the strength P, and length L; the vis viva $nnMV^2$ will be sufficient to close,

1. Either a spring of the strength *nn*P, and length L.
2. Or a spring of the strength *n*P, and length *n*L.
3. Or of the strength P, and length *nn*L.
4. Or if *n* be a whole number, the number *nn* of springs, each of the strength P, and length L, one after another.

For $MV^2 : nnMV^2 :: PL : nnPL$; and therefore, by corol. 32, the vis viva $nnMV^2$ will close any spring that has *nn*PL for the product of its strength and length. But *nn*PL is composed either of $nnP \times L$, or of $nP \times nL$, or of $P \times nnL$.

Also the loss of the vis viva, in bending a given spring, being always the same, by corol. 7; and the vis viva MV^2 being wholly lost in bending a single spring PL; the loss of the vis viva, $nnMV^2$, in closing one such spring, will be MV^2; and its loss in closing a second such spring, will again be MV^2, and so on: consequently the number *nn* of such springs will be closed one after another, by the time that the vis viva $nnMV^2$ is wholly consumed.

Scholium.—If the spring, instead of being at first wholly unbent, as we have hitherto considered it, be now supposed to have been already bent through some space CB, before the body strikes it; and the velocity of the body be required after the spring is bent through any further space, BD, fig. 11, this case, as well as the three others above mentioned, will be found to come under our theorem.

For if v be the velocity with which the body is supposed to strike against the bent spring at B; it is evident that this may be considered, either as the original velocity, or as the remainder of a greater velocity V, with which the body might have struck on the spring at C, and which, on bending the spring from C to B, would now be reduced to v. For in either case the effect in bending the spring from B to D, will be exactly the same.

In order therefore to determine this imaginary velocity V, let a middle proportional, BF, be taken between $CL \times \frac{M}{P}$, and 2α, α being the height to which a body will ascend in vacuo with the velocity v; draw BF perpendicular to CB, and with the radius CF describe the quadrant CGFEA. Then will our present case be exactly reduced to that of the theorem; CB and CD representing the spaces through which the spring is bent; BF and DE the velocities in the points B and D; GF and GE the times of bending the spring through the spaces CB, CD; and CG representing the imaginary velocity V, with which the body might have struck the spring at C.

For, by the theorem, $BF^2 : CG^2 :: v^2 : V^2$; and $v^2 : V^2 :: \alpha : a$. Therefore $CG^2 = BF^2 \times \frac{a}{\alpha}$. But $BF^2 = 2\alpha \times \frac{LM}{P}$, by the construction; and consequently $CG^2 = \frac{2\alpha LM}{P} \times \frac{a}{\alpha} = \frac{2aLM}{P}$, as in the construction of the theorem.

From this case we shall draw a few corollaries, as well for their usefulness on other occasions, as to show how the theory of springs may be safely applied to the action of gravity on ascending or falling bodies.

Corol. 37. If the body M, with the velocity v, sufficient to carry it to the height α, strike at B, on a string already bent through the space $CB = l$; and do thereby bend it through some further space $BD = s$; at the end of which space, or at D, the body has a velocity sufficient to carry it to some height, as ϵ; then $\epsilon = \frac{2\alpha ML - Ps \times \overline{2l + s}}{2ML}$.

For, by the theorem,

$\alpha : \epsilon :: BF^2 : DE^2$, or $DE^2 = BF^2 \times \frac{\epsilon}{\alpha} = \frac{2\alpha ML}{P} \times \frac{\epsilon}{\alpha}$, or $DE^2 = \frac{2\epsilon ML}{P}$.

Also, $DE^2 + CD^2 = CE^2 = CF^2 = BF^2 + CB^2$, that is, $\frac{2\epsilon ML}{P} + l^2 + 2ls + s^2 = \frac{2\alpha ML}{P} + l^2$; or $\frac{2\epsilon ML}{P} = \frac{2\alpha ML}{P} - 2ls - s^2$; or $2\epsilon ML = 2\alpha ML - Ps \times \overline{2l + s}$.

Corol. 38. If the motion of the body cease on bending the spring through the space BD $= s$, that is, if $\varepsilon = 0$; then the height to which the body might ascend in vacuo, with the velocity v, or $\alpha = \frac{PS \times \overline{2l + s}}{2ML}$.

For, by the last, when $\varepsilon = 0$, $2\alpha ML = PS \times \overline{2l + s}$.

Corol. 39. If p, the force of the spring when bent through the space CB, be equal to M the weight of the body; the height to which the body would ascend in vacuo with the velocity v, is to the space through which it will bend the spring, by striking it at B with that same velocity, as $2l + s$ to $2l$, or $\alpha : s :: 2l + s : 2l$.

For, by the last, $\alpha = \frac{Ps \times \overline{2l + s}}{2ML}$; and $\frac{P}{L}$ being equal to $\frac{p}{l}$, $\alpha = \frac{ps \times \overline{2l + s}}{2Ml}$; and if $p = M$, $\alpha = s \times \frac{2l + s}{2l}$.

Corol. 40. If $p = M$, and p also continue constantly the same, while the spring is bending from B to D (both which suppositions are necessarily made in reducing the action of a spring to that of gravity on an ascending body), the spring must be of an infinite length; and l, the space through which it was bent before the body struck it, must also be of an infinite length; and the space BD, through which the spring will be further bent, must be equal to the height the body can ascend to with the velocity v, or $\alpha = s$.

For, by the last, when $p = M$, $\alpha : s :: 2l + s : 2l$; and the resistances of the spring at D and B being respectively as CD and CB, that is, as $l + s$ and l; since those resistances are now supposed equal to one another, we must, on that supposition, consider $l \mp s$ as equal to l; and adding l to each, $2l \mp s = 2l$, that is, l must be infinitely greater than s; and then $\alpha : s :: 2l : 2l$, or $\alpha = s$.

In this proposition, and all its corollaries, except the last 4, we have considered the spring as being at first wholly unbent, and then acted on by a body moving with the velocity v, which bends it through some certain space: but as we suppose the spring to be perfectly elastic, the proposition and corollaries will equally hold, if the spring be supposed to have been at first bent through that same space, and, by unbending itself, to press on a body at rest, and thereby to drive that body before it during the time of its expansion: only, v, instead of being the initial velocity with which the body struck the spring, will now be the final velocity with which the body parts from the spring, when wholly expanded.

If the spring, instead of being pressed inwards, be drawn outwards by the action of the body, we need only make L the greatest length to which the spring can be drawn out beyond its natural situation, without prejudice to its elasticity, l any less length to which the spring is drawn outwards; P and p the forces which will keep it from restoring itself, when drawn out to those lengths respectively, and the proposition will equally hold good: as it will also, if the spring be supposed to have been already drawn outwards to the length l, and, in restoring itself, to draw the body after it: only, in this latter case, v, the initial velocity in the proposition, will now be the final velocity, as above.

Our proposition equally holds good, when the spring is of any form whatever, provided L be always understood to be the greatest length it can be bent or drawn to, from its natural situation, l any less length, and P, p, the forces which will confine it to these lengths. For Dr. Hook's principle extends to springs of any form.

Dr. J. has drawn so great a number of corollaries from this proposition, because, in the controversy about the force of bodies in motion, both parties support their opinion by arguments taken from the theory of springs.

Of a Steatomatous Tumour of the Ovarium, in which Hair was found; by Albert Haller, Professor of Anatomy and Botany at Gottingen, &c. From the Latin. N° 472, p. 71.

The subject of this communication was a maid servant, who died after a very lingering illness. On opening the body, the first thing which struck professor H. was a large tumour in the pelvis, which he suspected to be occasioned by pregnancy; but on examination, it was discovered to be a morbid enlargement of the right ovarium. Hydatids were found adhering to this tumour; and on cutting into it, it was found to consist of a hollow sac or cyst, divided by a septum into 2 cells. The smaller cell contained a matter resembling honey, and a substance resembling fat, together with a great quantity of brown, curled hair, like human hair: these hairs were nearly 2 inches long. Some of this honey-like matter, the meliceris of the ancients, being put into the fire, it burnt like fat. The larger cell contained a milky, purulent fluid, in which were suspended some brown, friable lumps, resembling portions of lung.

Professor H. supposed this double tumour to have been formed by the morbid enlargement of 2 ova or vesiculæ, which went on increasing until they occupied the whole cavity of the ovarium. He thought it not so easy to account for the presence of fat and hair. From this case he was inclined to doubt whether the cuticle be essential (as is commonly supposed) to the formation of the cortical part of the hair. He did not think the hair had belonged to a fœtus dissolved by putrefaction, as no teeth or other solid body was found with it.*

* Other instances of hair contained in the ovarium have been recorded by Dr. Tyson (Phil. Collections, N° 2, or vol. ii. p. 490 of these Abridgments), by Mr. Yonge (Phil. Trans. vol. xxv. N° 309, or vol. v. p. 347 of these Abridgments), and by Dr. Baillie. (Phil. Trans. vol. lxxix.) See also Trans. of the Royal Irish Academy, vol. i.

A Catalogue of the fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1741, *pursuant to the Direction of Sir Hans Sloane, Bart., &c. By Joseph Miller.* N° 472, p. 75.

This is the 20th publication of this kind, completing the number of 1000 different plants.

An Easy Method of procuring the true Impression or Figure of Medals, Coins, &c. By Henry Baker, F.R.S. N° 472, p. 77.

The first step is, to take a perfect and sharp impression, in black sealing-wax, of the coin or medal you desire the picture or figure of: when this is done, the chief trouble is over, and the rest of the operation may be executed at leisure.

Cut the wax away round the edges of the impression, with the point of a penknife, or a pair of sharp scissars; and, having ready a preparation in gum-water of the colour you would have the picture, spread your paint on the wax impression with a small hair-pencil, observing to work it into all the sinking or hollow places, those being the rising or projecting parts of the medal, and what only are necessary to be laid over with the colouring; for it must be entirely taken away from every other part before we can proceed.

The way of getting off the paint from the places where it should not be, is to moisten your fore-finger a little, but not too much, with spittle or water, and pass it gently, but nimbly, over the surface of the wax impression; wiping it each time on a cloth or handkerchief, till you perceive all the rising parts of it perfectly fair and clean, and the letters and sinking parts of it only coloured.

This done, take a piece of very thin post paper, a little larger than the medal; wet it in your mouth, or with water, till it be moistened quite through, but let not any water hang to it: place it on the wax impression, laying on the back of the paper 3 or 4 pieces of thick woollen cloth, or flannel, about the size of it.

You must have a couple of flat smooth iron plates, about 2 inches square, and $\frac{1}{10}$ of an inch in thickness. The wax impression must be placed, with its face upwards, on the middle of one of these plates, before you spread the paper and flannels on it, and the other plate must immediately be laid over them: then holding all tight together, put them carefully and evenly into a little press, made of two iron planks about $5\frac{1}{2}$ inches long, $1\frac{1}{4}$ inch wide, and half an inch in thickness, having a couple of long male screws that run through them, with a turning female screw on each, to force the planks together: and these female screws must have strong shoulders, to work them by.

Things being thus adjusted, hold the press in your left hand, and, with a little hammer, strike first on the shoulders of one screw, and then on the shoulders of the other, to bring the planks together parallel, and render the pressure every where alike; unless you find it requisite to give more force to one side than the other, which these two screws will put in your own power.

The press opens again, by a stroke or two of the hammer, the contrary way, on the shoulders of the screws: and then you will find a true and fair picture neatly printed off; which, if any deficiencies appear, you may easily repair, when dry, with a hair-pencil, and a pen, and a little of the same colour.

If your paper does not soak in the moisture well, by being over-sized, it is necessary to wet the flannels, or the paper will not come off strongly enough coloured: and if the relievo of your medal be very high, it is best to put a little cotton immediately on the back of the paper, between that and the flannels, that the paper may be duly pressed into the deep hollows of the wax mould.

This method is very easy and ready for taking the picture of a medal in any colour: but if you desire a relievo only, without any colour, the way is much shorter; for nothing then is necessary, but to place a piece of card, or white pasteboard, well soaked in water, on the wax mould, without any colouring, and letting it remain in the press a few minutes, the business is done at once.

Observations on the Manna Persicum. By John Fothergill, M.D. London. N° 472, p. 86.

In this paper Dr. F. endeavours to show that the ancient Greek physicians were unacquainted with manna, as an article of the materia medica; and that if the Arabians were not the first who introduced that substance into medicine, they at least rendered the use of it more common and extensive. He then states that the Arabians had 3 sorts of the drug, known by the names of manna, teren-jabin, and ciracost; that the manna Persicum is gathered from a prickly shrub called alhagi, and is the teren-jabin of the modern Arabs. This manna is much inferior to the Calabrian manna, which exudes from a species of fraxinus. This last is the manna officinarum, with the description of which this paper concludes.

*Cyanus Foliis radicalibus partim integris, partim pinnatis, Bractea Calycis ovali, Flore sulphureo; per Albert. Haller, Prof. Anat. et Bot. Gottingens. R.S. Ang. et Suec. S. descriptus.** N° 472, p. 94.

Concerning the Propagation and Culture of Mushrooms. By the Rev. Roger Pickering, V.D.M. of Deptford. N° 472, p. 96.

The rains having thrown up on his mushroom beds a great quantity of those plants, Mr. P. adds the following additional observations to those printed in these Transactions, N° 471.

* This plant is the *Centaurea orientalis* of Linneus. *Centaurea calycibus scariosis ciliatis, foliis pinnatifidis, pinnis lanceolatis.*

After having repeated the experiments, then made, on plants and seeds of this year, he finds no reason to alter any thing there mentioned, either as to the lamellæ or chives on the concave side of the umbella, being the siliquæ or seed-vessels; or the seeds falling from thence to a lodgement wisely prepared for it on the middle of the caulis, and from thence easily sliding to the earth contiguous to the mother-plant; or as to its propagation by fibrous runners, or stolones, like potatoes; all which, these following new observations sufficiently confirm.

1. On examination of several lamellæ, he not only distinctly observed seeds, of size and colour proportionable to the maturity of the plant, lodged in them, but also a siliquaceous aperture, with a row of seeds ready to fall through it; which is a very evident proof, that each distinct chive is a siliqua or seed-vessel.

2. On observation of the filament situated on the middle of the caulis, on which, he before observed, he at first discovered the seed, he found both its contexture and situation evidently demonstrating the end for which the wise Creator placed it there; viz. to intercept the seeds in their fall to the ground; by which the power which the wind would otherwise have on such minute bodies is lessened, and the seed, with little or no dissipation, securely directed near the stem of its mother-plant. For this filament is indented and pappous, to catch and lodge the seed as it falls from the siliqua; and is at first rigid, and standing horizontal to the umbella or head, and at right angles with the caulis; by which few or no seeds can fall without being intercepted: but as the plant comes nearer to its decay, this filament relents, falls down close to the sides of the caulis; and its several indentures then making parallel lines with the fibres of the stalk, the seeds are conveyed through them, as through little ducts or channels, to the ground.

It is further to be observed, that this filament is not of so succulent a contexture as the siliqua or seed-vessel; so that the seeds, which would otherwise rot in the siliqua, are here retained in full health, till the period of their falling to the ground.

3. On examination of the caulis in several sections, he found the mushroom a plant more perfect than has been thought. It has a perfect radix; a caulis consisting of fibres, the interstices of which are filled up by a parenchymous substance, leading from the radix to the umbella or head: it has its semen and siliquæ, and more regular periods of vegetation than is supposed. The common opinion of a mushroom's springing up in a night, and perishing in a day, has no foundation in fact. He had some near a fortnight old, and yet but just arrived to a fitness for the table.

4. On examination of several mushrooms, exposed to the open air, but kept from the injuries of the sun and rain, he found no animalcula bred in them, nor yet a tendency to putrefaction; though they had been exposed thus for a week. On the other hand, on examining a mushroom, very far from being full grown,

putrefied by the rain, and moisture of the dung in the bed, he found animalcula discoverable only by the third magnifier, floating in the liquor, squeezed out from it: from which he thinks it evident, that the dangerous consequences which history has informed us to have attended the eating of mushrooms, have not arisen from any poisonous quality essential to them, but from the accidental ova or animalcula, which the richness of their nutriment has allured to them, and which their contiguity to the ground, and the places they are produced in, render them obnoxious to.

It may not be amiss to subjoin a short account of the culture in the kitchen-garden of a plant which contributes so much to the delicacy of polite tables, which may be depended on, from personal trial and success; as those few writers on the subject, not being acquainted with the true mushrooms, are not entirely to be depended on.

In the melonry, or place allotted in the garden for hot-beds, the mushrooms must be thus ordered: having marked out a portion of ground one yard and a half broad, and of any length, as the ground will permit; fasten two sticks at each end of the diametrical distance already marked out, which shall, by inclining to each other on the top, form an isosceles triangle. To the breadth and height of these sticks must the bed be made, of old, rich, dry dung, closely trodden together: neither new nor moist dung is proper; for the mushroom being naturally of a succulent and spongy contexture, too much heat, and too much moisture, must necessarily injure it. Having raised the bed to the height and breadth proposed, cover it with fine screened mould, to the thickness of 3 inches, into which, at proper distances, put either that white fibrous substance, which may be collected from the place where mushrooms have formerly grown; or else water it with water in which the chives and parings of mushrooms have been steeped; or you may put in the chives in gross. If you take the first away, the mushroom is propagated by transplantation; that white fibrous substance, already mentioned, being no other than the stolones of old mushrooms, from which others are propagated, like potatoes: if you take the second, that is, by watering, the seeds lodged in the parings, being, by the water, separated from the siliquæ, and with it poured on the mould, are that which gives fertility to the beds thus managed. If you put the chives in gross into the mould, it is no more than sowing the seed in the pods, as in other plants it is sometimes necessary to do. Over the bed, thus prepared, must constantly be kept a covering of long new litter, to the thickness of one foot, to preserve the plant from the frost, the sun, and the wind. During the middle of summer, and the extremity of winter, it is best to make these beds under shelter; but at other times they are best exposed, the warm rains not a little contributing to their fertility; which, by the sloping fashion of the beds, are suffered to moisten them no more than necessary.

When speaking of the mushrooms, he means the fungus * porosus, crassus, magnus, called, by way of eminence, in England, the mushroom.

A Scheme of a Diary of the Weather; with Draughts and Descriptions of Machines subservient to it. By Roger Pickering, F.R.S, and V.D.M. N° 473, p. 1.

On a page of a folio paper book, opening broad-ways, are drawn, at proper distances, nine horizontal, and seven perpendicular lines; in the void square spaces of which the particulars of the diary are written down. The first of the horizontal lines is for the days of the month and week, on which the examination is made: the 2d for the hour of the day: the 3d for the weight of the air: the 4th for its heat: the 5th for its moisture, or dryness: the 6th for the quarter of the wind: the seventh for its force: the 8th for the weather; as whether it be rainy or cloudy, or clear: the 9th for the quantity of rain; and the space between the last line and the end of the paper, for the bill of mortality.

The 7 perpendicular lines are for the 7 days of the week; which, in our diary begins with the first day, or Sunday. If you therefore carry your eye along the paper from left to right, you may at one view see the weight of the air, and the degrees of heat and moisture, &c. for the whole week. If you carry your eye from top to bottom down the column, for any one day, you see regularly the whole of the observations in one line for that day. Four pages, or weeks, we allow to each month, and then leave a void page for the observations made in that month; and the overplus calendary days are carried on to the page allotted for the next month; only taking care to describe in every such page, where the ending and beginning of two different months are to be found, the names of both the months, directly over their final and initial day. The abstract of the weekly bill of mortality is apparently a part of observation peculiar to this plan, under which article all acute cases, depending on the state of the air, are set down.

Of the machines necessary in making observations for a diary of the weather, are these five: 1. The Barometer.—Those with open cisterns are more sensible than the portable ones; and with a micrometer, that divides an inch into 400 parts, renders them capable of showing the most minute alteration of the gravity of the air. 2. The Thermometer.—One made by Fahrenheit's scale on one side, with its correspondence to the graduation of the alcohol thermometer on the other, is recommended.

All the machines, except the barometer, are exposed to the open air. The thermometer and hygrometer are placed in a little shed, made for their reception, against the study window, where the graduation could be seen through the

* Mr. Watson, a very skilful and ingenious botanist, remarked, that the mushroom here meant, is the *Fungus campestris albus superné, inferné rubens*. J. B. See Raii Synops. Stirp. Brit. Edit. secunda, p. 11.—Orig.

glass; and, by lifting up the sash, they can be taken in, as occasion requires.

Of the Hygrometer.—Mr. P. had, for some time, used Dr. Hooke's hygrometer, made of the beard of a wild oat, set in a small box, with a dial-plate and an index; but he soon found an inconvenience, without the remedying of which no dependence could be had on this machine; viz. its making more than one revolution in a night. He endeavoured to remedy this by the following method, described in fig. 17, pl. 1.

At the vertical point, from which moisture and dryness are graduated, a small circle is described; the lower arch of which should just intersect with that arch, round which the index of the oat described its circuit. In the centre of this small circle is placed a pin, easily turning in the central cavity, and furnished with a flat piece of thin ivory on its head. This piece of ivory, intersecting with the index of the oat, by it was turned either to the moist or dry side of its graduation, as the index made a double revolution. Mr. P. flattered himself with success; but soon found, in the great fogs in the winter, that the wild oat is not a safe material to make an accurate hygrometer of. So he immediately turned his thoughts to some other for the diary, and reserved this for his study; where, or in any inclosed place, it does well enough, and may be very useful.

As a succedaneum to this, he thought on a statical one; it recurring to his mind, that the weight and moisture of the air being two properties of one and the same body, a statical hygrometer promised the best assistance towards a more complete knowledge of the barometer, which acts on statical principles; and that these two machines must have a reciprocal correspondence with each other. Remembering that Mr. Boyle had mentioned something of this nature; after consulting whom, he made the following machine, acting on his principles, but formed in a manner differing from his.

He procured a balance to turn with half a grain, the axis of the balance drawn out to the length of one inch, and its end furnished with a male screw, to which a light index with a female screw might be fixed. He had this balance fastened in a wainscot box, 12 inches in length, 9 in diameter, and 4 in depth at top, but gradually widening towards the bottom, with a back to slide up and down in a groove. The axis of an inch length, came through a hole in the front of the box, and then had the index fastened on, which described the segment of a circle on a brass plate, silvered and graduated into 180°, as if it had consisted of a perfect semicircle, or two quadrants.

The beam turning with half a grain; and every such turn, after repeated trials, moving the index somewhat more than one degree of the 180 described on the plate; he made a weight of 90 grains, which he fixed with a thread to one brachium of the balance, without any scale, the several threads or silk strings of which, as they would imbibe more moisture, would make the machine less accurate; and the other brachium he charged with a sponge, suspended likewise by a thread, of such a weight, when reduced to absolute dryness, as made an equilibrium; and then screwing on the index to the first degree of the 180, and exposing the machine, thus ordered, to the open air, in one night's time the index had got to the 70th degree; which, as the sponge had been absolutely dry, must have been the true state of the air, as to moisture, at that time.

He found this machine extremely sensible and accurate; it would alter 10 degrees in a night, and as many in a day.

The near correspondence between the degrees on the graduated plate, and the weight of the moisture necessary to be imbibed or exhaled, to make either brachium of the balance preponderate every such degree, gives it the preference to any other. For a more perfect idea of this machine, see fig. 12, pl. 1, where it is viewed on the inside, the back being slid up. At fig. 13 is represented the plate with its graduations and index, as it should appear on the front of the case.

Of the Anemoscope, fig. 14, pl. 1. The Anemoscope is a machine $4\frac{1}{2}$ feet high, consisting of a broad and weighty pedestal, a pillar fastened into it, and an iron axis, of about half an inch diameter, fastened into the pillar. On this axis turns a wooden tube, at the top of which is placed a vane, of the same materials, 21 inches long, consisting of a quadrant, graduated and bound with an iron rim, notched to each degree; and a counterpoise of wood, on the other. Through the centre of the quadrant runs an iron pin, on which are fastened 2 small round pieces of wood, which serve as moveable radii to describe the degrees on the quadrant, and as handles to a velum or sail, whose plane is one foot square, made of canvas stretched on 4 battens, and painted. On the upper batten, next to the bound rim of the quadrant, is a small spring, which catches at every notch corresponding to each degree, as the wind shall, by pressing against the sail, raise it up; and prevents the falling back of the sail, on the lessening of the force of the wind. At the bottom of the wooden tube is an iron index, which moves round a circular piece of wood fastened to the top of the pillar on the pedestal, on which are described the 32 points of the compass. By which this instrument shows both the direction and strength of the wind.

The Ombrometer, or Rain-gauge, fig. 16.—This machine consists of a tin funnel, its surface an inch square, a flat board, and a glass tube let into the middle of it in a groove, and an index. His board was about 3 feet long, to answer the height of the rails that go round the top of his house, to one of which it is hung, clear of any obstacle to prevent the free fall of the rain, with 4 small staples that slide over as many tenter-hooks. The bore of the tube is about half an inch; which, at a medium, is the best size, a larger bore obliging you to make your graduation the more contracted, and consequently the less plain and accu-

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rate; and a less one not permitting you to return the water out of the tube when full, without the adhesion of a great deal to its sides; which, when you have placed the tube in its perpendicular situation, subsides, and sometimes fills up $\frac{2}{32}$ of an inch; which, without care, must necessarily make great mistakes in the diary. The method of graduating the board is this:

He had a vessel of tin made, whose contents were exactly a cubic inch. With this vessel, filled with water exactly to its surface, he frequently gauged the tube, till, by repeated trials, he had found the height to which a cubic inch of water would rise in it. The space answering to this on the board he had graduated into 32 equal parts, and took the same method with the rest of the tube, till in the same manner he had graduated 4 such inches. Now the surface of the funnel being, as has been said, exactly a square inch, no rain can by it get into the tube, but such as falls within the square of one inch; which, as the shower is more or less, has its exact quantity shown on the board, on which a moveable index is placed.

Of the Monthly Observations.—The vacant page at the end of every 4 weeks, reserved for observations occurring in the preceding month, and giving a summary account of the greatest difference of the weather in it, is a method peculiar to this diary; and one which will be allowed exceedingly pertinent and useful. The great end of this, and all diaries, is to furnish materials for a set of sound observations, on which to build a thorough knowledge of the atmosphere, and its effects on mankind: and it is easy to see what great advantage to this part of natural knowledge must arise from a variety of observations, made by different men of application and judgment, on one and the same subject. Besides, in this portion of our design may be included, what could not well without perplexity be thrown into the columns of the diary, all the meteorological appearances of the aurora borealis, lightning, thunder, &c. with abstracts of the most authentic accounts of such phenomena, as at any time in the preceding month have been seen in different parts of our own country, or abroad.

A description of the meteorological figures in pl. 1. In fig. 12, aaaa shows the hygrometer seen in the inside; bb the balance; c a small piece of wood, by which the balance is fastened to the box; d the sponge; e the weight; f, f, two little rings, by which the hygrometer is hung up.

Fig. 13, the graduated plate on the front of the machine, with its index and divisions.

Fig. 14, the anemoscope; a the pedestal; b the pillar, in which the iron axis is fitted; c the circle of wood, on which are described the 32 points of the compass; d the index; e the wooden tube on its axis; f the velum; g the graduated quadrant; h the counterpoise of the vane.

Fig. 15, the velum taken off; a the plane of the velum; b the spring; cc the wooden radii; dd the holes, through which the pin, in the centre of the quadrant, goes.

Fig. 16, the ombrometer; aa the board; bb the tube; c the graduation; d the funnel fixed in the tube; e the funnel one inch square.

Fig. 17, the wild-oat hygrometer; a the box and plate; b the wild oat, with the index upon it; c the pin, with a small piece of ivory on its head.

Concerning some Persons being poisoned by eating boiled Hemlock. By Mr. William Watson, F.R.S.* N° 473, p. 18.

Notwithstanding the number of instances, which occur among writers, concerning the poisonous quality of our common hemlock, or cicuta major of Caspar Bauhin;† such as, that of Cardanus mentioning a man killed by a cake, in which this plant was an ingredient; that of Brassavola, who assures us, that it is mortal not to men only but to geese and swine; as well as those of Matthiolus, Scaliger, Kircher, Boccone, and others; yet the fatality of its poison, when growing in this kingdom, has been doubted by many; insomuch as that faithful collector the late Mr. Ray mentions, in his Synopsis, edit. 2, p. 326, that not only his friend Mr. Petiver ate $\frac{1}{4}$ oz. of the root of this plant, but that Mr. Henly, a friend of Mr. Petiver's, in his presence, ate, without any inconvenience, 3 or 4 oz. of the same root. From hence it has been thought, either that the root has effects different from the stalks and leaves, or that difference of climate varies the degree of the violence of the poison.

An observation indeed of the same kind occurs in the German Ephemerides. Linnæus, in the Hortus Cliffortianus, makes also some doubt concerning the malignity of this plant; and, in naming it, has kept to the old appellation of Theophrastus and Dioscorides, conium; and has transferred that of cicuta, to the

* Mr. Wm. Watson, apothecary, afterwards Dr. Watson, and after Sir Wm. Watson, was for many years one of the most useful and respectable members of the Royal Society. His close attention to science, and his numerous communications to the Society, and printed in the Philos. Trans. in most of the volumes, from the 41st to the 63d, on the various subjects of medicine, natural history, botany, zoology, electricity, meteorology, &c. as well as his extensive practice in his profession, will render his name justly celebrated among the physicians of Great Britain. We find no account of our author's birth. It seems he was bred an apothecary in Aldersgate-street, London, where he paid great attention to chemistry; and, from practising in this line, soon raised himself to the rank of M. D. after which, he practised as a physician with continually increasing reputation, till near the time of his death, which happened at his house in Lincoln's-inn-fields, from a paralytic affection, the 10th of May, 1787, at near 80 years of age. He had the honour to be one of the members of the committee of the Royal Society in 1772 and 1778, for reporting on the securing of the powder magazine at Purfleet, from lightning. He was also, at the time of his death, vice president of the Royal Society, member of the British Museum, and trustee of the College of Physicians. He was knighted on carrying up the address of the College on the king's escape from assassination.

† Conium maculatum. Linn.

cicuta aquatica of Gesner, and of Wepfer. Besides, many of the accidents, said to have proceeded from cicuta or hemlock, have been occasioned by different plants; some of the accidents, probably, from the common one, but many more from the cicuta aquatica beforementioned, and from the œnanthe succo viroso, cicutæ facie, of Lobel. This confusion appears manifestly in several authors, and some of them of the greatest credit. Which of these plants, or whether any of them, was the Athenian poison, nobody has determined.

Though the eating of the roots, as abovementioned, was attended with no bad consequences, a late melancholy accident has been sufficiently convincing of the poisonous quality of the leaves of the cicuta major.

On Sunday May 6, 1744, two of the Dutch soldiers lately arrived, who were quartered at Waltham Abbey in Essex, collected, in the fields adjoining, a quantity of herbs, sufficient for themselves and two others for dinner, when boiled with bacon. These herbs were accordingly dressed, and the poor men first eat of the broth with bread, and afterwards eat the herbs with the bacon. In a short time after, they were all seized with violent vertigos; they soon after were comatose, and two of them were convulsed, and died in about 3 hours.

The people of the town being exceedingly alarmed at this accident, a physician (Dr. Barrowby, jun.) being there, immediately went, and ordered the other two, at that time almost dead, large quantities of oil; by which means they threw up most of what they had eaten, and afterwards got better. In all of them the effects were the same as those from a large dose of opium.

The next day, being at the place, Mr. W. saw one of these men much recovered, and only complaining of a heaviness in his head; but the other was so well, as to be gone to perform exercise with the other soldiers. There was a 5th soldier, whom he saw, who told him, he eat some of the bread out of the broth, but felt scarcely any inconvenience from it. It so happened, that the 2 men, who gathered the herbs, were both killed.

As Mr. W. went down to the place to satisfy himself in this matter, a Dutch officer went with him very courteously to an inn, where there were 2 other soldiers, who had seen and knew the herbs which had been eaten: he was so kind also as to attend him with these soldiers into the fields, to show him the plants growing. They first gathered the cicutaria vulgaris of John Bauhin, or cow-weed; then, the myrrhis sylvestris seminibus asperis of Caspar Bauhin, or small hemlock-chervil. They then gave him some cicuta major, and, smelling it, immediately said, that this was the herb that killed their comrades; which he then had no reason to doubt of; as, of the two former plants, the first grows almost under every hedge, and is eaten by the cows, and the other is frequently given to tame rabbits for food; whereas cattle constantly refuse to eat hemlock.

Before Mr. W. was thus satisfied, he imagined this accident to have proceeded rather from Lobel's œnanthe; thinking, that as that plant grows near the sides of rivers, these soldiers might have gathered it by the river Lee, which runs by the town, and eaten it for smallage, to which it has some resemblance.

It is now known, that the cicuta major, the cicuta aquatica, and the œnanthe of Lobel, are certain poisons; but there are two others of the same class, growing common in England, and not much unlike these in smell and other circumstances, vehemently to be suspected; the one is the cicutaria tenuifolia of Mr. Ray, which grows frequently in waste places, and in gardens among pot-herbs, of which De la Champ gives some account of its malignancy; the other is the cicutaria palustris of Lobel and Tabernæmontanus, or phellandrium of Dodonæus, which grows in muddy ditches and ponds.

Mr. W. does not remember any history of the pernicious effects of the cicuta major in this kingdom; but as the detecting poisonous plants is of very great consequence, he presumed to lay this paper before the society.

A new Method of Calculating Eclipses of the Sun; also of Occultations of the Planets and Stars by the Moon. By Christian Lewis Garsten, F. R. S. and Prof. of Math. in the Academy of Giessen. N° 473, p. 22.

The more modern methods and tables for calculating such eclipses and occultations, being by far more correct than the present tedious paper, it is of no use to reprint it in this place.

Further Accounts of the Success of injecting Medicated Liquors into the Abdomen, in the Case of an Ascites. By Mr. Chr. Warwick, Surgeon at Truro in Cornwall. N° 473, p. 47.

Mr. W. here states, that the patient whom he tapped for a dropsy, and of whose case an account was inserted in Phil. Trans. N° 472, remained in the same state May 24th 1744, though she had laboured under a tertian ague ever since last January.

Mr. W. then acknowledges the kind communication of Dr. Hales' judicious remarks and improvement on his discovery, did him great honour and pleasure; and the more so, as he was so happy as to have discovered the use and efficacy of injections by means of one puncture only, on a poor woman, about 10 days before he received that communication; from whom he drew near fifty pints of dropsical lymph, by an easy transmutation thereof into an appropriated medicinal fluid; which was, without any difficulty, retained within the cavity near 2 hours, and, at the close of the operation, drawn all off at once, without the least symptom of a syncope from inanition; of which he intended to give a further account.

Some Observations concerning the Planet Mercury. By John Bevis, M.D. N° 473, p. 48.

The observations, to which the inclosed computations are made by Mr. Morris, from somewhat more correct elements than those in Dr. Haller's tables, were carefully taken by myself, with an excellent astronomical sector of 5 feet radius. You will perceive how far I am limited, by my friend's request; so must intreat you, if you think it worth while, to inform the Royal Society that Mercury's motion has not been at all disturbed, (by the late comet) to do it in what manner you shall think best.

Some calculated places of that planet are then set down, as well as the corresponding places, by observation, on the comparison of which, being found generally to agree together within a few seconds, it is inferred, that the planet's motions have not been disturbed, as above asserted.

A Rupture of the Navel, communicated to the Royal Society by H. W. Taube, Surgeon. N° 473, p. 50.

Ann Stubbensfull had a very hard labour 17 years before her death, and a little rupture appeared in her navel, and in the next labour it increased; which she endeavoured to cure by a bandage, but in vain; so it continued to increase more and more.

The first time Mr. T. was called to her, was on account of a wound near the place formerly made by uneasy trusses, where it looked as if it would mortify; which he cured, but left a place open as large as a half-crown, from which a great quantity of water would sometimes run out; but getting cold, it stopped; and the whole saccus was very much inflamed.

She had once an obstructio alvi for 15 days; and nothing would do, till he ordered her a glyster of tobacco boiled with urine. At last, she died maniacal. Dr. Douglas and Mr. T. would have opened her, but her children would not agree to it.

Further Remarks concerning Mushrooms: occasioned by the Rev. Mr. Pickering's Paper in the preceding Transactions, with Observations on the Poisonous Property of some Sorts of Fungi. By Mr. Wm. Watson, F.R.S. N° 473, p. 51.

With regard to the seeds of mushrooms, though they were never shown to the R.S. before, the fact was known to many of its members: for the industrious Micheli not only raised mushrooms from their seeds, but has, in his tables, shown the daily progress from their first point of vegetation, even to their perfect state.

The fungus porosus crassus magnus is not the mushroom usually raised in England for the table, as this gentleman imagined; that name being given by John Bauhin, in the 3d volume of his History (p. 833), to a species which is to be distinguished from all other funguses, by the inferior substance not being divided into lamellæ, or what we call in England gills; but has instead a great many papillæ; and being of a greenish yellow colour. But what is raised in England, is the fungus campestris albus supernè, infernè rubens, of John Bauhin, which differs toto cœlo from the former, and which Dr. Dillenius enumerates among the species of boletus, whereas the latter is a species of amanita.

Mr. W. differs from this gentleman also in regard to the use of the ring, which surrounds the stalk of this mushroom. He imagines it placed there, by the wise Author of Nature, to break the fall of the seeds when ripe; by which those light bodies may be preserved from the fury of the winds, in order to the abundant propagation of their species. Mr. W. has reason to believe, that those seeds, which fall on this ring, fall there by accident; and adhere there only from the viscocity by which they are entangled. But before examining this matter, Mr. W. makes a few observations on the economy of this plant. The fungi, then, are of that class of vegetables, which are ranged, by that most skilful botanist Linneus, under the appellation of cryptogamia, or those which perform their fructification in secret. Under this head we find the fig-tree, all the species of fern, mosses, mushrooms, and a few others, whose flowering and seeding are observed with more difficulty, than in those we usually call the more perfect plants. In some of this class, the fructification, notwithstanding the great assistance furnished to the modern botanists by microscopes, which the ancient were wholly destitute of, remains yet undiscovered.* This plant then being of this class, almost all those whose stems are thick and fleshy, as well as their umbels, have a ring on their stem; from which, when the plant is young, and until it arrives at a flowering state, there arises a membrane, which connects the rim of the umbel to the stem, and preserves the under part of the plant in this state: but when this is over, the umbel, which before was almost of an hemispherical figure, growing larger, and the membrane not giving way, is loosened from the rim of the umbel, and adheres only to the stem. Soon after this state, the seeds ripen, and the umbel losing its former figure, commences almost a plane; and the plant in this state is sold in our markets, by the name of flaps. Now, when the umbel is of this figure, the seeds, being perfectly ripe, must fall naturally on the whole space the umbel covers, which Micheli observed by placing leaves of trees under them; and on the ring, as well as any other part; though probably not more. As for those species of fungi whose stems are thin, and whose umbels are soft, and more ductile, they need not, nor have they, this ring or membrane; because, in their tender state, the rims of their umbels clap themselves quite close to the stalk, in the form of a contracted umbrella; and expand as the others do, when their seeds are ripe: yet the species of this tribe are as numerous as the former.

* Since this was written, much new light has been thrown on the fructification of plants belonging to the Linnean cryptogamia class, by the celebrated Hedwig.

Now to consider how far the poison of mushrooms can possibly proceed from animalcules: first, he doubts whether any person was ever injured from eating the common mushroom, or amanita; unless such accident may have proceeded either from eating too many at once, and thereby overloading the stomach; or from some particular dislike in the constitution; as we sometimes see, even with regard to honey, cheese, and some of the most innocent parts of our diet; but which, nevertheless, are by no means to be ranked among poisons. If there were many instances of their being pernicious, such must frequently occur to the practitioners in physic, on account of the vast quantity annually consumed in London; but he does not remember to have even heard of any such accident; but many instances occur of the noxious quality of many of the other species of this tribe, nor is it at all wonderful, that the different subjects of this class of vegetables should differ in their effects, more than those of the more perfect kind. The roots of carrot, parsnip, and many others of the umbelliferous class, are daily used as food; but the water-hemlock, and Lobel's oenanthe, though of the same class, are most certain poisons.

Here he observes what pains have been taken by naturalists, to distinguish the useful from the pernicious kinds. Among the Romans, the boletus mentioned by Juvenal, on account of the death of the emperor Claudius, is sufficiently described by Pliny; but among the later writers, Carolus Clusius was of the first of those who, about the middle of the 16th century, being tired with the critics and commentators of the time he lived in, presumed to believe that all knowledge was not confined to the writings of the Greek, Roman, and Arabian physicians; because, from the revival of letters in the western world to his time, nothing was regarded, as of any importance, but what was dignified with the authority of antiquity. And hence it came to pass, that when the clouds of ignorance began to disperse, the epocha of commentators took place; but many of the descriptions of the plants of Theophrastus, Dioscorides, and Pliny, were so very deficient, that little light could thence be acquired; especially from this last author, who is to be considered as the only Roman naturalist that we have handed down to us; and it is no wonder, if, among the vast variety of subjects that this most admirable historian treats of, he is, in many instances, rather to be considered as an enumerator, than as a describer.

There arose, then, such heats and disputations among the critics on those authors, very often about trifles, that they rather increased than diminished the ignorance of those times. This excellent Clusius, finding that a thorough knowledge of nature was necessary, not only to understand rightly the ancients, but to lay the foundation of future knowledge, was desirous to join careful observations of his own to those which were to be acquired from books. How much he travelled, and what progress he made in this undertaking, his many valuable works are the best testimony. Among them, his history of funguses bear not the least character; he there enumerates a great variety, not only of the esculent, but noxious kinds; but, as the different appellations of every species was not, at that time, much considered, he gives no other synonyms to either class, than that of, viz. esculentorum primum genus, noxiorum decimum genus, and such like. But this want of specific names has been sufficiently supplied by John and C. Bauhin, Ray, Morison, Tournefort, Vaillant; but, above all, by Dillenius, in his Catalogus Gissensis, and by Micheli, in his Nova Plantarum Genera. In most of these authors we find instances of mischievous effects from the pernicious kinds; which property some of them have equal to opium, aconite, or henbane; but how far this property proceeds from animalcules, the following instance will sufficiently demonstrate. We have a sort growing in England, called, by Caspar Bauhin, fungus albus acris; which M. Tournefort has rightly observed stimulates the tongue, and is almost as sharp as though it were steeped in spirit of nitre; and, being rubbed on paper dyed blue with turnsole, turns it as red as any violent acid spirit will. This caustic quality remains even after the fungus is dry. We need make no further inquiry for the cause of the poison in this plant; the above-mentioned is a sufficient criterion. John Bauhin likewise tells us, that after having handled this fungus, he rubbed his eyes by accident, and brought on a violent irritation on his eye-lids. Caspar Bauhin mentions a sort which kills the very flies. Micheli describes a species which, on eating them, almost killed the painter he usually employed, and an old woman, the painter's mother. This man, being sent by the author to delineate some of these funguses, and being taken with their appearance, ordered some of them to be fried, and he and his mother ate of them; but were, in about 2 hours, seized with violent pains in their bowels, from which they were with difficulty relieved.

Concerning the Island of Zetland, or Shetland. By Mr. Thomas Preston, N° 473, p. 57.

This island has little communication with the rest of the world for the 6 winter months; or rather for 6 of the winter months; for the year may be said to contain 10 months of winter, and 2 of cold raw weather. The inhabitants complained of heat at the same instant that Mr. P. complained of cold, and wished for a great coat. They are so accustomed to stormy bad weather that they will venture to sea in small boats, when one would not venture to cross the Thames.

This island is the northernmost belonging to Scotland, situated between the latitudes of 60 and 61 degrees: its length is, N. and S. 60 miles; its breadth 30; and so divided into head-lands and smaller islands, creeks, bays, inlets, and coves, &c. that you cannot place a compass on any the most inland parts of its chart that shall be 2 miles from the sea.

The land is wild, barren, and mountainous; not so much as a tree or bush to be seen. The shores are difficult, and in many places inaccessible, rude, steep, and iron-like; the sight of which strikes the mind with dread and horror; and such monstrous precipices, and hideous rocks, as bring all Brobdingnag before your thoughts.

In winter the sun sets soon after it rises; and in Summer it rises again soon after its setting; so that the nights at that season are nearly as light as the day; as, on the contrary, the day in December is nearly as dark as the night. About the solstice, we see, almost every night, the aurora borealis; which spreads a broad glaring appearance over the whole northern hemisphere, and looks somewhat terrifying to such as are not used to it.

There are 30 parish-churches, and about 80 gentlemen's houses, besides the towns of Lerwick and Scalloway: it was first inhabited by the Pights or Picts, who were driven out by the Danes. Christian, king of Denmark and Norway, whose daughter Margaret was given in marriage to king James III. of Scotland, in the year 1468, agreed, that the islands of Orkney and Zetland should remain in the possession of the said king James, until he had paid to him 50,000 Rhenish florins for his daughter's dower; and Christian afterwards, on the birth of a young prince his grandson, called James, renounced his title to the said island, in favour of King James; which has ever since belonged to Scotland.

The land is mountainons and moorish, abounding with moss and heather; under which they dig peat or turf for firing: under that is hard rock. Their horses are very small, but strong, and well mettled, which they call shelties. Their oxen, swine, and sheep, of which last they have plenty, and their cattle of all kinds are small. The seas abound with most kinds of useful fish; and the land is frequented by most wild fowls, especially the larger sorts. There are very large eagles, which are called earns, which prey on the young lambs, &c. There is a law in force, that whoever kills one of these eagles, is to have a hen out of every house in the parish wherein it is killed.

An Occultation of Jupiter by the Moon, observed at London. By Dr. Bevis. N° 473, p. 65.

1744, June 6^{d} 11^{h} 13^{m} 58^{s} Immersion of Jupiter's centre.
35 14 α Serpentaria culminated.
43 15 Emersion of the centre.

N.B. The clock was too slow by 1^{m} 25^{s}.

On the Easiest Method for calculating the Value of Annuities on Lives, from Tables of Observations. By Mr. Abraham De Moivre, F.R.S. N° 473, p. 65.

[This paper may well be supplied by the theorem in p. 86 of the author's Treatise on Annuities on Lives, being a demonstration and enlargement of that part fo his book.]

The Appearance of a fiery Meteor, as seen by Mr. Zachariah Craddock, of Somerset-house. Communicated by Mr. Henry Baker, F.R.S. N° 473, p. 78.

The head and body emitted an extremely lucid and white flame. The tail appeared of a transparent blue, like the flame of sulphur.

This phenomenon was seen on Sunday, May 27, 1744, at 11 minutes after 11 o'clock at night: its direction about from S. E. to N.W.; its height seemingly not half a mile.

It was seen, as here described, from the terrace in Somerset-gardens.

An Account of a Dissertation published in Latin by Dr. Weidler, F.R.S. in the Year 1727, concerning the Vulgar Numeral Figures. By John Ward, F.R.S. N° 474, p. 79.

Mr. W.'s former two Papers on this subject, gave the antiquity and use of the Arabian or Indian figures, and more especially in relation to England. And those papers, being afterwards published in the Phil. Trans., N° 439, gave occasion to Dr. Weidler, professor of the mathematics at Wirtemberg, to transmit to Dr. Mortimer a dissertation he had formerly printed on that subject. Which discourse coming before the Society, it was referred to Mr. W.'s perusal and consideration; of which the following is a brief account.

The author begins his discourse by observing the great inconveniencies that the ancients laboured under in their arithmetical computations, which were usually made with the letters of their several languages, differently applied in different countries. And he thinks it very strange, that, when it was always the custom to distinguish their numbers by decades, they should not more early have fallen into the method of using only 10 different characters, by means of which the largest sums are now computed with so much ease and expedition. But the Romans, as he observes, had some assistance from their abacus, or counting-table; a description of which, with the use of it, he has given from Velser, by whom it was first published, and afterwards by Gruter, and others.

Our author employs the remaining part of his discourse in treating of the antiquity and use of the Arabian or Indian figures. And here he has given a very particular account of the different opinions of several writers on this subject: but more especially of what Kircher and Dr. Wallis have said concerning it. The former of whom, as he observes, ventures to fix the precise time, when the Europeans learned them of the Arabians; which was occasioned by the assembly called together by Alphonsus king of Castile, for settling the astronomical tables, at which some Moors or Arabians were present. Now in those tables, which were finished and published in the year 1252, the numbers are expressed in these characters. Kircher thinks likewise, that the Arabians first borrowed them from the Indians about the year 900; when, having subdued Persia, Carmania, and the coast of India, they opened a commerce with that country. On the contrary, Dr. Wallis, as our author remarks, has shown, that these figures were known to the Europeans, and used by them in books of astronomy and arithmetic, long before the time assigned by Kircher. But, as Dr. Wallis suspects, that the characters found in some old editions of Boethius De Geometria, very like the Arabian figures, are different from the original, or other ancient manuscripts of that work; our author acquaints us, that he himself saw in the public library of the university at Altorf a copy of it, which by the form of the letters appeared to him to have been written in the 8th or 9th century; and that both the shape and situation of the numeral characters were the same, as in the first edition printed at Venice in 1492. He thinks therefore, that they might be the same, as in the original of Boethius; and endeavours to show, that they were then used in much the same manner as the Arabian figures now are, in sums of multiplication and division. And from thence he concludes, that such characters must have been known in Europe as early as the beginning of the 6th century; since Boethius was put to death by Theodoricus king of the Goths, in the year 524. As to the objection, which may be made to this opinion, from the silence of writers concerning it for several ages after Boethius; he observes, that the same has happened in other instances of a like nature. Though he supposes, that both the characters themselves, and the use of them, were a secret at that time, known only to philosophers and men of learning, and not introduced into the common affairs of life; and that the first invention of them was owing to the eastern nations, whence they came to the Greeks, among whom the Pythagoreans were particularly remarkable for concealing their knowledge from the vulgar, and imparting it only to their followers.

With this learned dissertation, he sent also to Dr. Mortimer a small brass quadrant, with the numbers engraven on it in Arabian figures, and the date when it was made, namely, 1306. In this quadrant, all the figures agree with those of Johannes de Sacro Bosco, except the 2; which in him is inverted thus, ζ, but on the quadrant has the present form.

Observations on the late Comet, made at Sherborn and Oxford; with the Elements for computing its Motions. By the Rev. Joseph Betts, M. A. N° 474, p. 91.

The comet which appeared towards the end of Dec. 1743, and in the following months January and February, 1744, was first seen in England, at the observatory of the Earl of Macclesfield, Dec. 23, between 5 and 6 o'clock in the evening. It formed, at that time, an obtuse-angled triangle, with α of Andromeda, and γ Pegasi, the comet being at the obtuse angle; and its passage over the meridian was observed at $5^h\ 32^m$, mean Oxford time.

A series of observations is then given, of the right ascensions of the comet, from which were calculated the following table of the places and elements, with the differences between the observed and computed places.

By the help of these observations, which were made by the Rev. Mr. Professor Bliss, the transits excepted taken at Sherborn, Mr. Betts, by the method delivered in the 3d book of the Principia, determined the comet's parabolic trajectory; and found the place of the ascending node to be in ♉ 15° 45′ 20″; the logarithm of the perihelion distance 9,346472; the logarithm of the diurnal motion 0,940420; the place of the perihelion ♎ 17° 12′ 55″; the distance of the perihelion from the node 151° 27′ 35″; the logarithm sine and co-sine of the inclination of the orbit to the ecliptic, 9,865138, 9,832616; and thence the time the comet was in the vertex of the parabola, or the time of the perihelion, Feb. 19d 8h 12m; the motion of the comet, in its orbit thus situated, was direct, or according to the order of the signs.

From these elements, by the help of Dr. Halley's general table, to which they are adapted, Mr. B. computed the comet's places for the times of observation, exhibited in the following table: to which are added the comet's longitudes and latitudes deduced from the observed right ascensions and declinations; with the errors between the observed and computed places; the observations being all reduced to Oxford mean time.

Equal Time at Oxford.				Longit. Comet observed.	North Latit. observed.	Longit. Comet computed.	North Latit. computed.	Diff. in Long.	Diff. in Latit.
1743, Dec.	23d	5h	32m	♈ 14° 10′ 2″	17° 33′ 11″	♈ 14° 10′ 3″	17° 33′ 37″	1″ —	26″ —
	27	5	7½	♈ 12 2 25	17 51 29	♈ 12 2 26	17 51 47	1 —	18 —
	28	5	1½	♈ 11 32 11	17 55 54	♈ 11 32 14	17 56 8	3 —	14 —
	31	4	44	♈ 10 4 57	18 9 3	♈ 10 5 16	18 8 53	19 —	10 +
		5	53	♈ 10 4 11	18 9 37	♈ 10 3 55	18 9 6	16 +	31 +
1744, Jan.	12	9	10	♈ 4 52 5	18 59 37	♈ 4 52 24	18 59 13	19 —	24 +
	13	6	20	♈ 4 31 40	19 2 31	♈ 4 31 13	19 2 49	27 +	18 —
		8	20	♈ 4 29 27	19 3 32	♈ 4 26 6	19 3 12	21 +	20 +
	16	6	33	♈ 3 18 43	19 15 47	♈ 3 18 27	19 15 13	16 +	34 +
		8	00	♈ 3 17 31	19 16 7	♈ 3 17 00	19 15 30	31 +	37 +
	23	6	11	♈ 0 19 45	19 42 30	♈ 0 19 16	19 42 1	29 +	29 +
	23	7	29	♈ 0 17 58	19 42 47	♈ 0 17 45	19 42 12	13 +	35 +
1744, Feb.	5	7	31½	♈ 21 52 37	19 35 00	♓ 21 52 56	19 34 42	19 —	18 +
	11	6	37½	♓ 14 42 45	17 23 30	♓ 14 42 58	17 24 5	13 —	35 —
	12	6	33	♓ 13 10 36	16 38 40	♓ 13 10 52	16 39 17	16 —	37 —
	13	6	25	♓ 11 32 50	15 43 45	♓ 11 33 16	15 44 16	26 —	31 —
	16	23	41½	♓ 5 9 14	10 17 40	♓ 5 9 1	10 18 8	13 +	28 —
	17	23	35	♓ 3 37 37	8 15 39	♓ 3 37 11	8 16 3	26 +	24 —

Perhaps it may not be thought improper to remark, that the nodes of the comet, and the planet Mercury, are situated within less than half a degree of each other; which probably gave rise to a report, that the comet had carried Mercury from its orbit. In order therefore to find how nearly they approached each other, Mr. B. brought the matter to calculation; and presently found, that there was above a week's difference in the times of their coming to the nodes; the comet passing its descending node, Feb. 22, about 2^h in the morning; and Mercury not coming to his till Feb. 29; the comet moving all that time southwards with a prodigious velocity. Again, computing their heliocentric conjunction, which happened Feb. 18, about 1^h in the afternoon, the comet was at that time distant from Mercury nearly $\frac{1}{5}$ part of the semidiameter of the orbis magnus; being almost twice as near to the sun as the planet ☿; and having then 31° 30′ of north latitude; Mercury's not exceeding 3° 58′ to an eye in the sun: whence it is easily collected, that the comet could have no sensible influence on ☿'s motion.

The elements above-given cannot possibly differ much from the true. For, after an interval of 2 months, in which time the comet had gone through almost $\frac{1}{7}$ part of its orbit, it is surprising to find the observed and computed places agree so accurately, that the difference no where amounts to a minute. In some parts of the orbit, the agreement is still greater; particularly in the observations made at Sherborn, which come within half that quantity.

The comet was in conjunction with the sun, Feb. 15, about midnight; and its perigee, Feb. 16, about 1^h in the afternoon; at which time it was somewhat nearer the earth than the sun is at its perigee; the comet's distance being them 83, and the sun's 98, such parts, as the semidiameter of the magnus orbis is 100; from which we may have some idea of the comet's magnitude; and therefore may suppose it at least equal to the earth.

Of a Scirrhosity of the Cerebellum. By Albert Haller, F. R. S. Professor of Physic at Gottingen, &c. N° 474, p. 100. *An Abstract from the Latin.*

Professor H. describes in this communication the appearances observed on opening the head of a beggar-girl who was supposed to be about 6 years old. Her body was much emaciated, and the mesenteric, inguinal and bronchial glands were found to be in a scirrhous state.

On examining the head, nearly the whole of the left lobe of the cerebellum was found to adhere strongly to the dura mater which lines the occiput. On making an incision into the tentorium, Professor H. discovered a very large scirrhous tumour, into which the whole of the medullary or of the cortical substance had degenerated. It was of an uniform density throughout, and of a fibrous appearance, not unlike the texture of the kidney. No blood vessel could be seen on cutting into it, nor was there any vestige of the cortical or medullary structure.

Whatever might be the cause of this morbid affection, it was evident that, in this girl, the middle portion of the cerebellum had been rendered useless, and that for a considerable length of time. Nevertheless she had continued to exist, and had been capable of going about, begging from door to door.

A parallel case of a scirrhous cerebellum is recorded in the Memoirs of the Parisian Academy of Sciences for 1705.

An Essay on the Causes of the different Colours of People in different Climates. By John Mitchell, M.D. N° 474, p. 102.

The cause of the colour of negroes being a subject so little known, but so much inquired after, and so curious and useful, as to excite the particular attention and inquiries of the learned in Europe, particularly the Academy of Bourdeaux, in their prize-problems, Dr. M. therefore offered his thoughts on that subject, having had frequent opportunities to make the proper and necessary observations. This problem supposes the knowledge of the causes of colours in general; so that if he can deduce the colour of the skin from its structure, &c. in the same manner, and for the same reasons, from which the great Newton deduces the colours of other substances, it is all he pretends to, which will be as much as that branch of philosophy will permit: and as this problem will include the cause of the colour of the skin in general, he first inquires into the cause of the colour of white people; with a change from that colour in some preternatural affections, whose causes seem not well understood.

PROP. 1.—*The Colour of white People proceeds from the Colour which the Epidermis transmits; that is, from the Colour of the Parts under the Epidermis, rather than from any Colour of its own.*—The truth of this proposition will plainly appear to those who consider, that the colour of white people is always more or less clear or vivid, as the skin is thinner or thicker, finer or coarser; that is, as it is more or less adapted to transmit the colour of the white parts below it. These parts are the parenchyma of the skin, corpus reticulare, papillæ nervosæ, the limpid and clear juices contained in the vessels, and perhaps the inner epidermis itself may appear through its outer porous coverlet; all which parts we know are white, and are what appear so in white people.

But this will be better confirmed, from the following considerations: 1. The palms of the hands, lips, &c. where the epidermis and skin are so thin as to transmit the colour from any thing below them, appear red, or of the colour of the red blood under them; especially in those in whom the skin is fine and thin; but where the skin is thick and coarse, those parts appear almost of the same colour with the rest of the body. 2. The blushings of the cheeks, and their redness in fevers, seem to be another proof of this cause of their colour; for, in a moment, they change from a pale to a deep red; but no one will imagine, that the epidermis then changes its colour, or power of reflecting the rays of light; but that it transmits the colour of the blood; which at such times is more forcibly driven into the capillary subcutaneous vessels, and shines through the epidermis; but before, these vessels contained only a serous liquor, and accordingly the skin appeared of that colour: which will further appear on squeezing such red parts, which drives the blood out of them, and makes them appear white; whereas, on removing such pressure, they recover their colour, as the blood does its place. 3. The yellow colour of the skin in the jaundice is a further proof of this assertion; where the yellow bile is diffused through the vessels of the cutis, and appears through the epidermis; but no one will imagine, that the epidermis itself receives this viscid bile into its vessels; which are so small, that many accurate anatomists, as Morgagni, have denied it to have any vessels at all; and the most accurate could never show them. 4. The pale look of those in whom the blood is viscid, or circulates with little force, shows that the epidermis then transmits the colour of the juices and fibres below it, which are then unmixed with red blood. 5. The same is manifest in those whose blood is poor and serous, as the leucophlegmatic, &c. in whom the epidermis transmits the colour of the water or serum under it.

Hence it appears, that the epidermis is a transparent membrane, which easily shows the colour of the parts under it, in the same manner as the cornea of the eye transmits the colour of the iris. And this will appear more plainly from some considerations below; where we shall assign the cause of this pellucidity; and show that the numbers of pores in the epidermis necessarily make it tranparent; and that the smallness of the particles, into which it is divided by them, make it unfit to reflect any rays of light, and consequently to manifest any colour of its own.

PROP. 2.—*The Skins of Negroes are of a thicker Substance, and denser Texture, than those of white People, and transmit no Colour through them.*—For the truth of the first part of this proposition, we need only appeal to our senses, and examine the skins of negroes when separated from the body; when not only the cutis, but even the epidermis, will appear to be much thicker and tougher than in white people. But because the substance and texture, especially of the epidermis, is not a little altered in anatomical preparations, and that in such a measure as to alter the texture perhaps, on which the colour depends, by boiling, soaking, peeling, &c. let us examine the skins of negroes on their body; where they will appear, from the following considerations, to have all the properties assigned: 1. In bleeding, or otherwise cutting their skins, they feel more tough and thick, than in white people. 2. When the epidermis is separated by cantharides, or fire, it is much tougher and thicker, and more difficult to raise, in black, than white people. 3. Negroes are never subject to be sun-burnt, or have their skins blistered by any such degree of heat, as whites are. 4. Though their skins, in some particular subjects, should not be so very thick in substance, yet in winter, when they are dry, and not covered with that greasy sweat which transudes through them in summer, their skins feel more coarse, hard, and rigid; as they do in ardent fevers, with a dry skin. 5. Their exemption from some cutaneous diseases, as the itch, prickly heat or essere, which no adult negroes are troubled with, but those of fine and thin skins are most subject to, show the thickness or callosity of their skins, which are not easily affected from slight causes. 6. And not only the thickness, but also the opacity of their skins, will appear, from their never looking red in blushing, or ardent fevers with internal inflammations, nor in the measles, nor small-pox; where, though the blood must be forcibly impelled into the subcutaneous vessels, yet it does not appear through the epidermis. The like may be said of their veins; which, though large and shallow, yet do not appear blue, till the skin is cut. 7. In the jaundice, anasarca, &c. the skin of negroes never shows the colour of the parts under it; though visible enough in the eyes: of which Dr. M. saw a more convincing proof in some negroes labouring under a bilious fever, in whom the serum of the blood, when let, was of a deep bilious yellow, but no yellow colour appeared on the skin, though plain enough to be seen in the eyes.

Corollary.—Hence might be deduced one plain cause of the blackness of negroes: for if the colour of the skin depends on what it transmits, and the skins of negroes transmit no colour through them, they must needs appear black; according to the known doctrine of light and colours, that wherever there is a privation of light or colour, there of course ensues darkness or blackness. But as most solid bodies, which are not pellucid, do generally reflect some colour, which we know no black body does, we shall next inquire into the particular make of their skins, by which they are rendered incapable to reflect, as well as to transmit, the rays of light.

PROP. 3.—*The Part of the Skin which appears black in Negroes, is the Corpus Reticulare Cutis, and external Lamella of the Epidermis: and all other Parts are of the same Colour in them with those of white People, except the Fibres which pass between those Two Parts.*—For a proof this proposition, we must examine the structure of the skins of negroes more narrowly, which may be done after blistering with cantharides, or after a scald or burn; when their skins appear in the following manner: the cuticle, which is separated, appears nearly of the same colour on the outside, as before such separation from the body; but on the inner side is almost as white as the same part in white people. This cuticle is almost always, in blistering with cantharides, divided into two lamellæ; especially on the thighs, where it is as thick almost as both the skin and scarf-skin of white people: the surfaces, by which these two parts or lamellæ of the epidermis cohere, are partly white, and partly black; for you may see many black fibres pervading the inner lamella, and perforating the upper one, which appear like so many black spots on these two surfaces, when separated from each other; but these black spots do not appear on the inner surface of this inner lamella; these fibres being, as it were, contracted within the two lamellæ, on the external surface of this inner one. The inner surface of the outer lamella of the epidermis, or at least of the outermost of the two into which it is divided by cantharides, appears to be a whitish membrane, like the other membranes of the human body; except the forementioned black spots, which appear on this likewise, and the colour it receives from its external black surface, which appears in some measure through the inner surface, and makes the whiteness on it appear very superficial. This outer lamella is thicker and tougher, and not so pellucid, as in whites.

By scraping these lamellæ of the cuticle of negroes, they may be made more white, and these black spots scraped off, by which the under lamella will become as white as any membrane almost of white people; and several white striæ may be scraped off from the outer lamella, by which both its surfaces will become nearer of the same black colour: whence the cuticle would appear to consist of, or be composed of, many different lamellæ, and those of different colours, so that the external one only is black; which blackness is easily scraped off from the membranes, by any thing that will abrade the fibrillæ.

Under the epidermis of negroes, when separated in a living subject, by blistering, appears as it were a third membrane, between that and the cutis vera: this is the corpus reticulare Malpighii, which differs from the same part in white people in two respects; for in negroes it is of a black colour all over the body, where they appear black; and whereas, in white people, it is of a soft, pappy, or mucous substance, and can hardly be separated but in pappy flakes, in blacks it is separated very often, by the force of epispastics, from both skin and cuticle, and may often be peeled off, like a membrane, from the cutis, as the epidermis is from it; while in other places, by a less force of the epispastic, it is closely adhering to the cutis, as the epidermis itself often is: this membranous expansion is of a much thicker substance, or denser texture, than the same part in whites; and from this seem to proceed the black fibres, which pervade the epidermis, and end in its external surface.

The cutis itself, which lies under this black membranous expansion, and to which it is closely connected, is of a white colour in negroes, somewhat like the skin of many brown-skinned white people; but when this black corpus reticulare is on it, after the epidermis is separated, they appear, when both connected together, of a brown copper-colour, somewhat like the colour of an Indian or mulatto; some of the colour of the white skin below being transmitted through this thin black membrane: which seems to show, in what manner the colour of these Indians and mulattoes may be occasioned, by the colour of the white membranes under their cuticles appearing partly and imperfectly through them, as the white skin does through this corpus reticulare.

Hence the formation of the epidermis seems to be more easily shown, and more completely deduced, than from any preparation of it in white people. For the external lamella of it manifestly arises from the corpus reticulare, by the intervention of the black fibrillæ, which has been shown to pervade the inner lamellæ of the epidermis; and this corpus reticulare itself arises from the subcutaneous nerves, so nicely and accurately delineated by Eustachius.

PROP. 4.—*The Colour of Negroes does not proceed from any black Humour, or fluid Parts contained in their Skins; for there is none such in any Part of their Bodies, more than in white People.*—It has been the generally received opinion, since Malpighi's time, that the cause of the colour of negroes is a juice or fluid of a black colour, which lies between the epidermis and cutis, in some aqueous vessels, which serve to lubricate those parts. But Malpighi seems to propose this rather as a probable opinion, to be more thoroughly examined, than as an established one to be confided in. But this opinion needs only to be well and more thoroughly considered, to be confuted: for if we consider the ill qualities, and pernicious effects to our bodies, of any such exalted sulphureous juices, no one will imagine that any animal can live in health, with any such fluids in his body; since all the fluids of the body constantly circulate, and communicate with each other; for such sulphureous juices seem to be the cause of black tongues in acute diseases, and of the blackness of gangrenes in some measure, which we know soon prove fatal, unless removed. Besides, these juices must be secerned from the blood, which seems to have no more disposition to turn black, in black than in white people: and as these black juices lie in the skin, it is very probable that they might often be exhaled, especially in sweating; and might leave the skin destitute of its black colour, in some measure, at such times; which it never is, but appears rather blacker at such times, than any other. And as this humour must be secerned from the blood, and constantly exhaled and renewed again, it is very likely that it would be often obstructed in its secretion, or altered in colour, in some morbid cases, like the other humours, as well as evaporated sometimes; which however we never see it to be.

But if there was any such thing as a black humour in the skins of negroes, no doubt it might be drawn out by some means or other; but, though Dr. M. had macerated the skins of negroes, and particularly the epidermis, in warm water, which readily dissolves the juices of the body, yet he never could extract any black juices from them, by any such maceration, or even by a more powerful expression; no more than Mr. Littre, as is related, could do, by more powerful dissolvents. Nothing seems more likely to extract this supposed black juice, than the action of fire, or cantharides, on the vessels which contain it, which abrade and tear the vessels and fibres of both the cuticle and corpus reticulare from the skin, but leave them both as black as ever they seem to have been, though they would doubtless extract whatever juices they might contain, as we see plainly they do, by the large blisters raised by such applications, from the abraded vessels spewing out their liquid contents; in which blisters there are no tokens of any black humours in negroes more than in whites, as he often found on proper trials; though if there was any such black humour contained in their subcutaneous vessels, there is no doubt but it would appear, in some measure, in the water of their blisters, as well as the yellow bile does, when diffused through the blood, and on the skin.

PROP. V.—*The epidermis, especially its external lamella, is divided into two parts, by its pores and scales, two hundred times less than the particles of bodies, on which their colours depend.*—Sir Isaac Newton informs us, that the particles of bodies, on which their colours depend, are about 600 times less than those which can be discerned with the naked eye; Opt. lib. 2, part 3, prop. 7. But Leuwenhoeck shows, that a portion of the epidermis, no larger than what can be discerned with the naked eye, is divided into 125000 pores; which pores must divide such a portion of the skin as can be discerned with the naked eye, into 125000 particles; therefore each of these parts of the skin, between its pores, must be about 200 times less than those particles, on which the colours of bodies depend; for $\frac{125000}{600} = 208\frac{1}{3}$; not to mention that such a portion of the epidermis is further divided into 250 scales, which must increase the number of parts into which it is divided. Nor will any one think, that the smallness of these parts and pores exceeds credibility, who considers that they convey the minutest particles of our last digestions; and were they even large enough to convey the particles of many waters, it is very probable, that all our fluids might in time evaporate through them. Nor is it any thing to our present purpose, whether these numbers be mathematically exact or not, all that is endeavoured to be proved, is, that the parts into which the cuticle is divided, are less, in some proportion, than the particles of bodies on which their colours depend.

PROP. VI.—*Problem: To determine and explain the proximate cause of the colour of Negroes, Indians, white people, &c. from the foregoing propositions.*—We have shown above, prop. 1, that the colour of white people depends on the colour which the epidermis transmits, and not on what it reflects: this pellucidity of bodies proceeds from the number of interstices between the particles which compose them, and the extreme smallness of those particles; for, in order to render a body of any colour, or fit to reflect the rays of light, its particles, and the intervals between them, ought not to be less than a certain magnitude; else they become incapable of making any reflexions, from their common surfaces, i. e. of appearing coloured: but, by prop. 5, the cuticle is divided into parts, and pores or intervals between these particles, far less than those on which the colours of bodies depend; that is, too small to reflect any rays of light from their common surfaces, or to appear coloured from such reflected rays: but as such porous bodies are always transparent, so the epidermis is transparent enough, to show any colour reflected from the parts below it: so that we must consider the epidermis of white people as a transparent pellicle, of too subtile or rare a substance, and too minutely divided, to reflect any rays of light from its surface; but consisting of numbers of pores, which readily transmit those rays through its thin and rare substance, by which it shows the colour of whatever parts are below it; on which the colour of white people depends.

But, as there are numbers of scales, or several strata of scaly lamellæ in the epidermis, so this transmission of the rays of light, from the subcutaneous parts, must be imperfect, some of these rays being intercepted in passing through the several lamellæ; and the thicker the cuticle is, i. e. the more there are of these lamellæ, or the denser their texture, the more the light will be intercepted in passing them, and the more the colour of the skin will degenerate from the pure white of the membranes below it. This is agreeable to experience; for Mr. Cowper tells us, in his anatomy, that the thickness of the skin proceeds from the number of the strata or layers of scales which compose it; and we may daily observe that those who have such thick and coarse skins, are never of so perfect and pure a white, as those who have a thin and fine skin, as Cowper observes. But the reason why such thick skinned people appear of a yellowish or tawny colour will be plain, from Newton's observations, Opt. lib. 2, p. 1, obs. 9 and 20; where he shows a faint yellowish colour to be the one that proceeds from an imperfect transmission of a white; for no one can say, but that both the internal membranes and humours of such swarthy people are of the same colour in time of health, with those of the perfectest white skins, as well as they are in Negroes. And this seems to be the cause of the pale yellow of dead bodies, whose skins are not perspirable, and consequently not so transparent, as in a living subject.

From this account of the cause of the difference in colour among those people that are white, we may account for the cause of the colour of Indians, and other tawny people, who seem to differ from each other in colour, and from white people only in degree, as they have more or less of this tawny yellow proceeding from the imperfect transmission of a white in their colours: thus, if we proceed from the swarthiest white person to the palest Egyptian, from thence to the fairest Mustee, Mulatto, Moor, &c. to the darkest Indian, we may plainly see, that they differ from each other only as they have more or less of the original white in their colour; and as we have shown this tawny colour in white people to proceed from the thickness or density of their skins, obstructing the transmission of the rays of light; so it is very plain that the same tawny colour, in these other tawny people, which seems to be of the same kind, but different in degree, must proceed from a like cause, that is, the thickness or density of their skins; and accordingly it will be found, that all such people have skins of a thickness or density proportional to the whiteness or darkness of their colours. The particular manner in which this opacity, or imperfect pellucidity of bodies is brought about, Sir Isaac Newton explains in his Opt. prop. 2, where he shows, that the opacity of bodies depends on the multitude of reflexions that are made in their internal parts; but it is very plain, that the thicker the skin is, the more reflexions the rays of light must suffer in passing through it, by which they will be extinguished, in proportion to the number of such reflexions; that is, the more opaque, or less white, it must appear: so that, though the particles, of which white and dark skinned people are composed, may not be very different from each other, as they seem not to be; yet a greater number of such combined particles, or more strata of them, in thick skins, and the smallness of their intervals in skins of a dense texture, will increase the number of reflexions made in their internal parts, or the opacity of them, which renders them less white, since their whiteness proceeds from the number of the transmitted rays.

In the same manner, by which we have accounted for the colours of tawny people, may we account for the colour of those that are black; for, if the skin appears darker and darker coloured, the more the rays of light are intercepted by it, of course it must follow, that when the rays of light are entirely intercepted by a body of the same structure, which the skins of negroes seem to be, it must be quite black; for blackness always proceeds from a suffocation of the rays of light, as those versed in the doctrine of light and colours are well acquainted with; but we have shown above, prop. 2, that the skins of negroes transmit no colour or rays of light through them, on account of the thickness of their substance, and density of their texture, in the same manner as they are imperfectly transmitted in some white or tawny people, whose skins appear to be of the same structure with those of negroes, and to differ from them in nothing but in degree of thickness and density, and in colour; which different density may therefore probably be one, if not the only cause of this difference of colour. So that the thickness and density of the skins of negroes seems to be the grand cause of their colour, in the same manner as it is of Indians, Moors, &c. Which may be further confirmed by the following considerations: 1. In the cicatrices of their ulcers, the thin and tender new-formed skin appears whitish, nay perfectly white in some, especially on the shins, or those places where these cicatrices are thin; but, where the skin is thick, or when these cicatrices turn more thick and callous, they grow blacker in proportion; as in those places where the scars grow thicker than the rest of the skin, they are likewise blacker. 2. The colour of the water, contained in the blisters of white people, may be plainly seen through their cuticles, especially if tinctured yellow, which cannot be perceived in the blisters of negroes: a plain proof, that their cuticles are not transparent, as those of white people are. 3. Infant negroes, whose skins differ from adults only in the thinness and rarity of their texture, look whitish, in comparison to adult negroes, but grow black, as their skins turn thicker and denser. These infant negroes, labouring under an icterus, look of a yellow colour, all over their body, which the adult do not, except in the eyes; a plain proof, that the colour of the skin proceeds from the colour which is transmitted through it; and that the skins of adult negroes transmit no colour of any sort. 4. But that the thickness of the corpus reticulare, the part which appears black in negroes, by prop. 3, may and does make it black, Malpighi gives an instance in the said part, in the tongue of a beef, in which it appears black, on the middle of the tongue, where it is thick; but is white on the edges and cheeks, where it is very thin. As for the manner in which this blackness or opacity is occasioned, by a thick or dense skin, it will appear from what has been said about the skins of tawny people; and it is very easy to conceive how the rays of light are intercepted, in passing through the thick and dense skins of negroes, which easily pervade the thin and rare cuticles of whites.

But, as the skins of negroes are of a denser texture than those of whites, they will be more apt to refract the rays of light; for the denser the body, the greater the power of refracting; and the greater the refraction of any body is, the more apt it will be to absorb the rays of light; which is another property of opaque bodies, by which they become black.

So that, from the whole, we may conclude, that the proximate cause of the colour of negroes is threefold, viz. the opacity of their skins, proceeding from the thickness and density of their texture, which obstructs the transmission of the rays of light, from the white and red parts below them; together with their greater refractive power, which absorbs those rays, and the smallness of the particles of their skins, which hinder them from reflecting any light.

Hence we may justly infer, 1. That there is not so great, unnatural, and unaccountable a difference between negroes and white people, on account of their colours, as to make it impossible for both ever to have been descended from the same stock, as some people, unskilled in the doctrine of light and colours, are very apt too positively to affirm and believe, contrary to the doctrine of the sacred pages. 2. That the epidermis, besides its other uses, tends to preserve the uniformity of the colours of people throughout the world.

Prop. vii.—*The influence of the sun, in hot countries, and the ways of life of the inhabitants in them, are the remote causes of the colour of negroes, Indians, &c. And the ways of living, in use among most nations of white people, make their colours whiter than they were originally, or would be naturally.*—As for what relates to the remote causes of the colours of negroes, it has been generally supposed, though not universally believed, that the power of the sun in hot countries is the principal, if not the only agent, in producing this effect: but as the authors of this opinion seem not to have understood what effect or alteration is produced in the make of the skin, in order to render it black, so they have not been able to satisfy any one in this point, and far less to vindicate their opinion from any material objections; for it would be very difficult, if not impossible, to show or prove, in what manner the sole effect of blackness is occasioned by the power of the sun, but not so difficult to show how it may make the skin thicker or denser, which we shall endeavour in the next place to do, by showing in what manner the power of the sun is able to cause that thickness and density of skin, which we have assigned as the immediate cause of its black or tawny colour. But as this subject is much plainer, and more obvious, than the other concerning the make of the skin, or the immediate cause by which its blackness is occasioned, so we need only give the principal heads of these arguments, which serve to prove this proposition: which arguments are of two sorts, viz. philosophical and historical.

The proof of the first part of this proposition will consist chiefly, in showing what effect of the sun it is which deprives the skin of its white colour, rather than what it is which causes it to be black; for, to prove the cause of blackness, is the same as to prove a negative; blackness being a negative with regard to colour. The skin then is deprived of its white colour, by the force and influence of the sun, these four ways: 1. By being rendered opaque, from a dissipation of its more aqueous and pellucid juices. The known effect of the sun's heat, and which will render all bodies opaque.

2. By a concretion of its vessels and glandules, from this dissipation of their aqueous contents, which renders the skin both thicker and denser, or more callous or rigid. For the skin being designed as a defence to the other subcutaneous parts, as the epidermis is to the cutis, they both wonderfully accommodate themselves to the nature and force of external injuries, so as to become capable to defend the body from them, as we see in smiths, &c. constantly used to handle hot and hard things, who have the skin of their hands become so thick and hard or cartilaginous by it, as to be able in time to handle even hot irons: and thus it is, in a great measure, with the skins of negroes, Indians, &c. constantly exposed, and generally naked, to the scorching heat of the sun in a perpetual summer.

3. By a new accretion of many new membranes, which render it thick and opaque. For the sun-beams act as a vibrating force, or external friction, on the skin, which derives fresh supplies of juices to it; by which new membranes, or lamellæ, are formed, in the same manner as the epidermis is renewed when abraded, which is very soon and easily done. Which derivation and concoction

of the humours on the surface of the body must occasion a thickness of their skins, as well as of their lips, and other muscles, especially of their face.

4. By increasing those parts or principles, in the composition of the epidermis, which have the greatest refractive powers. As the terrestrial, and fixed saline; but, especially, the tenacious sulphureous, which refract and absorb light more strongly than any other substances; while the more transparent and pellucid principles, as the aqueous, spirituous, and volatile saline, are evaporated by the heat, which causes the other more fixed principles to be accumulated in greater quantities, and combined in larger collections; and these particles, being likewise more comminuted by the sun, will on that account be black, as happens to oil when well boiled.

From what we have said above about the immediate causes of the colours of the skin, it will appear, that these several effects of the sun's heat contribute to make it of a darker colour; and no one will doubt, but that all of them, conspiring together, may make it quite black.

To these, perhaps, might be added another effect of the sun's power, a peculiar necrosis of the epidermis, occasioned by the forcible vibrations, contractions, and exsiccations of its fibres by the sun-beams, which cause it to turn black, as these, or the other parts, do by the heat of an inflammation or a fever, in gangrenes, black tongues, &c. Whence only the nervous parts of the skin come to be black, and more hard and callous, and less pellucid, than the rest; and the skins of negroes, besides their callosity, become more insensible than those of whites.

But as there are many degrees of whiteness and blackness in the colours of the people in the world, depending on the different densities and thicknesses of their respective cuticles, as we have above showed, it may not be improper, in the next place, to inquire into the more peculiar causes of this diversity, which will be found to be such as increase or diminish the power of the sun's heat, or its influence on the body; by which the only material objection that has been brought against this proposition may be answered, viz. that the sun's heat is not the cause of negroes, because several nations of people, in the same latitude with the negroes in Africa, are not made black by it.

The causes of this diversity may be referred to two heads, viz. 1. The nature and temper of the country. 2. The ways of living in it. Under the first may be included the following particulars:

1. The nature of the soil, and situation of the country, with regard to mountains, waters, &c. which very much alter the power of the sun's heat; for the differing degrees of heat and cold, in different places, depend in a great measure on the accidents of the neighbourhood of high mountains, whose height exceed-

ingly chills the air brought by the winds over them; and of the nature of the soil, which variously retains the heat, particularly the sandy; which, in Africa, Arabia, and generally where such sandy deserts are found, make the heat of the summers incredible to those who have not felt them, as the learned Dr. Halley has remarked. Whence it will appear, that the heat or influence of the sun is not always the same in the same latitudes, as those imagine who start this objection to this proposition; but that in Africa, where the people are black, the soil is as intemperately hot as the climate, occasioned by the scorching heat of its sands. This heat of the soil must much increase the heat of the sun, and its power on the body; and if the sun is the cause of blackness, must make the people blacker in such places than any where else; which we see to be true of the negroes in Africa, who are much blacker than the Indians of Asia or America, who live in the same climate, but inhabit more temperate countries. This power of the sun will be much increased in such sandy soils.

2. By the scarcity, if not entire absence, of large, spreading, succulent plants; which afford, in other moist and more fertile soils in hot countries, agreeable cooling shades, or a moist cool atmosphere, from their exhalations, which take off much of the scorching heat of the sun.

3. The want of water must much increase the heat of the body, if not of the sun; and conspires to the same effects as the more immediate heat of the sun itself. This is well known to be the case in Africa, from the many caravans that perish for want of water in travelling through its midland parts: besides, it rains so seldom in many places of Africa, as to make it generally believed formerly, that it never rained there at all; which must much more exsiccate the body, and parch the skin more powerfully, in those sandy regions, where no rain ever falls, but at a certain season or two in the year, than in more temperate regions, though in the same latitude.

4. The ways of living in many hot countries, particularly in Africa, very much contribute to increase the influence of the sun on the body, or to thicken and harden the skin, on which its blackness depends. These customs are—

1. The custom of going naked among most of the nations of Africa, especially those that are black, both in former times and at present. 2. Living not only without cloaths, but also without houses, in a very barbarous and rude manner, little better than the wild beasts. 3. The custom of wandering up and down in these sandy deserts, in the scorching heat of the sun, stark naked, with no house or cool shade to retire to, nor water to refresh themselves with, or cool their bodies in. 4. The custom of most people in hot countries of anointing their bodies with some greasy and unctuous epithems, to defend their skins from the scorching heat of the sun, will be found likewise to increase the darkness of their colours.

3

On the contrary, the customs and ways of life in use among the Europeans, and other nations of fair complexions, contribute to render their skins whiter than they otherwise would be, or than they were, in all probability, originally. These customs seem to be, an almost constant confinement, or rather imprisonment, from the open air; warm and soft cloaths; warm beds; sitting by fires; the custom of bathing, much in use formerly; a more succulent and nourishing diet; excess in strong liquors; frequent sipping of warm thin liquors; and, in general, more luxurious and effeminate lives; all which, with the absence of the sun, or defences from it, tend to soften, moisten, and relax the fibres of the body, and to render the fluids more thin and watery; and consequently the membranes composed of them, such as the skin is, must be more clear and transparent; on which we have shown its whiteness depends; and accordingly we constantly see that people of such constitutions, or ways of life among us, are always the whitest. We might indeed consider the effects of cold on the skin in these northern climes, where the people are white, were it not that those who are the fairest among them, are the least exposed to it, and seldom or never feel its effects; but the whiteness of their complexions seems rather to be occasioned by muffling themselves up against the cold, than from being exposed to its influence: for, as the cuticula is a sort of clothing to the other membranes of the body, and, by preserving their whiteness, serves, besides its numerous other uses, to keep up a uniformity and harmony in the colours of people; so there is no doubt, but that the cloaths with which we cover it, preserve its whiteness, or render it whiter, as every fair-one knows: so that the different customs of different nations, in this respect, will tend very much, besides other causes, to make that alteration and diversity so observable in their complexions. So that it seems to be but a small objection, if any at all, to this proposition, that the natives of Canada, though but a cold and northern clime, are of a swarthy colour, while others, in the same latitude in Europe, are white; for the customs and ways of life of these last seem very much to increase, if not occasion, the whiteness of their colours; whereas the hard lives, and savage customs of those Canada Indians, especially their going quite naked all over, seem to have no tendency to soften their skins, or refine their complexions; not to mention their custom of intermixing with the captive women of southern nations. But as the Canada Indians are the most northern, so they are the palest of all Indians.

Hence it will appear, that the power of the sun's heat in hot countries, and its more immediate application to the body, or the increase of its force, by the nature of the soil, or ways of life, is the remote cause of the blackness, and the different degrees of blackness, of the inhabitants of the torrid zone: whereas the luxurious customs, and the effeminate lives, of the several nations of white people, in the northern climes, are the remote causes of their respective fair complexions.

Hence, also, 1. White spots on the skins of negroes are as common, and proceed from the same causes with red spots on white people; viz. a distention, dilatation, and consequent rarity or pellucidity of the vascula of the epidermis: whence the physical causes of the total whiteness of some negroes, at their birth, may be accounted for.

2. The hair of negroes becomes short, stiff, and frizzled, from the exsiccation of its substance, and its excrementitious moisture, by the heat of the sun; with the thickness and density of the pericranium, which hinders it from being further protruded.

3. Many morbid discolorations of the body proceed rather from a preternatural thickness and density of the membranes of the skin, than from any humours lodged in them, as is commonly supposed; and may be accounted for in the same manner, as the different complexions in time of health.

4. The bodies of whites are more perspirable than those of negroes, but perspire less in hot weather, and more in cold.

5. White people are most healthy in cold, and black or tawny people in hot countries; each being subject to disorders, on a removal to these respective climes. The causes of the diseases of white people in hot countries, are often opposite and contrary to such as proceed merely from heat, which exalts the fluids, exsiccates the solids, and quickens the circulation, occasioning severe acute diseases; but the thin and rare skins, and large pores of white people, make them subject to too large cutaneous evacuations of the most subtil and active fluids; by which the body is enfeebled, and comes to be in an imbibing state, both on its external and internal surfaces; and too readily imbibes the humidity of the air and aliment, without a previous digestion; causing a cold and humid, rather than a hot and dry, state of the body; from whence proceed their lingering acute, and obstinate chronical maladies, more frequent in hot countries than the former, especially among the whites. Negroes, notwithstanding their hardier usage, are more apt to have their perspiration obstructed in cold weather, and thence contract fevers; whereas, in hot weather, their thicker hides serve as a coat, to keep off the power of the sun, and preserve the body against the moisture of the air, so remarkably great, and very pernicious in all hot countries, especially at certain seasons, which are always sickly. Hence white people should be best clothed in hot weather, and blacks in cold; a thing much neglected in Virginia, though the cause of one half of the untimely deaths of both sorts of people in it.

6. The perspirable matter of black or tawny people is more subtil and volatile in its nature; and more acrid, penetrating, and offensive, in its effects; and more of the nature, and more apt to degenerate to a contagious miasma, than

the milder effluvia of whites. The contagion of pestilential fevers proceeds from a subtilizition and volatalization of the perspirable humours, by the effects of a preceding fever, as often, if not more often, than from any external putrefaction, or mineral exhalation. Hence this acrimony of the perspirable humours of black and tawny people makes them subject to malignant and pestilential fevers, from the same causes which breed only putrid benign fevers among whites; and in them these fevers are more apt to turn contagious, as they themselves are to be infected with such contagion. Hence seem to have proceeded the first seeds of the measles and small-pox, with the African or true plague. Hence also proceeds the rank smell, or peculiar fœtor of dark-skinned people.

7. This acrimony of the perspirable humours, with the thickness and density of the skins of black and tawny people, or imperspirability of their bodies, makes them subject to many severe cutaneous diseases, accompanied with a contagion, which white people never feel, but by infection from them; and then these diseases appear in other shapes, with milder symptoms, than in the dark-skinned people which breed them. These diseases, which he has observed among them, may be referred to the elephantiasis Græcorum, or lepra Arabum, two species of which are called the yaws, and the joint-evil; with some others, not named, appearing in obstinate subcutaneous ulcers. But the elephantiasis Arabum, to which the negroes are also subject, is not a cutaneous distemper, as has been thought, but a peculiar kind of cachexy, accompanied with an atrabilious cacochymy, as in those afflicted with the hæmorrhoids; that being much the same distemper in the legs, as this is in the hæmorrhoidal veins. The peculiar diseases of white people analogous to these of the blacks, and which the blacks never have, are the lepra Græcorum, at least with furfurous desquammations, the itch, scurvy, essera, and some smaller ones of that kind. This cutaneous malady of the negroes, called the yaws, laid the first foundation of the lues venerea; which became to differ from it only by the part affected, and the particular manner of receiving the infection, after being transplanted into another colder clime, on people of a different complexion; the virulent acrimony of the cutaneous contagion being inviscated, and consequently mitified, by the semen which received it; the subtiler parts of the contagion being likewise exhaled in the white people, on account of the perspirability of their bodies, though the distemper was driven more on the internal organs, on account of the coldness of the climate; and so appeared to partake less of a true cutaneous malady, after this lues venerea was first propagated to Europe. Hence it is originally a cutaneous malady, only to be cured as such; the venom which attends it, and gives rise to it, being to be evacuated, most surely and effectually, by the pores of the skin, as it was originally bred by the acrid effluvia which pass through them. Hence the na-

ture, origin, progress, alterations, and different success of divers methods of cure of this lues, may be accounted for; and the most rational methods of cure deduced.

8. From what has been said about the cause of the colour of black and white people, we may justly conclude, that they might very naturally be both descended from one and the same parents; for the different colours of people have been demonstrated to be only the necessary effects, and natural consequences of their respective climes, and ways of life; as we may further learn from experience, that they are the most suitable for the preservation of health, and the ease and convenience of mankind in these climes, and ways of living; so that the black colour of the negroes of Africa, instead of being a curse denounced on them, on account of their fore-father Ham, as some have idly imagined, is rather a blessing, rendering their lives, in that intemperate region, more tolerable, and less painful: whereas, on the other hand, the white people, who consider themselves as the primitive race of men, from a certain superiority of worth, either supposed or assumed, seem to have the least pretensions to it of any, either from history or philosophy; for they seem to have degenerated more from the primitive and original complexion of mankind, in Noah and his sons, than even the Indians and negroes; and that to the worst extreme, the most delicate, tender, and sickly. For there is no doubt, but that Noah and his sons were of a complexion suitable to the climate where they resided, as well as all the rest of mankind; which is the colour of the southern Tartars of Asia, or northern Chinese, at this day perhaps, which is a dark swarthy, a medium between black and white: from which primitive colour the Europeans degenerated as much on one hand, as the Africans did on the other; the Asiatics, unless, perhaps, where mixed with the whiter Europeans, with most of the Americans, retaining the primitive and original complexion. The grand obstacle to the belief of this relation between white and black people is, that, on comparing them together, their colours seem to be so opposite and contrary, that it seems impossible that one should ever have been descended from the other. But, besides the falsity of this supposed direct contrariety of their colours, they being only different, though extreme degrees of the same sort of colour, as we have above proved; besides this, that is not a right state of the question; we do not affirm that either blacks or whites were originally descended from one another, but that both were descended from people of an intermediate tawny colour; whose posterity became more and more tawny, i. e. black, in the southern regions, and less so, or white, in the northern climes; while those who remained in the middle regions, where the first men resided, continued of their primitive tawny complexions; which we see confirmed by matter of fact, in all the different people in the world. Agreeably to this, we see, that the heat of the sun will tan, as the saying is, the fairest skin, of a dark

swarthy, even at this day; in which there is some degree of blackness; or at least this may well be said to be a tendency to their primitive swarthy complexions. So that if the heat of the sun will turn a white skin swarthy, as nobody in hot countries can doubt, the same cause might turn the swarthy and tawny black; for the effect seems to be the same in one as in the other, and may therefore be produced by one and the same cause. As for the black people recovering, in the same manner, their primitive swarthy colours of their fore-fathers, by removing from their intemperate scorching regions, it must be observed, that there is a great difference in the different ways of changing colours to one another: thus dyers can very easily dye any white cloth black, but cannot so easily discharge that black, and bring it to its first colour: and thus, though the skins of white, or even swarthy people, are easily affected by the greater power of the sun's beams than what they have been used to, and thereby become black; yet they are thereby rendered so thick and hard, or tough and callous, as not to be so easily affected, or readily wrought on, to render them again of their original swarthy or pale colour, by any of those causes, as the absence of the sun, coldness of the climate, or ways of life in it, which we have supposed to be the causes of the fair complexions of the Europeans; though probably it has never been tried what effect these luxurious customs, or soft and effeminate lives, which we have supposed to be the causes of mankind's turning to so tender and delicate complexions as the Europeans have, and to be the cause of all whiteness in the complexions of men, or changes from a dark to a fairer complexion, might have on the colour of negroes; but this we are assured of, that they are not of so deep a black, in cold northern, as in the hotter southerly regions. Besides, we want not some convincing instances, from the gleanings of a few historians, to show that such changes have happened in the memory of men, and within the compass of those records we have of time; for we could not suppose it to have happened all at once: thus Herodotus tells us, that the Colchi were formerly black, with frizzled hair; which he says he relates rather as a thing well known before, than a bare report; but there is no sign of any blackness in the complexions of their descendants, they being rather, especially about Circassia, reckoned some of the fairest people in the world at this day. Captain Smith tells us, that, even in Virginia, an Englishman, by living only 3 years among the Indians, became "so like an Indian, in habit and complexion, that he knew him only by his tongue:" and what might his children have turned to in a succession of many generations, by these same ways of life, which had so altered him in 3 years? The Moors and Lybians, being driven out of Africa, on the Turkish conquest, retired to the land of the negroes; but are no more to be found there of their original tawny colour. The king of Gualata is supposed to be lineally descended from these tawny Moors, but is even blacker than the original negroes. The

Abessines, who came from Arabia originally, are no longer of their swarthy complexion, but have got the black complexion of the Ethiopians, whose country they possess. The Mosemleeks' of Canada, who wear clothes, and are more civilized than the other savages their neighbours, who go quite naked, are so much more refined in their complexions by this usage, as to be taken for Spaniards, and not Indians. Nay the Spaniards themselves, who have inhabited America under the torrid zone for any time, are become as dark coloured as our native Indians of Virginia: and were they not to intermarry with the Europeans, but lead the same rude and barbarous lives with the Indians, it is very probable, that, in a succession of many generations, they would become as dark in complexion.

The Case of a Lad who was shot through the Lungs. By Mr. Nic. Peters, jun. Surgeon. N° 474, p. 151.

Dec. 28, 1737, James Channon, about 14 years of age, was accidentally shot in his back by another lad, at the distance of 2 yards from him; so that the whole load of shot, not having space to scatter, entered like a ball, by the edge of the left scapula, which it splintered; and, slanting upward, passed between the two superior ribs, and fractured the clavicle; the resistance of which bone hindered their passage through the skin; for some of them lay immediately on the fractured part, covered only by the cutis; which, with a touch of the incision-knife, Mr. P. took out, to the number of a dozen: they were the small mustard-seed shot.

After reducing the fracture, he drew off 10 oz. of blood, he having lost but very little by the wound; and treated the wound in his back as usual in gun-shot wounds; and the fever which attended it, as a common symptomatic fever. In 8 or 9 days time a plentiful suppuration came on, and his fever abated.

Towards the middle of January, the discharge of fetid pus was so great, not only through the wound, but also by expectoration, that he thought he could not long survive it: at each time of dressing, which was morning and evening, till the quantity lessened, full 6 oz. of pus were discharged: the like quantity he would generally cough up between the dressings. When the dressings were removed, he frequently made him force a cough, and try if he could not throw out any pus by his mouth; but, instead of passing that way, it flew out through the wound, like water from a pump: if he blocked up the wound with tow, he could then freely discharge it by the mouth. When the matter had done flowing, the air which was forced through the wound by coughing, would blow out a candle, as was often experienced. The matter was so very fetid, that, for some time after he was dressed, the stench in the chamber was scarcely tolerable;

and it was near the middle of March before the discharge began to abate. In this space of time he coughed up 25 shot; had frequently hectic heats, and night-sweats; a quick feverish pulse returned constantly towards evening, with great thirst; he had lost his appetite, and was greatly emaciated; his chief food was milk, and physic the bark.

In order to give a free passage to the matter through the wound, and prevent the stench from killing him, as the poor boy expressed it, by coming through the mouth, for some time a cannula was kept in the wound; but in less than a fortnight, it was obliged to be left quite out; for though it answered the end of giving the matter a free passage that way, and prevented its coming by the mouth, yet the quantity through the wound increased daily, and his hectic heats became more violent.

Seeing no prospect of any end to the discharge of matter, it keeping up to its usual quantity for a month or 6 weeks longer, and the poor boy reduced to a mere skeleton, he was determined, if possible, to heal the wound, and commit the event to nature; there not being one favourable symptom to give the least hope of his recovery.

About the middle of June the wound was quite cicatrized, 2 or 3 exfoliations being cast off from the scapula. His cough still continued with a discharge of the same fetid pus, but in 3 weeks it began to abate; and towards the latter end of July, he had gained flesh, and his cough had left him; he walked abroad, and was, to appearance, quite recovered. But this fair prospect did not last long; for towards the latter end of August, he was called to him in the night, and found him supported in the bed, with a half-pint basin in his hand, almost full of the same sort of stinking putrid matter, which he used to cough up: it had been emptied but a quarter of an hour before, so that in less than half an hour he had expectorated a full pint. This cough continued on him 16 hours longer; when, the load of matter being pumped up, he grew much better. Two or three days before this severe attack, he had complained of being faint, feverish, and strait at the breast, for which he was bled, &c. In this fit of coughing, he brought up with the pus 14 shot. He had three of these violent returns before the summer was quite over, which reduced him nearly to his former weak state, but discharged no shot.

In November following, he laid on a caustic to the cicatrix of the wound in his back; and kept it open with a large bean, to try if a discharge, by way of issue, might divert the matter from coming by the mouth: he had no such violent seizures afterwards, but still a hectic cough on him, which expectorated a small quantity of the same fetid pus: the discharge from the issue was pretty considerable, and he weathered out the winter tolerably well.

In March 1739, he became feverish, and complained of a great load and pain just above the diaphragm, on the left side: Mr. P. applied a warm plaster, and drew off 10 ounces of blood, which was pretty much inflamed. A few days afterwards an abscess formed between the ribs, where he had before complained of the pain; which was opened, and discharged about 4 oz. of the same fetid matter, and 18 shot. Here was a true empyema, and there were now great hopes of a cure; nature having pointed out such a depending part, for a discharge of what matter might be lodged in the thorax. He then healed up the issue in his back, and kept this new wound open with a cannula; but within 10 days, the matter had ceased flowing, his feverish symptoms again increased, and his cough returned with a discharge of the same putrid matter. He threw aside the cannula, and healed the wound between the ribs, it answering no end to keep it longer open. The remaining part of the year 1739, he had several returns of his cough, with pretty large expectorations, but they were not of long continuance; frequent bleedings, a milk diet, and vulnerary medicines, were made use of.

In the years 1740, 41, 42, towards spring and autumn, he generally was seized with a difficulty of breathing, and pain of the side: bleeding would relieve him for the present, but it seldom ended without a discharge of the same sort of pus by coughing, and with it sometimes one or two shot. At the latter end of the summer 1741, he had an abscess formed in the left side, between the true and spurious ribs. Mr. P. opened it, and with the matter discharged nine shot.

Between these grand fits of coughing, which happened 3 or 4 times in the year, he would gain strength, grow fat, and work at his trade of glove-making.

Towards the latter end of March 1743, his cough returned again with the same usual violence, and the discharge in one night was a full pint of fetid bloody pus; half that quantity was expectorated next day. He had the same feverish symptoms a few days before this cough, as usual, but rather more severe. It continued on him 8 days before it began to abate. No shot were discharged at this time, as was expected; but he coughed up a broad scale of a bone, ragged at the edges, and of an irregular shape, which Mr. P. imagined was a part of the scapula. A few weeks after this he was taken into the hospital.

Thus far Mr. Peters. He had the benefit of the Devon and Exeter hospital, under Dr. Hallett's care for 15 months; during which time he was hectical, had sometimes pleuritic pains, for which he was often bled, and took soft pectorals. He frequently spat pus in great quantities. He was confined to a milk diet; ordered balsamics, particularly bals. locatelli in an electuary. He was afterwards healthy, strong, and fat; and could walk from Topsham to Exeter, which is near 4 miles, returning the same day.

Observations concerning the Salt-Marsh Muscle, the Oyster-Banks, and the Fresh-Water Muscle, of Pennsylvania. By Dr. John Bartram. N° 474, p. 157.

There is something of an extraordinary nature in our salt-marsh muscle: by its fibrous roots, which strike deep into the soil, it seems to be of a vegetable nature; for, it is highly probable, the animal draws some part of its nourishment through them: they are fixed by these two thirds of their length in the sand, with their broad ends uppermost, which open at every return of the tide, to be replenished by the salt-water: when it is retreated, they are found lodged in the grass, sedge, creeks, and banks, singly and together in plenty.

On viewing one of them, we plainly observe the ligaments draw their origin from the principal parts of the animal, and unite near the extremity of the shell, which they pass through on that side of the muscle that opens to let in the water; then they divide again into many capillary roots or fibres, which penetrate and extend themselves into the mud or soil of the marsh; which, by long observation, seem intended for two uses; first, as I have above observed, to convey part of their nourishment; which seems probable, by their being dispersed through the body of the muscle.

The other use of these fibrous roots, for so I must call them, by their striking deep into the mud or sand, is to secure the creature from being carried away by the rapidity of the tide: so that, in this circumstance, they are somewhat analogous to plants, whose roots both nourish them, and secure them from the injuries of wind and flood.

Our oysters are of an oblong figure; they grow at the sides and bottoms of creeks, rivers, and bays, near the sea; but mostly in such a situation where they are near or quite dry at low water: they have the power of opening and shutting, like the muscle, to take in and retain the salt-water, which is their principal nourishment: though they stick in the mud, they are not so secured as the salt-marsh muscle beforementioned; and though these oysters grow in great clusters or heaps, commonly called oyster-banks, yet every one that is alive has free communication with the air and water, and liberty to open and shut. If the oyster's way of growing may be compared to that of a plant, I think there is great similitude between it and the opuntia, or Indian fig; a leaf produces and supports a leaf, and so on. thus the young oysters grow on the sides of the old one, which, by degrees, is so deeply immerged in the mud, that it dies; but yet it serves to support the young ones upright, till they come to maturity to produce others; and then those by degrees, subside; so that, by this method, banks of dead and living oysters are extended of an inconceivable length and breadth through all our coasts.

Our common fresh-water muscles differ from our salt-marsh muscle, in that they are not fixed to any place or thing, but have a method of trailing along on the sandy bottoms of creeks and rivers: they have the power not only of opening and shutting their shells at pleasure, but have also the power of creeping* along as it were like a snail, by turning on the upper edge that opens, and so work themselves along the soft yielding sand in little furrows about half an inch deep. I have traced them for several yards, by these little channels, when the tide is down, and left the sands bare.

Of an ancient Roman Inscription at Rochester in Northumberland, and two others at Risingham; dated Durham, Sept. 9, 1744. *By Mr. Christopher Hunter.* N° 474, p. 159.

[An imperfect inscription in a stone, unexplained.]

Some Magnetical Experiments, shown before the Royal Society, Thursday the 15*th of November,* 1744. *By Mr. Gowan Knight.* N° 474, p. 161.

Mr. Knight, of Magdalen-College, Oxford, being introduced to a meeting of the Royal Society on Thursday the 15th of November, 1744, produced, before the gentlemen there present, several curious artificial magnets contrived by himself; some of which consisted of plain bars of steel naked, and others of bars or blocks of the same substance, armed with iron after the common manner of natural loadstones: but as he was apprehensive the trials he had before made of the weights these magnets were respectively capable of lifting, could hardly be repeated with sufficient exactness and advantage before so large a company, he desired to refer himself, for those particulars, to what the president of the Society had seen at his lodgings on Wednesday the 7th, and on Tuesday the 13th of the same month of November.

On which the president acquainted the company, that he had lately been several times at Mr. Knight's lodgings, where he had seen many experiments made with his artificial magnets; and that, particularly on the days abovementioned, he had been present, and had taken minutes of the following trials then made by that gentleman; by which it appeared, that,

A small eight-cornered bar of steel, of the length of $\frac{7}{10}$ inches, and about half an ounce Troy weight, lifted by one of its ends about 11 of the same ounces.

That another plain bar of steel of a parallelopiped form, of the length of $5\frac{8}{10}$ inches, the breadth $\frac{4}{10}$, and the thickness $\frac{2}{10}$ of an inch, weighing 2 oz. $8\frac{1}{4}$ dwt. lifted by one of its ends 20 Troy ounces.

That a steel bar, almost of the same form as the last, but only 4 inches in length, capped or armed with iron at each end, cramped with silver, and weighing all together 1 oz. 14 dwt., liftod by the feet of the armour full 4 lb. Troy.

* I have seen this of our horse-muscles in ponds here in England. C. M.—Orig.

That a single block of steel of a parallelopiped form, almost 4 inches long, $1\frac{8}{10}$ inches in height, and $\frac{4}{10}$ of an inch in thickness, armed with iron, cramped with brass, suspended by a ring of the same, and weighing all together 14 oz. 1 dwt. lifted by the feet of the armour 14lb. $2\frac{1}{2}$ oz. Troy weight.

That a compound artificial magnet was also tried, consisting of 12 bars of steel armed, and that it was found to lift by the feet of the armour as the last, 23lb. Troy, $2\frac{1}{2}$ oz.

The 12 bars, composing this last magnet, were each a little more than 4 inches long, $\frac{3}{10}$ of an inch in breadth, and $\frac{16}{100}$ of the same in depth, weighing one with another about 25 dwt. each. They were all placed one on another, so as to make together one parallelopiped body, of the common length and breadth of the several bars, but of the height of near 2 inches, being the sum of the respective thicknesses of all the bars taken together: and this parallelopiped body, being cramped with brass, and fitted with a handle of the same metal, was armed at the 2 ends that were made up of the common extremities of all the bars, with 2 substantial pieces of iron, after the common manner of arming natural loadstones, the whole frame weighing together about 20 Troy ounces.

Besides these, the president made also the following report of some trials he had seen made at the same time of the effects of an art Mr. Knight is master of, by which he can improve or increase the lifting powers of natural loadstones.

He carried with him, on Wednesday the 7th of November, a small armed loadstone belonging to an acquaintance, which weighed, with its armour, 7 dwt. 14 gr.; but which, being reputed but of an ungenerous nature, took up, and with some difficulty, barely 2 ounces. Mr. Knight took it into his study, and returning it in about a minute, it then took up more than 4 ounces with ease: but, on his saying it would still gain some more strength, by remaining with him some time, it was left till the 13th, when it took up distinctly, with the same apparatus as before, 6 oz. 18 dwt. 3 gr.; since which time it has also several times been found to lift nearly the same quantity.

Mr. Knight further, at the same time, showed the president the following instances of his ability to invert or change the direction of the poles in natural loadstones.

Such a stone belonging to Mr. Francis Hauksbee, weighing about 5 oz. 14 dwts. of an irregular cylindrical form, with 2 of the sides somewhat flatted, on which armour had formerly been applied, had the direction of its polarity from one of these flatted sides to the other, notwithstanding the stone had a distinct grain running at right angles to that direction. It was tried, and observed that one of these flatted sides strongly attracted the north end, and repelled the south; and that the other attracted the south, and repelled the north end of the magnetic needle. The end of the stone, attracting the south end of the needle, was then marked, by the rubbing of a piece of silver upon it, as on a touchstone: after which, Mr. Knight carried the stone into his study; and reproducing it in about a minute, showed that the poles were then directly inverted; and that the same end, which before attracted the south end of the needle, now attracted the north, and repelled the south, and vice versâ.

After this, Mr. Knight again taking the stone, brought it back in as short a time as before, with the direction of its polarity turned at right angles to its former direction, and into the direction of the natural grain of the stone, the poles now lying in the flat ends of the cylinder; one of which, being the smoother end, attracted the south end of the needle, while the other, which was of a rougher texture, attracted the north end, and repelled the south end of the same: when it was also observed, that the polarity appeared stronger in this case, than either of the former.

Lastly, Mr. Knight, in about the same time, inverted this last direction of the poles, keeping it still parallel to the axis of the cylinder, but causing the smooth end of the stone to attract the north end of the magnetic needle, and the rough end to attract the south, and repel the north end of the same needle.

After this report, Mr. Knight proceeded to show, at the meeting, some of the same artificial magnets there mentioned; and it was found, that the compound magnet, consisting of 12 steel-bars, and which had, in the experiment made before the president, lifted 23 lb. 2¼ oz. Troy weight, did here, under all the inconveniencies and disadvantages of a crouded room, still lift a weight amounting to 21 lb. 11 oz.

It was also found, that the single armed block of steel, which had before lifted 14 lb. 2 oz. did here, under the same disadvantages as the former, lift 13 lb. 7 oz.

And lastly, Mr. Knight produced to the company the abovementioned natural loadstone belonging to Mr. Hauksbee, but with the direction of its polarity again altered from what it was, when it was last seen by the president.

P. S. Since the artificial magnets mentioned in the foregoing paper, Mr. Knight has caused some others to be made of a less size, but of a very great lifting power: and one of these, weighing, without its armour, just an ounce, and with the armour, cramps, and rings, 1 oz. 17 dwt. lifted 6 lb. 10 oz.

This magnet consisting of 3 plates of steel, each 2 inches long, $\frac{7}{10}$ of an inch in breadth, and not above $\frac{6}{100}$ of an inch in thickness: they were laid flat on each other, and screwed together by 2 small brass screws going through the 3 plates. After which, the little parallelopiped block so made up, was armed with iron at the 2 ends, cramped together with silver, and fitted with a double ring of the same metal, for the convenient holding of it.

Account of a Book concerning Electricity, just published at Leipzic, 1744. By John Henry Winkler, Greek and Latin Professor there. N° 474, p. 166.

The electrical sparks from metals, such as iron and silver, are capable of kindling all such fluids as may be otherwise kindled by actual flame. And this experiment succeeds best, when the quinta essentia vegetabilis is held in a spoon under the cross of a sword, its point being turned towards the electrifying glass. In like manner, the same spirits may easily be set on fire, by the sparks proceeding from an electrified tube of tin.

This experiment with the sparks coming from metals when made electric, was first made by Dr. Ludolph, of Berlin; who, toward the beginning of the present year 1744, kindled, with the sparks excited by the friction of a glass tube, the ethereal spirits of Frobenius. This account was not only related in the Berlin Gazette, of the 30th of May last; but has been since confirmed by several letters, sent from Berlin to Leipsic, to Count Manteufet, immediately after the experiment.

Mr. Marscall, who now studies here, also communicated to me a letter he had received from Berlin concerning the same; and I have since been also certified of it, by the account of several men of learning, that had seen the experiment at Berlin, and that have since visited me at this place.

Lastly, Mr. Reinhart, who came hither about last Easter, being told that the experiment was not difficult to be made; and that the liquor, called quinta essentia vegetabilis,* might very readily be kindled by the electrical sparks; Mr. W. immediately sent for some of that essence, and found the experiment succeed to his wish.

Red-hot iron sets no spirits on fire, though held very near to those spirits; but if that iron be made electric, its electric sparks very readily kindle all well-rectified spirits.

The sparks that proceed from the body of a man, made electrical, kindle spirits as quickly as those from electrified metal, whether the body of the man is rendered electric immediately by the glass tube, or by the intermediate tube of tin.

Mr. W. made this experiment with success on himself. Neither himself, nor any of the company, knew, at that time, that the electric sparks, from the body of a man, were capable of kindling spirits; but on seeing the quinta essentia vegetabilis kindled with extraordinary quickness, by the sparks proceeding from an iron tube that was rusty, one of the company started the question, whether the sparks from the body of a man, might not possibly do the same? On which Mr. W. immediately stept on to a frame, over which blue silken lines were ex-

* i. e. Spirit of wine so highly rectified, as, being poured upon gunpowder, and then being set on fire, will at last flash the gunpowder. C. M.—Orig.

tended: he took hold with one hand of the rusty iron tube, and held the fingers of the other over some of the quinta essentia; and the sparks from his fingers immediately struck with such violence into the silver spoon that held it, that the essence was in a moment set all in a flame.

This experiment, so unexpected, gave the greatest satisfaction to all the company; and an account of it was published in the Leipsic Gazette of the 21st of May; where it was also mentioned, that divers other experiments, with the sparks of electrified metal, had already been made both at Dantzic, and at Berlin.

Dead fowls, pork, and veal, both raw and drest, may be made electric by a tin tube, or by the hand of man; insomuch that the sparks, proceeding from those several bodies, will also kindle the same essence.

If such fluid bodies, as are usually kindled by flame, are not fine enough, they need only be warmed a little in the spoon: or the spirits may be lighted a little before, and blown out again, before they are brought to the electrical body. In this manner he kindled, with the electrical sparks, camphorated spirits of wine, coloured with saffron; the common essentia vegetabilis; and even French brandy, and corn-spirits, only taking the precaution of warming these liquors a little before. Even oil, pitch, and sealing-wax, may be lighted by the electric sparks, provided they are before heated to a degree that is next to kindling.

Observations on several newly discovered Species of Fresh-water Polypi. By M. Abraham Trembley, F. R. S. N° 474, p. 169.

We find, in divers places, on water-plants, and other bodies in the water, a whitish substance, that looks at first only like a sort of mould: we sometimes see plants, sticks of wood, snail-shells, and the like, that are entirely covered over with this substance. But if we take any of these, put them into a glass of clear water, and then examine with a magnifying glass what is upon them, we soon discover in the little bodies which by their assemblage, form this whitish substance, such motions as give sufficient reason to consider them as living animals; and this will appear yet more sensible, when they come to be observed with a microscope. We then find them to be minute bodies, severally fixed to the extremities of small stems, or pedicles, many of which are often so united, as to form together a sort of branches, or clusters; and this appearance determined Mons. De Reaumur to name the animalcula that appeared so fixed, clustering polypi; des polypes en bouquet. These clusters are larger or smaller, according to the species of the polypi that form them, and to the concurrence of many other circumstances.

To get a clear idea of the figure of these animals, it is best to observe the smaller clusters; as, in the larger, the great number of the polypi on the several stems, are apt to hide each other.

In one case where the polypi are simple, they are not above the 240th part of an inch in length, and are of a shape nearly resembling that of a bell: one of these is represented exceedingly magnified, fig. 1, pl. 2. The anterior part, ac, generally appears open, when it properly presents itself; the posterior part ib is fixed to a stem or pedicle be; and it is by the extremity e of this pedicle, that the polypus fastens itself to any other sort of body. The polypus of this sort generally appears to the microscope of a brownish colour, excepting at its smaller end b, where it is transparent, as well as its pedicle be. When the anterior part ac is open, we may perceive about its edges a very lively motion; and when the polypus presents itself in a certain manner, it discovers, on either side of these edges of its anterior part, somewhat very much resembling the wheels of a little mill, that move with great velocity. These polypi are able to contract themselves; and they do so often, and suddenly. They may be brought to contract at any time, either by touching them, or by moving the body to which they are fixed. When they contract, the edges of their anterior parts are drawn quite into their bodies; and when they resume, which they do soon after, their former posture, we distinctly see those edges come forth again, and put themselves in motion, as before.

On observing the anterior parts of these polypi, which are open, and whose edges are in motion, we may often remark a number of very minute bodies swimming in the water, that seem to be forced down with velocity into these openings of their anterior parts, and which sometimes are thrown out again from thence. To make this observation the most sensible, it is best not to look at a single polypus, but a cluster of a number of them together.

Though the polypi of the sort in question appear of a brownish colour when viewed with the microscope, yet, having left some of them for several days in the same water, they by degrees lost their brown colour, and became transparent; excepting only that a few grains or spots, of brown or black, still continued to be discernible in their bodies: but having afterwards removed these polypi into other water, newly taken out of a ditch, they in a little time resumed the same brown hue which they had before.

It may commonly be observed, that when the polypi are in water newly put to them, there fall upon their anterior parts far greater numbers of the above-mentioned minute bodies, than when they have been left for any time in the same water. It is very probable that these minute bodies are exceedingly small animalcules, on which the polypi feed; and that consequently the opening, which they have in their anterior part, serves them for the purposes of a mouth.

The polypi that have become transparent, and that have been left some time without the addition of such water as would make them recover their brown colour, have also, at the same time, left off multiplying. But others of them, to which has afterwards been given new water from the ditch, have soon after began to multiply again.

These polypi are capable of swimming about; and when they swim, they are no longer in clusters, but always single; and they do not then appear in the same form as when they are fixed, and open at their anterior ends. It is by swimming that they leave the place to which they first appeared fixed, and that they go and fix themselves to any other body that they find in their way.

We should begin to observe a polypus soon after it has fixed itself singly, in order to see regularly in what manner the clusters form themselves, and in what way these small creatures multiply. The stem or pedicle of a polypus that is yet single, and which has but lately fixed itself, is at first very short, but lengthens itself in a little time. After that, the polypus multiplies; that is to say, it divides or splits itself into two lengthwise. The lips are first drawn into the body, whose anterior part closes, and becomes round: the motion that was to be seen before the lips were drawn in, no longer appears; yet we may see, by looking with attention, a slow motion within the body, during all the time that the polypus remains closed. The anterior part of the polypus flats itself afterwards by degrees, and spreads in proportion, becoming broader as it shortens; it then gradually splits down through the middle, that is, from the middle of the head to the place where the posterior end joins to the pedicle: so that, in a little while, there appear two separate round bodies joined to the extremity of the pedicle that just before supported but one.

The anterior part of each of these bodies then opens by degrees; and, as they open, the lips of the new polypi show themselves more and more. Then is the time of observing these lips with attention, and of forming an idea of their true form, and of their motion. This motion is at the first very slow, it quickens as the polypi continue to open; and, as soon as they have done, it becomes as swift as that which appeared in the lips of the whole single polypus, before it began to divide: and then these new polypi may be considered as entirely formed. They are at first less than the polypus from which they were formed; but they grow to the same size in a very little time. A polypus is about an hour in dividing itself.

The lips of these polypi appear to be composed of 4 or 5 transparent stripes, having all an undulating motion. While the polypi are opening, and that the motion of their lips is yet but slow; we see on either side, when they are in a certain position, what seem to be the wheels of a mill, in the polypi that are quite formed, and whose lips move very fast: but we now see, while they are opening, what may be rather taken for 4 or 5 fingers on either side of their mouths; which alternately bend down and extend themselves every instant, and to which the transparent stripes abovementioned appear to be fixed.

When the first polypus is thus divided, and the two new ones produced by this operation are thus completed; we see on one pedicle two polypi, joined to its extremity by their posterior ends, and that show themselves on the sides of each other, as in fig. 2. Where the ordinary proportion between the length of the body of one of these polypi and the length of their pedicle, is pretty exactly observed. Soon after the separation is completed, each of the new polypi begins to show a pedicle of its own. And each of the new polypi has, the day after their separation, a pedicle of a tolerable length; and these new pedicles united at the extremity of the first pedicle, as the branches of a tree unite at its trunk. Several of the polypi have multiplied at the latest 24 hours after their first separation. The new cluster has then consisted of 4 polypi, each of which had its own pedicle; as every one has also had, that was afterwards produced by a new separation.

Fig. 3 represents a cluster of 8 polypi; and by this figure it may be apprehended in what manner the pedicles of the polypi become disposed, as their numbers increase. These several pedicles become so many branches of the cluster or sprig. This figure particularly represents a cluster, whose progress M. T. followed in the month of September last, 1744. It consisted, on the 9th day of that month, of only one single polypus, which was placed as at b: this polypus divided itself that evening, and at half an hour after 8 o'clock, there were to be discovered at b two perfect polypi, whose pedicles or branches, bd, bd, continued lengthening till the morning of the next day, being the 10th of the same month of September: at about a quarter after 9 that morning, these two polypi, which were then at d, d, began also each to divide; so that at a quarter past 11, there were at d and d 4 complete polypi, whose several pedicles di, di, di, di, formed themselves soon after. On the 11th of the same September, about half an hour after 7 in the morning, these last 4 polypi had already again divided themselves; that is, that there were at i, i, i, i, 8 distinct polypi; and this cluster, so consisting of 8 polypi, is here represented as it appeared on the 12th of the same month, between 10 and 11 in the forenoon. The polypi are not always ranged as they are disposed in this figure, for it often happens, that the pedicles and the polypi are behind each other, so as to form a groupe, in which some of the polypi may chance to be hidden or covered by others, either entirely or in part. This figure represents the polypi and pedicles as magnified to the same degree as those already exhibited in the former figures.

M. T. has taken notice of clusters, the numbers of whose polypi have constantly gone on doubling, from 2 to 4, from 4 to 8, from 8 to 16, from 16 to 32: after which he has no longer been able to count exactly the number of the polypi. Indeed the number is prodigious of those that are sometimes found in the water, thus multiplied. He had large glasses in which they had exceedingly multiplied; there was particularly, in one of them, a cluster composed of several less united clusters, which is above an inch over every way. There detach themselves from time to time single polypi, which go swimming about till they fix each on some body or other; and from these there again arise new clusters, in the manner as above. The branches, from which polypi have detached themselves, still remain fixed to the cluster, but they bear no more polypi; and after all the polypi of a cluster have thus detached themselves from it, the assemblage of the branches still subsists, but is of no further use.

Mr. T. knew of 4 other species of polypi, that all increase in the same manner as those abovementioned; that is, which split and divide themselves according to their length. Those which come the nearest to the first, are somewhat more slender, and the branches of their clusters are transparent; yet do they appear, when there is a number of them together, of a changeable violet-colour: the clusters of these bear a good resemblance to a sprig or aigrette of spun glass. When these last animals are completely formed, it is not so easy to see distinctly in them the motion of their lips, as it is in the other species beforementioned; yet may it be observed in these also, while they are still opening, and completing their formation: for at such times, this motion is but slow, whereas it becomes afterwards very quick in those that are entirely perfected.

The polypi of the other species that he has observed, are yet less than the last: they are shorter, but more open and hollowed at their anterior ends. These have a character that sufficiently distinguishes them from all the other species: their stems and branches have a motion that is not to be found in those of the other polypi. These stems draw themselves up, and shorten all at once, taking the form of a spiral wire or screw; and a moment after they again resume their former shape, stretching themselves out straight as before. These several species of polypi, all multiply in vast abundance; but they have also enemies that destroy immense numbers of them, and that in a very little time.

Mr. T. also observed regularly other small polypi, of a different sort from those that are found in clusters. These are nearly in shape like a tunnel, pretty long in proportion to the opening of their larger ends. For this reason, Mr. De Reaumur has thought proper to distinguish them by the name of tunnel-like polypi. He is acquainted with 3 species of these last polypi, which are respectively, green, blue, and white. Their anterior end particularly, is of a far more compounded shape, than may at first be imagined. There may be observed round the edges of this part, a sensible motion, much resembling that of an indented wheel, or rather of an endless screw, turned very fast about. These tunnel-like polypi form no clusters, like the others. The little bodies, that pass swimming near the anterior parts of these insects, are in some manner drawn into the mouths of their tunnels; and sometimes a considerable number of very small round animalcula fall one after another into these openings. Some of these were indeed afterwards let out again, at another opening: but it could plainly be seen, that many of these little round bodies remained within the bodies of the polypi; and it is therefore apparent that these little bodies, so taken in, became their food.

These tunnel-like polypi also multiply by dividing into two, but they divide otherwise than the clustering polypi: they neither divide longitudinally nor transversely, but sloping and diagonal-wise. Of two tunnel-like polypi, just produced by the division of one, the first has the old head and a new posterior end; and the other the old posterior end, with a new head.

We may call that which has the old head, the superior polypus; and that which has the new head, the inferior one. The first particulars observable in a a tunnel-like polypus that is going to divide, are the lips of the inferior polypus; viz. those transparent edges that are so conspicuous in the polypi when entirely formed. These new lips first discover themselves on the polypus that is going to divide, from a little below the old lips, to about two-thirds of the length of the polypus, reckoning from the head: but these new lips are not disposed in a straight line, according to the length of the polypus, but run sloping near half-way round about. These lips are known by the motion in them, but which motion is at first very slow. That portion of the body of the polypus, that is bounded by these new lips, then gathers itself up, the new lips insensibly draw together and close; by which there forms, at the side of the polypus, a swelling, that is soon found to be the head of the new one, bounded by the new lips first discovered. Before this swelling is become very remarkable, we begin to distinguish the 2 polypi which are forming; and when that swelling is considerably increased, the 2 polypi will be discovered, only joined by a small portion to each other. The superior polypus no longer adheres to the inferior one, but by its posterior extremity, which is still fixed on one side of the inferior polypus: the superior polypus then begins to make motions that seemingly tend to his separating from the other; and in a little time he becomes quite detached, swims away, and fixes himself elsewhere. Sometimes one has come and fixed at the side of the inferior polypus, from which he was just before separated. The inferior polypus remains fixed in the same place, where the polypus was that is now divided, and of which he was only the half, before the division took place.

Observations relating to Vegetable Seeds. By James Parsons, M.D., F.R.S. N° 474, p. 184.

The first here examined, is the seed of the musk scabious, which, for its shape and structure, is amazing. It resembles an octagonal vase with a scalloped brim: the whole is bell-shaped, having ribs or divisions, which run down from the mouth of the vase, and becoming narrower, form the bottom: between these ribs, down to the beginning of the narrow part, it is clear, though not quite transparent; and, from thence to the bottom, the ribs are hairy. This vase contains a seed, which is like a pestle standing in a mortar: the pestle is loose, in an octagonal case; but the narrowness of the mouth of this case hinders the pestle's being drawn out, because its extremity within is round and bulky. From its upper end arise 5 spiculated aristæ, whose little thorns are directed upwards, and are thereby prepared to cause the seed to recede from any thing that might injure it on being touched; and the basin, from which the aristæ rise, is of a fine green colour. They are of a shining brown.

The 2d specimen is that of the angelica. It is one of the most fragrant and agreeable seeds, for its smell, in the world. When the husk is pulled off, the nucleus appears of a brownish colour, and its shape is elliptical. By the help of the microscope, we know what produces that charming smell, being a fine amber-coloured gum, which appears in ridges disposed alternately, with others of a brownish colour, in a longitudinal direction all over the nucleus. What appears white, on the flat side, is a theca, which receives a very minute stilus from the pedicle that supports it.

The 3d is that seed which is vulgarly called grains of paradise. This seed, though promising from its aspect but very little that is curious, being only a brown irregular seed with flats and angles, and having an apex like the mouth of a purse drawn up with a string; yet, when dissected, nothing can produce a more beautiful appearance. In a longitudinal section, you see first the edge of the brown cortex; next to that, a black pitchy substance: and within that an exceedingly white radiated matter, which looks like a fine white salt, and is probably a mixture of a volatile pungent salt with a farinaceous substance: the radiation seems to confirm this opinion; for if it were only a farina, it could have no such appearance, and so does its exceedingly sharp taste. But the most remarkable and curious part of this seed, is a little piece of camphor, exactly shaped like a common vinegar crewet, having a round bottom, and a long taper neck. This is the constant form in hundreds of these seeds he has cut. These curious appearances were probably never observed before.

The next seed is that of the great maple-tree. It consists of a pod and its wing: two of these grow on a foot-stalk with the pods together, which makes them resemble the body of an insect with a pair of expanded wings. The wings are finely vasculated, and the pod is lined with fine silky down, which contains a round compact pellet covered with a brown membrane, that sticks very close to it. When this is peeled off, instead of discovering a kernel, as in other seeds, an entire green plant appears to be folded up in a most surprising manner, whose pedicle is about $\frac{1}{4}$ of an inch long, and its seminal leaves about $\frac{3}{4}$ each; between which the germina of the next pair of leaves are barely visible to the naked eye, but plain with a microscope. This discovery gave great pleasure, believing himself the only one who had observed it; but, some time after, looking into Derham's Physico-Theology, he found it mentioned, as if Dr. Highmore had seen and communicated it to Mr. Ray.

A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1742, *&c. By Joseph Miller, Apothecary.* N° 474, p. 189.

[This is the 21st presentation of this kind, completing the number of 1050 different plants.]

On the Manuring of Land with Fossil Shells. By the Rev. Roger Pickering of Hoxton, F.R.S. N° 474, p. 191.

At Woodbridge in Suffolk, in a farmer's ground, there are some pits, in depth equal to the usual height of houses, consisting of several strata of shells, from the bottom to within about 9 feet of the surface, where the natural soil of gravel and sand begins. The mass of shells here collected is prodigious; the sorts various; but the buccinum vulgare, or whilk, prevails the most. The depth to which these shells reach is not yet dug down to. Woodbridge is seated 7 miles N. E. from Ipswich; and is about the same distance from Orford on the sea coast, which bears from it due east. How such a mass of shells should get there at such a distance from the sea, when history has not informed us of any remarkable inundation in those parts, or that such a tract of land was ever recovered from the sea, appears difficult to determine, by any other than the Mosaic hypothesis of a universal deluge. Indeed the river Deben, which rises at Debenham some miles off, runs by Woodbridge, within half a mile of these pits, in its course to the German ocean, where it empties itself: but such a collection of shells can hardly be supposed to have been thrown up by it, and a surface of earth to the depth of 9 feet, settled over it, without allowing a space of time for such a circumstance, almost equal to the interval between us and the deluge.

The farmer of the ground has, it seems, laid the foundation of an ample fortune from them. He contented himself in the old beaten track of the farmers, till a happy accident forced him on a bold improvement. He used to mend his cartways, when broken up by harvest-work, with these shells; in which business his cart one day broke down, and threw the shells out of the cart-track into the cultivated part of the field. This spot produced so remarkable a crop next year, that he put some loads on a particular piece, kept the secret to himself, and waited for the event. This trial answering expectation, he directly took a lease of a large quantity of poor land, at about 5 shillings the acre; and having manured it heartily with these shells, in about 8 years it turned to so good an account, that he had 15 shillings the acre proffered to take the lease out of his hands.

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Of a Shuttle-spire taken out of the Bladder of a Boy. By Mr. Wm. Arderon, of Norwich. N° 474, p. 194.

Mr. John Harmer, a surgeon in this city, cut one Peter Riggs, a boy about 17 years of age, for the stone; at which time there was extracted from him an iron shuttle-spire, 4 inches long. He had, it is said, some time before, a stoppage of urine; and, by endeavouring with this piece of wire to relieve himself, and thrusting it too far along the urinary passage, he let it slip into the bladder, where it occasioned the same symptoms as a stone would have done. He underwent the operation with great fortitude; and said nothing of this accident till it was all over. He is now perfectly recovered.

A Remarkable Cure, performed on the Eye of a Young Woman in Scotland. By Tho. Hope, M.D. Communicated from Dr. Mead. N° 474, p. 194.

Jane Willson, a girl then 18 years of age, about 7 years before began to have her left eye turned towards the temple, occasioned by some tumour between the globe and the orbit. This tumour, for some years, did not appear outwardly; but increasing by degrees, at last a hard swelling appeared externally, reaching from the great angle almost to the little angle under the lower eyelid, and half an inch down on the cheek: it had forced the globe of the eye almost out of the socket, so that the pupil of that eye was, by measure, above $\frac{3}{4}$ of an inch further from the nose, than the pupil of the other eye; and the eye was more jetting out in proportion; so that it seemed to be out upon the temple, and quite immoveable; which, with the tumour, made a frightful sight. The patient had frequent pains in her head; but yet the sight of that eye was not lost, though a good deal impaired.

Dr. H. showed this patient to Mr. Alexander Monro, professor of anatomy at Edinburgh; who, after examining it very narrowly, gave it as his opinion, that this tumour had begun at the bottom of the orbit; and that the extirpation would be exceedingly difficult; and, as it seemed to be an incysted tumour, if any of that cystis remained at the roots, it would be apt to sprout up again; but concluded that there was room for a trial; and it would be a pity not to do something in order to save the patient's eye, and probably her life, which would be in danger if the tumour continued to increase.

Considering the great risk that the patient ran, if something was not speedily done, Dr. H. resolved to undertake it; having had a case of the like nature, but in a less degree, under his care about 12 years ago in London, the extirpation of which he performed without any bad consequence; and on consulting the book of his old master St. Ives, found almost a parallel case to this girl's, which he says he extirpated with success.

Accordingly Dr. H. performed the operation in the following manner: He turned the patient backwards on a chair on an assistant's lap, her head supported by pillows; then keeping the skin tense with his fingers, made an incision about an inch long with a small razor, beginning at the greater angle, and following the direction of the fibres of the orbicular muscle towards the lesser angle. He then passed a crooked needle armed with silk through the middle of the tumour as deep as he could go; and raising the tumour with the silk, with a fine bistoury, he separated all the lateral adhesions from round the tumour; and with the point of the scissars, he cut the deeper adhesions, which could not be so well reached with the bistoury, and brought away all that the thread had hold of. This seemed to be a tough membranous substance, independent of the real tumour: for, after this was quite taken out, there appeared a regular tumour, of a spherical figure, smooth and even, about the size of a small pigeon's egg: he passed the needle through the middle of it, as before, and plunged a lancet into it as deep as he could, in order to let out any fluid matter it might contain, but found nothing but a carnous substance; then lifting up the tumour by the thread, dissected it, with great care and caution, from the adjacent parts, as far as he could; in doing this he found several strong callous attachments on the side next to the globe, which felt almost as hard as a cartilage, and obliged him to change 2 or 3 instruments. He then, with the point of the scissars, cut the inward adhesions at the roots, and brought the tumour away entire. On putting in his finger to the bottom of the orbit, he could feel several hard callous substances still remaining; and keeping his finger on them, he slid a crooked needle armed with silk round the point of his finger, with which he hooked those callous roots; then making an assistant raise the thread, and directing the scissars on the point of his finger, where he felt the roots, with 2 or 3 snips he cut them quite away; so that he left the bottom even, and quite free, as far as he could judge. All this while he had no great effusion from any artery, but a good deal of black grumous blood from the varicose vessels.

He dressed it up the first time with dry lint, which he did not take off till the 3d day; when he found a soft swelling in the eye-lids and conjunctiva, with a slight inflammation and a pain in the forehead. He dressed the wound with a soft dossel, dipped in common digestive and warm brandy, and ordered an emollient fomentation to be applied every 2 hours: the pain in the forehead, and the swelling continued for 3 or 4 days, without any appearance of matter. He then touched the bottom of the wound with the lunar caustic, and some hours after there followed a pretty large discharge of blackish blood, and immediately her head was relieved, and the swelling subsided: a bloody sanies continued to issue out the 2 following days, for which he injected warm water, with a little brandy and honey of roses, after which it came to a pretty good digestion. As some spongy soft substances began to appear, he touched them with the lunar caustic, and the wound filled up apace. The eye still continued immoveable, them usculi adductores had been so long contracted, and the abductores so overstretched and lengthened, that they had lost their use; he could however observe that, by pressing with his hand on the globe of the eye with a little force, he could bring it a good deal more into the socket, but on taking away his hand, it would immediately return to its former place. This made him think that a constant and gradual pressure, by some proper bandage, might be of service to force the globe into its place, and keep it there till the muscles had recovered their tone: accordingly he got a steel bandage, with a concave brass plate corresponding to the convexity of the eye; which, by the means of a screw, bore on the side of the globe next the temple. He applied this bandage, by first gently forcing the globe more into its place with his hand; then putting a thick soft compress between the globe and the brass plate, screwed it down on the globe in such a manner, that it was impossible for it to start back again as it used to do.

He left an assistant with the patient all night, with instructions—if the bandage caused great pain, to ease the screw; and so, by gradually forcing it more and more, and keeping this bandage constantly applied day and night, in about 20 days the eye was brought quite into its place, so as to remain there of itself, had all its regular motions every way, and the patient saw with that eye as well as with the other.

In the morning, when he used to take off the bandage, he could observe that side of the globe which the plate bore on considerably flattened, and yet not attended with any pain, or bad consequence. In about a month the wound was quite healed up. A spongy carnosity had grown all along the inside of the lower eyelid, which, being long over-stretched by the tumour, was so relaxed, that after the operation it turned inside out, and occasioned that disorder which is called ectropion: the upper eyelid having been very much extended for so many years by the globe, on the eye returning to its place, was so relaxed, that its cartilage, on the contrary, turned inwards; by which the cilia or hairs on its borders rubbed against the globe of the eye, and occasioned the disease commonly called trichiasis. For the cure of the ectropion he passed a crooked needle through the middle of the carnosity, and raising it by the thread, he cut it off with the scissars; he afterwards touched the inside of the eyelid with the lunar caustic, to destroy what remained of the carnosity, and giving the eschar time to throw off, he repeated the same twice or thrice, by which the eyelid, in about a fortnight, recovered its healthful and proper situation. He did not think proper to torment the poor patient with the operation of the trichiasis; which, though very easy to the operator, is not so to the patient; and he found, by the application of proper topics, the eyelid recovered strength daily; and he judged, by the continuance of the same method, it would soon be well.

Of a Roman Inscription found on a Stone at Silchester in Hampshire. By John Ward, F.R.S. N° 474, p. 200.

The draught, which accompanies this paper, contains an exact copy of a Roman inscription. The account of it is this: the original stone, in which it is cut, was found at Silchester, within the ancient market-place, about 4 feet under ground.

From the usual form of such votive inscriptions, and the manner of expressing them, he apprehends there are not many letters lost at the end of the lines, and but one line wanting at the bottom; so that if all the words were written at length, and the line which is wanting supplied, the whole would run in the following manner:

Deo Herculi Segontiacorum Titus Tammonius, Saenius Tammonius Vitalis, cornicularius, honoris causa dedicarunt *vel* fieri curarunt.

We find no less than 6 altars dedicated to Hercules in Mr. Horsley's Britannia Romana, two of which have the title DEO prefixed to the name HERCVLI, as in this inscription. But as the thinness of the stone shows that it could not be part of an altar, it might probably belong to some public building erected to his honour at this place.

The word SEGON. in the second line, must probably be read SEGONTIACORUM as referring to the name of the people Segontiaci. Thus we have in Mr. Horsley DEAE NYMPHAE BRIGANTVM, and MOGVNTI CADENORVM; and in Mr. Camden DEO MOVNO CADENORVM; denoting the topical deities of those people. For as to the town Segontium, notwithstanding the affinity of its name with the Segontiaci, it was at a great distance from them, as appears by Antonine's Itinerary, being situated on the western coast over against the isle of Anglesea, where Caernarvon now stands; and therefore it could have no relation to this inscription.

The following lines contain the names of the two persons, who caused this dedication to be made in honour of Hercules, that is TITUS and SAENIVS TAMMONIVS, that of VITALIS in the first line being a cognomen of the latter, which often occurs as such in Gruter.

Had this inscription been sooner discovered, it would have saved our antiquaries much trouble in fixing the situation and limits of the Segontiaci; about which they have been greatly at a loss, and led into different opinions. Those people are first mentioned by Cæsar; who in the account of his second expedition into Britain says, that the Trinobantes having submitted to him, the Cenimagni, Segontiaci, Ancalites, Bibroci, and Cassi, followed their example. The Trino bantes are placed by Mr. Camden in Middlesex and Essex, and the rest in the neighbouring counties on each side the Thames; the Segontiaci particularly in the north part of Hampshire, in Holeshot hundred. And he rightly takes Vindomum or Vindomis, as it is called by Antonine, now Silchester, to have been their principal town. But though Dr. Gale agrees with Camden in making Silchester the same as the ancient Vindomis; yet he thinks that town did not belong to the Segontiaci. Mr. Horsley differs from them both; and neither admits Silchester to be the ancient Vindomis, nor to lie within the bounds of the Segontiaci; but takes it for Calleva Atrebatum, mentioned likewise in the Itinerary. From the difficulty therefore of fixing the situation of the Segontiaci, Dr. Clark contents himself with only placing the word incertum against their name. But had this short inscription, imperfect as it is, offered itself to these learned writers, none of them could have been at any further doubt, either in placing Vindomis, and not Calleva, which belonged to the Atrebates, where Silchester now stands; or including this town within the limits of the Segontiaci. The want thereof has likewise occasioned them no less to differ in settling some other neighbouring stations, which by this help might have been fixed with much more agreement and certainty.

The Case of one Hannah Hitchcock, one of whose Ureters was grown up; also a Present of a beautiful Stalactites; and a Drawing of an Extraordinary Calculus taken out of the Bladder of a Boy. By John Huxham, M.D., F.R.S. N° 474, p. 207.

Dr. H. here states that he had sent the case of Hannah Hitchcock with a box containing the stones, &c. That in the same box he had also sent a remarkable specimen of stalactites, which was found in a cavern, discovered amidst the vast marble rocks at Cat-down near Plymouth. It hung perpendicularly from the top of the rocky cavern, and was a cylindrical tube of 20 inches long at least, and quite hollow, but was unluckily broken into several pieces in bringing it away. He went to the cave the next day, and found 5 or 6 of such kind of tubes, but none above 2 inches long. They all sprang from a broad, hollow, protuberating basis, in some sort as a nipple arises from the breast. These also were cylindrical and hollow. There were in the same cavern many other petrifications, which had formed a kind of hollow pilasters against its sides; and also several large solid masses, which arose from the continual dropping of the petrifying water through the crevices of the superior rock. These all afford a very good alabaster.

The box contained an exact draught of a stone, fig. 4, pl. 2, which had been taken out of the bladder of a boy, about 12 years old; it was of an uncommon figure; though not indeed so very remarkable as that mentioned in the Philos. Trans. N° 450. The boy died 2 or 3 days after the operation.

Hannah Hitchcock, about 60, was from her youth a very sober industrious woman, of a thin habit of body, and biliqus constitution: but in her latter years she became unhealthy, and was frequently subject to fits of the gravel, and brought off some small stones. For about 3 or 4 years before her death, she was often afflicted with nephritic colics, great pains of the stomach and back, suppression of urine, and sometimes discharged bloody urine. She at length became ascitical and asthmatic; and, for more than 8 or 10 weeks before she died, she had a violent pain in the region of the stomach, and a hard swelling under the cartilago ensiformis, with almost perpetual vomitings, colic pains, extreme costiveness, and difficulty of urine. The last fortnight, or more, she vomited every thing, and had a total suppression of urine for 15 days. At last she died comatose and convulsed.

On opening the abdomen, soon after her death, there neither appeared stomach, liver, nor guts, but a large irregular mass, spread from one side of the abdomen to the other, and adhering firmly to both sides. This was, in great part, the omentum, grown, as it were, cartilaginous, and as tough almost as leather; having in it several large, very hard, scirrhous nodes, and some tubercles, full of fetid pus. The stomach was very much contracted, but its coats were very thick; and near the pylorus, very much inflamed, almost mortified; its glands in several places scirrhous, and as large as peas. The passage from the stomach to the guts was shut up, partly by the inflammation and thickness of its coats, and partly by its odd coalition with the liver and omentum. The liver was much shrunk and scirrhous, and rolled up into a kind of conical figure; in the vertex of which appeared the gall-bladder, of a dark green colour, and very turgid; and yet the ductus communis choledochus was near 4 times as large as usual. The ilium was thrust down much lower than ordinary; and the convolutions of the gut were, in several places, grown firmly together: the colon also, on both sides, was strongly attached to the peritonæum. It is almost constantly observed, where the omentum is either consumed, or greatly depraved, that the convolutions of the guts grow together, and adhere to the adjoining parts, for want of that oily mucus, which, in a natural state, in great plenty transudes from the omentum, to lubricate them, and render them fit for the regular performance of the peristaltic motion, &c. In the cavity of the abdomen there were near 6 quarts of putrid water, somewhat tinged with blood. In the right cavity of the thorax about a pint. The urinary bladder was quite empty, and half rotten, did not contain a drop of urine, but was smeared over with a sort of purulent matter. In the right kidney was found the larger stone, which took up almost all the pelvis renalis. In the right ureter were two or three small stones, which, with a sort of very tough mucus, had shut its passage entirely. Indeed the great stone in the pelvis had well nigh quite blocked up the mouth of the ureter. In the left kidney was the smaller stone, which had so entered the ureter with its conical part, as adequately to stop it, like a cork in a bottle. When we had removed this stone, though we found no other in the ureter, we could not force any water through it into the bladder, though a strong injecting syringe was used: for the ureter was greatly contracted, and there seemed an absolute coalescence of its sides.

It is certain that the canals of a human body, which cease to have any fluid transmitted through them, soon coalesce; as is particularly evident in the canalis arteriosus, urachus, umbilical vessels: nay we see the external coats of the intestines soon grow together, if they are not constantly lubricated with their proper oily mucus that prevents it. This will be much sooner effected in the ureters, if they happen to be excoriated by small stones, or gravel: for we find even the fingers, or other excoriated parts, soon grow together, unless prevented by due care. Doubtless this is sometimes the case in fatal ischuries; though more frequently they arise from obstructing gravel, stones, grumous blood, tough mucosity, or the like.

Of the extraordinary Effects of Musk in Convulsive Disorders. By J. Wall, M. D.* N° 474, p. 213.

[Dr. Wall in this communication gives an account of the beneficial employment of musk in certain convulsive disorders; but as this paper is reprinted in this author's works, with which every physician is now well acquainted, it is deemed unnecessary to retain it in these collections. Dr. W. prescribed the musk in doses of 10 or 12 grs. to adults, in the form of a bolus, where it could be given by the mouth, and when not in that way, he administered it by clysters.]

A Letter from Andrew Reid, Esq. to Dr. Wilmot, concerning the Effects of the Tonquinese Medicine. Dated London, Nov. 5, 1745.† N° 474, p. 225.

About 15 years antecedent to the above date, Mr. R. learned in China, that the Tonquinese had an infallible cure for the bite of a mad dog, and, being very desirous of possessing so valuable a receipt, he was 2 or 3 years after, favoured with it by the late Mr. Hart.

They take of the best musk about 16 grs.; of the purest native cinnabar, and finest vermillion, each about 24 grs.; and, having reduced them separately to impalpable powders, mix and administer them in about a gill of arrack; which, in 2 or 3 hours, generally throws the patient into a sound sleep, and perspiration; if not, they repeat the dose, and think the cure certain.

* Of this ingenious physician some authentic particulars will be given in the following (viz. the 44th) vol. of the Philos. Trans.

† Dr. Edward Wilmot remarks, that these experiments of Mr. Reid probably gave occasion to the liberal use of musk at the above period.—Orig.

As Dr. R. had no room to doubt the fact, he began to consider attentively the symptoms of the distemper, and the nature of the remedy. The former seemed to proceed immediately from the irritations of the nerves by the acrimony of the juices; which, being constantly and violently hurried about, are, by that motion, and the heat attending it, broken, colliquated, and gradually rendered rancid, putrid, corrosive, and even caustic: in the mean time, the nerves, being more and more vellicated by the increasing sharpness of the humours, become proportionably more rigid and constricted; at once augmenting the velocity of the blood, and shutting up all the pores and passages of the natural excretions and secretions; while what should, but cannot, pass off by them, exasperates the disorder, till the juices become so corrosive and caustic, as to produce mortal convulsions.

Believing this theory to be just, so far as it goes, he readily concluded, that a medicine capable of relaxing the nervous system could not fail of relieving it from the abovementioned effects of irritation, and thereby putting a stop to convulsions, opening the constricted passages of nature, moderating the velocity of the blood, and procuring sleep; imagining also that, by the same soothing quality, the juices themselves might, not improbably, be rendered more mild and innocent when impregnated with the medicine. And such a medicine he judged musk to be, on account of its known, and almost instantaneous, effects on persons of a lax habit; whose nerves are so suddenly slackened, and the motion of their blood so diminished by the least smell of it, that many of them faint away; besides, its odour is so exceedingly subtile, as to penetrate through the closest substances; and may therefore be supposed easily to pervade the minutest vessels of the human body, and to diffuse its softening balsamic virtue through all its juices.

The arrack seemed also a very proper vehicle for the musk; not only as they make together a very agreeable bitter, but also because inflammable spirits resist putrefaction, and also, in some measure coagulate animal juices, which are not already corrupted; by which effects the too much rarefied blood is condensed, and hindered from putrefying further; while the bad juices, being separated from the sound, are plentifully thrown off by the passages, which the musk has relaxed, and opened for them. The native cinnabar seemed to be sufficiently recommended by its known uses in physic, against acrimony, obstructions, and convulsions; but of the vermillion he could only say, that though it be a preparation of the former, yet, as the Tonquinese seem to think its virtue different, it were to be wished, that we knew their method of preparing it, in which they certainly excel.

After he had long considered and examined these principles in his mind, he satisfied himself, that they might justly be applied to many other cases; and that the medicine would be of especial service in malignant putrid fevers and convulsions; and having, in the year 1739, contracted with the government for the transportation of convicts, he communicated his opinion to Mr. David Ross, an ingenious and sensible surgeon, whom he had employed to take care of their healths; and prevailed on him to make the experiment in a very desperate case; for he did not care to venture on any other. Please, in the mean time to observe, that, as Tonquin vermillion was not to be had, he substituted an equal quantity of factitious cinnabar in its stead, and sometimes gave rum or brandy instead of arrack; in other things he generally adhered to the original prescription.

Case I.—In December 1739, 2 convicts in Newgate were at the same time very ill of the putrid, infectious, malignant fever, commonly called the gaol distemper. All the usual methods of practice having been tried in vain, their condition appeared to be quite desperate. One of them died in the evening, and the other was not expected to survive till morning, being covered with flat petechial spots, and delirious. Mr. Ross therefore administered to him the above described medicine about 9 or 10 o'clock at night; and next morning, to his great surprise, found him quite free from the fever, eating water gruel, and crying out for meat, after having slept well, and perspired plentifully. The spots on his skin rose, and the next day scaled off.

Case II.—Soon after this, a convict, who had lived in good credit, laid his condition so much to heart, and drank so freely of spirituous liquors to drown his care, that he fell into a violent fever. He was on the master's side, where his relations looked after him; so that he heard nothing of his case till it was very desperate. He was delirious to a high degree, and had catchings in his hands and face. He took the above medicine at night, slept and perspired well, and next morning waked entirely free from his distemper; excepting that he had such a tremor left in his hand, that he could not carry a glass to his head; on which account he ordered him a second dose, and he was perfectly cured.

Encouraged by these successes, they administered the medicine to a great many other transports, who had the gaol distemper, and generally found it to have the same salutary effects; more especially when the patients were delirious or convulsed; as can be attested by Mr. Louttil, apotheeary, who made up the medicines for the surgeons; and was himself a witness of several surprising cures performed by it, nor did Mr. R. ever hear of any bad effects from it.

Case III.—Mr. Ross, the surgeon abovementioned, having caught the gaol distemper, by attending those who were sick of it on board, came ashore at Gravesend, and desired Mr. Reid to provide another surgeon, for that he was so ill he could not go the voyage. He was blooded, and took 16 grs. of musk in a glass of rum, without the cinnabars, which were not to be had ashore, nor easily to be fetched from on board. This was at night, he slept, perspired, and waked about 10 in the morning so well, that he went on board directly, and continued his voyage.

CASE IV.—Encouraged by many instances of the first case, Mr. Reid ventured, about 3 years ago, to give half the Tonquin dose, but without the spirits, to his own child, then about 3 years old, who was seized with the small-pox and convulsions. She slept sound, and perspired plentifully after it: the small-pox rose kindly, and she did very well, having never had a single fit of convulsion after, though extremely subject to frequent and dangerous ones before.

CASE V.—About 2 years before, Mr. Ross gave much the same dose to Mr. Reid's servant's child, who was then about 8 months old, and had catchings in her hands, occasioned by a violent teeth-fever. She slept, perspired, and waked perfectly well.

CASE VI.—About the same time, Mr. Gordon, a clergyman, who lived near Greenwich, having come up to London, with a fever on him, became quite delirious, if he was not so before he set out. Mr. Ross gave him the Tonquin remedy, which made him sleep and perspire, so that by next morning he was perfectly cured.

CASE VII.—A lady being very subject to violent hysterical convulsions, was seized with a fit, as he sat at supper with her about 3 years ago, by which every part of her body was terribly agitated. Happening to have in his pocket a bolus made of the musk and cinnabar, he bruised it in the palm of his hand, and clapped it to her nose, which suddenly relieved her. He left it with her, and she afterwards told him, that she never went abroad, nor to bed, without it.

CASE VIII.—Observing the efficacy of this medicine in curing deliriums, he conceived that it would be of use against maniacal distempers; and happened to say so, in the presence of a gentleman of Oxford, whose son had been for some time exceedingly disordered in his senses, by a disappointment in love; being unable to sleep, refusing sustenance, and attempting to throw himself out of the windows of a high room where he was confined. The father begged Mr. R. to give him the recipe, and assured him he would make use of it, as the methods formerly tried had proved unsuccessful. He soon returned a letter of thanks, acquainting him, that the medicine had made his son sleep sound for 23 hours, that he had perspired plentifully, and waked in his senses. He had afterwards heard, that he continued well, and, from a skeleton, was grown fat.

CASE IX.—A particular friend went mad about a year and half ago, by too intense thinking. He mentioned the preceding case to Dr. Armstrong and Mr. Ferguson, who attended him; and, with their approbation, gave him musk, native and factitious cinnabar, of each a scruple, in about a gill of arrack. In about 3 hours he fell, or seemed to fall, asleep: on which, supposing the medicine had taken effect, they left him; but, soon after they were gone, he waked; and next day seeming very little, if at all, better, was removed to a private mad-house. There nothing else was done to him; but at night he slept tolerably well, appeared much better next day, and continued mending, till he was in a little time quite well, as he is now. How much of this cure may be attributed to the medicine he did not know, as it did not operate immediately, nor in the usual manner. He mentioned it chiefly to show, that even 20 grs. of musk had no bad effect on him, if they had not a good one.

CASE X.—Not long after, Mr. Louttit and he gave the same dose to a gentle-woman, whose brain had long been turned by religious terrors, which first affected her about the time that her menses ceased. He was at first surprised to find her suddenly become quite gentle, obliging, and reasonable; but these good effects, as she slept but little, went off next day, and she soon appeared neither better nor worse than she was before; and in the same condition she still continues; so that neither in this case did the large dose of musk do any mischief.

Mr. Reid adds, that where he thought the case required it, he had given as far as 24 grs. of musk to convicts, and never found any ill effects from it, though, on some occasions, it disappointed his hopes. Whether there was any error in altering the Tonquinese proportions, the cause of which he cannot recollect, or whether the medicine would have succeeded in the instances where it miscarried, had the original prescription been kept to, and repeated as directed, he leaves to others to judge. Mr. R. concludes with asking, whether the virtues of musk may not be applicable to many other cases, and particularly to the epi-lepsy and the plague.

De Planta minus cognita, et hactenus non descripta, Commentarius. Auctore Gulielmo Watson, R. S. S.* N° 474, p. 234.

This plant is represented fig. 5, pl. 2, where A is the pericarpium; B the oper-culum; C the volva.

Fig. 6 is the pericarpium seen in front, where A denotes the circular hole.

On sudden Freezing; and on the Electric Fire; also on the Application of a Micrometer to the Microscope. By Sam. Chr. Hollmann, Prof. Pub. Ord. Philos. Gotting. N° 475, p. 239. *From the Latin.*

The phenomenon related by M. Triewald, Trans. N° 418, was so extraordi-nary, that if Mr. H. had not had some further proof, that all congelations are

* The plant here described is the *lycoperdon fornicatum*, Linn. As it has been so often figured and described, it is unnecesary to reprint any thing but the title of this paper, and to retain the figure.

performed almost in a moment of time, it would have seemed to surpass all belief. But about the end of both the years 1742 and 1743, Mr. H. experimented something of the same kind himself.

He had some conical glasses of water, with some of those glass images in them, called Cartesian devils, which afforded several curious instances of sudden freezing, particularly on being touched with something warm.

1. When the water had received a sufficient degree of cold, whether the glass was then removed into a warm place, or was only taken in a warm hand, almost the whole water would in a moment be changed into ice: but more readily in the former case than in the latter. 2. It made no difference whether the glass was covered with a bladder or not; nor whether the bladder was pressed with the fingers or not. Neither did it signify whether any Cartesian devil, or any other such image, was in the glass or not; and if there was one, whether it kept at the bottom of the glass, or rose to the top. 3. When there was an image in the water, the freezing always began at some part of it, and diffused itself on all sides.

After M. du Fay had discovered by accident, that an electrified human body, if touched by another not electrified, would emit sparks that pricked pretty sharply, these experiments were repeated in the university of Leipsic; but instead of the glass tube, used by Mr. Gray and M. du Fay, they employed a glass ball, such as formerly was used by Mr. Hauksbee. On this occasion it was observed, that electrified bodies, especially those of animals and metals, emitted sparks so strong, as to set on fire, not only spirit of wine moderately warmed, but also, other inflammable bodies, such as gunpowder, pitch, brimstone, and sealing-wax, being first well heated. An iron tube turned, and suspended horizontally on lines of blue silk, having one end as near as possible to the glass ball, to receive its electricity, and the other end of it touched by a man standing on a cake of pitch; then the electrical force was so diffused through his whole body, that any part of it would attract and repel alternately leaf gold, and other light bodies; and either this man or the tube being touched by another person, emitted sparks extremely pungent. Also when the man on the cake of pitch had a sword on, sparks would spontaneously issue from the extremity of the sheath. When another person held spirit of wine warmed, in a spoon, and the electrified man brought his finger, or an iron key, or the point of a sword, near the surface of the spirit, it immediately inflamed. Or when the electrified man held the spoon with the spirit, and another person put his finger near it, the same effect ensued. And when several persons, standing on pitch, held each others hands, or were united by a cord, or an iron tube, &c. then the last would perform the same as the first or second.

Mr. Hollmann having observed, in Baker's Microscope made Easy, that Mr.

Martin had invented a micrometer, to be applied to any microscope; he announced that he had for several years made use of another kind of micrometer, which he applied to one of Scarlet's microscopes, and placed it in the focus of the first eye-glass. It was a very small piece of the thinnest black silk, divided into very minute squares, and extended on a little ring of wood or paper, so as to be conveniently placed in the focus of the first eye-glass. When he had found that N° 1 of the microscope magnified 250 times, and that the animalcula seminalia humana, without their tails, appeared hardly so large as a cheese mite does to the naked eye, it became evident that 15,625,000 of these animalcules were contained in the space of a cheese mite. And yet he observed much smaller animalcules than these, in an infusion of common pepper, or even of common hay, after standing some days. By means of the same micrometer he determined the number of seminal animalcules in the milt of a fish more accurately than had been done by Leuwenhoeck. Hence he found that one cubical decimal line of a foot in the milt of a carp, contained above 244 millions of seminal animalcules; and that the whole milt, weighing not quite 2 lb. made about 2000 decimal lines, and so contained more than 500000 millions of seminal animalcules.

Of the Gigantic Boy at Willingham near Cambridge. By the Rev. Mr. Almon, Minister of the Parish, and Mr. Tho. Dawkes, Surgeon at Huntington. N° 475, p. 249.

The mother of this boy had been a servant in Mr. Almon's family. He was her 2d child, and at his birth, had something very extraordinary about him above other infants; particularly in partibus generationis; besides being uncommonly large in his whole body. He grew wonderfully for $\frac{3}{4}$ of a year, having only the breast sustenance; when his mother died suddenly, and, as is supposed, by his drawing away her vital nourishment.

After her death, he has continued growing in proportion: and, though but 2 years and 11 months old, was 3 feet 9 inches high; and every part in proportion; his strength and courage such as to overcome boys of 6, 7, or 8 years of age; his voice like a man's, very coarse; his weight above 4 stone; and he appeared to have as much understanding as a boy of 5 or 6 years old. But, what is most surprising, his penis was 4 inches long when erect, and $3\frac{3}{10}$ inches as pendent; the hair on the pubis an inch long and thick.

He was very strong: he took up and threw from him, with much facility, a blacksmith's hammer, which weighed 17 lb.

The glans of the penis was quite uncovered; and his aunt and the midwife asserted that it was always so.

His father is a little man, a labourer: his mother was a woman of a middle stature. The midwife said, that when he was first born, he was what they commonly term, a lusty boy; except that the parts of generation were remarkably large, and that the lanugo first appeared when he was near a year old; which gave great uneasiness to his parents, who were very religious people.

To a second letter from the same were subjoined the affidavits and testimonials of the midwife, the minister, churchwardens, and others, that this child, Tho. Hale, was born on Oct. 31, 1741. Between Aug. 28 and Nov. 30, 1744, this child grew $2\frac{1}{4}$ inches, i. e. from 3 feet 8 inches and $\frac{8}{10}$, to 3 feet 11 inches.

A new Contrivance of applying Receivers to Retorts in Distillation. By Mr. Browne Langrish, F. R. S. N° 475, p. 254.

In fig. 7, pl. 2, A represents a common retort in a sand heat; B the first receiver, with an opening at the top and bottom; C a quart bottle fixed to a neck out of the bottom of the receiver; which, being tied on close by means of a bladder, may be removed at any time, and another instantly placed in its stead; by which means very little of the steam will make its escape; and any proportion of the volatile part to be distilled may be saved by itself, without unluting the recipient from the retort; D is a second receiver, inserted into the opening at the top of the first, to give more room to the rarefied and new generated air, and to receive the most subtile and volatile parts, which might not be so easily contained in the first recipient, without great danger of breaking it, or forcing the luting; E is a smaller bottle, for the same uses as the other, marked C; F is a bladder tied on to an opening, or upper neck, of the second recipient; which, as it is much thinner and weaker than any of the glasses, will always give way first, and prevent their bursting. Or, wherever the matter to be distilled is of such a nature, that we are sure all the glasses, put together, will not contain the fumes and air arising from it; then if the smallest pin-hole be made through the top of the bladder, as soon as the fumes begin to rise, it will be sufficient to let out the air as fast as it generated; and at that great distance from the fire, very little, if any thing, but air, can make its escape.

Hence great advantages will arise from this way of distillation: for 1. We can keep a greater fire, if occasion be, without fear of breaking our glasses. 2. The matter distilled may be removed as often as we please, by which we may always prevent the drawing off any spirit, &c. too low. And 3, any pure, fine, volatile salt, which shall arise into the second recipient, will not be so liable to be melted down, either by the heat, or too watery a fluid.

Of a Person bitten by a Mad Dog. By Charles Peters, M. D. N° 475, p. 257.

John Neale, of a robust constitution, aged 45, had for some years followed the occupation of curing dogs; and one day being employed in that calling, endeavouring to drench one supposed to be mad, he was bitten in the thumb.

The day following the dog was observed to droop, refuse his food, and at night he died.

The patient, having been frequently conversant with the like accidents, was sufficiently alarmed at the danger, and having been the year before received a patient into St. George's Hospital, he repaired thither for relief. Dr. Hoadly attending instead of Dr. Bailey, the physician of the week, he directed him to have the wound scarified, be blooded, use the pulvis antilyssus, and cold bath.

About a fortnight after the accident, being the full of the moon, his symptoms became so violent, that Mr. R. was desired to meet his brethren in consultation. He found him sitting on a bed, with one of his legs tied to the post; and on inquiring how he came placed in that position, he said, he himself fastened the cord, apprehending he might grew mischievous; and on proposing to him to put on the strapped waistcoat, he readily consented to it; expressing great dread of becoming hurtful. He said, he had felt a numbness in the wounded thumb, which shot up his arm to the shoulder; and that he was not sensible of having taken a moment's rest since the accident had happened; and that, on endeavouring to compose himself, he fell into startings, and dreadful apprehensions of mischief from dogs. His eyes looked wild, and he complained of an excruciating pain in the head. For some days past he had been troubled with a difficulty in swallowing; Mr. R. proposed to him to get down a small piece of bread; but he seemed to refuse it with great abhorrence; however, being encouraged to make use of his resolution, which he possessed to an extraordinary degree, he forced it into his mouth, where holding it for some minutes, he endeavoured to swallow, but was seized with violent spasms, beginning at the bottom of the abdomen; which, by a convulsive progression, heaved itself into the thorax, whence the spasms were extended to the pomum adami; when the patient fell into strangulation, and afterwards privation of sense. On recovering from the fit, as we perceived that, notwithstanding these obstacles, he had swallowed the bread, allowing him time to rest, we proposed to him to endeavour to swallow a spoonful of liquid, which he seemed most shocked at: he answered with fierceness, that he could not away with drink: however, on recollection, he said he would endeavour it: and taking a spoonful of alehouse drink into his mouth, he was instantly seized with convulsions, beginning from the bottom of the abdomen, and ascending with great violence to the head, till he fell into a fit of longer duration than the former; however he swallowed the liquor; and on recovering his senses, he pointed with great vehemence to his arm, signifying that he desired to be blooded; from which he afterwards told us he had before found relief. As his pulse was extremely hard, we directed the surgeon to take away 16 oz. which proved ad deliquium; however, that soon passing off, his pulse still continued hard, his flesh hot, with grievous complaints of the pain in his head.

On considering his case, as he had received no relief from the ordinary treatment of this distemper, and that his symptoms were become highly inflammatory, blood sizy, and flammeous urine, they resolved to lay aside all thought of infection, and to betake themselves to the method of cure in inflammatory fevers; with this difference only, that, as he had passed some days without stools, he was directed to take an enema immediately; and then the following bolus: nitri purif. ʒß, confect. mithridat. ℈1, sexta quaque hora, addendo dosi vespertin. extr. thebaic. gr j. et repetatur tertia quaque hora (nisi interveniant symptomata) donec concilietur somnus; epispastica applicentur brach. et crurib. intern.

On visiting him in the morning, the nurse said, that after having taken 2 boluses, he had slept about half an hour, to his infinite refreshment. His blisters discharged plentifully, his mind more composed, and his horrors were so far mitigated as to swallow half a pint of ale at one draught, though not without repugnance. He still complained of living in a flame, his eyes ready to start out of his head, where his pain still remained acute; with numbness in the diseased arm, inquietude, difficulty in swallowing and respiration. Mittatur sang. e br. ad ℥ xii. et pergat in usu præscriptorum.

The night following he took 2 boluses, and slept nearly 3 hours. The symptoms appeared less violent the next day, but still threatened mischief. Applicentur cucurbit. occipit. ad extract. sang. ℥ viii. epispastic. laterib. colli, &c.

His body was kept soluble with manna, and other cooling laxatives.

This method, with little variation, such as leeches, pulv. sternutatorius, &c. was continued for about 14 days, the blisters being kept open the whole time; during which the symptoms gradually abated. He fell into languors, which were easily removed by the use of assafetida, valerian, &c. During his illness, he voided so great a quantity of saliva, that his teeth, though naturally firm, became loose, and continued so till the abatement of the complaint.

As he was free from any disorders in his head, and his pulse beat with a natural softness, Mr. R. advised him to return to the use of the cold bath, pulv. antilyssus, with a caution to bleed, and discontinue the use of them, whenever he found himself heated.

He was restored to a tolerable state of health, except at the new and full moon: for, though he felt some alteration in the quarters, they were not so considerable: at which time his symptoms returned in some degree, but so slightly, as not to prevent him from following his calling, which he changed to selling of greens, not being entirely freed from the dread of dogs.

During his illness, he complained of coldness in the extremities, with sour eructations; so that, as soon as it was judged safe, he was directed to make use of a vomit, which was repeated many times with success.

When any liquid was offered him, he poured it into his mouth with uncommon hastiness, and on inquiring the reason, he said, he had experienced, that by throwing in a large quantity of liquid into his mouth at once, his faculty of swallowing became more easy; and that whenever any hindrance happened in the performance, it was not without difficulty that he recovered himself.

Mr. R. desired he might in nowise be thought to depreciate the efficacy of the pulv. antilyssus * and cold bath; for he believed them more generally successful than any other means; yet he thought it clear in the above case, that they were so far from alleviating the complaints, that they tended evidently to promote them; the patient never making use of the cold bath, but his head-ach increased,† and his feverish complaints grew more violent.

As the contra rabiem powder now stands in the Lond. Pharmacop. it is compounded of two drugs only. Mr. R. had endeavoured to discover what effect might be procured by the liverwort; but on trying it, for experiment's sake, in several different cases, even in large doses, he could never perceive the least alteration either in the pulse or secretions.

A new Species of Fungus.‡ *By John Martyn, F. R. S.* N° 475, p. 263.

In the latter part of the summer of the year 1744, a fungus of a very extraordinary shape and size, which had been found growing on a piece of the trunk of an elm, in a damp cellar in the Haymarket, was brought to Mr. Martyn. The whole plant was about 2 feet in height, and at first sight seemed not very unlike the horns of some deer, being variously branched, and covered with a thick down. It was of a spongeous substance, and of a dusky red colour, inclining to black. The tips of the smaller branches were of a cream colour. The larger branches, or rather the tops of the whole plant, were expanded in form of a funnel, smooth on the concave, and full of pores on the convex side. The inner and lower part of the funnel was of the same colour with the stalk, the rest of it was of a cream colour.

Mr. M. had not been able to find that this plant has been mentioned by any author, and was persuaded that it is a new species; and perhaps the remarkable branching of the stalks may induce some to think it a new genus. As the funnel may be esteemed a cap, and as this cap is not lamellated, it will be a boletus,

* Of Dampier, as altered by Dr. Mead.—Orig.

† Therefore, adds Dr. Mortimer, the secretary, in a note, in my Thes. Inaug. Ludg. B. 1724. I proposed the use of warm baths; for by them heat and thirst will be abated, and the blood diluted, not rendered still more thick by sweating, as is the effect of cold baths. See these Trans. N° 443.—Orig.

‡ This plant was probably no other than a gigantic specimen of the *clavaria hypoxylon* of Linnæus; though it has sometimes been considered as a species of boletus.

according to the method observed in the 3d edition of Ray's Synopsis. According to Micheli, it seems to belong to the genus of polyporus.

And as Mr. M. does not think it necessary to constitute a new genus, he called it, Boletus caule ramosa, summitatibus concavis expansis, ramis minoribus in acutum mucronem desinentibus. See fig. 8, pl. 2.

On the Vegetation of Melon Seeds 33 *Years old; and on a Fossil of a Man. By Roger Gale, Esq. F. R. S.* N° 475, p. 265.

About Jan. 1742-3, Mr. G. accidentally found a paper of melon seeds, that he had laid by, with the date of the year 1710 on it. He sowed some of them, not with any great hopes of their coming up; but to his great surprise, he had a fine number of plants from them, which all prospered very well, till they had put out 4 leaves, when they were all lost by an accident. This long retention of their vegetative quality may probably be ascribed to the oiliness of the seed, and the hardness of its outer coat.

We have few or no fossils in this country (Yorkshire); but a friend in Staffordshire says that that country abounds much in fossils; such as sea-shells, rock plants, and other marine bodies left at the deluge. Near Bakewell in Derbyshire was lately found the skeleton of a man, with some stags' horns, as the workmen were driving a sough, or drain, to a lead mine, about 9 yards deep from the surface of the earth, and about 40 fathoms from the beginning of the sough. There were found with the skeleton stags' horns; two pieces of which Mr. G. had; viz. the brow-antler, which was 9 inches long, and seemed to have about 2 inches broken off the tip end; the other a piece of the large horn near the head, and was 3 inches diameter. Both the horns of the stag, and the rib-bones of the skeleton, were much decayed; and as soon as the head of the latter was exposed to the air, it crumbled all away, except a piece of the lower jaw. Several of the larger teeth were taken out, which were covered with their natural enamel, and perfectly sound. The place where these things were found, is on every side surrounded with a rocky petrified substance, or terra lapidea, by the miners called tuft, so hard, as they say, as to strike fire against their tools. This substance lay above the bones and horns a yard and half thick or more, and on either side; and beneath them to a breadth and depth uncertain; so that it appears, that the skeleton and horns lay in a cavity, which was not however contiguous to them, there being a sort of soft coarse clay or marl, interspersed thick with little petrified balls, or pellets of the same kind of substance as the tuft, for near a quarter of a yard round them; but none of the bones seemed in any degree to be petrified. The workmen conjectured there was more of the skeleton to be found; but they dug no farther than was necessary to complete their sough.

The interment of this man and stag seem to me to have been accidental, by their falling into a chasm, or wide cleft of the rock, in very early times; which has since closed up, and grown over them, by the accretion of the marly substance, which environs the skeleton, &c.; and in time perhaps will grow as hard as the tuft, and the rest of the rock.

Concerning a large Stone found in the Stomach of a Horse. By Mr. Wm. Watson, F.R.S. N° 475, p. 268.

Mr. W. here offers some observations on the calculus, sent him for examination by Mr. Woolaston, who informed him that it was found in the stomach of a coach-horse; and that it then weighed 3 pounds $2\frac{1}{8}$ oz. Avoirdupois. It seems the poor creature was observed frequently to be in violent pain; and would sometimes eagerly turn his head to one of his sides, and sometimes to the other, as though he endeavoured to bite out that which annoyed him; and that he died, after having taken various remedies, which the farriers administered. When Mr. W. weighed this stone about half a year after the former its weight was 2 lb. and $\frac{3}{8}$ oz.; so that in about half a year, it had lost 1lb. 1oz. $\frac{7}{8}$. Its figure was spheroidal, as these sort of stones generally are; its periphery $17\frac{1}{4}$ inches, by $16\frac{1}{8}$ inches; which are very near the same dimensions this stone had when first found. Its surface irregular, somewhat resembling the inequalities on the surface of the brain: all the projecting parts of which are polished, from their friction against the sides of the stomach. It was of a dark-brown bilious colour, and very like to a species of pyrites, and was by much the most specifically heavy of this sort he had ever seen.

Having, from sawing the stone, a quantity of its powder, he was induced to an inquiry into its constituent parts by way of analysis. He first let fall 2 small pieces of this stone into water almost boiling: they immediately sunk, but rose again, and continued alternately rising and sinking a considerable time. This was occasioned by the quantity of air bubbles, which the heat rarefied; but the air was detained by the mucus, which seemed to connect the particles of the stone together; and which, though diluted by the hot water, was tenacious enough to form bubbles of size sufficient to buoy up the pieces of stone; the rarefaction becoming greater, the bubbles burst, and the stone fell to the bottom: but rose again, in like manner, at the expulsion of more air. Dr. Hales likewise found great quantities of air in the human calculus.

Mr. W. infused 2 drams of this powder in 2 oz. of boiling water: this infusion he filtered when cold. It was of a light brown colour, and of a bitterish saline taste. He calcined what remained of the powder after the infusion, till the whole was black, and then it weighed a dram and 3 grains. He made the following trials with the infusion:

1. Mixed with spirit of violets, it became green. 2. With oil of tartar, the colour was deeper without ebullition, but the mixture sent forth immediately a strong urinous smell; the same smell arose from rubbing some of the powder with oil of tartar. 3. With oil of vitriol, and spirit of salt, it lost its colour; but no ebullition ensued. 4. With a solution of sublimate in water, the mixture curdled, and let fall a light grey sediment, leaving the liquor quite transparent. 5. With a solution of sublimate in lime water, the mixture grew turbid, and let fall a deep yellow sediment, in a much greater quantity, and of a deeper colour, than a solution of sublimate and lime water alone.

From these inquiries he concluded that the stone was compounded of an earth, air, mucus of the stomach, and a saline principle bearing great resemblance to sal ammoniac.

Of a Porcupine swallowed by a Snake. In a Letter from Bombay, dated Jan. 23, 1743-4, communicated by Francis Woolaston, Esq. F. R S. N° 475, p. 271.

Some time ago there was found, on an island adjacent to this, a large snake dead, with a porcupine in its belly. The snake had seized the porcupine by the head; and had so sucked it in. When it was quite in, the quills, which were flatted down while it was going in, rose; ran through the snake's belly, and killed it: so that there was a monstrous large snake dead, with the quills of a porcupine sticking out of it in many places.

Concerning the Bologna Bottles. By Dr. Josephus Laurentinus Bruni, of Turin, F.R.S. N° 475, p. 272.

The curious people in this country talk much of a phenomenon, called the Bologna bottle, because it was first discovered at Bologna. If you let these bottles fall perpendicularly from some height on a brick floor, they will not be broken; but if you drop into them some little hard bodies, they will burst in pieces. Dr. B. took one of these glass bottles, in form resembling Florence flasks, and in capacity about three quarters of a pint, and let it fall down from the height of 5½ feet on a floor of brick, and it was not broken: he then let fall down into it, from the mouth to the bottom internally, a piece of flint-stone, weighing 11 grains; and immediately the bottle burst into many pieces.

He took one of those pieces, weighing a dram, and let it fall in the same manner into another bottle, which he moved circularly for a minute; and then putting it on a table, in about a quarter of an hour it broke in pieces. Into a third bottle he dropped a piece of whetstone, weighing 40 grains; and in a few minutes the bottle was broken. He filled another bottle half full of water, and let fall into it a small piece of flint-stone; and after 4 hours it burst. He let fall into 3 other bottles a piece of wood, weighing 50 grains, a piece of brass weighing 300 grains, and a ball of lead weighing 140 grains; and neither of them was broken.

These bottles are thicker at the bottom than the neck. The glass-maker blows them, and lets them cool, without putting them again into the oven. And from the experiments Dr. B. noticed, that what is capable of breaking them, ought to have some roughness: and he was told that a grain of river-sand will break them.

Concerning two Ancient Camps in Hampshire. By Mr. Tho. Wright. N° 475, p. 273.

Common report, and his own natural curiosity, led Mr. W. to a place in Hampshire called Buckland castle, or, more vulgarly, the Rings, where he found 2 neighbouring camps, about 3 furlongs asunder: the one very strong, with double ditches, and triple vallums, on the top of a hill, 3 ways guarded by a natural ascent; the other on a lower ground, close by a river, which defends it on one side, with a ditch and vallum half round, and a kind of morass on the other. The first contains about 10 acres; the latter about 7; and the land lying between them is, and has been time out of mind, called Ambrose farm: besides an arm of the river, or rather of the sea, it being salt water, running close up to the latter, is called Ambrose dock. From all which it appears not improbable, that this may have been a principal station belonging to Aurelius Ambrosius.

Camden, though he takes no notice of these camps, yet mentions one of much less consequence, at about 12 miles distance, called Castle Malwood, says, "It is most certain, that about the year 508, Aurelius Ambrosius had here many conflicts with the Saxons, with various success." But, again, the people of the country have a tradition, that 3 miles to the west of this camp, a famous battle was fought between the Saxons and the Danes; in which so much blood was spilt, that a little river is said to have run blood, now called from thence Danes Stream.

Observations on a Case published in the last Volume of the Medical Essays, Edin. 1744, &c. of Recovering a Man dead in Appearance, by distending the Lungs with Air. By John Fothergill, M.D. N° 475, p. 275.

There are some facts which, in themselves, are of so great importance to mankind, or which may lead to such useful discoveries, that it would seem to be the duty of every one, under whose notice they fall, to render them as extensively public as possible.

The case which gave rise to the following remarks, seems to be of this nature: it is an account of "a man dead in appearance, recovered by distending the lungs with air; by Mr. William Tossack, surgeon in Alloa;" printed in part 2, p. 605, vol. v. of the Medical Essays, published by a society of gentlemen at Edinburgh; an abstract of which will be sufficient in this place.

A person suffocated by the nauseous steam arising from coals set on fire in the pit, fell down as dead; he lay in the pit between half an hour and three quarters, and was then dragged up; his eyes staring and open, his mouth gaping wide, his skin cold; not the least pulse in either heart or arteries, and not the least breathing to be observed.

In these circumstances, the surgeon, who relates the affair, applied his mouth close to the patient's, and, by blowing strongly, holding the nostrils at the same time, raised his chest fully by his breath. The surgeon immediately felt 6 or 7 very quick beats of the heart; the thorax continued to play, and the pulse was soon after felt in the arteries. He then opened a vein in his arm; which, after giving a small jet, sent out the blood in drops only for a quarter of an hour, and then he bled freely. In the mean time he caused him to be pulled, pushed, and rubbed, as much as he could. In one hour the patient began to come to himself, within 4 hours he walked home; and in as many days returned to his work.

Hence it naturally appears how much ought to be attributed to the sagacity of the surgeon in the recovery of this person. Anatomists have long known, that an artificial inflation of the lungs of a dead or dying animal, will put the heart in motion, and continue it so for some time; yet this is the first instance he remembered to have met with, wherein the experiment was applied to the happy purpose of rescuing life from such imminent danger.

Bleeding has hitherto been almost the only resource on these occasions: if this did not succeed, the patient was given up. By bleeding, it was proposed to give vent to the stagnating blood in the vein, in order to make way for that in the arteries a tergo, that the resistance of the heart being thus diminished, this muscle might again be put in motion.

But in too many instances, we are every day informed, that this operation will not succeed, though the aperture is made with ever so much skill; nor is it likely that it should, when the blood has lost considerably of its fluidity, the motion of the heart, and the contractile force of the solids, are at an end. And chafing, rubbing, pulling, and the application of stimulants, are too often as ineffectual as bleeding.

The method of distending the lungs of persons, dead in appearance, having been tried with such success in one instance, gives just reason to expect, that it may be useful to others. Hence it may be a proper inquiry, in what cases, and under what circumstances, there may be a prospect of applying it with success?

It will at once be granted, that when the juices are corrupted, where they are rendered unfit for circulation by diseases, where they are exhausted, or where the tone and texture of the solids is injured or destroyed, it would be extreme folly to think of any expedient to recover life. But where the solids are whole, and their tone unimpaired by diseases, the juices not vitiated by any other cause than a short stagnation; where there is the least remains of animal heat; it would seem wrong not to attempt so easy an experiment.

This description takes in a few diseases, but a great number of accidents. Among the first are many of those which are called sudden deaths from some invisible cause; apoplexies, fits of various kinds, as hysterics, syncopes, and many other disorders, where, without any obvious pre-indisposition, persons in a moment sink down and expire. In many of these cases it might be of use to apply this method; but without neglecting any of those other helps, which are usually employed on these melancholy occasions.

It is not easy to enumerate all the various casualties, in which this method might be tried with a prospect of success; some of them are the following: suffocations from the sulphureous damps of mines, coal-pits, &c. the condensed air of long-unopened wells, or other subterraneous caverns; the noxious vapours arising from fermenting liquors received from a narrow vent; the steam of burning charcoal; sulphureous mineral acids; arsenical effluvia, &c.

Perhaps those who to appearance are struck dead by lightning, or any violent agitation of the passions, as joy, fear, surprize, &c. might frequently be recovered by this simple process of strongly blowing into the lungs, and by that means once more communicating motion to the vital organs. Malefactors executed at the gallows would afford opportunities of discovering how far this method might be successful in relieving such as may have unhappily become their own executioners, by hanging themselves. It might at least be tried if, after the criminals have hung the usual time, inflating the lungs, in the manner proposed, would not, sometimes, bring them to life. The only ill consequence that could accrue from a discovery of this kind, would be easily obviated by prolonging the present allotted time of suspension.

But this method would seem to promise very much in assisting those who have been suffocated in the water, under the above-mentioned circumstances; at least it appears necessary to recommend a trial of it, after the body has been discharged of the water admitted into it, by placing it in a proper position, the head downwards, prone,* and, if it can be, across a barrel, hogshead, or some such-like convex support, with the utmost expedition.

It does not seem absurd to compare the animal machine to a clock; let the wheels of which be in ever so good order, the mechanism complete in every part, and wound up to the full pitch, yet without some impulse communicated to the pendulum, the whole continues motionless.

* This was a very injudicious direction, founded on a mistaken supposition that the death of drowned persons was owing to the quantity of water admitted into the stomach and lungs.

Thus, in the accidents described, the solids are supposed to be whole and elastic, the juices in sufficient quantities, their qualities no othewise vitiated than by a short stagnation, from the quiescence of that moving something which enables matter in animated bodies to overcome the resistance of the medium it acts in.

Inflating the lungs, and, by this means, communicating motion to the heart, like giving the first vibration to a pendulum, may possibly, in many cases, enable this something to resume the government of the fabric, and actuate its organs afresh, till another unavoidable necessity puts a stop to it entirely.

It has been suggested by some acquaintances, that a pair of bellows might possibly be applied with more advantage in these cases, than the blast of a man's mouth; but if any person can be got to try the charitable experiment by blowing, it would seem preferable to the other; 1st. as the bellows may not be at hand; 2dly, as the lungs of one man may bear, without injury, as great a force as those of another man can exert; which by the bellows cannot always be determined; 3dly, the warmth and moisture of the breath would be more likely to promote the circulation, than the chilling air forced out of a pair of bellows.*

Concerning a Moving Moss in the Neighbourhood of Church-town in Lancashire. By the Rev. L. Richmond. N° 475, p. 282.

Jan. 26, 1744-5, a part of Pilling moss was observed to rise to a surprising height: after a short time it sunk as much below the level, and moved slowly towards the south side: in half an hour's time it covered 20 acres of land. The improved land adjoining that part of the moss, which moves is a concave circle containing near 100 acres, is well nigh filled up with moss and water. In some parts it is thought to be 5 yards deep. A family is driven out of their dwelling house, which is quite surrounded, and the fabric tumbling down. Mr. Buttler, Whitehead, and Stephen White, are the first sufferers by this uncommon accident. An intense frost retards the regress of the moss to-day; but it is feared it will yet spoil a great deal of land. The part of the moss which is sunk like the bed of a river, runs north and south; is above a mile in length, and near half a mile in breadth; so that it seems there is a continual current to the south. A man was going over the moss when it began to move: as he was going eastward, he perceived, to his great astonishment, that the ground under his feet moved southward. He turned back speedily, and had the good fortune to escape being swallowed up.

* Would it not seem to be a circumstance in favour of the use of the bellows, and against that of blowing the breath from the mouth, that this latter air, having been already breathed, is thus rendered noxious, and little suited to restore a person almost dead? Besides, warm air may be blown from bellows as well as cold. Dr. F. seems to have attended only to the mechanical part of the respiratory process, without being aware of the chemical changes produced on the blood by atmospheric air, whether drawn or forced into the lungs.

A brief Inquiry into the Reading of two Dates in Arabian Figures, cut upon Stones found in Ireland; communicated Nov. 10, 1743, and December 6, 1744. By John Ward, F.R.S. N° 475, p. 283.

Two dates in Arabian figures, transmitted from Ireland, were some time since laid before the R. S. But as the reading, which had been given them, seemed doubtful to the gentlemen then present, they desired Mr. Ward's further thoughts concerning them. Both those dates are said to be cut on stones, and in relief.

He considered them as carefully as he was able, and after the strictest examination could see no sufficient reason to think either of them so old, as had been represented.

For several considerations then he is induced to think, that the sculptures were made in the 16th century, and probably not before the reign of Queen Elizabeth, considering the size of the ruffs, and some other circumstances, which seem not to suit with more early times.

On firing Phosphorus by Electricity. By the Rev. Henry Miles, D.D., F.R.S., N° 475, p. 290.

Mr. M. took a small bit of phosphorus, which had lain by him 10 years; and having nothing at hand convenient for holding it, he rolled it up in a small piece of white paper; and applying it to the excited tube, it immediately took fire, emitting a considerable quantity of flame and smoke: after some time he quenched it, by dipping it into water; and taking it out again without staying any longer than to be satisfied it was not on fire, he applied it as before, when it suddenly took fire, as at first. This he repeated in the same manner for 6 or 7 times with the like effect; though the phosphorus could not be drained of the water, especially as the paper about it was wet.

The phosphorus was held generally about 5 inches from the tube; but once or twice bringing it nearer, he could perceive a continued ray of light from the tube to the phosphorus. He was minded to try whether the air alone would have that effect on the phosphorus, and accordingly took it out of the water, with a forceps, and laid it down on a shelf, so as nothing touched it but the instrument which held it; but he could not perceive the least glimmering of light, though the place was sufficiently dark, after it had lain there for the space of half an hour, which he thought long enough to satisfy him, that it was not kindled by the action of the air in the abovementioned experiment.

An Observation of a Fracture of the Os Humeri by the Power of the Muscles only. By the late Claudius Amyand, Esq. N° 475, p. 293.

An extraordinary Case of the Bones of a Fœtus coming away by the Anus. By John Still Winthorp, Esq. N° 475, p. 304.

There are several instances of the bones of fœtuses, which have died in their mothers' bellies, making their way out by preternatural ways; some by the navel, some by the groin, and some by the anus. Of this last sort is the following instance, which happened in New London in New England, in the year 1737. A negro wench was thought to have conceived with child; and about 3 months after, she had some appearances of a miscarriage, but no fœtus was observed to come away. This therefore made the good women now alter their opinion; thinking that she was not with child, but only had not been regular from having taken cold: therefore remedies, proper in such a case, were given her; but she found no relief from exceedingly great pains she complained of, in the bottom of her belly, and in the small of her back, more particularly when she went to stool. Her flesh wasting extremely, a skilful woman was sent for, who found milk in her breasts, and other certain tokens of her being with child. She continued wasting in a miserable condition, growing less in her belly, and her breasts falling, and was at last given over: but at length, at the end of about 8 months, she brought away much blood by stool, on which her pain in those parts abated; and then she voided with her stools these bones with flesh and rotten skin about them. After this she soon grew well, and quite recovered. All the parts of the fœtus were found in her stools, except the head; which is supposed to have come away by the vagina, when she had the symptoms of miscarrying abovementioned; for it was now recollected, that she then said, something came away with her water as large as a great nut, but it was not then attended to.

An extraordinary Cystis in the Liver, full of Water. By Charles Jernegan, M.D. N° 475, p. 305.

Mrs. A. B. deceased, aged near 40, had been affected with a constant acute pain on the region of the liver, with a swelling, or more than ordinary fullness on that side; by pressing of which was perceived a fluctuation of some fluid lying deeper than just under the first teguments. The body was opened by Mr. Sherwood, junior, when the liver was found of a prodigious size, and there was a small adhesion to the peritonæum without inflammation: it spread over the stomach quite to the spleen on the left side, and contracted much the cavity of the thorax, by pressing and thrusting up the diaphragma. On opening the great lobe of the liver, there issued out above 4 quarts of a limpid water, from a cavity formed by the proper containing coat of the liver; though the water itself had been contained in a single conglobated gland, and there formed a cystis, which had burst, and was found loose at the bottom of this large cavity. This skin or cystis was not so thin but still capable of further expansion.

The liver still did its function of separating the gall: the gall-bladder and its ducts were in a good state: the lobulus spigelii was much enlarged, and crumbled easily like a mass of congealed blood.

The patient had no particular thirst; nor was there any alteration in the urine, as to quantity more or less. But she had this symptom, common in the hydrops pectoris, of not bearing any other posture than that of leaning forwards on her breast.

The left kidney, being longer than usual, was examined and found to have two ureters; and each had its separate pelvis.

New Observations on Electricity. By Jo. Henry Winkler, Gr. et Lat. Literarum Prof. Publ. Ordin. et Academiæ Lipsiensis h. t. Rector. N° 475, p. 307. *From the Latin.*

Hollow glass balls, &c. by the friction of the hand, excite such an electricity in metals and persons near them, that the electrical fire, emitted on the approach of a body void of electricity, bursts out in a continued stream. But when glass tubes are rubbed up and down, the sparks are emitted by intervals.

M. Winkler describes several machines for conveniently rubbing of tubes, globes, and cylinders of glass, for exciting electricity. And by various ways of increasing electricity, he set fire to several substances, as spirit of wine, &c.

Description of a Machine to blow Fire by the Fall of Water. By James Stirling, F. R. S., N° 475, p. 315.

ABCD, fig. 9, pl. 2, is a pit dug in the ground, its surface higher at D than on the other side at A. The bottom BC is strongly rammed with clay, on which are laid thin deals. In this pit is fixed a tub GHKI without a bottom, having a hole I at the lower part of the side, and all round the tub is rammed with clay, except at the hole I. In the middle of the upper end of the tub is fixed a pipe PQRS; at the higher end of which are 4 holes pointing downwards, two of which are represented by S and R.

SRTU is a funnel, fixed on the top of the pipe, with a throat XZ narrower than the bore of the pipe. In the upper end of the tub towards one side is fixed a crooked pipe at LM, tapering to the end at N. It is made of wood so far as O, but from O to N of iron, the fire being supposed at N. EF is the surface of a plain stone, raised up in the middle of the tub, directly under the pipe PQRS.

The running water, being let in at the top of the funnel, falls through the pipe on EF, the stone in the tub; it runs out at the hole I, but cannot get off till it rises as high as A. This raises it in the tub almost up to the surface of the stone, and it must not rise higher. So much water must run in at the top of the funnel, as will keep it always full, or nearly so. This height of water forces it into the pipe with a great velocity; but, since it passes through the throat of the funnel, which is of a smaller bore than the pipe, room is left all round the vein of water for the air to enter at the air-holes.

It no sooner enters than it mixes with the water, on account of the rapidity of the motion; and both together make a white froth, and entirely fill the bore of the pipe. When this froth falls on the stone in the tub, it is dashed into small particles, which disengages the air from the water. The air cannot get out at PQ, the end of the pipe, because it is filled with the froth, which falls with a great force; neither can it get out at the hole I, because the surface of the water is kept so high above it; for which reason it rushes out at N; and if the hole N be stopped, the air will soon force all the water in the tub out at I, and then follow it. The most convenient way of regulating the blast, is to bore a small hole in the blast-pipe; and, by the help of a pin in it, to let out what air there may be more than is wanted.

The dimensions of such an engine sufficiently large to smelt harder ore than any in lead-hills, are as follow:

Height of the funnel 5 feet; length of the pipe 14, 15, or 16; height of the tub 6; diameter of the tub 5½; height of the stone in the tub 2.

Diameter of the throat of the funnel 3¼ inches; diameter of the bore of the pipe 5¼; diameter of the blast-hole at N 1½; hole at I about 5 inches square; diameter of the air-holes 1¼.

This engine is also of great use to convey fresh air into the works; which saves the double drifts and shafts, and cutting communications between them. A small one will do very well for a blacksmith.

Some Additions to the Statical Experiments printed in N° 470. *By Dr. John Lining, at Charles-Town, South Carolina.* N° 475, p. 318.

These additions are of no use now. See the former part at p. 683, vol. 8 of these Abridgments.

Concerning an extraordinarily large Fossil Tooth of an Elephant. By Mr. Henry Baker, F. R. S. N° 475, p. 331.

The fossil tooth Mr. B. received from Norwich. It seems to be a grinder belonging to the left under-jaw of a very large elephant, as its own size and weight may show: for the circumference, measured by a string drawn round the edge, is 3 feet, wanting 1 inch; in length it measures 15 inches; in breadth, where widest, 7 inches, in thickness about 3; and its weight is upwards of 11 pounds.

On one side it is convex, and on the other concave, with 16 ridges and furrows running on each side transversely, and corresponding with the same number of eminencies on the grinding edge, which appears furrowed like a millstone. On the bottom of the part that lay within the gum are several cavities for the insertion of the nerves. The whole tooth is almost entire, and seems very little, if at all, petrified; but, since its being exposed to the air, several little cracks appear. Other monstrous bones were found with it; and particularly thigh bones, 6 feet long, and as thick as the thigh of a man; all which belonged probably to the same animal, and may be considered as farther proofs of the creature's enormous size.

The place where, and the manner how, these bones were discovered, are curious particulars. A little town, called Munsley, is situated close to the sea-shore, on the north-east coast of the county of Norfolk, where the sea is bounded by exceedingly high rocky cliffs: some of these being gradually undermined by the continual dashing of the waves when the tide comes in, great pieces frequently tumble down on the shore: and by the tumbling down of one of these the above-mentioned bones and grinder were discovered.

This discovery seems a convincing demonstration, that the earth has undergone some very extraordinary alterations: for the remains of animals, of quite different climates and regions, and of kinds which, in the present situation of the world, could never possibly come over hither, must either imply their having been placed here by Providence originally, or that this island must heretofore have been contiguous to the continent: but since we find these creatures in very hot countries only, it is highly probable they were never placed here by Providence.

What changes have happened to our earth, and how they have been produced, no human wisdom can possibly find out with any certainty. But suppose only the polar points, or the axis, to have been shifted at any time but a few degrees, and its centre of gravity to have been altered, which some great men have imagined not improbable, what convulsions in nature, what a universal change in the face of things, must thus have been occasioned! what inundations, or deluges of water, bearing every thing before them! what breaches in the earth, what hurricanes and tempests, must have attended such an event! for the waters must have been rolled along, till, by them, an equipoise was produced. In short, all parts of the world would thereby acquire degrees of heat and cold different from what they had before. Seas would be formed where continents had been: continents would be torn in sunder, or perhaps split into islands. The ancient bed of the sea would be changed into dry land, and appear covered at first with shells, and other marine bodies; of which the action and nitrous salts of the air would in a few years moulder away, and turn to dust those on the surface; but such as were buried deep would be preserved and remain for many ages.

Such would probably have been the fate of inanimate things: and as to living creatures, they must have been almost universally destroyed and buried in the ruins of the world, as perhaps this elephant may have been. Some few however would probably escape, either by swimming to, or being left on, rising lands; where, if they met with proper food, and an agreeable climate, they would continue and increase; or otherwise would wander till they found such a country, unless prevented by interposing seas, or impassable rivers.

All this indeed is barely conjecture: but the bones and teeth of fishes, the multitudes of sea-shells, some of which are petrified, and others not, and the many sea productions found buried in the earth in almost every country, at vast distances from the sea, and even in the midland parts, are demonstrations of the surprising alterations that must have happened as to the disposition of sea and land.

The present grinder and bones, however they came thither, must have lain in this cliff for many ages; and that the grinder in particular is much larger and heavier than any our late worthy president Sir Hans Sloane has mentioned in N° 403 and 404, of the Philos. Trans. where he gives an account of all the fossil teeth of elephants that had come to his knowledge. None of those mentioned by Mr. Molineux, in his History of Ireland, come near it in weight or size. Our thigh-bones of 6 feet long exceed also, by 2 feet, any ever yet heard of: and, according to Mr. Blair's osteology of an elephant 9 feet high, which died at Dundee in Scotland, in the year 1706, and whose thigh-bones were 3 feet in length (Vide Phil. Trans. N° 326,* we may suppose, by the rules of proportion, that the elephant, to which our bones and tooth belonged, was 18 feet in height.

Of an extra-uterine Conception. By Starkey Myddelton, M. D. N° 475, p. 336.

On the 28th of October last, Dr. M. was sent for to a woman of about 42 years of age. She had been taken with a flooding the day before; which a little surprised her, having been very irregular in her menstrual discharges for near a year before. At the same time she complained of a great pain in her belly and loins, with a continual forcing both forward and backward; which still continued, though her flooding was then in a manner stopped.

He ordered her a gentle paregoric for that night, and the next day he found her in great pain; at which time she said, she had some reason to believe she had conceived with child.

He then examined her, and found the os tincæ entirely close. He was not very curious in examination at this time; taking it for granted, that nature would soon dispose the uterus to discharge its contents, though at present there was not the least appearance of it. He ordered her an anodyne clyster, and a paregoric to be

* Vol. v, page 557 of these Abridgments.

taken after it; and the next day he found her pains continued, which now appeared like a tenesmus, though so violent, as to prevent her rest all that night. He then ordered her a repetition of the clyster and paregoric; and the next day, finding her in great pain, and still without any rest, and beginning to be a little feverish, he ordered 8 oz. of blood to be taken away, and continued the paregoric, which he likewise ordered her to repeat as she found occasion; from the use of which she sometimes rested tolerably well; but as the power of the opiate went off, her pains always returned.

Several days having now passed without any alteration, he again examined her, and found the os tincæ still as close as ever; but, on a stricter inquiry, he felt something, which seemed to be the head of a child fluctuating in its membranes.

He told her his thoughts of her case, and that it was not in his power to help her; but that nature must take its course, or at least point out a method how to act. She seemed very much surprised at his opinion, and asked, if ever he had met with such a case before? He told her, he had been engaged in the practice of midwifery upwards of 20 years; in all which time he had never met with a case of the like nature: being positive he felt the head of a child, but could not absolutely determine whether it was in the uterus or extra uterum.

He then told her, he would desire the favour of Dr. Bamber to give her a visit; which he did; and the next day they went together; when, on examination, he confirmed what he had before asserted; but seemed more inclinable to believe the child was extra uterum. Indeed he proceeded in his inquiry at that time further than before, having passed his finger into the anus, where he could distinguish the head more plainly. They then both left her, after having ordered her to repeat the paregoric, when in more pain than ordinary; and once in 2 or 3 days to take a gentle lenitive purge, to keep her body soluble; because the continual use of opiates would naturally tie her up.

In this manner she went on for about 3 weeks longer; when he waited on Dr. Nichols, and desired the same favour of him as he had before asked of Dr. Bamber, that he might have his opinion also of a case which appeared so very singular. The next day they went together; and when they came, he desired he would examine her, which he did; and after having heard all her complaints, said, he was of opinion, that there was some abscess forming in, or in contact with, the uterus, which very likely in a little time would break and discharge itself; but as, at that time, nothing of a child could be perceived by the touch, so he was obliged to submit that to the credit of his opinion, who had before frequently felt it.

Thus she continued for about a fortnight after this visit; when, calling on her one day, she told him, she was much easier than she had been; and that something came constantly draining away by the anus, of a very offensive smell, which on examination appeared to be true pus. He now began to think Dr. Nichols's opinion of her case the most eligible, and the rather, as it was not inconsistent with his own sentiments, that there had been a child; which, being now dead, might have given occasion for the forming such an abscess.

In this state of violent pain she continued to the time of her death, which happened on the 28th of January, being 13 weeks from the first of her illness; when, by her particular desire, he opened her. After having divided the integuments of the abdomen, every thing, at first view, appeared in a healthy state. On turning aside the intestines, he found the uterus sound and perfect, and of a size common to women who have had children; but, in the place of the right Fallopian tube, there appeared a large tumour, formed by the expansion of the tube extending itself from the os ilium towards the extremity of the sacrum. Opening it, he discovered a mass of fetid pus, in which the bones of a fœtus, of about 5 or 6 months old, were buried. These bones were, for the most part, wholly divested of their flesh; so that the edges of the thin bones must, of necessity, cut and irritate from every motion of the body. The pus had made its way through the rectum, in which there was a small passage a little above the sphincter.

On examining the bones, after having washed them in water, a new matter of surprise appeared; viz. the inferior jaw was consolidated with the os temporis and superior maxilla; and 6 of the ribs, with their correspondent vertebræ, were united into one bone.

Concerning a Rotatory Motion of Glass Tubes about their Axes, when placed in a certain Manner before the Fire. By the Rev. Granville Wheler, F. R. S. N° 476, p. 341.

About 4 years before, Mr. Charles Orme, of Ashby de la Zouch in Leicestershire, acquainted Mr. W. that in drying the glass tubes for his diagonal barometers, he had observed a rotatory motion about their axes, and at the same time a progressive one towards the fire. And a little above a year since, Mr. W. making some stay at Ashby, he went to see the experiment, which answered fully to the description: the tubes, which were about 4 feet long, and half an inch over, moving at 6 or 8 inches distance from the fire, not only progressively, and about their axes along the side-wall they leaned against, but along the front-wall of the chimney, which made an obtuse angle with the other; so that they seemed to move up hill, and against their weight.

Surprised at this, he thought the case deserved a little further examination; and proposed placing 2 tubes horizontally, parallel to each other, and at right angles to the face of the fire, to be supporters to a third, which was to be placed upon them parallel to the fire. They did so, and with pleasure observed the supported tube turn about its axis, and move on towards the fire in such a manner, as made him still less inclined to think either of the motions owing to the draught of the fire, and certainly not to the whole weight of the moving tube; a fine spirit-level informing them that the supporting tubes leaned from the fire; so that the motion was a little up-hill.

This success determined him to go on further; and, furnishing himself with tubes of several lengths and thicknesses, he made several trials; and found, that with a moderate fire the experiment succeeded best, when the supported tube was about 20 or 22 inches long, the diameter about $\frac{1}{10}$ of an inch, and had in each end a pretty strong pin, fixed in cork, for an axe to roll with upon the supporting tubes; which, to lessen the contact, had nearly the same diameter with the moving one. Under these circumstances the tube would begin to move at 18 inches distance from the fire; and continue to do so, with little intervals, till it touched the bars; and moved much in the same manner, when a little ball of cork, an inch or more in diameter, was fixed in the middle of it. But what surprised him still more, and seemed to take off the objection of the draught of the chimney, was, letting it once stay a little while against the bars, its still continuing its motion about its axis in the same direction.

This put him on making little rings of wire, to fix on and move along the supporting tubes, so as to stop the moving tube at any distance from the fire. Stopped with these, the motion of the tube about its axis still continued.

Desirous to try what would be the effect in or near an upright position, he made the pin at one end of the tube rest on a China plate, that at the other turn in a silver socket (that carried his pencil) fixed in a horizontal arm of wood, but so as he could slip it up and down, to adapt it to the length of the tube. Here he found, that if the tubes leaned to his right hand, the motion was from east to west; but if they leaned to his left, the motion was from west to east; and the nearer he could get to the perfectly upright position, the less the motion seemed to be either way.

He now placed the tube horizontally on a glass plane, a large fragment of a coach-side window glass. The tube, instead of moving towards the fire, moved from it, and about its axis, in a contrary direction to what it had done before. Observing that this glass plane was broader at one end than the other, and that the rotation backwards was more sensible when the narrower end was towards the fire, he placed a triangular piece of the same glass with its vertex towards the fire nearly horizontal, but rather rising from the fire; so that its base was a little higher than its vertex; and on it a tube of glass, about 22 inches long, and $\frac{1}{8}$ of an inch diameter, near the vertex and the fire. This tube receded from the fire, moving about its axis till it came to the distance of 8 inches; which is 4 inches more than it receded the day before on the same piece of coach-glass, before it was broke into this triangular form.

He was naturally led now to make use of 2 supporting tubes, instead of the triangular glass plane. These were about 18 inches long each, and $\frac{1}{10}$ of an inch in diameter, and placed parallel to each other at the distance of about 2 inches, so as to support the moving tube near the middle of it. When very nearly horizontal by the level, the supported tube moved from the fire about its axis to the distance of 13 inches: when the supporters were a little raised at their remote ends, so as manifestly by the level to descend towards the fire, it receded to the distance of 10 inches, moving as before about its axis; but in this latter case the fire had declined a good deal; otherwise probably the tube would have receded farther though up-hill.

The next day, the same tube, when the same supporting tubes were $8\frac{1}{4}$ inches distant from each other, receded nearly as before: when $12\frac{1}{4}$ inches from each other, it stood still; and when removed to the distance of $16\frac{1}{4}$ inches, the supported tube very manifestly changed its motion, and went towards the fire; as it did afterwards, when the inclination of the supporting tubes was altered, so as to ascend towards the fire.

When the tube had 4 others under it, all supporting, one near each extremity, and one on each side of its centre, no motion at all was perceived; and when 2 of them on the same side of the centre were taken away, the supported tube moved into an oblique situation with regard to the fire, the unsupported half receding from the fire. So that on the whole, it appears sufficiently plain, that the stream of air up the chimney is not the cause of the rotation.

Mr. W. next made an experiment or two, to show that the motion is not owing to any attraction or repulsion in the tubes. He suspended two fragments of small tubes, 8 inches long, and about $\frac{1}{10}$ of an inch in diameter, near the fire, from 2 pins, by blue silk lines, which had each a loop at one end, were tied at the other to the top of the tubes, and hindered from slipping off by a little sealing-wax. The tubes came together at the upper end, and receded manifestly from each other at the lower, appearing to be in a state of attraction above, and a state of repulsion below: but suspecting this to be owing to the sealing-wax, which soon began to melt, he scraped it off both, leaving only as little as was possible, to hinder the silks from slipping. The consequence then was, they came together at the lower ends, and very near so at the upper; and when suspended from one pin, so that the loops of the silks touched each other, the tubes seemed equally close all the way down, without any appearance either of attraction or repulsion. But imagining still that a repulsive power in the heated supporting tubes, when placed nearly together, might possibly be the occasion of the receding of the upper tube at contact with them. To put the matter out of all doubt, he wet the 3 tubes all over; yet the regressive and rotatory motion was still manifest, with very little, if any difference; not more than might be well accounted for, from the increase of resistance by wetting.

These 2 experiments fully convinced him, that neither attraction nor repulsion would be of any assistance in solving the rotation. On considering therefore the matter further, he found nothing was wanting, but that the moving tube should swell towards the fire; and indeed he thought he could perceive such a swelling in Mr. Orme's long tube of 4½ feet, which he saw first placed near a good fire in the manner before described. For, allowing such a swelling, gravity must draw the tube down, when supported near its extremities horizontally; and a fresh part being exposed to the fire, and swelling out again, must fall down again, and so on successively; which is, in other words, a rotatory motion towards the fire.

When the supporting tubes are brought near each other, as well as near the centre of the supported tube, then the parts hanging over on each side, being larger than the part which lies between the supporters, will, by their weight, draw downwards, and consequently force the middle part, resting on its two fulcra, upwards; and being less advanced towards the fire, as being less heated, will, by their oblique situation, draw the middle part backward also from the fire: which effects, being successive, will exhibit a rotatory regressive motion, quite contrary to what the tube had when supported near its extremities: and when a single tube lies inclining opposite to the fire, either to the right-hand or the left, out of a plane perpendicular to the surface of the fire, gravity will not permit the curved part to rest, but draw it down, till it coincides with a plane perpendicular to the horizon; and consequently as new curves are generated, new motions will be so too; that is, the tube will be made to move about its axis; but with this difference, when the tube inclines to the right-hand, the motion about the axis will be from east to west; when to the left-hand, from west to east. The justness of this reasoning is made manifest with a very little trouble; only bending a wire, and supporting it first near its extremities, then near its centre on each side, afterwards inclining it to the right, and then to the left; the bending in every case representing the curved part of the tube next the fire. And that this solution is the true one, seems further probable from hence, that when 4 supporters were made use of, one at each extremity, and two near the middle, there was no motion at all either backward or forward. Now is it of any service to object here, that the increase of contact hinders the motion? because, on the plane of glass, so large as to have a much greater contact with the tube, both a rotatory and regressive motion was manifest.

An Attempt by Mr. John Ward, F. R. S. to explain some Remains of Antiquity lately found in Hertfordshire. Communicated by William Freeman, Esq. F. R. S. Feb. 14, 1745. N° 476, p. 349.

By the account, which Mr. Ward delivered with them, these are said to have been found in a chalk pit, near the side of Rooky Wood, in the parish of Barkway in Hertfordshire. A farmer's man, digging chalk there about 2 years since, brought them with the chalk into his master's yard, and taking no notice of them mixed them with the dung. They consist of a small brass image, an oblong piece of brass, and 7 silver plates, very thin; which have all suffered more or less by time, and other accidents. The figures and ornaments on the front of the plates are all in relief, and seem to have been made by a stamp impressed on the other side. Two of them have inscriptions in a compartment, written with the point of a style, and the letters flatted behind. And one of these, with 3 others of the remaining 5, have the figure of Mars, and the other 2 that of Vulcan, impressed on them.

The image represents Mars, fig. 1, pl. 3, looking to his right side, with a helmet on his head, and his body naked; his right arm extended, as if he had held a spear, and a thong round his left, like the remains of a shield now broken off with the hand; his right shoulder supporting a belt, which crossing his body descends on the left side; his right leg broken off at the knee, and his left foot lost. The height of what remains, from the end of the left leg to the top of the crest, is about 7 inches. It has been observed by Montfaucon, that though Mars is frequently represented on coins, yet his statues are not very common. (Sup. vol. i. p. 93.)

The other piece of brass, fig. 2, pl. 3, is about 4½ inches long, and seems to have been the handle of a knife, or some such utensil.

The plates are for distinction sake numbered in the following order. 1. The first is broken into two parts, see fig. 3, which put together resembles the form of a leaf, and is near 21 inches high, and about 10 broad in the widest part. It contains an inscription inclosed in a compartment, addressed to Mars Jovialis.

2. The second, fig. 4, is 8 inches in height, and 4 in breadth, where it is widest; and retains pretty much of the gilding, which none of the rest now do: though all of them it seems, when first discovered, appeared to have been gilt, but in washing them the gilding came off. It has on it an image of Mars in a military habit, with a helmet on his head, a spear in his right hand, and his left resting on a shield; in the manner he is often seen upon coins. This image is placed in the front of a temple, between two pillars, with a fastigium or pediment over them. And beneath the temple in a compartment is an inscription to Mars Alatorum.

3. In the third plate, see fig. 5, which is 3 inches high, and almost 2 wide, is an image of Mars placed between two pillars without a pediment, in much the same attitude as the former, with a parazonium over his shield.

4. The 4th plate, see fig. 6, which is $4\frac{3}{4}$ inches in height, and $1\frac{3}{4}$ inch in breadth, has the figure of Mars in a like attitude, inclosed only in a plain compartment.

5. The height of the 5th, see fig. 7, is 8 inches, and the breadth near $4\frac{1}{2}$; which has also a figure of Mars, much like the former, but turning to the left hand, with a chlamys hanging down on his right side. It stands in the front of a temple, having two pillars on each side, called by Vitruvius tetrastylos, and a double pediment over them.

6. The 6th plate, see fig. 8, is $6\frac{3}{4}$ inches in height, and $3\frac{1}{4}$ in breadth. It differs from all the former, as it represents the figure of Vulcan, having his usual attributes, a thick beard, high cap, short tunic, femoralia, and half boots; a forceps in his right hand, and a hammer in his left, with a chlamys thrown over his left arm. He looks to the right, and has before him a vessel like an altar, from which a flame ascends. He is placed in the front of a temple, between two pillars under a pediment, like Mars.

7. The last plate, see fig. 9, is $3\frac{1}{4}$ inches high, and near 2 inches wide. It contains also an image of Vulcan in the front of a temple, with his several attributes, like the former, but differs from it in the other ornaments.

The design of both the inscriptions is to return thanks for some favour ascribed to the deity, to whom they are addressed. That on the first plate runs thus:

MARTI
IOVIALI
TI . CLAVDIVS . PRIMVS
ATTII . LIBER
V . S . L . M

That is,

Marti Joviali Titus Claudius Primus, Attii libertus, votum solvit libens merito.

The word IOVIALI, in the second line, seems to have been an epithet given to Mars in compliment to the emperor Diocletian, who assumed the name of Jovius; as his colleague Maximian did that of Herculius. Hence we meet with some military bodies in the Notitia, and elsewhere, called Joviani and Herculiani from those emperors; like the Flaviani, Æliani, and the like, which were so denominated from the names of other preceding princes. There are also other epithets of the same form with that in the inscription, taken either from the names of deities, or emperors deified; such were the sacerdotes Augustales, Flaviales, Hadrianales, and others, which often occur in Gruter. In like manner Cicero gives the title of ministri Martiales to the priests of Mars; and calls the company of merchants at Rome Mercuriales, as being under the protection of Mercury. And Ganymedes is stiled by Macrobius, Jovialium poculorum minister. Now as these several appellations took their rise from the peculiar relation and subserviency of the persons to those deities, from whom they were denominated; so Mars himself, being here called Jovialis, is by an excess of flattery represented as subservient to this emperor Jovius or Jupiter. For so he was also called, as we find in some like instances of fulsome compliments paid to him by the panegyrist Mamertinus; as when addressing to him, and his colleague Maximian, he says, Sancte Jupiter et Hercules bone. And in another passage, non opinione traditus, sed conspicuus et præsens, Jupiter cominus invocari, non advena, sed imperator, Hercules adorari. And as if no degree of flattery could be too extravagant for this emperor, there is an inscription in Gruter, which begins thus: ÆTERNO IMPERATORI NOSTRO MAXIMO OPTIMOQVE PRINCIPI AVRELIO VALERIO DIOCLETIANO. The epithets OPTIMVS MAXIMVS, usually ascribed to Jupiter, had indeed been applied to some former emperors; but ÆTERNVS, as a personal title, seems to have been first attributed to this prince; though, like other ill examples, it was soon imitated, and given to some following emperors.

The third line contains the names of this votary, TITVS CLAVDIVS PRIMVS, each of which is separately found in Horsley's Britannia Romana; and in one of Gruter's inscriptions they all three meet in the same person, in the order they stand here. The next line tells us his character, that he was the freedman of ATTIVS, that is, probably, of TITVS CLAVDIVS ATTIVS; it being customary for freedmen to assume the first two names of their patrons, as TIRO the freedman of Cicero was called MARCVS TVLLIVS TIRO. Indeed ATTIVS generally stands as a family name, but we find it in the place of a cognomen in Gruter, MARCVS TVLLIVS M. L. ATTIVS. The last line contains the usual form of such addresses.

The inscription on the second plate is thus expressed:

D . MARTI . ALATORV
DVM . CENSORINVS
GEMELLI FIL
V . S . L . M

That is, it seems, it may be read:

Deo Marti Alatorum Dum, Censorinus, Gemelli filius, votum solvit libens merito.

The word ALATORV in the first line must probably stand for ALATORVM, the letter V being joined to the R in one character; as we find them in the Britannia Romana, where they make part of the word INSTITVERVNT for INSTITVERVNT.

Now as to the design and use of these plates. The ancient pagans had not only their national but domestic deities, to whom they addressed in private, and sometimes carried their images about with them, as their guardians and protectors. And it appears to have been a custom among them to place their images in shrines, made in the form of temples, both for public and private devotion. The tabernacle of Moloch, mentioned in the Acts of the apostles, is generally taken to have been of the former sort. And Herodotus informs us, that the Egyptians, on a solemn day, carried in procession the image of a goddess, said to be the mother of Mars, in a wooden temple gilded over, which was drawn in a chariot. Such shrines are mentioned likewise by later writers. And others of a less size seem to have been made in imitation of them for private use. The silver shrines of Diana, mentioned also in the sacred history cited already, are by most interpreters said to be of this kind. And Mr. Kemp had in his collection of antiquities one made of brass, only 5 inches high, with a goddess, supposed to be Isis, sitting in it; as it is described in the printed catalogue. Another of the same deity, but of a different form, and somewhat less, is now in the possession of James West, Esq. a worthy member of this Society. And sometimes they were placed in the monuments of deceased persons; an instance of which we find mentioned in an inscription published by Reinesius, which is there said to have been made of marble. The persons employed in making those sacred images were called by the Greeks ἀγαλματοποιοὶ, and by the Romans sigillarii, as we find in the ancient glossaries. And one of these artists is mentioned in two inscriptions of the Britannia Romana, where he is stiled SIGILLARIVS COLLEGII LIGNIFERORVM, who are more usually called by the Greek name DENDROPHORI; part of whose business might be to carry, or attend, the shrines in their processions at public festivals.

Whether these plates ever belonged to shrines, cannot be determined; but probably they did not; except perhaps the first, which from its size, and having no image stamped on it, but only a written inscription in honour of Mars, might possibly have been laid over part of a wooden shrine, within which the brass image was placed, that was found with it. As for the rest, they were probably designed as partial representations of shrines for the use of private persons; having only the front of them with the image of the deity placed in it; which, being fixed on wooden tablets, might either be set up in their houses, or carried about with them, in devotion to those tutelar deities. And when any fortunate event happened, which they attributed to the success of their addresses made to them, they might sometimes express their acknowledgment of it by hanging them up in their temples, among other donations, making them a sort of votive tables. That they were intended for some such uses seems the more probable, from the number of them found together. They have a similitude with the reverse of many Roman coins, where the images of their deities are represented in the same manner; from an imitation of which they might perhaps be introduced at first, as well for cheapness, as ready convenience, in some of the more remote provinces. And it is very remarkable, that no two impressions on these plates are in all respects exactly alike; as we do not often meet with two Roman coins struck from the same die.

As to the time when the plates were made, the inscription on the first fixes it to the reign of Diocletian; and as not only the characters of the other inscription exactly correspond with that, but also the manner of the work on each plate is the same; it is highly probable they were all made about the same time, which was near the end of the third century. And to this likewise the form of the letters, particularly A and M, very well agrees. Nor ought it to seem strange, if more of them have not been preserved; since from their nature they appear so liable to be destroyed, either by the injuries of time, or for the sake of the silver.

Concerning the Poles of Magnets being Variously placed. By Gowim Knight, M.B.
N° 476, p. 361.

1. Mr. K. cut a piece of natural loadstone into the shape of a parallelopiped, 1 inch $\frac{8}{10}$ in length, in breadth $\frac{4}{10}$ of an inch, and $\frac{2}{10}$ in thickness: its weight was 3 drams 10 grains. In this stone he placed the magnetical virtue in such a manner, that both the two opposite ends became south poles; and the middle was, quite round, a north pole.

2. Another stone was in length 1 inch $\frac{1}{10}$, in breadth $\frac{7}{10}$, and in thickness about $\frac{2}{10}$ at a medium, it being thicker at one end than at the other: its weight 1 dram 57 grains. The 2 opposite ends of this stone he made both north poles, and the 2 opposite sides south poles.

3. An irregular stone, that weighed about $5\frac{1}{4}$ oz., had 2 broad flat surfaces opposite to each other, at the distance of 1 inch and $\frac{3}{10}$. He made half of each of these surfaces a north pole, and the other half a south pole; so that the north pole of one surface was opposite to the south pole of the other surface, and vice versa.

4. He took a stone of a pretty good kind, that had a grain very apparent, running the length ways of it: it was 1 inch $\frac{4}{10}$ in length, 1 inch $\frac{3}{10}$ in breadth, and its thickness at the sides was $\frac{6}{10}$ of an inch; but in the middle $\frac{7}{10}$; it being tapered away from the middle to the sides: its weight was 3 ounces wanting 4 grains. At one end of it he placed a north pole surrounded by a south; and at the other end a south surrounded by a north pole; so that the edges of each surface had a pole of a different denomination from that which occupied the middle.

A great many varieties of this kind might be easily devised; but these examples seem sufficient to show how manageable the magnetic virtue is, in respect to its direction; and how defective most of the hypotheses are, which have been raised to account for the phenomena of the loadstone.

Of some very Curious Wasps' Nests made of Clay in Pennsylvania. By Mr. John Bartram.* N° 476, p. 363.

Mr. Bartram sent from Pennsylvania 2 sorts of curious wasps' nests made with clay, which are commonly built against the timber under the roofs of houses and pales, to shelter them from the weather. They feed, as the bees, on flowers; but whether they sting like them is not yet known.

The plain clay nest is fabricated by a small black wasp, of the same species of that in fig. 10, pl. 2, but less, having a speck or stripe of yellow in his tail; and the cells are made 4 or 5 together, joining side by side to each other. But the clay nests, that are so elegantly wrought, are built by a purplish black wasp, such as represented fig. 11: after one cell is formed they stop it up, and join another to its end, and then add another to that; which makes these wrought clay fabrics longer than the plain ones.

Their method of working is much alike, and it is very diverting to see them at it: their art and contrivance is wonderful; and, as if it was given to cheer them at their labours, they make a very particular musical noise, the sound of which may be heard at 10 yards distance.

Their manner of working is, to moisten clay, and temper it up into a little lump, of the size of swan-shot. This they carry to build with; they begin first at the upper end of the cell, and work downwards, till it is long enough to contain the nymph or chrysalis: after they have spread out the little lump in a proper manner to form their little fabric, they set up their musical notes, and return to temper and work up more clay for the next course. Thus they continue alternately singing and working, till a cell is finished; which is made delicately smooth withinside; then, at the further end of each cell, they lay an egg; after this, by surprising instinct, they go and catch spiders, and cram the cell full of them: but it is further wonderful to observe, that they only in some manner disable the spiders, but not kill them; which is to answer two purposes; first, that they should not crawl away before the cell is finished; and next, that they may be preserved alive and fresh till the egg hatches, which is soon.

The spiders, by wonderful instinct, are provided for the embryo to feed on: having stored up sufficient for its support, she very securely closes up the cell, and then proceeds to build the next in the same manner.

The maggot or embryo, having eaten up all its provision before October, pre-

* These insects belong to the Linnean genus *Sphex.*

pares for its change, and spins itself up in a fine soft silken case, in which it lies all the winter in the chrysalis state, till the spring, when it eats its way out of its clay dwelling.

For several other curious particulars, with the figures of such clay nests, and the wasps, see M. Reaumur's excellent work, vol. vi. on the clay nests from St. Domingo.

Extract of a Letter from Mr. B— B—r, containing an Account in Pounds and Ounces, of the Surprising Quantities of Food devoured by a Boy, 12 *Years old, in Six Successive Days, who laboured under a Canine Appetite, at Black Barnsley in Yorkshire. Communicated by Dr. Mortimer, Sec. R.S. Dated April* 15, 1745. N° 476, p. 366.

The boy was regular as other children, till about a year before the above date, when this extraordinary craving of appetite first began; which afflicted him to such a degree, that if he was not fed as he called out for it, he would gnaw the very flesh off his own bones; so that, when awake, he was constantly devouring; it could hardly be called eating, because nothing passed his stomach; all was thrown up again.

Of the various substances, bread, meat, beer, milk, water, butter, cheese, sugar, treacle, pudding, rye, fruit, broth, potatoes, &c. he swallowed in the 6 successive days, as follows: viz.

Thursday	69 lb	8 oz.	
Friday	61	14	
Saturday	58	8	
Sunday	77	0	
Monday	60	12	
Tuesday	55	8	
Salt	1	0	in the six days.
Total	384	2	

Of an Iliac Passion, occasioned by an Appendix in the Ilion. By the late Claudius Amyand, Esq. F.R.S. N° 476, p. 369.

A lad about 10 years of age, seemingly in perfect health, after drinking some sour small beer, complained of a violent colic: which increasing with great tension of the belly, and continual vomitings of excrements, he died 3 days after of a miserere, ending in a mortification of the inflamed guts.

He was suspected to have been poisoned; which occasioned Mr. Maccullough's being sent for to open him. All the large guts were found empty, up to an appendix, or a hernious expansion in the ilion, about 3 inches long, and of the same dimensions with the gut itself; which was so contracted and shut by a spasm, that as nothing could pass downwards, so all the fæces were retained in the intestinal pipe, between this and the stomach, which was greatly distended as far as the pylorus; the opening into the stomach there, and that in the guts below, which were contracted and spasmed; being so close, as hardly to admit of a small probe.

The frequent vomitings of excrements, during life, showed that the stricture at the pylorus had occasionally given way to their coming into the stomach; but as these were thrown up as soon as let in, this viscus was found as empty as the guts were below the strangulated gut; through which nothing had passed during the course of the disease.

This appendix of the ilion, more capacious and longer than that usually observed in the cæcum, being supported by no mesentery, lay loose and floating. At its opening into the gut, it made an acute angle, determining the course of the fæces from the upper gut into it, and obstructing their descent into the natural pipe: where the current was made difficult, from a defect in some segment of the fibres inservient to the vermicular or peristaltic action; which probably was the occasion of the spasm, the contracted fibres of the gut having no antagonist above, and the compelling force to conquer the resistance being wanting.

In the liver there was a steatomatous tumour stretched out through its substance, containing in its cystis about 8 ounces of matter; but so disposed, that the course of the fluids and juices in and out was not impeded.

A Proposal for Warming Rooms by the Steam of Boiling Water conveyed in Pipes along the Walls: and a Method of preventing Ships from Leaking, whose Bottoms are eaten by the Worms. By Col. Wm. Cook. N° 476, p. 370.

This is an engine for giving a sufficient heat to all the rooms in the house from the kitchen fire. It consists of a copper or boiler, of water, on the kitchen fire, having a still-head. From the top of this passes a pipe of lead or copper, to convey the hot steam from the boiler, through the several rooms, in a very zigzag manner; being fixed to the wall or side of the room in the place of the chimney; the pipe terminating in an opening or exit. In several parts of the pipe are placed turncocks, either to shut or contract the passage, to adapt the heat more or less according to circumstances.

When a ship's bottom is so eaten by the worms, that she is no longer fit for service, try the following method; viz. first calk well the inside planks or lining; then fill the vacant spaces between the timbers, and the out and inside planks, with boiling pitch or resin, as high as the main gun deck. The pitch, being put in very hot, will run into every the smallest cranny, and make the ship as tight as a bottle. By this the ship is ballasted; there will be no room left for vermin, as rats, &c. and the pitch will serve for other uses when taken out; therefore it will be but little expence.

On Osteocolla. By Ambrose Beurer of Norimberg. N° 476, p. 373.

The author of this communication endeavours to prove, that osteocolla is the petrified root of the black poplar tree (populus nigra). This paper concludes with some attempts towards a chemical analysis of this petrifaction; but these imperfect trials it is unnecessary to reprint, after the very complete and satisfactory experiments of Margraaf, inserted in the Memoires de l'Acad. de Berlin for the year 1748.

Concerning the Boy with an Extraordinary Boulimia, or Craving Appetite. By J. Cookson, M.D. Dated April 24th, 1745. N° 476, p. 380.

The following relation respecting the boy at Barnsley, 6 miles from Wakefield, contains an account of his eating and drinking for 6 days successively.

Matthew Daking, a healthy and sprightly boy, about 10 years old, was about 15 months since seized with a fever, which continued above a fortnight. In the beginning he had frequent provocations to vomit, which induced his apothecary to give a gentle vomit of ipecacuanha. The retchings continuing, he gave him another: they seemed to operate well, but yet did not answer the end in settling his stomach: however, the fever gradually went off, but the vomiting rather increased, notwithstanding some other methods were used.

He then began to have a craving appetite; to satisfy which he was indulged in eating and drinking more plentifully, but always vomited most of what he had taken, almost immediately. His appetite kept increasing, so that in a few weeks his eating was come to the pitch before mentioned. Thus he has continued above a year. His urine and stools did not exceed those in health; so that he vomited most of what he took in.

He had tried crude mercury, and all sorts of medicines, and mineral waters. He looked pretty well in the face, and was cheerful; but had lost the use of his legs and thighs, which were much emaciated. He was sometimes so hungry, that he said he could eat them all: he often wished he were in the king's kitchen.

Medico-physical Observations on Ipecacuanha. By Fred. Gmelin, Med. Licent. Wurtemb. From the Latin. N° 476, p. 382.

Dr. G. had several microscopes, both simple and compound. The latter was Mr. Martin's portable reflecting one. He exposed to the focus of this instrument a very small, pure, thin bit of the bark of this root, very carefully scraped off. Its external surface was opaque, very uneven, of the colour of dry earth; but the inner surface, viewed in the same manner, appeared a confused heap of very short and thin small masses, or small purple atoms, connected by interspersed variegated threads, chiefly of a whitish colour; but at the edge there appeared red prickles; and the whole contexture seemed like that which in the animal economy anatomists describe in the fat; where they say this oil is lodged in cells, which cohere with that lanuginous soft cellular substance.

From these, and several other microscopical observations, M. G. concludes, that the bark must be more efficacious than the whole root, especially when we consider that the purging principle of vegetables resides chiefly in their resinous parts.

Remarks on the Operation of Cutting for the Stone. By Claud. Nic. Le Cat, M.D., F.R.S. Translated from the French by T. S., M.D., F.R.S. N° 476, p. 391.

These remarks have been published in a separate tract, to which the surgical reader is accordingly referred. They form an entire pamphlet, and would be very improperly retained in a collection (such as the present.) of short dissertations or memoirs.

Extraordinary Electrical Phenomena. By M. De Bozes, Professor of Experimental Philosophy at the Academy of Wirtemberg. N° 476, p. 419.

A hollow globe of glass, of 6 or 8 inches diameter, being swiftly turned round on its axis, by means of a large wheel, in the manner Mr. Haukesbee formerly advised; and being rendered as electrical as possible by the application of a dry woollen cloth, or rather of a very dry hand; if, while in this swift rotation, it be brought near the end of an iron bar, suspended by silken strings exceedingly well dried, such an electric power will be communicated to the iron, that on touching the other end of it with a finger, not only sparks of fire, in the usual manner, will be emitted very briskly, but even blood will be drawn from the finger; the skin of which will be burst, and a wound appear as if made by a caustic.

2. If highly rectified spirit of wine heated in a spoon, the ethereal spirit of Frobenius, oil of turpentine, sulphur, pitch, or resin melted, be applied to the iron bar, instead of the finger, the sparks proceeding from it will set it on fire instantly.

3. A chair being suspended by silken ropes made perfectly dry, a man placed in it is rendered so much electrical by the motion of the globe, that in the dark a continual radiance, or corona of light, appears encircling his head, in the manner that saints are painted.

4. When several such globes, or electric tubes, are brought near the man

suspended in the chair, the motions of the heart and arteries are very sensibly increased; and when a vein is opened under the operation, the blood that comes from it appears lucid like phosphorus, and runs out faster than when the man is not electrified.

5. Water, in like manner, spouting from an artificial fountain suspended by silk lines, scatters itself in luminous little drops; and a larger quantity of water is thrown out, in any given time, than when the fountain is not made electric.

N.B. If 3, 4, or 5 globes be employed, the effect will be proportionably better: and M. l'Abbé Nollet has found, that globes or tubes made of glass, coloured blue with zaffer, are preferable to others; for when the glass is blue, the experiments succeed in all weathers; whereas in damp weather the white glass loses much of its electric power.

A Catalogue of 50 Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1743, pursuant to the Direction of Sir Hans Sloane, Bart., P.R.S. By Jos. Miller, Apothecary. N° 476, p. 421.

[This is the 22d presentation of this kind, and completes the collection up to the number of 1100 different plants.]

An Inquiry into the Measure of the Force of Bodies in Motion: with a Proposal of an Experimentum Crucis, to decide the Controversy about it. By James Jurin, M.D., F.R.S. N° 476, p. 423.

Mechanical forces may be reduced to 2 sorts; one of a body at rest, the other of a body in motion. The former is called by the name of pressure, tension, force, or vis mortua; and is measured by the weight of the body.

The force of a body in motion is on all hands agreed to be a power residing in that body, so long as it continues its motion; by means of which it is able to remove obstacles lying in its way; to lessen, destroy, or overcome, the force of any other moving body, which meets it in an opposite direction; or to surmount any dead pressure or resistance, as tension, gravity, friction, &c. for some time; but which will be lessened or destroyed by such obstacles, or by such resistance, as lessens or destroys the motion of the body. This is called moving force, vis motrix, and by some late writers, vis viva, to distinguish it from the vis mortua spoken of before: and by these appellations, however different, the same thing is understood by all mathematicians; namely, that power of displacing obstacles, withstanding opposite moving forces, or overcoming any dead resistance, which resides in a moving body, and which, in whole or in part, continues to accompany it, so long as the body moves.

But about the measure of this kind of force, mathematicians are divided into two parties: and, in order to state the case fairly between them, it will be necessary to show how far the two parties agree, and in what point their disagreement consists.

Both sides agree, that the measure of this force depends partly on the mass or weight of the body, and partly on the velocity with which it moves; so that, on any increase, either of the weight or of the velocity, the moving force will become greater. They also agree, that if the velocity continue the same, but the mass, or weight of the body, be increased in any proportion, the moving force is increased in the same proportion: so that, in this case, the measure of the moving force is the same with that of the weight: or, when 2 bodies move with the same velocity, if the weight of the second be double, triple, quadruple, of that of the first, the moving force of the second will also be double, triple, quadruple, of that of the first.

But when two bodies are equal, and the velocities with which they move are different, the two parties no longer agree about the measure of the moving force. One side maintains, that when the velocity of the second body is double, triple, quadruple, of that of the first, the measure of the moving force of the second is also double, triple, quadruple, of that of the moving force, being the same with that of the velocity. The other side pretends, that in the same case the moving force of the 2d body is 4 times, 9 times, 16 times, as great as that of the first; the measure of the moving force being the same with that of the square of the velocity.

In consequence of the agreement in the first of these two cases, and the disagreement in the second, the one side pretends, that the measure of the moving force is, in all cases, the product of the weight into the velocity; and the other, that it is the product of the weight into the square of the velocity.

This controversy was first started by the famous Mr. Leibnitz, and has been carried on by him and his followers for near 60 years; during which time a great number of pieces have been published on both sides of the question, and a great number of experiments have been made, or proposed to be made, in order to decide it. But though both parties agree in the event of the experiments, whether actually made, or only proposed; yet, as the writers on each side have found a way of deducing from those experiments a conclusion suitable to their own opinion, the disagreement still continues as wide as ever, to the great scandal of the learned world.

Now, if we examine carefully into the reason of this, and would see by what means it happens, that two opposite conclusions are so often drawn from the same experiment, we shall find it not so much owing to false reasoning on either side, as to another cause; namely, to their disagreement in the principles on which the reasoning is founded. For whereas whatever is laid down on either side as a principle, ought to be something all the world agrees in, at least what is admitted by the other party; without which, all reasoning on it is to no purpose; this conduct has been so little observed in the present dispute, that what has been offered on the one side as an undoubted principle or axiom, has commonly been something that the opposite party does not admit, nay even absolutely denies.

Of this it were easy to produce a number of examples; but two only may suffice. Those who maintain that the moving force is as the weight into the velocity, lay down for a principle or axiom, that when two bodies meet one another in contrary directions, if their moving forces be equal, neither body will prevail over the other: and if their moving forces be unequal, the stronger will always prevail over the weaker.

This the Leibnitian party deny. They maintain, that one of these bodies may prevail over the other, though their moving forces be equal: nay, that in many cases the weaker will prevail over the stronger.

It is therefore to no purpose to allege, that the principle above laid down is founded on common sense; or that it was always universally received, till this dispute began: for since the opposite party now reject it, all reasoning upon it can have no weight with them: we must have recourse to something else.

On the other hand, those who adhere to Mr. Leibnitz's sentiment, lay down for a principle, that equal effects always arise from equal causes; provided the causes be entirely consumed in producing those effects.

This their opponents do not admit, unless in the case where those equal effects are produced in equal times: and therefore, till both sides shall agree in admitting this principle, no argument can be drawn from it by one party, that will be of any service to convince the other.

But as this principle is chiefly used in reasoning on experimenis made with springs, many of which have been produced by both parties, in support of their opinions, it may be worth while more particularly to consider, what right there is on the one side to impose this principle, and what reasons may be given on the other for rejecting it.

When one end of a spring, wholly unbent, leans against an immoveable support, and the opposite end is struck on by a body in motion, which, bending the spring to some certain degree, thereby loses its whole moving force; the moving force of the body may be considered as the cause of bending the spring; and the bending of the spring as the effect of that cause which is wholly spent and consumed in producing it.

Now if two unequal bodies, moving with unequal velocities, strike in this manner on 2 equal springs, and each of them bend the spring it strikes on, exactly to the same degree; and by so doing, the moving force of each body be entirely consumed; here, say the Leibnitian writers, are 2 equal effects produced;

for the springs are equal, and are now equally bent; and the moving forces, which are the causes of those effects, are wholly consumed in producing them; and therefore, by virtue of the principle above laid down, those causes must be equal; that is, the moving forces of the 2 bodies must be equal.

But their antagonists reply, that this principle is not admitted by them, except the times of producing those effects are equal; and that they are not so in the case before us; for the greater body takes up a longer time in producing its effect, or in bending its spring.

If therefore the Leibnitian party pretend, that equal effects, when produced in unequal times, do always arise from equal causes, they must not impose this on their opponents by way of principle or axiom, but must demonstrate it. Till this be done, there will be room to doubt, at least, whether the two bodies have equal moving forces, though they bend equal springs to the same degree. For the larger and slower of these two bodies will bend the one spring more slowly; and consequently will be resisted for a longer time, than the smaller and swifter body will be resisted in bending the other spring to the same degree.

May not therefore the total resistance of a spring be greater, if that resistance continues for a longer time? And if the total resistance be greater, must not the moving force, which is destroyed and consumed by that resistance, be also greater? Is there not reason then to doubt, whether the moving forces of these two bodies be equal, though they bend equal springs to the same degree?

In like manner, when a spring, already bent to some certain degree, does, by unbending, drive before it a body which gives way to its pressure, is there not room to doubt, whether the pressure of the spring may not produce a greater effect, when that pressure continues for a longer time?

That pressure may be said to produce three effects, all of which may, if we please, be considered as different from one another. 1. The pressure carries the body through a certain space; by which space the length of the bent spring is increased, in returning to its natural situation. 2. The pressure gives to the body a certain quantity of motion. 3. It gives the body a certain moving force.

Now, the first of these effects is greater, when the pressure acts for a longer time. For, if the pressure of a bent spring, by acting for one second on the body 1, carry that body 1 through the space 1; the pressure of the same, or of an equal spring equally bent, by acting for 2 seconds on the same body 4, will carry that body 4 through the same space 1.

Likewise the 2d effect is greater, when the pressure continues for a longer time. For in the case just now mentioned, the body 4 will have twice the quantity of motion that the body 1 has; though these two quantities of motion arise from the pressure of the same, or, which is all one, of equal springs equally bent.

Why therefore are we to take it for granted, or to have it imposed on us by way of principle or axiom, that the third effect is not greater, when the time, in which it is produced by the pressure of the same, or equal springs, is longer, nay infinitely longer?

But we are told that all the force, which resided in the spring, while bent, is now, on the unbending of the spring, communicated to the body moved. What then was that force, or what kind of force was that, which resided in the spring, while bent, and without motion? Was it a bare pressure, or a moving force? A vis mortua, or a vis viva? It must be acknowledged, it was a vis mortua, a bare pressure, and nothing more. But the force communicated to the body, and which now resides in the body in motion, is a vis viva, a moving force. This therefore is not the same force, nor a force of the same kind, as that which resided in the bent spring.

It will be said however, that the force of the bent spring is entirely exhausted in giving the body its moving force. What then is it that is to be understood by these words, the force of the spring is entirely exhausted? If the meaning be, that the spring could not possibly give that same body any greater moving force, than what it has already given, it is allowed: but this does not prove, that the same spring, bent afresh to the same degree, or an equal spring equally bent, cannot give a greater force to a greater body.

But if the meaning of these words be, that the spring cannot give a greater moving force to any body whatever, that this is taking for granted the very point which is in dispute. For the opposite party pretend, that a body of 4 times the bulk, will receive twice the moving force in twice the time, from the pressure of the same spring in unbending itself, or in exhausting all its force.

It is plain therefore, that the followers of Mr. Leibnitz have no right to say, a body has such or such a force, because such or such a spring has put it in motion by unbending itself, or can be bent by it. This is not a position to be taken for granted, but stands in need of a demonstration, which nobody has as yet attempted to give, at least from any uncontroverted principle; and, till this be done, the laying down any such position can have no other effect, than to perplex the controversy more and more, without hopes of ever coming to an end of it.

For which reason Dr. J. takes a quite different method in what follows, and lays down nothing, by way of principle or axiom, but what is allowed of by all the world, or at least has never yet been contradicted a priori.

Axiom 1.—When a bent spring, by unbending itself, pushes a body before it, the greater the body is, the more slowly will the spring unbend itself.

Axiom 2.—The more any spring is bent, the greater is its pressure.

Axiom 3.—A greater pressure produces a greater moving force, if the time be given.

Prop. 1.—Moving forces are not proportional to the masses of the bodies, and the squares of their velocities.

Demonstr.—Let there be two springs, equal, and equally bent, A and B, which, by unbending themselves, push before them two unequal bodies, the spring A pushing before it the greater body.

Now, by axiom 1, the spring A will unbend more slowly than the other; from which it follows, that at every instant of the time which the spring B takes up in unbending itself, the spring A will have unbent itself less than B, or will be more bent than B. Therefore, by axiom 2, the pressure of the spring A will, at any instant of that time, be greater than the pressure of the spring B at that same instant. Hence, by axiom 3, the nascent, or infinitely small moving force, which is produced by the pressure of the spring A, in every infinitely small part of that time, will be greater than that produced by the pressure of the spring B, in the same infinitely small part of the time.

Therefore, the sum of the infinitely small moving forces, that is to say, the whole moving force, which is produced by the spring A, during that time, will be greater than the moving force produced by the spring B in that same time: or the moving force of the greater body will be greater than that of the less, at the instant that the spring B, being now wholly unbent, ceases to act any longer on the body it has pushed before it; and as, after that instant, the spring A, not being yet wholly unbent, continues to act on the greater body, the moving force of the greater body will still continue to increase, and consequently will more and more exceed the moving force of the smaller body.

But every one knows, that the products of the masses and squares of the velocities are equal in the two bodies. Therefore the moving forces, which we have proved to be unequal, are not proportional to the products of the masses and squares of the velocities. Q. E. D.

To consider this in a particular example, let us suppose the masses of the two bodies, exposed to the pressure of the springs A and B, to be 4 and 1 respectively; and let the spring B unbend itself, and thereby give the body 1 its whole moving force in one second of time. Then, at the end of that second, the moving force of the body 4 will already exceed that of the body 1, and will still grow greater during another second of time. For the times are as the square roots of the masses. Also, if the masses be 100 and 1, the moving force of the body 100, will, at the end of the first second of time, be greater than that of the body 1, and will continue to increase during the space of 9 other seconds.

Corol.—When a bent spring, by unbending itself, drives a body before it, the larger that body is, the greater will be the moving force which it receives from the spring.

Having now clearly proved, that the moving forces are not proportional to the squares of the velocities, Dr. J. proceeds next to demonstrate, that they are proportional to the velocities themselves: and, in order to this, he makes use of no other principles or axioms, than such as are admitted on both sides, or at least have never yet been controverted a priori by either party.

Axiom 4.—Springs of unequal lengths, when bent alike, have equal pressures. We speak here of springs equal in all respects, except the length only; and, by being bent alike, we mean, that they are so compressed, as that the lengths they are now reduced to, are exactly proportional to their natural lengths, or to the lengths they are of when no way compressed.

In this condition, if one be directly opposed to the other, they will mutually sustain each others pressure, so as to maintain a perfect equilibrium; or, if each be placed separately in a vertical situation, they will sustain equal weights. And in one or the other of these cases, it is evident that they must exercise equal pressures.

Axiom 5.—Equal pressures in equal times produce equal moving forces.

Prop. 2.—Moving forces are proportional to the masses and velocities jointly.

Demonstr.—Let there be two springs, of the lengths 1 and 2, but equal in all other respects, and bent alike; and, in unbending themselves, let the spring 1 drive before it a body whose mass is 2; and the spring 2 another body of the mass 1.

Now, by corol. 2 of his general theorem concerning the action of springs, these two springs will unbend themselves exactly in the same time; and consequently the spring 2 will unbend itself with a velocity double of that of the spring 1; and, by corol. 12 of the same theorem, it will give to the body 1 a velocity double of that which the body 2 will receive from the spring 1.

Also, as the two springs were supposed to be bent alike at first, and the spring 2 unbends itself with a velocity double to that of the spring 1, it is manifest, that during the whole time of their expansion, they will be always bent alike, one to the other.

Therefore, by axiom 4, their pressures will be constantly equal to each other; and hence, by axiom 5, the infinitely small moving forces produced by each of these springs, in every infinitely small part of time, will be equal to each other. Consequently the sums of those infinitely small moving forces, that is, the whole moving forces produced by the two springs, will be equal to each other. And the masses of the two bodies being 2 and 1, and their velocities being 1 and 2 respectively, it is plain, that the moving forces are proportional to the masses and velocities jointly. Q. E. D.

As we do not think, that any flaw can be found in either of the demonstrations above laid down; and the axioms, on which they are founded, have never yet been disputed, as far as we know; we presume that the Leibnitian opinion about the measure of moving forces, is incontestably overthrown by the first proposition, and the opposite sentiment is as evidently established by the second. But if any reader shall be of a different opinion, we must beg leave to propose to his consideration the following experiment, which we hope may justly deserve the name of an experimentum crucis, and, as such, may put a final period to this controversy.

Exper.—On a horizontal plane at rest, but moveable with the least force, suppose on a boat in a stagnant water, let there be placed, between two equal bodies, a bent spring, by the unbending of which the two bodies may be pushed contrary ways.

In this case it is evident, that the velocities, which the two bodies receive from the spring, will be exactly equal, and their moving forces will also be exactly equal; and that the plane they move on, and also the boat on which it lies, will have no motion given them either way. Let us call the velocity of each body 1, and the moving force also 1.

Now let us suppose the spring to be bent afresh to the same degree as before, and to be again placed between the two bodies lying at rest; then let the plane, on which the spring and the bodies lie, be carried uniformly forward, in the direction of the length of the spring, with this same velocity 1. In this case it is manifest, that each of the bodies will have the velocity 1, and the moving force 1, both in the direction of the axis of the spring. During this motion, let the spring again unbend, and push the two bodies contrary ways, as before, the one forward, the other backward: then the spring will give to each of these bodies the velocity 1, as before, when the plane was at rest.

By this means the hindmost body, or that which is pushed backward, will have its velocity 1, which it had before by the motion of the plane, now entirely destroyed, and will be absolutely at rest. But the body, which is pushed forward, will now have the velocity 2, namely 1 from the motion of the plane, and 1 from the action of the spring.

Thus far every body agrees in what will be the event of this experiment. But the question is, what will be the moving force of the foremost body, or of that which is pushed forward, and which has the velocity 2; viz. 1 from the motion of the plane, and 1 from the action of the spring. By the Leibnitian doctrine, its moving force must be 4: and, if so, it must have received the moving force 3 from the action of the spring; for it had only the moving force 1 from the motion of the plane.

Let us examine, whether this be possible, or reconcileable to their own doctrine. Their doctrine is, that equal springs, equally bent, will, by unbending themselves, give equal moving forces to the bodies they act on, whatever those bodies are. We agree to this, not generally indeed, but in the case before us, where the bodies are of equal masses or weights, we agree to it.

Let us therefore imagine the bent spring, which is placed between the two bodies, to be divided transversely into two equal parts. In this case it is plain, that the two halves of the spring may be considered as two entire springs, equal, and equally bent, each of which rests at one end in equilibrio against the other spring, and at the opposite end presses against the body it is to move. Consequently, by the Leibnitian doctrine, to which, in this particular case, where the bodies are equal, we also agree, the two springs will give equal moving forces to the two bodies.

But the moving force received by the hindmost body from the hinder spring, was undoubtedly the moving force 1: for by that force given it in the direction backward, the moving force 1, which it had before from the motion of the plane in the direction forward, is exactly balanced and destroyed; the body remaining, as was observed before, in absolute rest. Therefore the moving force received by the foremost body, from the foremost spring, was also the moving force 1. And this, added to the other moving force 1, which it had before from the motion of the plane, makes the moving force 2, and not the moving force 4, as the Leibnitian philosophers pretend.

Consequently, that body, which had before the velocity 1, and the moving force 1, and now has the velocity 2, has also the moving force 2: that is, the moving forces are proportional to the velocities.

Observations on Luminous Emanations from Human Bodies, and from Brutes; with some Remarks on Electricity. By the Rev. Henry Miles, D.D. and F.R.S. N° 476, p. 441.

In the late edition of the works of Mr. Boyle, vol. 5, p. 646, is a letter from Mr. Clayton, dated June 23, 1684, at James city in Virginia; in which he gives Mr. Boyle an account of a strange accident, as he calls it, and adds, that he had inclosed the very paper Colonel Digges gave him of it, under his own hand and name, to attest the truth; and that the same was also asserted to him by Madam Digges, his lady, sister to the wife of Major Sewall, and daughter of the Lord Baltimore, to whom this accident happened. This paper came not to hand till after Mr. Boyle's works were printed, and therefore could not be inserted with Mr. Clayton's letter, but having since met with it, the following copy of it is here inserted.

"There happened in Maryland, about the month of November 1683, to one Mrs. Susanna Sewall, wife to Major Nic. Sewall, of the province aforesaid, a strange flashing of sparks, seemed to be of fire, in all the wearing apparel she put on, and so continued till Candlemas: and, in the company of several, viz. Captain John Harris, Mr. Edward Braines, Captain Edward Poulson, &c. the said Susanna did send several of her wearing apparel, and, when they were shaken, it would fly out in sparks, and make a noise much like unto bay-leaves when flung into the fire; and one spark fell on Major Sewall's thumb-nail, and there continued at least a minute before it went out, without any heat, all which happened in the company of Wm. Digges."

* "My Lady Baltimore, her mother-in-law, for some time before the death of her son, Cæcilius Calvert, had the like happened to her; which has made Madam Sewall much troubled at what has happened to her.

"They caused Mrs. Susanna Sewall one day to put on her sister Digges's petticoat, which they had tried beforehand, and would not sparkle; but at night, when Madam Sewall put it off, it would sparkle as the rest of her own garments did."

Bartholin of Copenhagen, in his collection of anatomical histories that are unusual, century 3, hist. 70, which he entitles Mulier Splendens, gives a parallel instance in a noble lady of Verona in Italy, which he says he had from an account of the phenomenon published by Petrus à Castro, a learned physician of the same place, in a small treatise intitled De Igne Lambente.

There is another author, Dr. Simpson, who published a philosophical discourse of fermentation, Anno 1675, who takes notice of light proceeding from animals, on their frication or pectation, as he calls it; and instances in the combing a woman's head, the currying of a horse, and the frication of a cat's back, the last two of which are known to most. According to this gentleman's hypothesis, he would assign the principles of fermentation, which he supposes to be acidum et sulphur, as the cause of these lucid effluvia in animals. But more probably the properties of the effluvia in animal bodies are many of them common with those produced from glass, &c.: such as their being lucid, their snapping, and their not being excited without some degree of friction, and electricity, for a cat's back is strongly electrical when stroked.

In the account of some of the earlier electrical experiments made by Mr. Gray, Phil. Trans. N° 366, we are informed, that he electrified several other bodies, besides animal substances, by drawing them between his thumb and fingers; in particular, linen of divers sorts, paper, and fir-shavings, which would not only be attracted to his hand, but attract all small bodies to them, as other electric bodies do. Now, notwithstanding this last circumstance of their attracting as well as being attracted, may it not be questioned, whether, in this

* The additional lines are not in Colonel Digges's hand, but seem to be in Mr. Clayton's.—Orig.

way of trial, it appears that they are electrical bodies, or electrics per se? is it not doubtful, since his fingers must be excited considerably in this experiment, whether he did not communicate electricity to them from his hand, rather than excite it in them? Doubtless the principle is inherent in many other bodies besides animal, possibly in all bodies whatever. But as it is allowed that animals have a greater quantity of it residing in them than other substances, there seems room to admit the above doubt.

An Extract, by Mr. Paul Rolli, F. R. S. of an Italian Treatise, written by the Rev. Joseph Bianchini, a Prebend in the City of Verona; on the Death of the Countess Cornelia Zangári and Bandi, of Ceséna. N° 476, p. 447.

The Countess Cornelia Bandi, in the 62d year of her age, was all day as well as she used to be, but at night was observed, when at supper, dull and heavy. She retired, was put to bed, where she passed 3 hours and more in familiar discourses with her maid, and in some prayers; at last, falling asleep, the door was shut. In the morning, the maid taking notice that her mistress did not awake at the usual hour, went into the bed-chamber and called her; but not being answered, doubting of some ill accident, opened the window, and saw the corpse of her mistress in the deplorable condition following:

Four feet distance from the bed there was a heap of ashes, 2 legs untouched, from the foot to the knee, with their stockings on: between them was the lady's head: whose brains, half of the back part of the skull, and the whole chin, were burnt to ashes; among which were found 3 fingers blackened. All the rest was ashes, which had this particular quality, that they left in the hand, when taken up, a greasy and stinking moisture.

The air in the room was also observed cumbered with soot floating in it: a small oil-lamp on the floor was covered with ashes, but no oil in it. 2 Candles in candlesticks on a table stood upright; the cotton was left in both, but the tallow was gone and vanished. Somewhat of moisture was about the feet of the candlesticks. The bed received no damage; the blankets and sheets were only raised on one side, as when a person rises up from it, or goes in; the whole furniture, as well as the bed, was spread over with moist and ash-coloured soot, which had penetrated into the chest-of-drawers, even to foul the linens; nay the soot was also gone into a neighbouring kitchen, and hung on the walls, moveables, and utensils of it. From the pantry a piece of bread covered with that soot, and grown black, was given to several dogs, which refused to eat it. In the room above it was noticed, that from the lower part of the windows trickled down a greasy, loathsome, yellowish liquor; and thereabout they smelt a stink, without knowing of what, and saw the soot fly around.

It was remarkable, that the floor of the chamber was so thickly smeared with a gluish moisture, that it could not be taken off; and the stench spread more and more through the other chambers.

Remarks.—It is impossible that, by any accident, the lamp should have caused such a conflagration. There is no room to suppose any supernatural cause. The likeliest cause then is a flash of lightning; which, according to the most common opinon, being but a sulphureous and nitrous exhalation from the earth, having been kindled in the air, penetrated either through the chimney, or through the chinks of the windows, and produced that effect. All the abovementioned effects prove the assertion; for those remaining foul particles are the grossest parts of the fulmen, either burnt to ashes, or thickened into a viscous bituminous matter.

In the Acta Medica et Philosophica Hafniensia, published by Thomas Bartholin, 1673, is such another accident related in these words. "A poor woman at Paris used to drink spirit of wine plentifully for the space of 3 years, so as to take nothing else. Her body contracted such a combustible disposition, that one night she, lying down on a straw couch, was all burned to ashes and smoke, except the skull and the extremities of her fingers."

John Henry Cohausen relates this fact in a book printed at Amsterdam 1717, intitled, Lumen novum Phosphoris accensum, and in the first part p. 92, relates also, "That a Polish gentleman, in the time of the queen Bona Sforza, having drank 2 dishes of a liquor called brandy wine, vomited flames, and was burnt by them."

Remarks.—Such an effect was not produced by the light of the oil-lamp, or of any candles; because common fire, even in a pile, does not consume a body to such a degree; and would have besides spread itself to the goods of the chamber, more combustible than a human body. It seems also, that it was not what is commonly taken for a fulmen; for there was not left in the place any sulphureous and nitrous smell; there did not appear any blackish tracks on the walls, all signs of the fulmina, as they have been remarked by the exactest observer of phenomena, the celebrated Mr. Boyle. But if it was not a real fulmen, it was certainly of such a nature.

The author's opinion is, that the fire was caused in the entrails of the body by inflamed effluvia of her blood, by juices and fermentations in the stomach, by the many combustible matters which are abundant in living bodies for the uses of life; and, finally, by the fiery evaporations which exhale from the settlings of spirit of wine, brandies, and other hot liquors, in the tunica villosa of the stomach, and other adipose or fat membranes, within which, as chemists observe, those spirits engender a kind of camphor; which, in the night-time, in sleep, by a full breathing and respiration, are put in a stronger motion, and consequently more apt to be set afire.

Proofs.—Fat is an oily liquid separated from the blood by the glands of the membrana adiposa; and it is of an easily combustible nature, as common experience shows. Our blood is of such a nature; as also our lymph and bile; all which, when dried by art, flame like spirit of wine at the approach of the least fire, and burn away into ashes. Such a drying up of matters may be caused in our body by drinking rectified brandy, and strong wines; as Mons. Litre observed in the dissection of a woman 45 years old, in the History of the Royal Academy of Sciences, 1706, p. 23. Which effect may oftener happen, if the spirit of wine has any mixture of camphor; for that liquor is but a sublimated oil, whose sulphureous particles, being attenuated by the fermentation, when separated from fixed and salt matters, are easily put in motion, and, rolling through the air, become flame and fire.

Besides, though the salts which are in living and vegetable creatures are not naturally inclined to kindle, yet they often contribute to it, particularly when there is joined some strong boiling fermentation. It is from such a cause that we know how the mixture of two liquors, though cold to the touch, produces a flaming fire. Becher was the first discoverer of this marvellous phenomenon, by mixing oil of vitriol with that of turpentine. Borrichius afterwards did the same, by mixing oil of tupentine with aquafortis; and at last Mons. Tournefort, by joining spirit of nitre with the oil of sassafras; and Mons. Homberg with this acid spirit, together with the oil and quintessences of all the aromatic Indian herbs; nay, Mr. Homberg asserts, that with a certain cold water cannons were fired, Anno 1710, in the aforesaid History of the Academy of Sciences, p. 66. It is out of question how, by a strong fermentation, magazines of gunpowder, barns, paper-mills, and haycocks, have been set on fire.

The acid particles in our bodies are much united with the fat and oily parts; nay all our limbs abound with oil and acid. What wonder then if they kindle? as Mr. Homberg well observes, in the aforesaid history, 1712, 1717, from p. 13 to 31, where he takes notice, that all our limbs have abundance of fetid oil, and volatile salt, and therefore easily combustible.

We ought not to omit how the teeth are formed by so many short tubes, the bones by long ones, and easier therefore to be set on fire. Malpighi observed also, that the bones contain a fat oily matter. Besides all this, we know that the sebaceous glands are spread all over the body, and that an oily moisture, with now and then a nitrous sulphureous smell, perspires from our skin, to which Dr. Blancard ascribes the whole circulation. Abundance of combustible matter, shut up in a great number of cells, lies in the omentum.

There is further to be considered the vast quantity of effluvia that emanate from our bodies. Sanctorius observed that, of 8 lb. of food and drink in a day, there is an insensible perspiration of about 5; computing with them those effluvia which go out of the mouth by breathing, and which might be gathered in drops on a looking-glass. As also, that in the space of one night, it is customary to discharge about 16 ounces of urine, 4 of concocted excrements by stool, and 40 and more by perspiration [Aphor. 59]. He teaches also, that numbness is an effect of too much internal heat, by which is prevented such an insensible transpiration as is proved. On this supposition, that the effluvia of such an insensible transpiration are an inflammable mine, easily apt to kindle, whenever a friction, be it ever so small, puts them in quick motion, and increases their velocity. We acknowledge the discovery of this evident truth from Mr. Hauksbee, in the experiment of the glass globe, p. 30.

The friction of the palms of our hands, or of any other parts of our body, may produce those fires commonly called ignes lambentes. We learn of Eusebius Nierembergius, that such was the property of all the limbs of the father of Theodoricus: such were those of Charles Gonzaga, Duke of Mantua, as Bartholin notices. By the testimony of John Fabri, M. D. a noted philosopher, who saw it, sparkles of light flashed out of the head of a woman, while she combed her hair. Scaliger relates the same of another woman. Cardanus, of a Carmelite Monk, whose head continued 13 years to flash out sparkles, every time he tossed his cowl on his shoulders. Ezekiel à Castro, M. D. a famous Jew, and afterwards a Christian, wrote a little treatise, entitled, Ignis Lambens; on the occasion that the Countess Cassandra Buri, of Verona, when she rubbed her arms with a cambric handkerchief, all the skin shined with a very bright light. Eusebius relates the same of Maximus Aquilanus. Licetus heard his father say, that he saw the same quality on Francis Guido, a civilian; and that he himself knew Antony Cianfio, a bookseller in Pisa, who, when he shifted, shined all over with great brightness. Libavius relates the same of a youth; and Cardanus of a friend of his; saying, that, when he shifted, clear sparkles of fire shot from his body. Father Kircher, a Jesuit, relates, that going in company into a subterranean grotto at Rome, saw sparkles of fire evaporate from the heads of his companions, become warm by walking. Father Alphonso d'Ovale was eye-witness on the highest mountains of Peru and Chili, how both men and beasts there seem shining with the brightest light from top to toe.

These flames seem harmless, but it is only for want of proper fuel. Peter Bovisteau asserts, that such sparkles reduced to ashes the hair of a young man. John de Viana, in his treatise entitled, De Peste Malagensi, p. 46, relates how the wife of Dr. Freilas, physician to Cardinal de Royas, Archbishop of Toledo, sent forth naturally, by perspiration, a fiery matter, of such a nature, that if the roller that she wore over her shift was taken from her, and exposed to the cold air, it immediately was kindled, and shot forth like grains of gunpowder.*

* Pet. Borelli gives an instance of such effluvia not only producing light, but likewise fire. See

The author even asserts, that a feverish fermentation, or a very strong motion of combustible matters, may rise in the womb of a woman, with such an igneous strength, as to reduce to ashes the bones, and burn the flesh. Two such cases are known, one in the Acta Medica et Philosophica Hafniensia, Anno 1673, by the observation of Matt. Jacobei; and the other in M. Marcell. Donato, de Medic. Hist. Mirab. lib. 4, cap. 25, p. 248, et lib. 7, Cosmog. c. 1, of Cornel. Gemma. He says also, that the bile, which is a necessary juice for our digestion, was observed by Peter Borelli, that, being vomited up by a man, it boiled like aquafortis. [Centur. 2, Obs. 1, p. 109].

Besides, very strong fires may be kindled in our bodies, as well as in other animals of a hot temperament, not only by nature, but also by art; which, being able to kill, will serve for a better proof of the argument. See also the 77th observation of John Pisano, in the German Ephemerides, printed in Lipsic 1670. Tie the upper orifice of the stomach of an animal with a string; tie also its lower orifice; then cut it out above and below the ligatures, and press it with both hands, so that it swells up in one side; which done, let the left hand keep it so that the swelled part may not subside; and, with the right, having first, at an inch distance, placed a candle, open it quickly with an anatomical knife, and you will see a flame there conceived, coming out in a few seconds of time: and such a flame may, by the curious, be perceived not only in the stomach, but also in the intestines.* The first discoverer of this was Andrew Vulparius, Anatomy Professor at Bologna in Italy 1669. Thus we see, that a quick and violent agitation of spirits, or a fermentation of juices in the stomach, produces a visible flame. Pisano was an eye-witness of the above-related operation.

In the German Ephemerides, Anno 10, p. 53, of the continuation by John Christopher Sturmius, we read, that often, in the northern countries, flames evaporate from the stomachs of those who drink strong liquors plentifully. About 17 years before, says the author, 3 noblemen of Curland, drank by emulation strong liquors; and 2 of them died scorched and suffocated by a flame issuing from the stomach.

Borelli relates how a woman vomited flames in the point of death: you may read, says he, in Bartolinus de Luce, and in Eusebius Nierembergensis's History Nat. peregrin. how such accidents often happened in great drinkers of wine and brandy: where is related also, how fire issued from the privy parts of a woman. Lord Bacon, in his Nat. Univ. Hist. assures us that he had seen a woman's belly

his Obs. Cent. 2, Obs. 75, p. 174, where he says, that there was a certain peasant, whose linen, hempen thread, &c. if laid up in boxes, though wet, or hung upon sticks in the air, soon took fire; which has been seen by a great number of spectators.—Orig.

* To inflame this air, it is necessary it should issue into, or be mixed with common atmospherical air.

sparkling like fire; and truly such flames would often rise in us, if the natural moisture did not quench them; as Lucretius observes, from verse 868, lib. 4, and verse 1065 lib. 6. Moreover, Marcellus Donatus, in his Mirab. Hist. Medic. lib. 6, cap. 4, entitled, of a new distemper, says, Albertus Krantzius, lib. 5, of his Saxon History, that, in the time of Godfrey of Bologne's Christian War, in the territory of Niverva or Nivers, people were burning of invisible fire in their entrails, and some had cut off a foot or a hand where the burning began, that it should not go further. Ezekiel de Castro, in the abovesaid work of his, of lambent fire, relates the famous instance of Alexandrinus Megetius, a physician, who, from the vertebra of the coxa, after great pain, relates how fire came out, which burned the eyes, as Simplicius and Philaseus, eye-witnesses, attested.

After all these instances, what wonder is there in the case of our old lady? Her dulness before going to bed was an effect of too much heat concentrated in her breast, which hindered the perspiration through the pores of her body; which is calculated to about 40 oz. per night. Her ashes, found at 4 feet distance from her bed, are a plain argument, that she, by natural instinct, rose up to cool her heat, and perhaps was going to open a window. The Marquis Scipio Maffei was told by Count Atimis of Gorizia, who passed through Cesena a few days after the accident, that he heard say there that the old lady was used, when she felt herself indisposed, to bathe all her body with camphorated spirit of wine; and she did it perhaps that very night. This is not a circumstance of any moment; for the best opinion is that of the internal heat and fire; which, by having been kindled in the entrails, naturally tended upwards; finding the way easier, and the matter more unctuous and combustible, left the legs untouched; which may have been saved also, by remaining cut off at the combustion of the tendons, where they join with the knees. The thighs were too near the origin of the fire, and therefore were also burnt by it; which was certainly increased by the urine and excrements, a very combustible matter, as we may see by its phosphorus. Galenus (Class. 1, lib. 3, de Temperam.) says, that the dung of a dove was sufficient to set fire to a whole house: and the learned father Casati, a Jesuit, in his Phys. Dissert. part 2, p. 48, relates that he heard a worthy gentleman say, that, from great quantities of the dung of doves, flights of which used, for many years, nay ages, to build under the roof of the great church of Pisa, sprung originally the fire which consumed the said church.* After all this, the author concludes, that to be sure the lady was burnt to ashes standing; drawing the consequence from her skull fallen perpendicular between her legs; and that the back-part of her head had been damaged more than the fore-part, was because of her

* Which effect is confirmed by Galen, lib. 2, de Morb. Diss. cap. 2; where he says, that he has seen pigeons dung take fire, when it was become rotten.—Orig.

hair, and of the nerves, whose principal seat lies there: and besides, because in the face there were many places open, out of which the flames might pass; as it happened in the time of the Roman consuls T. Gracchus and M. Juventius, when a flame came out of a bull's mouth, without hurting the beast, by not finding any resistance to its way.

Extract from a Pamphlet, entitled, "Fire from Heaven burning the Body of one John Hitchell, of Holnehurst, in the Parish of Christ-Church, in the County of Southamptom, the 26th of June, 1613." By John Hilliard. Printed at London, 1613. N° 476, p. 461.

John Hitchell having been, on Saturday the 26th of June last, at work at the house of one John Deane of Parly Court, where he truly and painfully laboured at his trade, being a carpenter, and having ended his day's work, went home to his house. After being in bed with his wife and child, in the middle of the night, the lightning came on so fiercely, that an old woman, named Agnes Russell, his wife's mother, being wakened by a terrible blow on her cheek, cried to the said John Hitchell and his wife to help her: but they not answering, the poor old woman started out of her bed, and went unto the bed where they lay, and awakened her daughter, who was on a sudden most lamentably burnt all on one side of her, and her husband and child dead by her side. Yet nevertheless his poor wife dragged him out of the bed into the street; and there, by reason of the vehemency of the fire, she was inforced to forsake him; where he lay burning on the ground for about 3 days after. Not that there was any appearance of fire outwardly to be seen on him, but only a kind of smoke ascending upwards from his carcase, till it was consumed to ashes, except only some small part of his bones, which were cast into a pit made by the place.

An Extract from the Minutes of the Royal Society, of Nov. 8 and 15, 1744, concerning the Woman at Ipswich, who was found burnt to Ashes on April 10 preceding. N° 476, p. 463.

The first account of this extraordinary accident was in a letter from Mr. R. Love to his brother, Mr. Geo. Love, apothecary at Westminster, dated Ipswich, June 28, 1744, which was laid before the Society by the president, on Nov. 8, following; wherein Mr. Love says, "That it appeared, on the Coroner's Inquest concerning the death of this woman (at which he attended), that she, having gone up stairs with her daughter to bed, went down again from her, half undressed; and that the next morning early her body was found quite burnt, lying on the brick-hearth in the kitchen, where no fire had been, with the candlestick standing by her, and the candle burnt out, with which she had lighted herself down; and that the daughter could assign no reason for her going down, unless it were to smoke a pipe; but said she was not addicted to drink gin. The jury brought it in accidental death."

Nov. 15.—Dr. Lobb communicated two letters concerning the same woman; one from the Rev. Mr. Notcutt at Ipswich, to the Rev. Mr. Gibbons; dated July 25, 1744; and the other from the said Mr. Gibbons to a friend, dated Sept. 2 following.

They both agree in all the material circumstances relating to the fact; both giving their relations from the mouths of the eye-witnesses, who viewed the body when it was first found burning; particularly Mr. Gibbons from the woman's own daughter, and from 2 other persons living in the same house, whose names are Boyden. The case was this; one Grace Pett, a fisherman's wife, of the parish of St. Clement's in Ipswich, aged about 60, had a custom, for several years past, of going down stairs every night, after she was half undressed, to smoke a pipe, or on some other private occasion. The daughter, who lay with her, fell asleep, and did not miss her mother, till she awaked early in the morning, April 10, 1744; when dressing herself, and going down stairs, she found her mother's body lying on the right side, with her head against the grate, and extended over the hearth, with her legs on the deal-floor, and appearing like a block of wood burning with a glowing fire without flame; on which quenching it with two bowls of water, the smother and stench almost stifled the neighbours, whom her cries had brought in; the trunk of the body was in a manner burnt to ashes, and appeared like a heap of charcoal covered with white ashes; the head, arms, legs and thighs were also very much burnt.

It was said, that the woman had drank very plentifully of gin over night, on the occasion of a merry-making, on account of a daughter who was lately come home from Gibraltar. But the difficulty is to account for the fire by which she was burnt; since there was none in the grate, and the candle was burnt out in the socket of the candlestick, which stood by her; and a child's clothes on one side of her, and a paper screen on the other, were both untouched: and though the melting of the grease had so penetrated into the hearth, as not to be scoured out, yet they observed that the deal-floor was neither singed nor discoloured; and the manner of the fire burning in her body is described as the working of some inward cause, and not from the burning of her clothes, which were only a cotton gown and petticoat.

Of a Quadruped brought from Bengal, and shown in London. Presented by James Parsons, M. D., F. R. S.* N° 476, p. 465.

This creature is not mentioned by any natural historian, nor any figure ex-

* This animal is the *antilope tragocamelus*. Lin. Gmel. the *Indostan antelope* of Pennant.

hibited, in the least like it. Nor is it indeed to be wondered at, since the beast was brought to Bengal, from a very remote part of the Mogul's dominions; insomuch that no person at Bengal had the least knowledge of him. The only hint that seems to point at this creature, is that mentioned by John Albert de Mandelsloe, in his voyages through the Indies, published in Harris's Complete Collection of Voyages and Travels, N° 52, p. 775; where he says, that among the horses in the stables of the viceroy of Goa, he saw "a beast called a biggel, a creature much about the colour and size of a rain-deer: its head like that of a horse; its main like that of an ass, with black cloven feet, and two black horns on his head." This is the whole of his account, which is so imperfect, that it can hardly be thought absolutely to mean this very beast before us.

The creature is a male, having the penis and testicles like those of a deer; but, as the penis does not come very forward, it cannot be seen in a side view of the animal.

The head is formed like that of a deer, with a rhomboidal spot of black hair on his forehead; his ears are dark without, and yellowish within, with dark spots toward their edges; and the horns rise about 7 inches high, bending forwards; which is very particular, because those of all other horned animals are directed sideways or backward, except the brow-antlers of some kinds of deer. He keeps his ears in continual motion, which is an action common with deer, and butts with his horns as they do. His eyes are black and lively, and the rictus oris is long.

His neck bends forward like the deer kind, but is thick and strong, somewhat resembling that of a male deer in rutting time. His mane is thin of hair like that of an ass, and on the convex part of his neck forward he has a tuft of black hair. His shoulders are thick, and his breast pretty broad and strong, from which a piece of loose skin hangs like the dewlap of a cow. His legs are slender, with cloven hoofs like those of a deer. His back rises, directly over his shoulder, pretty high, on which the mane, continued from his neck, ends in a tuft of hair. From the back of this bunch or rising, his posterior parts resemble those of an ass, having a tail like that of the ass, only it is flat on the side next the animal, and convex on the back. It is about 22 inches long, and ends with some long hairs.

He is of a light ash-colour, having a smooth coat of short hairs, which grow darker, inclining towards a black, on some parts of his limbs. He has some white under his belly towards his breast, and under his tail, with white testicles. He feeds on hay, grass, or any kind of greens; and being tried whether he would eat raw flesh, refused it. His keeper says, when he lies down he chews the cud; and his excrements are like those of a deer. He is about 12 hands high to the top of the bunch in his back. His keeper says, he never lies down on either side, but directly upon his limbs like the camel, and that he rises as suddenly as that beast. There is something very particular in his voice, which imitates the creaking noise of a child's rattle, or the croaking of some birds, rather than the voice of any quadruped except the deer, which exhibits something like it in rutting time.

Of certain perfect minute Crystal Stones. By J. Parsons, M.D., F.R.S. N° 476, p. 468.

Fig. 12, pl. 2, represents a small crystal magnified. It is one of a great number brought by a very curious gentleman from Gibraltar, who has caused many of them to be set in buckles of different kinds, for the wear of his lady and himself: and though they are not polished by art, yet they look very bright, and produce a very good effect in the buckless.

They were found accidentally. This gentleman saw a man cleaving a rock near that town, and observed a great quantity of fine black powder fall from its crevices; which he examined, and found these little stones in clusters, consisting of no more than 12 or 14 each; and each cluster lying at considerable distances from one another. They are all of the same form, some less perfect than others, and are in general hexagonals.

Concerning the specific Gravity of Diamonds. By Mr. John Ellicot, F.R.S. N° 476, p. 468.

From some experiments Mr. E. made, it appears highly probable, that what has formerly been published concerning the specific gravity of diamonds, is not to be depended on. Those in Mr. Boyle's works he thinks very erroneous, and the specific gravity too low.

The scales in which the diamonds were weighed by Mr. E. turned very sensibly with the 200th part of a grain; and as one of the diamonds weighed above 92 grains, it was capable of being weighed to less than the 18000th part: several of them were weighed twice over both in water and air, and the weights found to agree to the greatest exactness; and if to this is added the very near agreement of the weights of the several diamonds, though weighed at different times, and at a considerable distance from each other, he thinks it highly improbable, that there could be any considerable mistake in these trials; and therefore their specific gravities, as in the following table, may fully be depended on.

He sets down the weights of the several diamonds both in air and water, that if any mistake should have happened, it may be the more easily rectified.

	In Air	In Water	Specif. Grav.
Water			1000
	Grains	Grains	
N° 1. A Brazil Diamond, fine water, rough coat	92.425	66.16	3518
2. A Brazil Diamond, fine water, rough coat	88.21	63.16	3521
3. Ditto, fine bright coat	10.025	7.170	3511
4. Ditto, fine bright coat	9.560	6.830	3501
5. An East India Diamond, pale blue	26.485	18.945	3512
6. Ditto, bright yellow	23.33	16.71	3524
7. Ditto, very fine water, bright coat	20.66	14.8	3525
8. Ditto, very bad water, honeycomb coat	20.38	14.59	3519
9. Ditto, very hard bluish cast	22.5	16.1	3515
10. Ditto, very soft, good water	22.615	16.2	3525
11. Ditto, a large red foul in it	25.48	18.23	3514
12. Ditto, soft bad water	29.525	21.140	3521
13. Ditto, soft brown coat	26.535	18.99	3516
14. Ditto, very deep green coat	25.25	18.08	3521
The mean specific gravity of the Brazil diamonds appears to be			3513
The mean of the East-India diamonds			3519
The mean of both			3517

Concerning the Natural Heat of Animals. By Cromwell Mortimer, M.D., Sec. R.S. N° 476, p. 473.

Since the complete and full demonstration of the circulation of the blood in animals by our illustrious countryman, Dr. Harvey, the generality of medical writers have attributed the natural heat of animals to the motion of the blood in the blood vessels, or rather to an attrition of all the fluids in the animal arising from it; which fluids, from the later discoveries by injections and microscopes, are found to move in conical canals communicating one with another near the apices, or where the arteries are the narrowest, soon afterwards growing wider and wider, when the same continued canals obtain the name of veins, and convey back the fluids they contain to the heart. They ascribe heat in an animal to strong and frequent contractions of the heart and arteries: which heat will be the greater, the more dense the humours are, the more strongly they are propelled, and the greater the resistances are, near the ends of the arteries. From this supposition they conclude, that the heat arises from attrition; that, by a violent agitation of the particles of the blood and humours against one another, and especially by their attrition against the sides of the containing blood vessels, there must be great friction excited, and from that friction heat generated; as is easily done by rubbing two pieces of wood together, or a piece of wood and a piece of metal, or two pieces of metal, or hard stones: but it is known, by daily experience, that either any watery fluid, or oily or greasy substance, applied to these bodies while rubbed, will prevent the excitation of heat; as for instance, the use of water in polishing of glass or marble, and the greasing or oiling all manner of wheel-machines, many of which, for want of that application, have heated, taken fire, and been even consumed in flames of their own exciting. There is no experiment, by which it appears, that any the least degree of heat has been generated by the simple or mere mechanical agitation or friction of the particles of any fluid, either by itself, or mixed with various fluids; water, wine, vinous spirits, oils, quicksilver, either agitated singly or mixed, will by no force, or velocity of motion ever heard of, produce heat; nor can the blood of animals, when once let out of the body, be kept either fluid or warm by any of the most violent agitation. Indeed heat is generated in fluids in some particular circumstances, as in those two so commonly known cases of fermentation and effervescence; which, as they are frequently confounded by persons not thoroughly versed in chemical matters, Dr. M. explains the difference.

Fermentation is that spontaneous intestine motion, which, in the degree of heat of the universal temperature of subterraneous caverns, will, in a few hours, bring on such a change in vegetable juices, or in water charged with a strong tincture of vegetable particles (for fermentation is confined to the vegetable kingdom solely) as from a vapid must or wort quenching fire, to make it become more or less inflammable and nourishing of fire, as it is impregnated with more or fewer of the vegetable particles, and in the alembic to afford that volatile subtil inflammable liquor commonly called vinous spirits. The heat produced by fermentation never exceeds that of the human body.

Effervescence arises from an intestine motion, to be excited in various sorts of fluids, either by the mixture of fluids with fluids of different natures together, or by dropping in salts or powders of different natures together into different fluids: the two most common opposites, acids and alcalis, on being mixed, cause a great ebullition or frothing, but no great heat; but the solutions of some metals in aquafortis cause intense heat, and emit flame: the mixing aromatic oils with acid mineral spirits actually kindle, and burn with violent explosions; and some vegetable substances, putrifying with moisture, will sometimes heat so, as to kindle what lies dry above that part of the heap where the putrefaction happens. Thus dung heaps will heat, and haycocks often kindle into actual fire.

In these cases of effervescence, as there is no adventitious heat or fire applied, there must be the elements of fire lying hid or dormant in one or other of the bodies; and it is sufficiently known, by experiment, that there is abundance of air lies dormant in all bodies both solid and fluid; and it is likewise known, that fire cannot exert itself without the elastic assistance of common air; for wood will not burn, nor even gunpowder fire, in the artificial vacuum. It being therefore granted, that there are the elements of fire and of air lying dormant in all bodies; there is only required such an action as may set at liberty the particles of air and the particles of fire; by which action the particles of air will recover their elasticity, and, putting the particles of fire in motion, cause heat or warmth, but not incension or inflagration; unless the fire thus agitated meets with a proper pabulum, which pabulum is sulphur only, though differently modified, whether under the appearance of brimstone, bitumen, oil, vinous spirits, vegetable substances when deprived of their water, metalline sulphurs, or the most inflammable of all, animal sulphur, commonly called by modern chemists phosphorus.

Thus in fermentation, the fire and air being let loose, produce a warmth, but do not kindle, because of the water predominating; whereas in the effervescence produced by the solution of metals, the fire meets with the metalline sulphur, which it kindles, and sometimes causes explosions; the aromatic oils containing but little water, being almost entirely composed of the sulphureous parts of the vegetables, immediately kindle, and break out into flame; and phosphorus, which is nothing but the animal sulphur, as appears by the curious account of it given us by that late ingenious chemist Mr. Godfry, a worthy member of this Society, (see Trans. N° 428,) is so greatly disposed to take fire, that if it be only exposed a few minutes to the open air, it kindles and flames.

Now all animals, on which experiments have been made, are found to contain more or less of the phosphoreal principles; some insects constantly shine, or emit light in the open air; many sorts of fish are luminous, if exposed to the air a short time; nay, even the bubbles of the sea water appear like fire in the dark: some quadrupeds have been observed to emit light on very slight friction of their hair, as the necks of horses, the backs of cats, and the like; and there are many instances in our own species, of many parts of the body appearing luminous, and even of the exhalations from it adhering to the clothes, causing them to shine likewise; of which several curious observations have lately been laid before this Society. These are convincing proofs of phosphorus existing, at least in a dormant state, in animal fluids; and as it is likewise certain that they all contain air, it is only necessary to bring the phosphoreal and aërial particles to contact, and heat must of consequence be generated; and was it not for the superabundance of aqueous humours in animals, doubtless fatal incensions would frequently happen. This explains evidently the cause of animal heat: indeed the heart and arteries are the instruments which excite this heat; but that is not done by the friction caused by the circulation of the humours, but only by the intestine motion, which the circulation gives to the several particles which constitute the mass of animal fluids; and as the velocity of these fluids is increased, so must the different particles, of which they consist, come oftner into contact; and consequently the phosphoreal and the aërial meet, the more frequent and greater must the nisuses be to create heat.

Hippocrates, Aph. 1, 14, mentions the Θέρμον ἔμφυτον, calidum innatum.

Galen takes it for the soul, and more modern writers have supposed it to be the very spirit, the archeus, and others the vital heat; but have all treated of it as a certain degree of fire existing in animals; not having any notion that the element of fire might be absorbed, or lie latent, in fluid bodies, ready to become active as soon as it meets with air, or even to kindle, if it meets with sulphureous particles under proper circumstances. This probably the ancients, in the very earliest ages of the world, had some notions of; when they thought proper to communicate to the vulgar some shadows only of more profound and real knowledge under types and fables, as handed down to us in the fictions of the poets. Of this kind, and quite to our purpose, seems the fiction of Prometheus stealing fire from heaven to animate his men with, to be one. And on this principle of phosphorus existing in animals, we may easily explain the cause of those melancholy accidents which have happened to some of the human species, as that of the lady at Cesena in Italy, the carpenter in Hampshire, and the woman lately at Ipswich; who, it is most probable, were all set on fire by lightning. It may be said, many are struck by lightning, but not set on fire: but it is to be remarked, that the lady at Cesena had charged all her pores and absorbent vessels with a great quantity of camphor; the woman at Ipswich had drank plentifully of gin; and as for the carpenter, that circumstance is not recorded of him, whether he was a hard drinker or not; which circumstances must greatly promote the kindling the phosphoreal fire in them; and, as this pabulum was conveyed into the most minute capillary vessels, might produce an almost instantaneous deflagration and dissolution of all the solid containing parts.

Animals appearing more susceptible of electric fire than other bodies, greatly confirms these conjectures of the phosphoreal principles; and probably being rendered electric to any high degree might prove a dangerous experiment to a person, habituated to a plentiful use of spirituous liquors, or to embrocations with camphorated spirit of wine; on the contrary, in some languid, cold, or worn-out constitutions, possibly future experiments may evince, that electricity may be used medically, in order to renew and regenerate a proper quantity of vital fire, such as is necessary for the conveniently carrying on, and performing the animal functions.

Experiments and Observations, tending to illustrate the Nature and Properties of Electricity. By Wm. Watson, F.R.S. N° 477, p. 481.

The Society having heard, from some of their correspondents in Germany, that what they call a vegetable quintessence had been fired by electricity, Mr. W. succeeded, after having been disappointed in many attempts, in setting spirits of wine on fire by that power.

The preceding part of the week had been remarkably warm, and the air very dry; than which nothing is more necessary towards the success of electrical trials. The wind also was then easterly, and inclining to freeze. He that evening used a glass sphere, as well as a tube; but he always finds himself capable of sending forth much more fire from the tube than from the sphere, probably from not being sufficiently used to the last.

He had before observed, that though non-electric bodies, made electrical, lose almost all that electricity, by coming either within or near the contact of non-electrics not made electrical. It happens otherwise with regard to electrics per se, when excited by rubbing, patting, &c.; because from the rubbed tube we can sometimes procure 5 or 6 flashes from different parts; as though the tube of 2 feet long, instead of being one continued cylinder, consisted of 5 or 6 separate segments of cylinders, each giving out its electricity at a different explosion.

The knowledge of this theorem is of the utmost consequence towards the success of electrical experiments; inasmuch as you must endeavour, by all possible means, to collect the whole of this fire at the same time. Professor Hollman seems to have endeavoured at this, and succeeded, by having a tin tube; in one end of which he put a great many threads, whose extremities touched the sphere when in motion, and each thread collected a quantity of electrical fire, the whole of which centered in the tin tube, and went off at the other extremity. Another thing to be observed is, to endeavour to make the flashes follow each other so fast, as that a second may be visible before the first is extinguished. When you transmit the electrical fire along a sword, or other instrument, whose point is sharp, it often appears as a number of disseminated sparks, like wet gunpowder or wild-fire: but if the instrument has no point, you generally perceive a pure bright flame, like what is vulgarly called the blue ball, which gives the appearance of stars to fired rockets.

The following is the method Mr. W. made use of, and succeeded in. He suspended a poker in silk lines; at the handle of which he hung several little bundles of white thread, the extremities of which were about a foot at right angles from the poker. Among these threads, which were all attracted by the rubbed tube, he excited the greatest electrical fire he could, while an assistant, near the end of the poker, held in his hand a spoon, in which were the warm spirits. Thus the thread communicated the electricity to the poker, and the spirit was fired at the other end. It must be observed in this experiment, that the spoon with the spirit must not touch the poker; if it does, the electricity, without any flashing, is communicated to the spoon, and to the assistant in whose hand it is held, and so is lost in the floor.

By these means he fired several times not only the ethereal liquor or phlogiston of Frobenius, and rectified spirit of wine, but even common proof spirit. These experiments, as before observed, were made last Friday night, the air being perfectly dry. Sunday proved wet, and Monday somewhat warm; so that the air was full of vapour, wind south-west, and cloudy. Under these disadvantages, on Monday night, he attempted again the experiments; they succeeded, but with infinitely more labour than the preceding, because of the unfitness of the evening for such trials.

April 15, wind E.N.E.; about 4 o'clock in the afternoon, Mr. W. fired the spirit of wine 4 times from the poker, as before, 3 times from the finger of a person electrified, standing on a cake of wax, and once from the finger of a second person standing on wax, communicating with the first by means of a walking-cane held between their arms extended. The horizontal distance in this case between the glass tube and the spirit was at least 10 feet.

There is the repulsive power of electricity, as well as the attractive; so that we are able, when a feather, or such like substance, is replete with electricity, to drive it about a room, which way we please. This repulsive power continues, till either the tube loses its excited force, or the feather attracts the moisture from the air, or comes near to some non-electric substance; if so, the feather is attracted by, and its electricity lost in, whatever non-electric it comes near. In electrified bodies, we see a perpetual endeavour to get rid of their electricity. This induced Mr. W. to make the following experiment.

He placed a man on a cake of wax, who held in one of his hands a spoon with the warm spirits, and in the other a poker with the thread. He rubbed the tube among the thread, and electrified him as before. He then ordered a person, not electrified, to bring his finger near the middle of the spoon; on which, the flash from the spoon and spirit was violent enough to fire the spirit. This experiment he then repeated 3 times.

In this method, the person by whose finger the spirit of wine is fired, feels the stroke much more violent, than when the electrical fire goes from him to the spoon. This way, for the sake of distinction, we will call the repulsive power of electricity.

The late Dr. Desaguliers has observed, in his excellent dissertation concerning electricity, "that there is a sort of capriciousness attending these experiments, or something unaccountable in their phenomena, not to be reduced to any rule. For sometimes an experiment, which has been made several times successively, will all at once fail." Now Mr. W. imagines that the greatest part, if not the whole of this matter, depends on the moisture or dryness of the air; a sudden though slight alteration in which, perhaps not sufficient to be obvious to our faculties, may be perceived by the very subtle fire of electricity. For, 1st, he conceives that the air itself, (as has been observed by Dr. Desaguliers) is an electric per se, and of the vitreous kind; therefore it repels the electricity arising from the glass tube, and disposes it to electrify whatever non-electrical bodies receive the effluvia from the tube.

2dly, That water is a non-electric, and of consequence a conductor of electricity. This is exemplified by a jet of water being attracted by the tube, from either electrics per se conducting electricity, and non-electrics more readily when wetted; but what is more, if you only blow through a dry glass tube, the moisture from the breath will cause that tube to be a conductor of electricity.

These being premised, in proportion as the air is replete with watery vapours, the electricity arising from the tube, instead of being conducted, as proposed, is, by means of these vapours, communicated to the circumambient atmosphere, and dissipated as fast as excited.

This theory has been confirmed by divers experiments, but by none more remarkably than on the evening of the day Mr. W. made those before mentioned; when the vapours, which in the afternoon, by the sun's heat, and a brisk gale, were dissipated, and the air perfectly dry, descended again in great plenty, on the absence of both, and in the evening was very damp. For between 7 and 8 o'clock, he attempted again the same experiments in the same manner, without being able to make any of them succeed.

Mr. W. remarks, that no inflammable substance will take fire, when brought into or near the contact of electrics per se excited to electricity. This effect must be produced by non-electrical substances, impregnated with electricity received from the exciting electrics per se.

1st, Mr. W. supposes that inflammable substances are fired by the attractive power of electricity, when this effect arises from their being brought near excited non-electrics.

2dly, That inflammable substances are fired by the repulsive power of electricity; when it happens, that the inflammable substances, being first electrified themselves, are fired by being brought near non-electrics not excited.

This matter will be better illustrated by an example. Suppose that either a man standing on a cake of wax, or a sword suspended in silk lines, are electrified, and the spirit, being brought near them, is fired, this is said to be performed by the attractive power of electricity. But if the man electrified, as before, holds a spoon in his hand containing the spirit, or the same spoon and spirit are placed on the sword, and a person not electrified applies his finger near the spoon, and the spirit is fired from the flame arising from the spoon and spirit on such application, this he calls being fired by the repulsive power. Of the two-mentioned kinds he generally finds the repulsive power strongest.

Mr. W. has fired the spirit both by the attractive and repulsive power, through 4 persons standing on electrical cakes, each communicating with the other, either by means of a walking-cane, a sword, or any other non-electric substance.

It has likewise been fired from the handle of a sword held in the hand of a third person.

He not only fired Frobenius's phlogiston, rectified spirit, and common proof spirit, but also sal volatile oleosum, spirit of lavender, dulcified spirit of nitre, Peony-water, Daffy's elixir, Helvetius's styptic, and some other mixtures, where the spirit has been very considerably diluted; likewise distilled vegetable oils, such as that of turpentine, lemon, orange-peels, and juniper; and even those which are specifically heavier than water, as oil of sassafras; also resinous substances, such as balsam capaivi, and turpentine: all which send forth, when warmed, an inflammable vapour.

But expressed vegetable oils, as those of olives, linseed, and almonds, as well as tallow, all whose vapours are uninflammable, he has not been able yet to fire; but these indeed will not fire on the application of lighted paper. Besides, if these last would fire with lighted paper, unless their vapours were inflammable, he can scarcely conceive they would fire by electricity; because, in firing spirits, &c. he always perceives, that the electricity snaps before it comes in contact with their surfaces, and therefore only fires their inflammable vapours.

There is considerable difficulty in firing electrics per se, such as turpentine and balsam capaivi, by the repulsive power of electricity; because, in this case, these substances will not permit the electricity to pass through them: therefore, when you would have this experiment succeed, the finger of the person who is to fire them, is to be applied as near to the edge as possible of these substances when warmed in a spoon, that the flashes from the spoon (for these substances will emit none) may snap, where they are spread the thinnest, and then fire their effluvia. This experiment, as well as several others, serves to confute that opinion, which has prevailed with many, that the electricity floats only on the surfaces of bodies.

If an electrical cake be dipped in water, it is thereby made a conductor of electricity; the water hanging about it transmitting the electrical effluvia in such a manner, that a person standing on it can by no means be electrified enough to attract the leaf gold at the smallest distance; though the person standing on the same cake when dry, attracted a piece of fine thread hanging at the distance of 2 feet from his finger. We must here observe, that the cake being of an unctuous substance, the water will no where lie uniformly on it, but adhere in separate moleculæ; so that, in this instance, the electricity jumps from one particle of water to another, till the whole is dissipated.

If two persons stand on electrical cakes at about a yard distance from each other, one of which persons, for the sake of distinction, we will call A, the other B; if A, when electrified, touch B, A loses almost all its electricity at that touch only, which is received by B, and stopped by the electrical cake: if A be immediately electrified again to the same degree as before, and touch B, the snapping is less on the touch; and this snapping, on electrifying A, becomes less and less, till B, being impregnated with electricity, though received at intervals, the snapping will no longer be sensible.

If a number of pieces of finely spun glass, cut to about an inch in length, and little bits of fine wire of the same length, of what metal you please, and small cork balls, be either put all together, or each by themselves, into a dry pewter plate, or on a piece of polished metal, they make, in the following manner, a very odd and surprising appearance. Let a man, standing on electrical cakes, hold this plate in his hand, with the bits of glass, wire, &c. detached from each other, as much as conveniently may be; when he is electrified, let him cause a person standing on the ground to bring another plate, or his hand, or any other non-electric, exactly over the plate, containing these bodies. When his hand, &c. is about 8 inches over them, let him bring it down gently: as it comes near, in proportion to the strength of the electricity, he will observe the bits of glass first raise themselves upright; and then, if he brings his hand nearer, they will dart directly up, and stick to it without snapping. The bits of wire will fly up likewise, and as they come near the hand snap aloud; you feel a smart stroke, and see the fire arising from them to the hand at every stroke: each of these, as soon as they have discharged their fire, falls down again on the plate. The cork balls also fly up, and strike the hand, but fall again directly. You have a constant succession of these appearances, as long as you continue to electrify the man in whose hand the plate is held; but if you touch any part either of the man or plate, the pieces of glass, which before were on their ends, immediately fall down.

Some few years ago, Sir James Lowther* brought some bladders filled with inflammable air, collected from his coal-mines, to the Royal Society. This air flamed, on a lighted candle being brought near it. This inflammability has occasioned many terrible accidents. Mr. Maud, a worthy member of this Society, made at that time, by art, and showed the Society, air exactly of the same quality. Mr. W. was desirous of knowing if this air would be kindled by electrical flashes. He accordingly made such air, by putting an ounce of filings of iron, an ounce of oil of vitriol, and 4 ounces of water, into a Florence flask; on which an ebullition ensued, and the air, which arose from these materials, not only filled 3 bladders, but also, on the application of the finger of an electrified person, took flame, and burnt near the top and out of the neck of the flask a considerable time. When the flame is almost out, shake the flask, and the flame revives. You must, with your finger dipped in water, moisten the mouth of the flask as

* See these Trans. N° 442.—Orig.

fast as it is dried by the heat within, or the electricity will not fire it: because the flask, being an electric per se, will not snap at the application of the finger, without the glass be first made non-electric by wetting. It has sometimes happened, if the finger has been applied before the inflammable air has found a ready exit from the mouth of the flask, that the flash has filled the flask, and gone off with an explosion equal to the firing of a large pistol; and sometimes has burst the flask. The same effect is produced from spirit of sea-salt, as from oil of vitriol; but as the acid of sea-salt is much lighter than that of vitriol, there is no necessity to add the water in this experiment.

Those who are not much acquainted with chemical philosophy, may think it very extraordinary that, from a mixture of cold substances, which, both conjunctly and separately, are uninflammable, this very inflammable vapour should be produced. In order to solve this, it may not be improper to premise, that iron is compounded of a sulphureous as well as a metallic part. This sulphur is so fixed, that after heating the iron red-hot, and even melting it ever so often, the sulphur will not be disengaged from it; but, on the mixture of the vitriolic acid, and by the heat and ebullition which are almost instantly produced, the metallic part is dissolved, and the sulphur, which before was intimately connected with it, being disengaged, becomes volatile. This heat and ebullition continue, till the vitriolic acid is perfectly saturated with the metallic part of the iron; and the vapour, once fired, continues to flame, till this saturation being perfected, no more of the sulphur flies off.

A dry sponge hanging by a packthread at the end of an electrified sword, or from the hand of an electrified man, gives no signs of being made electrical; if it be well soaked in water, wherever it is touched, you both see and feel the electrical sparks. Not only so, but, if it be so full of water that it falls from the sponge, those drops in a dark room, received on your hand, not only flash and snap, but you perceive a pricking pain. If you hold your hand, or any non-electrical substances, very near, the water, which had ceased dropping when the sponge was not electrified, drops again on its being electrified, and the drops fall in proportion to the received electricity, as though the sponge was gently squeezed between your fingers.

Mr. W. considered, in what manner he could give a tenacity to the water sufficient to make the drops hang a considerable time; and this he brought about by making a mucilage of the seeds of fleawort. A wet sponge then, squeezed hard, and filled with this cold mucilage, was held in the hand of an electrified man, when the drops, forced out by the electricity, assisted by the tenacity of the liquor, hung some inches from the sponge; and by a drop of this he fired not only the spirit of wine, but likewise the inflammable air beforementioned, both with and without the explosion. What an extraordinary effect is this, that a drop of cold water (for the seeds contribute nothing, but add consistence to the water) should be the medium of fire and flame?

Camphor is a vegetable resin, and of consequence an electric per se. This substance, notwithstanding its great inflammability, will not take fire from the finger of a man, nor any other body electrified, though made very warm, and the vapours arise therefrom in great abundance; because, neither electrics per se excited, nor electrified bodies, exert their force by snapping on electrics per se, though not excited. If you break camphor small, and warm it in a spoon, it is not melted by heat like other resins; but, if that heat was continued, it would all prove volatile. To camphor thus warmed, the finger of an electrified man, a sword, or such like, will, in snapping, exert its force on the spoon, and the circumambient vapour of the camphor will be fired by it, and light up the whole quantity exposed. The same experiment succeeds by the repulsive power of electricity.

A poker, thoroughly ignited, put into spirit of wine, or into the distilled oil of vegetables, produces no flame in either. It indeed occasions the vapours to arise from the oil in great abundance; but if you electrify this heated poker, the electrical flashes presently kindle flame in either. The experiment is the same with camphor. These experiments, as well as the following, sufficiently evince, that the electrical fire is truly flame, and that extremely subtile.

Mr. W. has made several trials in order to fire gunpowder alone, which he tried both warm and cold, whole and powdered, but never could succeed; and this arises, in part, from its vapours not being inflammable, and in part from its not being capable of being fired by flame; unless the sulphur in the composition is nearly in the state of accension. This we see, by putting gunpowder into a spoon with rectified spirit, which when lighted will not fire the powder, till, by the heat of the spoon from the burning spirit, the sulphur is almost melted. Likewise if you hold gunpowder, ground very fine, in a spoon over a lighted candle, or any other flame, as soon as the spoon is hot enough to melt the sulphur, you see a blue flame, and instantly the powder flashes off. The same effects are observed in the pulvis fulminans, composed of nitre, sulphur, and fixed alkaline salt. Besides, when the gunpowder is very dry, and ground very fine, it (as you please to make the experiment) is either attracted, or repelled; so that, in the first case, the end of your finger, when electrified, shall be covered over with the powder, though held at some distance; and in the other, if you electrify the powder, it will fly off at the approach of any non-electrified substance, and sometimes even without it. But he could at pleasure fire gunpowder, and even discharge a musket, by the power of electricity, when the gunpowder has been ground with a little camphor, or with a few drops of some inflammable chemical oil. This oil somewhat moistens the powder, and prevents its flying away; the gunpowder then being warmed in a spoon, the electrical flashes fire the inflammable vapour, which fires the gunpowder; but the time between the vapour firing the powder is so short, that frequently they appear as the same, and not successive operations, wherein the gunpowder itself seems fired by the electricity; and indeed the first time this experiment succeeded, the flash was so sudden and unexpected, that the hand of the assistant, who touched the spoon with his finger, was considerably scorched. So that there seems a 4th ingredient necessary to make gunpowder readily take fire by flame; and that such a one as will heighten the inflammability of the sulphur.

In common cases, the lighted match, or the little portion of red-hot glass, which falls among the powder, and is the result of the collision from the flint and steel, fires the charcoal and sulphur, and these the nitre. But if to these 3 ingredients be added a 4th, viz. a vegetable chemical oil, and gently warm this mixture, the oil, by the warmth, mixes intimately with the sulphur, lowers its consistence, and makes it readily take fire by flame.

A Proposal to bring Small Passable Stones Soon and with Ease out of the Bladder. By the Rev. Stephen Hales, D.D. N° 477, p. 502.

Dr. H. having been present, Feb. 4, 1744-5, when the late Earl of Orford, after having taken for 2 months Dr. Jurin's lixivium, voided at once 11 pretty large nearly cubical fragments of larger stones, which were involved in coagulated blood and urine; and, a few hours after, 15 more at once, in the same manner, in all 32 that day; some of which were as large as were possibly passable; it hence immediately occurred to him, that all passable stones which have lately fallen from the kidneys into the bladder, or which have broken off from larger ones, might readily and easily be brought out thence, by conveying into the empty bladder, by a catheter, some very mucilaginous substance, such as syrup of marsh-mallows, or a solution of gum Arabic, or barley-water. Such substances would bring the stones away soon, and with great ease to the patient; and so not only prevent much teasing pain, by fruitlessly endeavouring to bring them away with the weak force of thin urine, but also effectually to secure the patient from the danger of their growing too large to come away, by long continuing in the bladder. And what strongly evinced the reasonableness of this proposal is, that on opening the bladder of his lordship, there were no stones found remaining, except 2 small grains, which were involved in the folds in the neck of the bladder.

If on trial any stones shall be found too large to pass off, the patient is but where he was before; and if any shall be of such a size as to enter the urethra but part of the way, they may be pushed back, or cut out, according as their situation shall happen to be.

And further to evince the reasonableness of this proposal, Dr. H. made the following experiments, viz. in order to show the comparative force, with which fluids of different degrees of density and tenacity will impel stones, he took a glass tube, which was an inch in diameter within, and 14¼ inches deep; and, having filled it full of urine, he put into it a nearly cubical piece of a large stone, taken out of a human bladder, which weighed 7¼ gr.; and, standing by a clock whose pendulum beat seconds, he found, by repeated trials, that the stone was a second and a quarter, in descending through the 14¼ inches depth of urine.

The experiment being tried with the same stone in oil of olives, it was 5¾ seconds in descending; so that the resistance of the oil to the falling stone was 4.6, that is, more than 4 times greater than the resistance of the urine; and consequently the impelling force of oil to propel a stone in passing through a narrow tube, would be proportionably so much greater than that of urine, were their velocities equal.

When 1 oz. of gum Arabic was dissolved in ¼ pint of water, the stone descended in 2 seconds; with 2 oz. in 3 seconds; with 3 oz. in 4 seconds.

In a decoction of warm barley-water, which was so thick as to be a tender jelly when cold, the stone was 45 seconds in descending, that is, 35 times slower than in urine; and, consequently, the impelling force of urine is 35 times less than that of this mucilage, in case their velocities were equal.*

This mucilage was, as he guessed, of a due consistence for the purpose; for it was about the thickness of Lord Orford's coagulated blood and urine. Equal quantities of blood and urine will continue a thick coagulum for many weeks, without any separation.

But as the velocity, with which such mucilaginous substances pass through small tubes is considerably less than the velocity with which urine will pass; supposing the forces with which they are impelled to be equal; it was requisite to determine those different velocities by experiments; and, in order to it, he put ¼ pint of the same blood-warm decoction of barley into a glass vessel, where its depth was near 8 inches, and therefore its mean depth near 4 inches. It run out at the bottom in about 50 seconds through a glass tube, whose bore was ¼ inch diameter; its length 2 inches; and, on repeating the same experiment twice, as the decoction grew cooler and cooler, it was about 80 and then 90 seconds in running out; whereas a like quantity of urine ran out through the same tube in 18 seconds.

Now, supposing the velocities at a medium, through the urethra, to be as 72 to 18, then the velocity of the urine will be ¾ greater than that of the mucilage of barley. Taking therefore ¾ from 36, the force of the mucilage, the remainder

* Should be 36 times.

9 is the force with which the mucilage will impel the stone; and, consequently, the impelling force of the mucilage, in the neck of the bladder and in the urethra, will be 9 times greater than that of urine; besides the advantage of greater slipperiness which it gives to the urethra.*

Of some Experiments, lately made in Holland, on the Fragility of unannealed Glass Vessels. By Mons. Allamand. N° 477, p. 505.

This paper contains an account of several experiments of an odd nature, that have lately been tried both in Italy and Holland, on some unannealed glass phials; that is, such as have been exposed to the air as soon as blown, without passing through the operation commonly called annealing.

The excessive fragility of these sorts of glasses must have been observed, as long as the art of making glass has been in use: it having been found, that almost all the vessels that were made of such glass were entirely useless on that account; being apt to break and fly, almost constantly, of themselves, and that even frequently before they were well cold.

It was therefore to remedy this inconveniency that the practice of nealing or annealing them was devised; by which, passing very gradually, in the space of some hours, through what is called the leer, from a very intense degree of heat to the temperature of the common air, they were found to acquire such a toughness or tenacity, as fitted them for the several uses for which they were respectively designed.

But some of the phenomena depending on their first brittleness, or at least very nearly connected with it, have been often judged to deserve the attention of the curious. One of the first very worthy founders of the Royal Society, the Right Honourable Sir Rob. Moray, very early gave in his experiments, which appear in the register, on those drops or lachrymæ of glass, which, instead of being nealed, had been immediately quenched in water, or some other fluid. And the same learned person further observed, that hollow balls, made of unnealed glass with a small hole in them, would fly in pieces with the heat of the hand only, if the small hole, by which the internal and external air communicated, was but stopped with the finger.

The glasses which the following paper concerns, have been already mentioned to the Society by Mr. Baker, who communicated the extract of a letter he had just

* There appears to be a strange confusion among the numbers in the above calculation, as well as great inaccuracies in the estimate of the comparative forces of the different fluids, to impel small bodies before them. The force of moving fluids to impel bodies before, and the resistance they make to small bodies moving or descending through them, may be very different, and have different ratios. The former is as the density of the fluid, but the latter, in the present instance, depends chiefly on its viscosity.

received from Dr. Laurentius Bruni of Turin, taking notice of the same; and relating their remarkable property of resisting very hard strokes that were given them from without, notwithstanding they at the same time shivered to pieces on the shocks they received by the fall of very light and minute bodies dropped into their cavities. And Mr. Ellicot, having very soon after caused some unnealed glasses to be made here, repeated with them some of the same experiments, which he found to answer agreeably to what Dr. Bruni had mentioned.

But it will further appear to be remarkable in the present paper, that, according to the experiments made abroad on those glasses, it is not the weight alone of the bodies severally dropped into them, which occasions their rupture; for some certain bodies break them with abundantly more ease than others of the same or even much greater weights; insomuch that such phials as are shivered to pieces by the fall of very small particles of flint and some other substances, are nevertheless capable of resisting the much greater shock they receive, in like manner, from a leaden bullet, though some hundreds of times heavier than the flint.

The author of the paper is Mons. Allamand, a gentleman of distinction, merit, and learning, in Holland, a person of great curiosity, and particularly well versed in all the parts of natural and experimental knowledge. This gentleman communicated his observations to the Hon. William Bentinck, Esq. of the Hague, a worthy member of the Royal Society, and who was pleased immediately not only to transmit them over to the president, but also to oblige him at the same time, with a number of glass phials, of the very same sort as those on which Mons. Allamand's experiments had been made, that he might thereby be enabled both to report to the society the facts he should take notice of, and to repeat some of the experiments themselves in their presence.

Mons. Allamand's paper is in French, but the substance of it in English is as follows:

These glasses differ from ordinary phials only in this, that they have not been set to cool gradually in what is called the nealing furnace, but have been immediately exposed to the open air as soon as formed. They may be made of any shape; and all that needs to be observed in the making of them is, to take care that their bottoms may be thicker than their sides, and indeed the thicker the bottom is, the easier the glasses break. Mr. A. had one in particular, whose bottom was above 3 fingers breadth in thickness, and which flew with as much ease at least as the thinnest glass. He had some others equally thick all over; these have flown also, but with more difficulty than the former.

These glasses are capable of resisting very hard blows coming from without: he has given to some, with a mallet, strokes sufficient to drive a nail into wood tolerably hard, and they have held good without breaking. They also resist the shock of several heavy bodies, that are let fall into their cavities. Thus he has dropped, from the height of 2 or 3 feet, musket balls, pieces of iron, brass, tin, silver, gold, antimony, bismuth, pyrites, jasper, and several sorts of woods, ivory, and bone; all which is indeed nowise extraordinary; for other glasses equally thick would also bear the strokes of the same bodies, but herein consists what is more surprising.

He took a shiver of flint, of the size of a small pea; and let it fall into the glass, from the height of 3 inches, and in about 2 seconds the glass flew. Having repeated the same experiment on several other glasses with the same piece of flint, the greatest part broke in the moment of the shock, and the others 1 or 2 seconds after it.

He let fall into different glasses a shiver of flint, of but half the size of that used in the former experiment, and the glasses flew in the same manner. Another bit of flint, of the size of a small lentil, also produced the same effect.

Being encouraged with this success, he let fall into one of his glasses a piece of flint no larger than a grain of sand: this was too light to produce any sensible shock, and accordingly the glass did not break. In order to try further, he shook the glass with the small piece of stone in it; and nothing following, he repeated the same experiment on 4 other glasses, none of which broke. He then judged the experiment to have failed, and set aside those 5 glasses; but, about half an hour after, one of the glasses flew, and the other four soon after; insomuch that the glass which remained the longest entire, broke also, about 3 quarters of an hour after its being shook.

Though flint is, of all the bodies that he had employed, that which has hitherto broken these glasses with the greatest ease, it is not however the only body that produces this phenomenon. He let fall into one a sapphire set in a ring; and though the bottom of the glass was near an inch in thickness, the sapphire passed through it as through a spider's web. The glass was dispersed on all sides, and the ring remained on the table just where the glass rested.—A bit of porcelain, of the thickness of half a line, and the breadth of 2 lines, broke also several glasses; but that only some seconds after the shock.—A bit of glass of the same size, produced the same effect; and so did a very small pebble.—Diamonds of several sizes have constantly done the same.—A very small piece of hard tempered steel has broken all the glasses into which he dropped it.—One of those pellets also that boys play with, and which they commonly call marbles, broke a glass into which it was dropped, but not till 4 minutes after its fall.

Being desirous to know if the bodies, on which the glasses rested, contributed any thing to the ease of their breaking, Mr. A. repeated the same experiments, holding the glasses in his hand, and setting them upright in clay, and placing them on a down pillow, and putting them in water; in all which cases they broke in the very same manner. He then half filled one of them with water, and a piece of flint, about the size of a pea, broke it.

All the bodies with which he had yet broken glasses having been elastic without being ductile, he was willing to inquire if those qualities were essentially necessary, though he was already satisfied that all the bodies that had those qualities, such as ivory, for example, would not produce the effect. After many trials, none of which succeeded, he thought of slightly rubbing the bottoms of some of the glasses with his finger, and all those on which he made that experiment broke, though some of them did not fly till about half an hour after they had been so rubbed. Thinking, that perhaps the heat he communicated to them with his hand might occasion their breaking, he poured into several some almost boiling water, which certainly gave them a much greater heat than he could have given them with his hand; but none of those glasses broke.

He found in the animal kingdom only one sort of bodies capable of breaking these glasses, which are pearls: he dropped one of near a line diameter into a glass, which broke in about half an hour.

Though the experiment of rubbing with the finger had convinced him that the stroke or shock of a falling body is not always necessary to break these phials, he thought of scratching with a flint the bottom of the glass, and it immediately broke. To assure himself whether the scratch he had made was the occasion of its breaking, he took a rod of iron having a rounded end; he pushed it strongly against the bottom of the glass, and the glass flew. He then did the same, and even pushed much harder, against the bottoms of several ordinary glasses, but without any effect; for though these glasses were much thinner than the others, yet none of them broke.

If the glasses in question are every where extremely thin, they do not break in the circumstances abovementioned, he frequently dropped into such glasses the same sorts of bodies as had broken the thicker ones, but without any success. He only met with one that split, and he was not even sure but that the weight of the body dropped into it, which was a stone of some size, might occasion its breaking.

All the phials on which he made these experiments were of white glass; he had not an opportunity of trying those made with the green.

The author of the dissertation, published at Padua on this subject, pretends to account for all these singular phenomena by saying, that the bodies dropped into these phials cause a concussion that is stronger than the cohesion of the parts of the glass; and that consequently, a rupture of the same must ensue. But why does not a ball of gold, silver, iron, copper, or any of the other bodies that have been tried unsuccessfully, though 1000 times heavier, equally cause this concussion, and break the glasses? Shall it be said, it is because they are not elastic? copper, iron, silver, and ivory, are elastic; and as much so as flint and porcelain, and surely much more so than the end of one's finger.

It appears that, before we undertake to give the solution of these phenomena, we should apply ourselves to the making a much greater variety of experiments about them; that we should both try a greater number of glasses, and those with a greater variety of differing bodies, that we may be able thence to collect at last, in what classes the several bodies are to be ranged, that are either fit or unfit for these purposes: and then it may perhaps be time to inquire, whether it is from the principles of chemistry, or from those of mechanics, or any other branch of natural philosophy, that we are to seek for the reasons of the several facts.

The president (Martin Folkes, Esq.) and the R. S. also repeated the same experiments, with the like success, both foreign and English phials, made as before.

Mr. Allamand observes, that he had yet only tried these experiments on phials made of white or crystal glass. But the president since received from the Rev. Dr. Littleton, F. R. S. some large hollow cups, made at Worcester, of the common green bottle glass; all which, though of a much greater size than the others, and some of them above 3 inches thick at bottom, were instantly broken with a shiver of flint, weighing only 2 grains; though they had before resisted the shock of a musket-ball from the height of near 3 feet.

On some Improvements which may be made in Cyder and Perry. By the Rev. Henry Miles, D. D., F. R. S. N° 477, p. 516.

The design of communicating the following paper to the Royal Society is, to invite gentlemen, after the example of a practice that has long obtained in Herefordshire, to attempt an improvement of their waste lands, by planting such kind of fruit trees, as are mentioned, in hedges and barren places; which, for aught appears, would thrive as well in other counties, perhaps in some parts of most counties in England, as in that of Hereford.

Extract from a manuscript, written Anno 1657-8, by Dr. John Beale, F. R. S. concerning an excellent liquor made of a mixture of rough pears and crabs.— The author undertakes to evince, "That crabs and wild pears, such as grow in the wildest and barren clifts, and on hills, make the richest, strongest, the most pleasant and lasting wines that England yet yields, or is ever like to yield."—"I have so well proved it already (says he) by so many hundred experiments in Herefordshire, that wise men tell me, that these parts of England are some hundred thousand pounds sterling the better for the knowledge of it."

He mentions, of these kinds of austere fruit, the Bareland pear and the Bromsbury crab, of which notice is taken page 4th of the tract entitled Herefordshire

Orchards; and intimates, "That though the discovery of them was but then lately made, yet they had gotten a great reputation."—He adds, "The croft crab and white or red horse-pear excel them, and all others, known or spoken of in other countries." Of the red horse-pear of Felton or Longland he observes, "That it has a pleasant masculine vigour, especially in dry grounds, and has a peculiar quality to overcome all blasts."—Of the quality of the fruits he says, "That such is the effect which the austerity has on the mouth on tasting the liquor, that the rustics declare it is as if the roof were filed away; and that "neither man nor beast care to touch one of these pears, though ever so ripe." Of the pear called imny-winter, which grows about Rosse, in that county, he observes, "That it is of no use but for cyder, that if a thief steal it, he would incur a speedy vengeance; it being a furious purger; but, being joined with well chosen crabs, and reserved to a due maturity, becomes richer than a good French wine; but if drank before the time, it stupifies the roof of the mouth, assaults the brain, and purges more violently than a Galenist."

Of the quality of the liquor he says, "That, according as it is managed, it proves strong rhenish, backrac, yea pleasant canary, sugar'd of itself, or as rough as the fiercest Greek wine, opening or binding, holding one, two, three, or more years—that no mortal can yet say at what age it is past the best. This, adds he, we can say, that we have kept it till it burn as quickly as sack, draws the flame like naptha, and fires the stomach like aqua vitæ." He says, "That he made trial at his own house with wine d'Hay, by a merchant of Bristol highly extolled, which, compared with a liquor made of crabs and wild pears, was so much inferior, in the judgment of all, that the comparison was ridiculous." And he further relates, "That a gentleman (Sir H. Lingen) a great planter, and expert in many experiments, had then by him many tuns of liquor made with this mixture of fruit, which he, by a designed equivocation, called pearmaine cyder, that carried the applause from all palates—that all his common hedges yielded him store of the said fruit."

To recommend this easiest, cheapest, and most profitable kind of agriculture, as he calls it, he says, "That the best of these pears grow on very bare and sandy hills, or vales; crabs on any mound or bank that may be raised on a heath; that one pear tree ordinarily bears yearly 40, 50, 60, 70 gallons of statute-measure, and some 5, 6, or 7 times as much. Since I undertook this argument, adds he, within 10 miles of this place we made in one year 50,000 hogsheads, as I examined, not by fancy, but by rule and inquiry, and this shows the hardiness of the fruit. Let our noble patriots weigh, that this is not a thing in the air, but a most certain and apparent truth, importing no less than the art of raising store of rich wines on our common arable, on our hills, and waste grounds; the charge a trifle, the pains very small, the profit incredible. Hence my design is to urge the incredible benefit that would redound to these nations, if leading persons would make themselves, their tenants and cottagers, all happy by following our example. I leave the reader to cast up how many millions of hogsheads of wine, in a few years, would be raised in the land. And truly I conceive it the chief cause, that, in all these times of late wars, none of our poorest cottages saw want; in all houses they had the same number of meals, and the same constant fare: our arable seems not a jot the less, nor our pasture the less; and for some uses the shadow of the orchard brings on the grass a fortnight the sooner, as commonly for ewes and lambs."

Concerning the Ancient Bridewell at Norwich. By Mr. Henry Baker, F. R. S. N° 477, p. 520.

This account it seems Mr. B. received from a Mr. Wm. Arderon of Norwich, as follows: The city affords a remarkable instance of an art now lost, viz. the wonderful art, which our ancestors knew, of cutting or rather breaking flint stones into uniform figures, of equal sizes, and with smooth and plain surfaces. Many remains of this sort are to be seen in our old buildings; but none is more artfully and regularly finished than the north wall of Bridewell at Norwich, which in length is 114 feet, and in height 30.

This ancient structure is one of the greatest curiosities of its kind, either in this city or county, and is not perhaps to be exceeded in any part of the known world.* It was built by William Appleyard, the first Mayor of Norwich, who in the year of Christ 1403, held his mayoralty there: and yet this flint-work appears now as perfect as if it had been finished but yesterday; whereas the bricks, which were, after a certain manner, wrought-in near the bottom of the wall, as a ground work, are almost entirely rotted away. The windows and mouldings, which were built at the same time, of freestone, are nearly in the same condition. But these flints have hitherto defied the devouring teeth of time, and will probably continue untouched for many ages; being perhaps the most durable way of building that ever was yet invented.

These beautiful flint-stones are squared to such a nicety, that the thin edge of a knife cannot be insinuated between the joints without a great deal of difficulty; and it is no easy task to make out that they were laid with lime. Most of them are about 3 inches square, and as smooth and level as if they had been ground. They are also laid with such great exactness, that no brick-work, or hewn-stone, appears more regular in its courses.

* The gate of the Austin Friars at Canterbury, that of St. John's Abbey at Colchester, and the gate near Whitehall, Westminster, are in the same taste. But the platform on the top of the Royal Observatory at Paris, which, instead of being leaded, is paved with flint after this manner, is an instance that the French have, in some measure, recovered this art.—Orig.

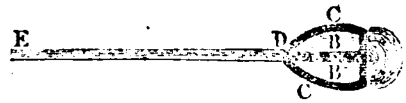
Concerning a fiery Meteor seen in the Air on July 14, 1745. *By the Rev. Geo. Costard.** N° 477, p. 522.

Mr. C. coming from his living, just before he reached a place called Stanlake-broad, and a little before 8 o'clock in the evening, was on a sudden surprised to see a long stream of fire, of a colour resembling molten glass, and of a figure like that in the margin, which shot down from A to B, in length about 20 degrees, and seemed immediately to run up again from B to A; where it turned to a sort of smoke, or rather to a fine lambent flame, like that of an aurora borealis; which continued for some time in a sort of oblong shape, but afterwards by degrees, changed successively into various other shapes; becoming fainter and fainter, till it entirely vanished about 9 o'clock.

There was a fine gentle breeze all this time; but it did not appear to affect the phenomenon so far as to make it change its place, which was to the eastward of the north: unless perhaps this change of figure might, in some measure, be owing to it.

A Note by Dr. Mortimer the Secretary.—As I was returning home from the Royal Society to Westminster, on Thursday, Dec. 16, 1742, at $8^h\ 40^m$, p. m. being about the middle of the parade in St. James's Park, I saw a light arise from behind the trees and houses in the S. by W. point, which I took at first for a large sky-rocket; but when it had risen to the height of about 20 degrees, it took a motion nearly parallel to the horizon, but waved in this manner,

and went on to the N. by E. point over the houses. It seemed to be so very near, that I thought it passed over Queen's Square, the island in the Park, cross the Canal, and I lost sight of it over the Haymarket. Its motion was so very slow, that I had it above half a minute in view; and therefore had time enough to contemplate its appearance fully, which was as in the annexed figure. A seemed to be a light flame, turning backwards from the resistance the air made to it. BB a bright fire like burning charcoal, inclosed as it were in an open case, of which the frame CCC was quite opaque, like bands of iron. At D issued forth a train or tail of light flame, more bright at D, and growing gradually fainter at E, so as to be transparent more than half its length. The head seemed about half a degree in diameter, the tail near 3 degrees in length, and about one eighth of a degree in thickness.—Orig.



* Mr. Costard was born at Shrewsbury 1710, and educated at Wadham College Oxford, of which he became a fellow and a tutor; and he died at his vicarage at Twickenham, Jan. 10, 1782, in the 72d year of his age. Mr. Costard was a man of general learning; deeply read in divinity and astronomy; well versed in the Greek, and a perfect master of the Hebrew and other oriental languages. His private character was amiable, and his correspondence with the literati was extensive, both at home and abroad. And from some passages in his writings he appears to have been strongly attached to the principles of public liberty. He had a great veneration for the Greeks and their literature; and was generally skilled in the learning of most of the ancient nations. Besides various publications on divinity, &c. he was author of the History of Astronomy, 4to, 1767, with its application to geography, history, and chronology. His communications to the R. S. which are chiefly on astronomical subjects, are contained in vols. 43, 44, 48 and 68 of the Philosophical Transactions.

Concerning the Effect which the Farina of the Blossoms of different Sorts of Apple-trees had on the Fruit of a neighbouring Tree. By Mr. Benj. Cook, F. R. S. N° 477, p. 525.

Mr. Cook sent to Mr. Peter Collinson some russetings changed by the farina of a next neighbour, whose name he wanted skill to know; but could only say, that the russeting had exactly acquired his face and complexion.

[Mr. Collinson then produced several samples of the apples; an unteinted russeting; a russeting changed in complexion, which grew among a great cluster of unaltered brethren; and some apples of the other tree, which had caused the change in the russetings, and whose fruit had in return received a rough coat from the russetings.]

Theophrastus takes notice of this Παραλλαγη, as he calls it; and tells us that the old divines were wont to make a great pother about it, and foretel great events by it: Pliny informs us, that there was one who wrote a whole book about such changes. But the use now made of it, is chiefly this, that it may be of importance to the curious in fruits, to take care how their trees are sorted, and what company they keep. For though this change be not so conspicuous in apples which have a smooth green coat, as in the russet-breed, yet one may suppose impressions of this sort often made on them; and perhaps their juices altered for the better or worse.

Note.—Sir Jos. Ayloffe, a worthy Member of this Society, communicated, on July 1, 1731, from the Rev. Mr. Henchman, prebendary of Salisbury, some observations on pease of different colours infecting each other in the same manner as the apples abovementioned. Mr. Henchman, in the spring 1729, sowed a piece of ground in his garden with white pease, and two double rows of blue pease, with an alley 4 feet wide between; in autumn, on gathering some for seed, he opened one of the pods, and was surprised to see one blue pea at the end next the stalk, with 6 white pease: but after having examined several other shells very carefully, he found a great variety of intermixtures of the white and blue pease in the same shells, sometimes one white (or blue) only at one end, sometimes at each end; sometimes two white (or blue) with one of the other colour interchangeably; and thus the whole parcel that was rubbed out for seed was intermixt white and blue. The next year, he says, not having plots of white and blue pease standing near each other, he did not find any such mixture in the several parcels then saved for seed. But it is pity he did not pick out a sufficient number of the blue pease from among the white, and sow them by themselves, in order to see what coloured pease this mixed breed would have produced.—Orig.

On the Sinking down of a Piece of Ground, at Horseford, in Norfolk. By Mr. Arderon, of Norwich. N° 477, p. 527.

In the night between the 24th and 25th of June last [1745] a violent storm of thunder and lightning happened at the city of Norwich, and the places adjacent; though at Norwich it seemed extraordinary only for the loudness of its claps, and the length of several of the flashes; some continuing near half a minute, which were so extremely bright, that they caused some thin deal shutters to the windows of his bed-room (which then happened to be unpainted) to appear almost quite transparent.

But at Horseford, a small village, about 4 miles north-west of this city, a remarkable phenomenon appeared the next day. A sudden lapsus, or sinking down of the earth, happened in the night abovementioned, and left a round hole 12 feet deep, and 12½ in diameter. Its sides are nearly perpendicular; and no ruffles, cracks, or chasms, are to be found near it, but the ground appears entirely firm and solid; and for miles around is a fine champaign country, of a dry sandy soil, but not hilly; neither is there any watercourse above ground near it. The first yard from the surface downward, is corn-mould earth; the other 3 are composed of brown and yellow sand, disposed in several different strata.

He does not pretend to account for this accident, but queries might it not be occasioned by some subterraneous current, washing away the sandy matter by little and little, till it had left only a crust, which the tremor of this terrible thunder had thrown down from the very surface; though on this conjecture, one would expect some overflow or appearance of water; whereas there was not perceived here the remains of a single drop.

Concerning the Bones of a Fœtus voided per Anum; and of some Fossils found in Ireland. By Mr. James Simon. N° 477, p. 529.

A clergyman of the county of Armagh, sent to Mr. S. a parcel of bones, with the following account of them, viz. "A woman in the barony of Clare, in the year 1741, about the latter end of May, or the beginning of June, being in the 37th year of her age, and mother of several children, conceived, as usual; but, in 2 or 3 days after, felt an excessive unnatural kind of pain in the matrix, which continued, with frequent faintings, a depraved appetite, and an exceedingly great weakness, till her child quickened; after which she proceeded reasonably well in her pregnancy to the end of 9 months; and then, her child alive, and every thing right, as the midwife thought, she fell in labour, which lasted, with proper child bearing pains, for 24 hours, but could not be delivered; and her labour leaving her, the child was no more observed to stir. In a month after, her labour returned, and, with many regular throes, continued 24 hours more; but to no purpose, save the discharging of some quantities of black corrupted clods of blood; of which kind also she threw up much by vomit: then her labour left her entirely; and soon after she felt the decaying of the flesh of her infant, and the discharge of it both by the matrix and anus, with so putrid and deadly a smell as was extremely nauseous both to herself and others about her.—Thus she lived for upwards of 12 months, and at that period her pains increasing to excess, she began the discharging of the bones, which, to the number of 80 and upwards, she voided wholly by siege; 14 the first day, and 2, 3, or 4, at a time afterwards for the space of 12 months or more, with most intolerable pains at the voiding of each bone, especially a broad piece of the skull: so that, from her conception to the day of her death, which was the 4th of April last, makes up near 4 years; during most of which time, never was a more calamitous creature: for 3 years, scarcely a day without suffering most exquisite torture, being also attended with frequent faintings, a continual want of appetite, and an almost perpetual looseness; so that it is wonderful how she lived, not eating all that long space so much as would have sustained a sucking infant; even the very liquids at length not lying a moment in her stomach; by which means she became quite emaciated, and dismal to look at, not being able to move from one position to another, or to be moved, without fainting at the least touch or motion."

In his excursions in quest of fossils, Mr. S. found what naturalists call lac lunæ; but he thinks Dr. Plot is mistaken, when he gives it as a criterion or sign of good lime-stone; for the two quarries where found were building stone, but will not burn into lime. This matter or earth makes a strong ebullition with vinegar and spirit of vitriol. Some of it was as soft as cream-cheese, when taken out of the fissure of the rock; the other was hard, some in thin crusts, and some in pretty thick lumps. It never was taken notice of in this kingdom before. He also found white native vitriol, which he takes to be the capillaris sort.

Some Account of the Distemper raging among the Cow-kind in the Neighbourhood of London; with some Remedies proposed for their Recovery. By Cromwell Mortimer, M.D., Secr. R.S., &c. N° 477, p. 532.

After remarking that physicians, both in ancient and modern times, have not deemed it any derogation from the dignity of the profession to turn their thoughts to the disorders of cattle, Dr. M. proceeds to the description of the distemper then raging among the cows in the neighbourhood of London. He was informed by the cow-keepers that a cow would seemingly be well, and feed heartily over night, or in the morning, and give the usual quantity of milk; that in 12 hours time they shall all of a sudden abate in their milk near half, and entirely fall off their stomach, so as neither to eat nor drink, and then gradually lose all their milk. As soon as they perceive this, they give them a warm mash of malt, or the following drench: "take two oz. of caraway seeds, boil them in a quart of water, and strain it; add a gill of white-wine and ¼ lb. of honey."

Their teeth are commonly observed to be loose; for which they lance the gums, and rub them with salt and vinegar. The very first day they have a huskiness, breathe short, and wheeze, but have no great cough; for which they have blooded them (in too small quantities), and rubbed their noses with tar, but with no success. Some hang down their heads, and run much at the nose; for which they lay a bag of scalding hot malt to their heads, tying it between their horns. This has sometimes relieved this symptom, but the beasts have not recovered.

The 2d or 3d day most of them fall into a purging, groan much, and seem to be in great pain. The stools seem to be bilious, have cakes of jelly come away with them, and some were streaked with blood. They soon died after these stools came on. Those that are kept out in the cold air seldom live beyond the third day; those that are kept warm in houses, and cloathed, live 5, 6, or 7 days.

Many of the cows have a wild stare with their eyes; the whites of the eye, and the skin of the eyelids, looked yellowish: their tongues looked white; they had no extraordinary heat in their mouths, or at the roots of their horns (a place where they usually feel to judge of the heat of cattle), or in the axilla or arm-pit. The mucus running from their nose is very thick and ropy: their milk is thick and yellow.

In the 2 he had seen opened, the flesh and blood looked much darker coloured than usual; the fat of the first looked yellow; the lungs were much inflamed in many places, and had several large blisters, two or three inches over, full of water, on their outer surface: there was no water in the thorax, little or none in the pericardium; the heart looked well, but the blood in it was not at all clodded, being exceedingly fluid and dark coloured; the paunch was very full of food, and greatly distended; the stomach looked well; the liver was full of scirrhous swellings and chalky knobs; the gall-bladder larger than usual; the gall fluid, but dark coloured; the intestines inflamed in many places; the fat about the kidneys was distended with air; the kidneys were sound, as was the bladder and uterus. This cow was not with calf. On opening the skull, much water gushed out.

In the 2d cow, the fat was not yellow; the lungs, heart, paunch, and stomach, were like the former; the liver was pale, flabby, not scirrhous; but the gall-bladder very large; the intestines inflamed, and in some places livid; the fat of the kidneys in this was sound, but one of the kidneys was mortified. This cow was about a month gone with calf.

The man who flayed and opened these cows said, these were the general appearances in most he had flayed; only that in some he found water in the cells of the cores of the horns.

From these circumstances he thinks it evident, that this distemper began by an inflammation of the lungs, attended with a catarrh or flux of humours from the nose; that in the progress of it there came on an inflammation of the guts, and a purging, caused by an acrimony and overflowing of the gall, which ended in stools tinged with blood, exciting great pain in the bowels, and so brought on death.

Bleeding (in small quantities) has not been found effectual, nor in short any of the remedies yet made use of; therefore, having a chief regard to the ultimate effort of nature, which seem to be to carry off the distemper by an extraordinary discharge of gall, he hoped the use of crocus metallorum, a medicine made use of with success in horses, and a great discharger of gall, as he had known its good effects in the jaundice in men, might be attended with success: he had therefore proposed to some cow-keepers to give to a cow, as soon as taken ill, one of the following balls.

Take crocus metallorum ¼ oz.* in powder; make it into a ball with dough or crumb of bread moistened; give the cow a draught of bran and warm water after it, and repeat the draught after every purging stool.

For the running at the nose, he was told, that pouring a pint of warm vinegar with an ounce of salt into the nostrils, had proved successful in making the cow sneeze, and discharge a great quantity of thick yellow mucus, and other matter, from the nose, after which the cow recovered.

For the shortness of breath, he had advised the giving, whale-oil, treacle of sugar, each a pint; flower of brimstone 4 oz. Give it in a mash of malt, or bran and water, twice or thrice a day.

For the scouring, first give the crocus purge above mentioned; then give them every 6 or 8 hours the following draught.

Take whiting 1 lb., bruise it; pour boiling water on it, a quart or more; let it stand to settle; pour off the clear water, and fling it away; then put a quart of warm water to the wet whiting; and add bole-armeniac in powder 2 oz., Venice treacle 1 oz., English malt spirits half a pint. †

An Account of a Fracture of the Os Ilium, and its Cure. By Mr. D. P. Layard, Surgeon. N° 477, p. 537.

Feb. 8, 1745, one John Easdon, about 22 years of age, was jammed between a waggon and a coal cart, as he was getting up into the waggon; the cart-wheel pressed on the upper part of the left os ilium, and, by a sudden jolt, squeezed him against the waggon, so as to raise him from the waggon-wheel on which he stood; then the cart going on, the poor man fell on the ground.

Mr. L. examined the part, and found just below the contusion made by the pressure of the cart wheel, a fracture running quite across the costa of the left os ilium, about 3 fingers breadth below the crista of the said bone; the end of the upper fractured part being forced in towards the cavity of the abdomen.

* Or more, according to the size and strength of the cow; or as the first dose is found to operate. —Orig.

† See a former account of a distemper among cows by Mr. Bates, in vol. vi. p. 375 of these Abridgments.

The patient being laid on his back, on the edge of the bed, he applied a napkin on the false ribs, which was pulled tight by 2 assistants, in order to press the contents of the abdomen downwards: another assistant pressed the abdomen on the right side, while, by pressing the crista of the fractured os ilium gently inwards, he brought both the edges of the fracture to a mutual contact.

The fracture being reduced, the napkin applied on the false ribs was tightened, and kept on during the whole cure with the scapulary. He applied proper compresses, and a pasteboard cut according to the figure of the bone, over which he applied the spica bandage. The patient was kept in bed for about 3 weeks, lying on his back, the affected side being supported with a soft pillow. By this means, and by observing what is generally recommended in all fractures, the patient was perfectly cured, and walked very well at the month's end.

Of a curious Tripos and Inscription found near Turin, serving to discover the true Situation of the ancient City Industria. By David Erskine Baker. N° 477, p. 540.

Dr. Joseph Laurentius Bruni, F.R.S., and physician of the College at Turin, having, in the month of March, 1744-5, sent from thence the description of a most curious antique tripos of metal, found, some little while before, together with a plate of the same, bearing an extraordinary inscription, at a village called Monteu, on the right side of the river Po, about 16 miles from Turin; and the same gentleman having lately sent us likewise an Italian dissertation printed at Turin, wherein the learned authors (Paul Ricolvi and Anthony Rivautella) undertake to discover, from the said inscription, and other concurring circumstances, the true place of the ancient city Industria, mentioned twice by Pliny; a short account, collected from the whole, and translated into English, may prove not unacceptable.

This tripos, they say, far exceeds every thing of its kind, preserved hitherto in any of the cabinets in Europe, as well for its structure, as for the variety and elegance of the several relievos with which it is adorned. Each of its 3 pillars has on it 4 small figures: the first, which is placed at the top, represents a terminus of Venus; the second is a Victory, or a winged Fortune rather, standing with her feet on a globe; the third, which is near the middle of the pillar, is a Harpy, winged, with a woman's face; and the fourth figure, at the foot of the pillar, appears to be an old Silenus or Satyr, crouching himself together in an odd manner.

The pillars are joined to each other by little bars of metal, fastened by rivets at top, and rings at bottom, in such a manner that they may be closed together, or drawn asunder, at pleasure; and when they are extended to the utmost, the size of the tripos is somewhat more than a Turin foot, which, Dr. Bruni says, is equal to 20 English inches.

EQ. ROM. EQ. PVB.

These words imply that Lucius Pompeius, the person to whose honour this plate is inscribed, was a Roman knight, who had a stipend from the public. The Roman knights served at their own expence till the year of Rome 451, when their horses began first to be maintained at the expence of the commonwealth; and it appears, from various inscriptions under the emperors, that the words, eques publicus, equo publico donatus, or ornatus, &c. always mean a military dignity, and must be distinguished from the Roman knights towards the end of the commonwealth, who were a degree of citizens between the senators and the plebeians.

Q. ÆR. PET. ALIM.

It hence appears that Lucius Pompeius was quæstor ærarii, though only of the finances of the city Industria, and not of the emperor under whom he lived. Several other inscriptions are also produced, to prove the office of quæstor alimentorum; and a great deal of reading is introduced, to show that the quæstor alimentorum was sometimes understood to be an officer having the care of the public allowance for bringing up children; and that at other times his office was understood to be the procuring all sorts of provisions for the use of the emperor's troops.

Passing by his office of ædilis and duumvir, we find he presided likewise over the receipt of the taxes, by this address to him,

CVRATORI

KALENDARIORVM. REI. P.

The days fixed for payment of the taxes and debts were registered in the public calendars; and creditors usually demanded their interest on the kalends, or first day of every month: whence the register of the debtors, and the sums due, or the tribute to be paid by particulars to the public, and indeed the general state of the debts, and credit of every community, came to be called calendarium.

COLLEGIVM PASTOPHORORVM.

The college of priests called Pastophori, a name taken, as some suppose, from a very rich and ornamental upper garment termed pastos. As Pastophorus was a name given to Venus, these priests may have belonged to her, or else to the goddess Isis, whose chief priests, as Lucius Apuleius informs us, were called pastophori, by way of pre-eminence, "Unus——cætu Pastophororum, quod sacrosancti Collegii nomen est, velut in concionem vocato, indidem de sublimo suggestu——renunciat, &c." He says also, that the god Osiris had a college of them. His words are, "Osiris —— in collegium me Pastophororum suorum, imo inter ipsos decurionum quinquennales elegit." This body of priests had various offices; one might probably be the conferring honours on persons of great merit, as we find from our inscription those of Industria had done on Lucius Pompeius.

INDVSTRIENSIVM
PATRONQ.
OB. MERITA.

Hereby the city Industria acknowledges Lucius Pompeius as its Patron or protector, and shows its gratitude for singular benefits received. Patrons and clients were in the earliest times of the commonwealth; but under the emperors inscriptions show us frequently, that cities and nations chose for their patron some eminent Roman citizens in favour with the prince, on whom they often conferred great honours.

T. GRAE. TROPHIMVS. IND. FAC.

These words on the cornice our authors wondered to find, as it was unusual for any but the most eminent painters and sculptors to put their names, and that only to the most famous and perfect of their works. Whence they conjecture, that this Titus Græcus Trophimus of Industria, might be not only the engraver of the inscription, but likewise the sculptor of some image to which this may have been the pedestal.

These gentlemen, who are authors of the Marmora Taurinensia, went to this village of Monteu in the Autumn of the year 1743; where they found many inscriptions, with the names of various magistrates, both civil and ecclesiastical; which were certain proofs that some considerable city had been in that place formerly: and returning thither the Autumn following, they found a broken stone; on which, by putting the pieces together, they could plainly read, that there had been decreed to a person named Cocceia, at the expence of the public, a statue AB. IND., which they interpret ab Industriensibus, and suppose to mean the citizens of Industria.

. . . . . COCCEIÆ . . . . .
HA . . . EC . . AB. IND.
. . . . FVNERE. PV.
ET . STATVAM.

Some peasants about the same time, digging in the plain between the hills near the Po, discovered the vestiges of an ancient fabric, with some medals; and in the middle of the following February, found the traces of a large room, other medals, and some pieces of wrought brass; and in March they discovered this plate and tripos. Our authors mention also the discovery of many medals, a Mosaic pavement, the remains of an ancient temple, basso-relievos, little images, ruins of edifices, and inscriptions found here; and give two passages from Pliny, mentioning this place.

In the first of these quotations, the city Industria is spoken of, as one of the noble cities that flourished in its time along the banks of the river Po, a little way to the south side of the Apennines. In the other he explains himself more fully, describing it to be near the Po, where that river begins to acquire a greater depth: and as a confirmation, gives its name still more ancient than that of Industria, viz. Bodincamagus, signifying in the Ligustine tongue, the river's being deeper at that place. And our authors affirm, that even at this day the Po, above and near Turin, is hardly navigable; but at Monteu, after having received not only the Dora, but the Stura, the Orco, the Mallone, and the Dora Balteo, it becomes much larger both in depth and width. They also take notice, that the hill near the plain of Monteu is called Mondicoi, which they suppose a corrupted remain of the ancient word Bodincomagus. They find also in the bulls of this parish, that the parochial church is called Sancti Joannis Baptistæ de Lustria; which they conjecture may, by length of time, have been formed from the ancient name Industria.

From all these circumstances put together, they seem confident of their having discovered the real spot where this ancient city stood; and bring several reasons to prove, that Casal cannot possibly be the place, as some writers have imagined; and in order to show more fully the grandeur, magnificence, and antiquity, of this ancient city, they add several other inscriptions found at the same place.

Further Observations on the Distemper now raging among the Cow-kind. By Dr. Cr. Mortimer. N° 477, p. 549.

Since his former paper on this subject, Dr. M. had opportunities of being present when 3 cows had been flayed and opened; the lungs in all were inflamed and blistered, and the guts in some places inflamed, in others livid, the gall-bladders exceedingly large: a collar-maker's man, who has been assisting in flaying above a hundred dead cows, assured him these were the general appearances in them all; except that in one he met with a large bag full of corruption, between the bag inclosing the heart and the back bone; in another he found the gall-bladder quite contracted and shrivelled up, having little or no gall in it; and in several he found scirrhous knobs in the livers.

Nov. 26. Dr. M. desired Mr. Hill, an ingenious apothecary in Westminster, to accompany him to see a cow dissected, and to help in examining every thing very carefully, having got her drawn into a shed, to defend him from the weather.

When the skin was taken off, she appeared very fat; the muscles looked of a darker colour than usual. On opening the abdomen, the caul appeared very fat; the paunch was greatly distended; on making a puncture, much air gushed out: it had in it a great deal of food; the inside looked well, and did not peel:

the 2d and 3d stomach, or the omasum, as also the fourth stomach or abomasum, were almost empty, but looked well; the liver was firm, well-coloured, and sound, except a few scirrhous knobs about the size of nutmegs: the gall-bladder was exceedingly large, and full of very fluid gall; the guts were inflamed in many places, the colon and cæcum livid: he had the curiosity to have them measured; from the anus to the insertion of the cæcum, there were 12 yards (the cæcum was an ell long), and from the cæcum to the pylorus, there were 52 yards. The midriff was much swelled and inflamed: the lungs were swelled, inflamed, adhered in some places to the pleura, and almost wholly covered with bladders of water: there was no appearance of any inflammation on the pleura, or in either the internal or external intercostal muscles: the windpipe was inflamed greatly throughout its whole course, especially its inside; but the gullet, which lay so near it, was not in the least inflamed: the heart was of its natural size, the pericardium full of very fluid blood, probably from the bursting of some branch of the coronary artery, caused by the extraordinary accumulation of blood in the right ventricle; for the vena cava, and right ventricle of the heart, were turgid, and full of black coagulated blood, though this cow had been dead but 12 or 14 hours; the lungs were likewise turgid with blood, but little or none was found in the left ventricle or aorta; the obstruction seemed to have been so great in the lungs, that very little blood could pass through them from the right to the left ventricle of the heart, and therefore evidently evinces the existence of a confirmed peripneumony. All the membranes lining the nostrils, and the spongy bones there, were quite turgid with blood, and in the highest state of inflammation. The greater and less brain looked fair and well, seeming no way distempered.

Dr. M. had not seen, in any cows he had examined, any cutaneous sores or exulcerations, nothing like the boils, carbuncles, &c. described by authors as the constant concomitants of the plague in men: nor does there seem to be any attempt of nature to fling off the distemper by any internal imposthumation, or discharge, unless by the running at the nose, and by the bilious stools, or bilious urine. The few, which had recovered, had been such as had been kept within doors very warm, had been blooded once, twice, or oftner, had had warm mashes of malt and bran given them, and warm drenches of warm herbs, such as rosemary, wormwood, and ground-ivy, with honey or treacle, and had neither purged at all, or but little; and when they had not purged at all, their urine had been observed to be as high coloured as Porter's beer.

He was informed, by the farriers and cowleeches, that a horse or a cow would bear to have near 2 gallons of blood taken away without fainting. One cow he had seen, within about a month or six weeks of her calving-time, was taken with the running at the nose, and shortness of breath; the owner of her immediately took away out of the neck 5 quarts of blood by measure, and gave her a warm mash of malt once in 6 or 8 hours: next day he cut her tail, and let her bleed 2 hours; the day after he took away 2 quarts from under the tongue, and so continued bleeding her, at 14 or 15 hours distance, for 7 times. She did not purge at all; her urine was as high-coloured as coffee at first, but grew paler and paler every time of bleeding: she soon recovered, afterwards ate heartily, looked brisk, and had not slunk her calf.

The concern the cow-keepers were under for the loss of their substance, the various methods offered to them, and their want of judgment either to chuse the most rational, or their want of accuracy in making experiments, and following directions, were quite discouraging, which was the reason why none of them have pursued any regimen so steadily as to give us an opportunity of making conclusions from it: indeed several owned that they were quite bewildered, not knowing which way to turn themselves, or whose advice to follow, what one said being quite contrary to the directions given by another. Some to whom he had given his directions had blooded once, had given the purge once, but had not given the oily drench; or had given this once, and had not repeated it; others had given the chalky drench once, and not repeated it; and had not followed the other parts of his instructions; so that he was sorry to find that he could have no satisfactory experiments made: yet, as the state of the disease seemed so evidently to be a peripneumony, or inflammation of the lungs, windpipe, and nostrils, attended with a redundance of gall, he could not forbear urging to the public the following method.

"Give to all cows in general, while well, ½ oz. or 1 oz. (according to the size of the cow) of crocus metallorum. As soon as a cow falls off her meat, give her another dose of crocus metallorum; and give her warm mashes of malt, bran, &c. When she runs at the nose, lay a bag of malt-meal, wetted with boiling water, on her forehead and nose, tying it to her horns morning and evening; pour warm vinegar and salt into the nostrils: if a short cough, or difficulty of breathing, come on, bleed her one quart twice a day, for 3 or 4 days, and every 6 hours give the oily drench: if a purging come on, give another dose of the crocus metallorum; if it continue, give the chalky drench every 6 hours, and if it do not abate in 24 hours, inject the same mixture by way of clyster; and if the husky cough continue with the purging, give the oily drench one 3 hours, and the chalky drench the next 3 hours."

Most of the cows, which have recovered from this distemper, recover their milk again, as their appetites mend; but they are observed to have scabby eruptions come out in their groins and axillæ, that itch much; for a cow will stand still, hold out her leg, and show signs of great pleasure, when a man scratches these pustules or scabs for her.

He was informed, that some cow-leeches had given coloquintida and salt of tartar, each 1 oz., in a quart of warm ale; but he imagined it must be too griping a purge, and improper where the guts are inflamed. Indeed he had not heard of any cows recovering which took it.

As for the cause of this distemper, he was still at a loss: he thought it could not be owing to the food, because the cows which had it first in Essex ate only grass, turnips, and hay or straw; the cows about London ate, some, grass; all grains and hay, some, little or no grass, but lived chiefly on grains, turnips, off-falls from the garden-grounds, and hay.

He was in doubt as to the air; the spring and summer were very wet, and the ground very damp; the autumn was very dry and cold; the beginning of winter very damp and cold. The cows in Essex had the distemper in summer; it first began about London in autumn: it had spread itself equally among cows which had lain in the fields a-nights, and those which stood in stables or sheds: it spread itself in Essex, at first into such farms where they bought in strange calves, or lean cows, at market, which they did not know where they came from; but most probably from the hundreds where the disease first broke out; but how it got thither, whether by importing any cattle from Flanders, he knew not; for surely there is too wide a tract of sea for any infectious miasmata to be wafted over to that part of the country by the winds! This was certain, the viscera concerned in respiration are the parts chiefly affected. Its spreading in England had been progressive; and therefore one may reasonably think it was not constitutional in the air, for then it ought to be universal every where; but that it was contagious, and propagated by infected cows being mixed with well cows: therefore the not buying in calves, or strange beasts, but every farmer keeping his herd by itself, must be a great means of preventing the propagation of it; and housing the cows a-nights might be a proper preservative against it.

An Account of the Weaver's Alarm, vulgo Larum. By Mr. Arderon. N° 477, p. 555.

Nothing is more true, than that necessity is the mother of invention; among the many instances of which, the useful contrivance described below may serve as one remarkable instance.

This little apparatus goes commonly by the name of the weaver's larum, from its being chiefly or originally made use of by persons employed in that trade, who have frequently occasion to rise very early to their work: and Norwich may boast of its first appearance there, though the inventor's name be not known. However, the simplicity of the thing itself, and the singular service it may be of to multitudes of people, render it not undeserving notice.

The materials necessary to compose this little time-piece or monitor, are nothing more than a small candle, of 14 or 15 inches in length, a piece of thread or packthread, a graduated board, and a common stone, or any other ponderous body: but the drawing here added, fig. 1, pl. 4, will fully explain it.

A represents a board, which hangs commonly against a wall, divided and figured according to the size of the candle made use of.* B, a little shelf to place the candle on. CC, a thread or packthread, tied fast at D, and hanging over a pulley at E, to which a weight is hung at F.

By sliding the spring of the candlestic G, up or down, as occasion requires, the flame of the candle is raised as many hours above the thread as the person that adjusts it designs to lie before he is called up. At the desired hour the candle burns the thread in two, the weight falls, and by its noise seldom fails to wake the person.

But if the man, who makes use of this contrivance, happens to be of a more than commonly sleepy disposition, in such a case another thread is tied to that part of the line CC which is next the pulley, and its other end is twisted round the thumb or wrist of the sleepy person; by which, when the candle burns the line, and the weight falls, he receives such a sudden pull as can hardly fail to wake him; as the drawing will easily explain.

If the line for a few inches on each side of the candle be wire, with a short thread only just in the middle where the candle is placed, there can be no danger of doing mischief by the fire running along the line.

And thus may the poorest mechanic provide himself with a useful servant at a very small expence.

Of some Human Bones, Incrusted with Stone, now in the Villa Ludovisia at Rome. Communicated to the R. S. by the President, Martin Folkes, Esq. N° 477, p. 557.

Something like the body of a petrified man being mentioned by several authors, as preserved in the Villa Ludovisia at Rome, and the same having been lately referred to in a discourse read before this Society; he thought that a drawing o. that curiosity, which he procured at Rome might possibly deserve the notice of the gentlemen, especially, as it will hence appear, that the several accounts hitherto given of it are not very accurate, or at the best convey but a very imperfect idea of the truth.

The following passage occurs in the journal-book of the Society, for April 17, 1689. "Mr. Henshaw related, that he had seen, in the Villa Ludovisia at Rome, the body of a man incrusted with a sort of a white marble or alabaster case, supposed to have been a man frozen in the alps, and after, in long process of time, this incrustation to have grown upon him; and that one of his arms was broken off, purposely to show that it was no imposition."

* For want of such a board a common ruler is frequently used, to set the number of hours between the flame of the candle and the thread.—Orig.

Mr. Richard Lassels, in his Travels to Italy, printed at Paris in 1670, p. 180, tells us, that in the lesser Casina, belonging to the Ludovisian villa, he saw, "in a large square box lined with velvet, the body of a petrified man, that is, a man turned into a stone; one piece of the leg (broken off to assure an embassador doubting of the verity of the thing) showed plainly both the bone and the stone crusted over it. The head and the other parts lie jumbled up together in the box."

Father Kircher says, in his Mundus Subterraneus, l. viii. ch. 2. "Spectatur et hic Romæ in horti Ludovisiani palatio, corpus humanum totum in saxum conversum, ossibus adhuc integris, et lapideo cortice obductis." And in the following page he gives an imperfect sketch of the same thing, under the title of "Sceleton humani corporis in saxum conversum, ex palatio Pinciano principis Ludovisii." But the truth is, there is nothing like the body of a man, but only a cluster of disjointed bones, cemented together by the same matter that incrusts them over. Mr. Misson in his Travels has more truly described them, when he says, that "in the same room they show a small heap of bones, said to be the skeleton of a petrified man; which is a mistake, for the bones themselves are not petrified, but there has gathered about them a sort of candied crust, or stony incrustation, which has made them pass for being of real stone." Mr. Wright also, in his late observations made in travelling through Italy, &c. has taken notice, that in the Villa Ludovisia "they showed some bones of a human body all crusted over with a petrified substance."

When Mr. F. was at Rome in the year 1734, he saw this curiosity, which is still preserved in the same casina of the Ludovisian gardens; and in the very square box lined with velvet, mentioned by Mr. Lassels, and represented by father Kircher. The stony substance that joins the bones together, is of a whitish colour, and the same as that which incrusts the bones themselves: small fractures in several places discover the natural bones; and the size of the whole mass may be judged of, by considering the skull, which is of the common dimensions, as a scale to the other parts.

END OF THE FORTY-THIRD VOLUME OF THE ORIGINAL.

Description of a Water-wheel for Mills. And concerning the Bark preventing catching Cold. By Mr. Wm. Arderon, F.R.S. N° 478, p. 1. *Vol. XLIV. Anno* 1746.

Mr. Philip Williams, chief engineer to our water-works at Norwich, a man of great ingenuity, who, in his time, has been author of many curious inventions, has contrived lately a machine for the raising of water to supply cities and drain marshy grounds, or other useful purposes, where no head of water can be procured, and the current runs very slowly: circumstances which render most other engines useless.

The axis of the first mover is cut into the form of an hexangular prism, of dimensions suitable to the force required, as is represented by the letter A, fig. 2, pl. 4. Into this, several sets of holes are mortised, as BBB. These are intended to receive different sets of sails made of iron plates, one of which is represented at C; all which sails are weathered in the same manner as those designed for windmills; only in these, the extremity of their ends stands parallel to the planes of each end of the axis; viz. those ends which are farthest from the centre. This hexangular axis, when employed, must be placed parallel to the moving stream, and may lie even with its surface: but the engine will act most vigorously, when it and all the sails employed are entirely under water, as is easy to comprehend.

Each set of the sails before described contains 6 in number, and are so contrived as to be put in and taken out at pleasure. Whence it follows, that when a single set of sails is made use of, the engine produces a single effect; when 2 sets, a double; and so on, till the desired momentum is acquired, with the same quantity of running water, provided there be room to fix a sufficient number of sails.

It is further to be observed, that when this engine is placed with its sails, made and weathered as above directed, they will move with equal velocity, even supposing the current should change its course, and come upon them in a quite contrary direction; as the case really happens in rivers where the tide ebbs and flows; where most other engines yet invented are of little use.

About 6 weeks ago a model of this engine was tried. It was fixed in our river, in a place where the water moved only 27 feet in 20 seconds; in which time the first mover made 6 revolutions. Its diameter was no more than 2 feet 2 inches; yet it would have lifted 14 pounds 2 yards high in the above-mentioned time, had not a misfortune happened to its case, which made it not perform quite so much.

It appeared somewhat extraordinary, that the circumference of its first mover, viz. any determined part of it, passed through a space of 42 feet in 20 seconds; which is nearly twice as fast as the motion of the water: and as the momentum will be in proportion to the number of the sets of sails that are employed, its force is capable of being greatly augmented with the same quantity of water: a thing not to be admitted without sufficient experiment, but what seems extremely plain in theory, and what probably will answer when brought to practice.

This engine, when once seen, requires little skill for constructing it, is made at a small expence, and easily kept in repair.

Dr. Salter, one of the prebends of Norwich cathedral, writes, in a letter to Mr. Arderon, that, on seeing Mr. Baker's communication of the use of the jelley, or rather rob, of black currants in curing sore throats, Trans. N° 459, he thought it might be of service to take notice of the following effect of the Jesuit's Bark: the Doctor used to be subject easily to take cold, and in consequence to be subject to have a sore throat to a very great degree; but the last time, above 15 years ago, after his recovery, he was advised by Sir Benj. Wrench, to take 2 oz. of the bark, after due preparation by bleeding, or purging, or both, when he was altogether without complaint, every spring and fall. This he said would more effectually guard him against taking cold, which he has found so far to answer, that he is now able to go 500 miles with less hazard of cold, than he could go 20 before; and he has never since had what he can strictly call a sore throat.

A third Account of the Distemper among the Cows. By C. Mortimer, M. D. Fell. of the Royal Coll. of Physicians, and Secr. R. S.* N° 478, p. 4.

During the Christmas holidays Dr. M. sent for some milk, as usual, to the Vineyard in St. James's Park, none of the cows belonging to that house having as yet caught the distemper, though 3 had already died in the Park: part of the milk was used for chocolate, and part was set by for cream for the next morning. The milk had a rank sourish smell and taste, like rank butter: the cream next morning was more so; they boiled the milk, which did not curdle; so they used the cream with tea, though the taste was not very agreeable. The boiled milk curdled in the tea; neither any of Dr. M.'s family, nor a friend who drank of it, found any inconvenience from it. On sending the morning following for more milk, the people refused selling any, saying one cow was taken ill, and another was nearly dry.† This was the cow whose milk Dr. M. had had, and she died in 48 hours. Next day another fell ill, and was knocked on the head by the public officer, in about 48 hours after her being seized. Dr. M. had the curiosity to see this cow opened, which was done the next day but one. The inflammation in general in this creature was greater than what he had before seen in any of those which died of the distemper; this cow had been blooded about 3 weeks before she was taken, and once as soon as taken: the caul was greatly

* See the 1st and 2d account at p. 171 and p. 177 of this abridged volume.

† Dr. M. sent for some of the milk of the sick cow, after she had been about 12 hours ill: they could not get above 2 oz. which was as thick as cream, and yellow like cheese: it curdled, being put into bohea tea next morning. In about 3 days keeping, it turned of itself into a substance like cream cheese, without separating any serum. In 8 days it dried away to a hard cheesy substance, and in 14 days became quite dry, like the rind of Gloucester cheese; it smelt like rank butter at first, but never corrupted or stunk.—Orig.

inflamed, the paunch inflamed, and the inner coat peeled off, especially that of the (abomasum) faidle; the guts were all inflamed; the liver was much inflamed in some parts, in others was turned livid; the gall-bladder was very large, and the gall very liquid; the lungs adhered, in many places, to the pleura, were greatly inflamed, and turgid with blood, and were in many places quite black; he did not find any of the watery bladders on the surface of these, as he did on all the others he had seen opened.

Here is an instance of the most surprisingly quick progress of this distemper, and to such a violent degree, that he does not think it in the power of medicines to have prevented death; but he thinks this case is still a further confirmation of the necessity of plentiful bleeding as soon as a beast falls sick, especially if shortness of breath ensues: this cow was not come to the stage of purging.

From the distemper getting into the Park, he thinks there is reason to conclude it cannot arise solely from any fault in the food, because the pasture is always good there, and, from the great number of horses, always kept low; and the soil never dunged or manured; and the cows have plenty of hay in winter. How it got into the Park is very strange; there having been no fresh cows brought in there since Welsh fair in August. And this is further very observable, that though this distemper seems so very infectious among the cow-kind, yet he did not hear that any of the deer had fallen ill; which is much more likely to happen to them than to the horses, because they chew the cud, these do not. He therefore suggests whether it would not be the most likely means to put a stop to the spreading of the distemper, to forbid any cows or calves being brought to market to be sold alive, or that any farmers should buy in any fresh cattle for 6 months, or till it is found, that the distemper is entirely ceased; and that all fat cattle should be kept carefully separated from the cows and calves, and that under severe penalties.

An Appendix to the foregoing paper.—On reading the foregoing paper, some gentlemen present favoured the company with the following information and remarks.

Mr. Theobalds, a worthy member of the Society, and a diligent observer of remarkable occurrences, informed the gentlemen present, that the first infection of this dreadful distemper among the cow-kind was brought over from Holland, in April 1745, by means of 2 white calves, which a farmer at Poplar, near London, sent for, in order to mix the breed, and that the infection was got to Maidenhead in Berkshire, by 2 cows brought out of Essex, and sold at the fair there; that there was observable a very disagreeable smell in the clothes of persons, who had been very conversant with sick cows; and that the infection had been propagated by means of sheep, who, it is presumed, carried it in their wool. On the mention of this scent in clothes, Dr. M. remarked, that Dr. Lobb, in his late book, intitled, "Letters relating to the Plague, and other contagious Distempers, London 1745, in 8vo. in his letter to John Milner, Esq. p. 388," recommends to persons conversant about sick cows to wear a linen garment, over their other clothes, wetted with a mixture of salt and vinegar; and, p. 383, he gives many prudent useful rules to farmers for the management of their sick cattle.

Dr. Parsons, another ingenious Fellow of the Society, said, that the cattle in the high grounds about Hampstead, Highgate, Millhill, and Hembdon, had hitherto remained free from the infection; but that it had spread all about in the lower grounds.

Mr. Hoffman, a learned Danish gentleman present at this meeting, said, this infection was first carried into Denmark by raw hides of cattle dead of this distemper, rubbed with wood ashes, in order to preserve them fit for tanning, which were brought from Flanders; that some cows sickened in a few days after the unpacking of these hides in Denmark; and that they have lost above 50,000 head of cattle in that kingdom.

At another meeting, Mr. Collinson, a member greatly deserving of the Society, acquainted the company present, that a farmer in Essex, who had the distemper among his cows, invited a neighbouring farmer to come and assist him in giving drenches to some of his sick cattle; the good natured man went accordingly, and spent best part of the day with his neighbour, to lend him his help in his distress, little dreaming of what ill consequence this friendly act was about to prove to himself; for, being so many hours conversant with the diseased cows, so much of the infectious effluvia adhered to his clothes, that as he was walking home, which was about a mile and half, his way lying through a field in which several of his own cows were feeding, he no sooner entered the field but the cattle all left off their grazing, ran to the farther end of the field snorting and flinging up their noses, showing the greatest uneasiness at their master's approach, and endeavouring as much as possible to avoid him, as though they smelt something very disagreeable; and so indeed it proved to them, for the very next day many of them fell sick, and died in a few days.

A certain cow-keeper in Tothill-fields, Westminster, had 30 cows, out of which number 4 only have survived, 2 never took the infection, 1 had it and recovered; and he said, that one had the distemper 4 several times; for that, as soon as she was well for a week or 10 days, she relapsed, and went through all the stages of the disease, but now continues well.

In St. James's Park are kept 17 cows, of which number 4 were bought in new at Welsh fair; out of these 11 are dead; 4 never had the distemper, and 2 recovered from it. These are the cows which were so plentifully blooded, mentioned in the former paper, N° 477; and one of them, then said to be very big with calf, being recovered, went the proper time, had a living calf, and is well and thrives; indeed they knocked the calf on the head, because they wanted the milk.

Dr. M. was informed, that a farmer at Little Chelsea, who had but 10 cows, has not had any fall sick, though his neighbours had cows sick all around him. His management was, not to let any of his cattle have any communication with his neighbours, to keep them within doors, littered like horses with clean straw, to feed them with good hay, and give them plenty of clean water to drink; to turn them out every day at noon into his yard to air themselves; and, in the mean time, to clean out the cow-house carefully; removing all the litter, washing the pavement clean with a birch-broom, laying clean litter, and keeping them warm a nights.

As a contagious distemper among the cow-kind is no new thing, Dr. M. thought proper to look into the Auctores de Re Rustica; but found none so full in the account of the pestilence among cattle as Columella is, in lib. 7, cap. 5. He advises, as soon as any signs of an infectious distemper are perceived, to drive the cattle immediately into a different air, at as great a distance as can conveniently be done, to separate the sick from the sound; and that there should be no intercourse between them, lest the infection be carried to the sound. If these cautions only were strictly observed by our farmers, Dr. M. thinks there is reason to hope the contagion would soon be extinguished. He would advise the building several small huts with faggots and broom, at a distance from each other, in some fallowed field, and there keep a man constantly to attend the sick cattle, and to have every beast, as soon as it begins to sicken, removed into one of these huts, as into an infirmary; by which means the cow-house will be kept clear from infection: and never let this man go near the well cattle, but keep them in the most distant pastures, and let them have huts run up likewise to shelter themselves under from the inclemencies of the weather, providing them with clean straw to lie on. He heartily wishes we had the experience to say with Columella, Evincendi sunt autem quamvis pestiferi morbi, et exquisitis remediis propulsandi. He recommends a drench made of a wheat-mash made with all-heal, eringo-roots, and fennel-seeds; and he says he has known as an immediate remedy, a rowel made in the ear with the roots of the larger black hellebore; and he says, that Celsus advises the pouring into the nostrils wine, in which misseltoe-leaves have been bruised.

These infectious diseases have not been confined to the cow-kind alone, but sometimes the contagion has been so virulent as to attack all sorts of brutes, as well as men. Ovid mentions a dreadful instance in his Metamorph. lib. 7, l. 536.

Strage canum prima, volucrumque, oviumque, boumque.

And ibid. l. 538.

Concidere infelix validos miratur arator
Inter opus tauros; medioque recumbere sulco.

Virgil gives an account of such another contagious sickness in his Georgic. lib. 3, l. 515.

Ecce autem duro fumans sub vomere taurus
Concidit, et mixtum spumis vomit ore cruorem,
Extremosque ciet gemitus:——

Lucretius, mournfully describing the plague at Athens, of which Thucydides has left us so ample a relation, records the infection being likewise spread among the cow-kind: see lib. 7, l. 1129.

Consimili ratione venit bubus quoque sæpe
Pestilitas, etiam pecubus balantibus ægror.

Soon after the times of Constantine the Great, one Severus Sanctus, a Christian poet, left a melancholy account of a murrain then reigning among the cows, in a Latin eclogue, intitled, Carmen de Mortibus Boum, which was reprinted at Leyden, in the year 1715, 8vo.

In the beginning of his poem the author describes the sudden destruction that distemper carried with it, and the progress it made in Europe, so like what it has now done.

We see, by these accounts of the murrain among beasts and cattle, that this dreadful distemper has often accompanied or preceded the plague among the human species: what pains therefore does it behove us to take to prevent the spreading of this disease among brutes? and what warning ought man to take, lest the pestilence should come home to him?

Of a Rupture of the Diaphragm, and Displacement of some of the Viscera, observed in the Body of a Girl 10 Months old. By Dr. John Fothergill. N° 478, p. 11. *An Abstract from the Latin.*

A delicate lady, about 21 years of age, after miscarrying of her first child, and recovering with much difficulty from the weakness consequent to the flooding, became again pregnant, and was in due time delivered of a perfect, but small and delicate female infant. From the moment of its birth the respiration of the child was observed to be more frequent than natural; and soon afterwards there came on a defluxion of a mucous humour from the mouth, eyes, and nose, so that the child was almost suffocated when she attempted to suck, and hence would cry and burst out into fits of passion, so violent as to threaten immediate death. The defluxion was in some degree relieved by proper medicines; but the child continued to be troubled with vomitings, which came on more suddenly and more frequently than is usual with infants. Its bowels were likewise a good deal disordered; and there was much difficulty of breathing, especially at night.

About the 7th month from its birth the child had an eruption of hard red pimples, (tubercula) on the face, neck, shoulders, breast, and other parts. These pimples, the eruption of which was preceded by fever, were accompanied with a most troublesome itching, and rose gradually into vesicles filled with a thin pellucid serum, after which they dried up and scaled off, leaving behind them dark red marks. One crop of these pimples succeeded another for some time; at length, however, they were entirely removed by the use of mild evacuating medicines. The child was weaned about the beginning of the 10th month from its birth; it seemed not to suffer much from weaning; but on the 6th day after, it was seized with a violent vomiting, which in the course of 24 hours terminated fatally.*

On opening the abdomen, the liver was found to be large and pallid. The gall-bladder was enlarged and turgid with thick, black bile. All the adjacent parts were tinged of a yellow colour. The ileon was inflamed, and the colon was not in its place.

On removing the sternum, they were astonished to find that a large portion of the stomach had forced itself into the left cavity of the thorax, concealing from their view the left lobe of the lungs, with the pericardium and heart. The stomach being drawn aside to see what was under it, they found that a large portion of the ileum, together with the cæcum and its appendage, and a portion of the colon, had been likewise forced up into the same place. On further inspection, they found that this had happened in consequence of a laceration or rupture of the diaphragm, which ruptnre must, from the appearances of the edges of the ruptured part, have taken place a considerable time before the death of the child. It (the rupture) extended from the sternum and cartilages in front, down to the tendinous centre of the diaphragm, so as to form an unequal opening (sinus), wider on the left than on the right side. Through this large opening, the beforementioned viscera had got admission into the left side of the thorax; and were retained there so securely, as not to slip back again in any posture or under any agitation of the body. Dr. F. imagined that this rupture of the diaphragm, and consequent displacement of the stomach and intestines, happened at the time of the child's birth.

The heart was exceedingly small, and was forced against the spina dorsi; and the lungs on the left side were compressed into a very small compass. From the preternatural situation of the lower part of the stomach, the passage of the bile through the ductus communis choledochus was interrupted, whence the high coloured urine, the jaundiced appearance of the skin, &c. are easily accounted for.

* In addition to the disordered action of the bowels, at one time costive at another time loose, the urine was observed to be bilious, and the skin was sometimes yellow.

The lungs on the left side being further examined, it was found that they had formed such strong adhesions, and were otherwise so diseased as to be incapable of expansion; but the lungs of the right side were in a sound and natural state.

A little below the lower margin of the lungs and above the diaphragm, they found a cyst, seemingly formed from the cellular membrane, which contained about 2 oz. of a yellowish green fluid, as thick as the white of egg. Dr. F. asks, whether the humour which caused the beforementioned eruption of pimples might not proceed from this source?

The lower part of the stomach was in a putrid state.

From the appearances observed on dissection it is easy, Dr. F. remarks, to account for all the symptoms with which this child was affected. He concludes with the following reflections:

1. From this case it is evident, that an animal may live, and even enjoy some share of health after a rupture of the diaphragm.

2. Hence there is less reason to be afraid of wounding the diaphragm in performing the paracentesis pectoris, in cases of empyema or dropsy of the chest.

3. That the diaphragm is so injured, (if the wound be large) may perhaps be known from the lengthened figure of the thorax itself, from the less oblique situation of the ribs, and from the concomitant difficulty of respiration.

4. In those disorders of infants where much obscurity prevails, some light may possibly be derived from an accurate examination of the external parts, and therefore such an examination should never be omitted.

5. Whenever tubercles, pustules, or exanthemata occupy any one part of the body in particular, it may be suspected that the source of such eruption lies in some neighbouring part underneath.

On keeping of small Fish in Glass Jars: and on an easy Method of catching Fish. By Mr. Wm. Arderon, F. R. S. N° 478, p. 23.

In the beginning of September 1744, Mr. A. procured a small dace, about an inch in length, which he put into a glass jar, that held near a quart, and kept it till the latter end of May following; in which time it grew full half an inch in length, but very little in breadth. All this time it seldom or never ate any thing, excepting the small animalcula which happened to be in the water given it; which he found necessary to do once every day in winter, and twice or thrice a day in the spring, as the weather grew warmer. When the water was fresh, it would come up to the top about once in an hour, to blow out some small bubbles of air; then, putting its nose near the surface, it would take in a fresh supply; and when it had taken in a sufficient quantity, it would retire to the bottom again. But, as the water became more and more vitiated by its use, its returns to the surface were more frequent, till at last it would remain there continually, till a fresh quantity was given it.

Thus he might probably have kept this fish for years; but a multitude of business one day prevented him from giving it clean water in due time; which unfortunately put a period to the life of his little companion.

When first caught, this silver-coloured fish would not suffer him to come near the glass which included it, without the utmost confusion and surprise; but at last, by gentle usage, and a little art, it grew so tame, that when he came but in sight, it would be sure to come to the same side of the glass, and there lie gazing at him till he was weary of observing of it.

In the same month of September 1744, Mr. A. likewise put a ruff, about 3 inches in length, into another glass, which held about 3 quarts. This fish at first appeared mighty reserved, and would not eat any thing, nor suffer him to come near it, for several days; but, in a very short time, all-powerful hunger assisted him to make it so tame, as is hardly credible. Though the dace found, among the minute animalcula, the little inhabitants of our river-water, enough to subsist on, this ruff found nothing by which it could satisfy the calls of nature; so was obliged to take what he provided for it, and in what particular manner he was pleased to give it.

After this method he brought it to be so tame, that it would not only eat small worms thrown into the glass for it, but would also take them out of his hands, or off a quill, just as he thought proper to give it them: nay it would even rise out above the water for its prey: which is quite contrary to the way this kind of fish takes its food. And at last it would come to his hand whenever he put it into the glass, and suffer him to handle it. But when he had made all the observations he thought necessary, and in pity to its confinement, when he had kept it about 7 or 8 months, he gave it its liberty.

Of all the kinds of fish he viewed the circulation of the blood in, none show it in a finer manner than ruffs, whose fins are exceedingly transparent: besides, it is a fish vastly tenacious of life, and will live 20 or 30 minutes out of water, without receiving much damage.

At a country town about 5 or 6 miles from Norwich, the poor people have a very cheap and expeditious method of catching small fish, such as dace, roach, &c. out of a little rivulet which runs close by.

They procure a bough of white-thorn, which abounds with numbers of thorns; one of which they cut off. To this they tie a piece of thread. Then they take a worm, and slip it on the thorn and thread together: the other end of the thread they fasten to some small twig that hangs over the rivulet. Thus they do by some hundreds at a time; by which means they seldom fail of catching a great many fishes: for no sooner does a fish take the worm into its mouth, and endeavour to be gone, than its mouth is gagged quite open, and it is presently drowned.

On the Effects of a Stick of black Sealing-wax, and a Stick of Brimstone, in Electrical Experiments. By the Rev. Henry Miles of Tooting, D.D., F.R.S. N° 478, p. 27.

Dr. M. procured a stick of the best black sealing-wax, of about an inch in thickness, and of a convenient length; and exciting it with white-brown paper, or clean dry flannel, both equally good, he made the following trials.

He kindled common lamp-spirits, both by attraction and repulsion, the electrified person standing on a cake of bees-wax. He made trial, at the same time, with a glass tube, which kindled the spirits more easily: perhaps from some circumstances hereafter to be mentioned.

He then repeated that experiment of the late ingenious and industrious Dr. Desaguliers, and others; by which it appears, that when any light body is put into a state of repulsion by vitreous electricity, it is in a state of attraction, in respect of resinous electricity, and so è contra. This he found constantly to hold good. He made this trial with a down-feather, and also with a small piece of writing paper, of about the same dimensions as the feather. The feather retained the effluvia, whether of the glass or wax, about 5 or 6 minutes longer than the paper would; that is, the feather remained so much longer in a state of repulsion. The time in which the paper was in a state of repulsion, after many trials, he found to be about 20 minutes.

He made another trial with the wax and tube in a dark room; being led to it from a suspicion, that the effluvia from the wax were grosser, and more in quantity, than those from the glass tube; and on exciting both very quickly, he found the luminous effluvia, when he brought his forefinger near the wax, to proceed in a much greater quantity to the wax from the tip of the finger, than they did on the same trial with the glass tube. And he several times observed a small globular spot of fire to appear first on the finger, from which issued regular streams in form of a comet's tail.

When he used the glass tube, as the quantity was less, so the sparks were finer, less in thickness and in length, but much more active; nor did they proceed so regularly towards the tube, nor make so regular an appearance; frequently breaking in pieces, as if by collision, or not altogether unlike the sparks from a brand in a wood fire, which has lain long without being stirred. The resinous effluvia were also more deeply coloured than the vitreous.

A remarkable Case of a Person cut for the Stone in the New Way, commonly called Lateral. By William Cheselden, Esq. N° 478, p. 33.

Mr. Simpson, aged 75, after having been afflicted with the stone above 5 years, and taking Mrs. Stevens's medicines about a year before for 7 months successively, without receiving any benefit, was cut by Mr. Cheselden, March 13, 1741-2, at which time he had a fit of the stone on him, which had continued for 10 days; and when consequently the bladder and urinary parts were very much inflamed; which, together with his great age, made the success very doubtful. However, at his own earnest request, after consultation, the operation was performed, and a large flattish round stone was extracted, weighing very near 4 oz.

The wound bleeding plentifully from the small vessels, only a piece of thin wet sponge was introduced, that it might bleed for a while through; intending, if there should be occasion, to tie any vessel afterward, that should require it. But, contrary to expectation, this proved the means of stopping the effusion of blood; and from experience, in many cases since, it has been observed that nothing is so useful as this method, thus accidently discovered. About 6 hours after the operation, the patient having lost but little blood, it was thought proper to take 12 oz. of blood from his arm. The urine flowed freely through the wound in about 2 hours after his being cut, and also through the penis; and continued to do so, though with great pain from the heat and sharpness of it; which excoriated the parts about, notwithstanding his being dressed every 2 or 3 hours, and the parts being anointed with a cooling ointment.

On the 4th day, not having had a stool since the operation, an emollient clyster was ordered; which purged him twice, gave him a great deal of ease, and abated some slight feverish symptoms that were observed. This was repeated once in a day or 2 for about 3 weeks, he being naturally costive.

About a week after the operation he complained of a pain in his sides, and had little convulsive motions in his bowels, with faintings, and the abdomen swelled: but, on bleeding him 8 oz. his pain was removed, and the other symptoms went off on taking the confect. raleigh. in aq. pæon. comp. every night for a week.

During this time the wound began to digest, a large slough cast off, and in it came away several very small bits of stone, which had crumbled off in extracting the stone with the forceps. These, by obstructing the free passage of the urine, had given him sometimes great pain, but which was removed by frequently injecting oil and warm barley-water through the penis and wound.

At the beginning they made use of a fomentation made of absinth. roman. et flor. chamæmæl. but it proving too sharp for the excoriated parts, it was left off, and the parts bathed with warm milk, and sometimes barley-water. His diet, the first 3 weeks, was nothing but spoon-meats and light pudding, afterward a boiled chicken, &c. and in a fortnight after the operation, asses-milk night and morning, from which he received great benefit, being very thin, and having a cough, which had afflicted him many years, and being of a hectic disposition.

At the end of 3 weeks, the wound was above half healed, the urine began to come chiefly through the penis, daily lessening through the wound; and when that was near cicatrized, it all came the natural way, and had lost its heat and sharpness: then his mouth blistered within, from the saltness of the saliva. For which, during the whole course of the cure, he drank plentifully of cooling emulsions with gum Arabic, &c. In 5 weeks he was perfectly cured, and so continued without any return of his distemper.

On the Effects of the Lixivium Saponis, taken inwardly by a Man aged 75 *Years, who had the Stone, and in whose Bladder, after his Decease, were found* 214 *Stones. By William Cheselden, Esq.* N° 478, p. 36.

May 21, 1739, Laurence Welch, aged 75, born in Denmark, but at that time a pensioner in the Royal Hospital at Chelsea, after having had symptoms of the stone for 8 years, but never known to void any in all that time, began to take 10 drops of lixiv. sapon. in ¼ pint of water, 3 times a day; and on the 23d in the evening voided several small scales of stone, with little or no pain.

On the 24th the dose was increased to 15 drops; but, making him very sick, it was reduced on the 25th to 10. He complained of great pain in his back this day, and at night passed a whole stone, the size of an orange-kernel, and several little bits.

On the 26th he complained of great soreness in the urethra when he made water; for which a little oil was injected, and he took 8 oz. of a solution of gum Arabic inwardly, omitting this day the lixivium. Being very easy on the 27th, he took the lixivium as before: his urine was thick and plentiful, but had no gravel or sand in it.

On the 28th he took the lixivium, and about 4 in the afternoon voided several bits of small stone, of different shape and texture, some as large as tares; which making the urethra very sore, on the 29th he omitted the lixivium, and took a solution of the gum, and had the oil injected as before. Being searched this afternoon, Mr. C. perceived a great quantity of stone in his bladder. On the 30th he took the lixivium again, was very sick, but easy. The 31st, taking the lixivium after great pain in his back, belly, and penis, he passed a large piece of stone, with several smaller bits.

June 1, he took nothing, was very easy, and made a great quantity of thick turbid urine. He took the lixivium again the next day, was very easy, and continued so all the 3d; but on the 4th was in great pain all day and night. On the

5th, the medicine having sat well on his stomach these 3 days, Mr. C. ventured to give him 15 drops of the lixivium; and, after having great pain in his loins and penis, at night he voided several large bits.

Early in the morning, after very great pain, on the 6th, he voided 3 large bits of stone, and in the afternoon several more pieces came away; one of which was the largest yet voided. He took the solut. gum. Arab. this day, and no lixivium; and at night, being faint and fatigued with pain and want of rest, he had an opiate and cordial julep. On the 7th he likewise took no lixivium, having great pain and sickness at his stomach. His urine to-day appeared a little bloody, and had a tough mucus in it, which sunk to the bottom of the pot, and was very fetid. In the afternoon a large piece of stone stopt at the extremity of the penis; which, after endeavouring for 2 hours to press out with his fingers in vain, was extracted, by enlarging the orifice. This stone weighed only 9½ grs. though near as large as a die; and two which he voided before, nearly as large, and of a triangular form, weighed, the one 8 grs. the other 7¼ grs. This day he took the solut. gum. Arab. and at night had an opiate and cordial julep, to take a little of now and then.

On the 8th he took nothing; and though he had a great deal of pain, and was very weak, had a better appetite than any day since he began the course. He complained of pain the morning on the 9th; so took no medicine; was very easy the rest of the day, and passed some very small scales. On the 10th he took the lixivium, and was pretty well. The 11th had great pain, and was very sick; and also on the 12th: therefore he omitted the lixivium on the 13th; and, being very faint and low, had a cordial julep. On the 14th he took the lixivium, and was very easy every day till the 20th, when he complained his water made him smart; and also the 21st.

The 22d he was very full of pain, and sick; therefore on the 23d he took only the solut. gum. Arab. was easy, and had a great deal of mucus come away with several small scaly bits of stone in his urine; and also on the 24th and 25th. He complained of great pain in the glans penis on the 26th; and his belly swelled, and pitted just above the os pubis; and he voided some small bits in a great quantity of mucus. On the 27th a few little bits were voided, and he had no pain all day. The 28th he had great pain, and made very little water; but on the 29th made a great deal of water. His belly was swelled, and he complained of great pain in his loins.—He voided some small bits of stone on the 30th, which was the last day he took the lixivium.

The 1st of July he took the solut. gum. Arab. and was in great pain. On the 2d, being very faint, and wanting rest, he had a cordial julep and quieting draught; and the 3d and 4th; which day he complained of great pain in his limbs; and on the 5th had several loose stools; which however stopt on the next day.

He continued languishing, and in extreme pain all over his body, even his fingers and toes, from this day to the 24th of July, not being able to bear being touched in any part of his body or limbs, some days before his death, which was on that day, being 64 days since the commencement of this course: in which time he took 81 doses of the lixivium, being one day with another (till he left off entirely, which was 24 days before he died) about 20 drops a day.

During the course, his water was generally thick and muddy; though when in great pain, sometimes clear and high coloured. In about 3 weeks after he began, there was a great deal of that tough fetid mucus in his urine, which continued till he left off taking the lixivium, and then decreased considerably, till a fortnight before his death, when it entirely disappeared, and his urine had no smell or taste, and came away involuntarily.

On straining and filtrating the urine, Mr. C. often found little scaly bits, very small, which, when dry rubbed, became an impalpable powder. After he left off the medicine, Mr. C. could never find this in the urine, though he searched very carefully. His diet the whole time was chiefly spoon-meat; now and then a little meat, but very seldom; and he had but little appetite when he began, and was very weak.

On opening the body Mr. C. found his kidneys very sound, and neither stone nor gravel in them: but in his bladder there were 214 stones; the 2 largest about the size of nutmegs, the others gradually less, to the size of a pea, and smaller, but each whole, not pieces. They were of a soft chalky substance and colour, and weighed all together 6 oz. 2 scr. 4 grs. Avoird; besides which, there were several small scales, such as he found in his urine. The other viscera had nothing remarkable. The quantity of small chalky and scaly bits and powder that he could save out of the urine, was about as much in 24 hours as would well cover a shilling; and perhaps there might be as much more lost in his urine, when at stool, and on the sides of the pot and urinal.

Further Experiments and Observations, tending to illustrate the Nature and Properties of Electricity. By William Watson, Apothecary, F. R. S. N° 478, p. 41.

As water is a non-electric, and of consequence a conductor of electricity, there was reason to believe that ice was endowed with the same properties. On making the experiment, the conjecture was verified; for on electrifying a piece of ice, wherever the ice was touched by a non-electric, it flashed and snapped. A piece of ice also, held in the hand of an electrified man, as in the beforementioned processes, fired warm spirit, chemical vegetable oils, camphor, and gunpowder prepared as before. But here great care must be taken, that by the warmth of the hand, or of the air in the room, the ice does not melt; if so, every drop of water from it considerably diminishes the received electricity. To obviate this, he caused the assistant, while he was electrifying, to be continually wiping the ice dry on a napkin hung to the buttons of his coat; and this being electrified as well as the ice, prevented any loss of the force of the electricity. The experiment will succeed likewise, if, instead of the ice, you electrify the spirit, &c. and bring the ice not electrified near them. But ice is not so ready a conductor of electricity as water.

In the first paper (Trans. N° 477) on this subject, Mr. W. took notice of having observed 2 different appearances of the fire from electrified substances, viz. those large bright flashes, which may be procured from any part of electrified bodies, by bringing a non-electric unexcited near them, and with which we have fired all the inflammable substances mentioned in the course of these observations; and those, like the firing of wet gunpowder, which are only perceptible at the points or edges of excited non-electrics. These last also appear different in colour and form, according to the substances from which they proceed: for, from polished bodies, as the point of a sword, a silver probe, the points of scissars, and the edges of the steel bar made magnetical by the ingenious Dr. Knight, the electrical fire appears like a pencil of rays, agreeing in colour with the fire from Boyle's phosphorus; but from unpolished bodies, as the end of a poker, a rusty nail, or such-like, the rays are much more red. These pencils of rays issue successively, as long as the bodies, from which they proceed, are exciting; but they are longer and more brilliant, if any non-electric not excited be brought near them, though it must not be close enough to make them snap. By holding a hand at about 2 or 3 inches distance from these points, you not only feel successive blasts of wind from them, but hear also a crackling noise. Where there are several points, you observe at the same time several pencils of rays.

It appears from experiments, that besides the several properties that electricity is possessed of peculiar to itself, it has some in common with magnetism and light.

Prop. 1.—In common with magnetism, electricity counteracts, and in light substances overcomes the force of gravity. Like that extraordinary power likewise, it exerts its force in vacuo as powerfully as in open air, and this force is extended to a considerable distance through various substances of different textures and densities.

Corol.—Gravity is the general endeavour and tendency of bodies towards the centre of the earth: this is overcome by the magnet, with regard to iron, and by electricity, with regard to light substances, both in its attraction and repulsion. But he has never been able to discern that vortical motion, by which this effect was said to be brought about by the late Dr. Desaguliers and others, having no, other conception of its manner of acting than as rays from a centre, which indeed is confirmed by several experiments.

Prop. 2.—In common with light, electricity pervades glass, but suffers no refraction from it; he having, from the most exact observations, found its direction to be in right lines, and that through glasses of different forms, included one within the other, and large spaces left between each glass.

Corol.—This rectilineal direction is observable only as far as the electricity can penetrate through unexcited originally-electrics, and those perfectly dry; nor is it at all material, whether these substances are transparent, as glass; semidiaphanous, as porcelain, or thin cakes of white wax; or quite opaque, as thick woollen cloth, as well as woven silk of various colours; it is only necessary that they be originally-electrics. But the case is widely different with regard to non-electrics; wherein the direction, given to the electricity by the excited originally-electric, is altered as soon as it touches the surface of a non-electric, and is propagated with a degree of swiftness, scarcely to be measured, in all possible directions, to impregnate the whole non-electric mass in contact with it, or nearly so, however different in itself; and which must of necessity be terminated by an originally-electric, before the electricity exerts the least attraction; and then this power is observed first at that part of the non-electric the most remote from the originally-electric. Thus, for example, by an excited tube held over it, leaf-gold will be attracted through glass, cloth, &c. held horizontally in the hand of a man standing on the floor; and this attraction is exerted to a considerable distance. On the contrary, the rubbed tube will not attract leaf-gold, or other light bodies, however near, through silver, tin, the thinnest board, paper, or any other non-electric, held in the manner beforementioned.

Prop. 3.—Electricity, in common with light likewise, when its forces are collected, and a proper direction given to it, on a proper object, produces fire and flame.

Corol.—The fire of electricity, as before observed, is extremely delicate; and sets on fire, as far as yet experienced, only inflammable vapours. Nor is this flame at all heightened, by being superinduced on an iron rod, red-hot with coarser culinary fire, as in a preceding experiment; nor diminished by being directed on cold water. However, being desirous of knowing if this flame would be affected by a still greater degree of cold, Mr. W. made an artificial cold; by which the mercury, in a very nice thermometer adjusted to Fahrenheit's scale, was depressed in about 4 minutes, from 15 degrees above the freezing point to 30 degrees below it; that is, the mercury fell 45 degrees. From this cold mixture, when electrified, the flashes were as powerful, and the stroke as smart, as from the red-hot iron.

A Description of a curious Sea-plant; Frutex Marinus Flabelliformis Cortice Verrucoso obductus. Doodii. Raii Hist. Tom. iii. p. 7, et Synops. Edit. 3, p. 32. Coralloides granulosa alba. J. B. Tom. iii. p. 809. Erica Marina alba frutescens. Mus. Pet. 30. Keratophyton Flabelliforme, Cortice Verrucoso obductum. Raii Syn. Edit. 3, p. 32. By Sir Hans Sloane, Bart., M.D. late P.R.S.* N° 478, p. 51.

King Charles II. had got, in his closet at Whitehall, this coralline, as Sir Hans calls it; which he supposes had been presented to him by some of his sea-officers, appointed to cruise in the soundings, lying off the west of England, towards the Atlantic ocean. Sir Hans had it from thence entire, and in perfection, from some of the late commanders on that station (of which fig. 4, pl. 4, represents an entire figure when young), who, by their sounding lines, brought it up from the rocks at the bottom of the sea; and which being a very curious coralline, it is extraordinary it has been so little taken notice of.

It rises to 4 feet high, from a woody basis, near an inch diameter, giving it a firm foundation on the rocks in the bottom of the sea, spreading out its branches like a fan, the substance or inner part of which is woody, of a light brown, or blackish colour, as at a, b, covered all over with a thin tuberculated crust, of an ash-colour, or sometimes yellowish, seldom joined together, as the rete marinum, but loose, and distorted; and not straight, as most of this kind.

Sir Hans has had it from Tangier, Antigua, and Newfoundland; from which last place, one with the stella arborescens Rondeletii, p. 121, (mentioned by Mr. Winthrop, in these Transactions, N° 57,) having its branches fastened several times round those of this coralline; a branch of which is here figured, with the animal sticking to it, at fig. 5, in which a is the mouth, and fig. 6 represents the back part of it, having a crack in it by some accident.

John Bauhin is the first author who describes it plainly, both by words, and an imperfect figure of a small piece or branch.

It is likely, that many of the coralline substances mentioned by authors, may be this, or parts of it, the crust being rubbed off more or less, and its colour changed, and so described for different corallines.

Several Electrical Experiments. By the Rev. Henry Miles, D.D. and F.R.S. N° 478, p. 53.

The stick of Brimstone with which Dr. M. kindled lamp spirits so readily, having been set up in a cupboard in an erect position, lost all its electric virtue, and could not be made to attract a down-feather, or a fine thread. This was to him unaccountable, unless it be that the exposing it to the air, by its not being

* The zoophyte here described, is the *Gorgonia verrucosa*, Lin. Gmel.

wrapt up in any thing, may have deprived it of its power. The cupboard is small, and never cold. Whereas his stick of wax kept in his desk, not wrapt, will attract a thread at any time, without rubbing at all.

Wanting to try if he could not kindle spirits of wine with an icicle; but not being able to get one, he attempted it with a thick piece of ice, and immediately succeeded, in the presence of 7 or 8 persons; and the sparks of fire from the ice, when the finger of a non-electric person was brought near it, were as large and as powerful as any he ever saw; so that their power is no ways diminished by the coldness of the ice.

By accident one of the gentlemen approaching the electrified person with his hand, near his shoulder, the gentleman felt a very pungent stroke on his flesh, through his coat and waistcoat, which were both cloth. This was repeated several times, and in every one's opinion, on whom trial was made, the repulsive stroke was as smart as it is usual on the end of the finger, when nothing intervenes; and the sensation continued as long.

Dr. M. took a clamp of iron, such as is used for heating box-irons for smoothing linen clothes; and having heated it red-hot, applied it to the spirits of wine, as he stood on the cake of wax electrified, holding it in a pair of tongs. But he could not kindle the spirits during the time the redness continued in the clamp; but as soon as that disappeared, and it began to look blackish, the spirits were kindled as usual.

On the Light caused by Quicksilver shaken in a Glass Tube, proceeding from Electricity. By Mr. Trembley, F.R.S. Dated the Hague, 4th Feb. 1745. N.S. N° 478, p. 58.

Mr. l'Allamand inclosed some mercury in a tube close-stopped; and, when he afterwards rubbed this tube, it gave a great deal more light, than when the same had no mercury in it. When this tube has been rubbed, after raising successively its extremities, that the mercury might flow from one end to the other, a light is seen serpenting all along the tube; that is, the mercury, as it runs along, is all luminous.

Mr. l'Allamand then made the mercury run in the same manner along the tube without rubbing it, and it still gave some light, but much less than before. This last experiment persuaded him, that the friction of the mercury against the glass might electrify that glass, like the rubbing of the hand. And he has been confirmed in the same notion by another experiment: he brought some down near to the tube, and then made the mercury run from one end to the other; and the down was attracted, as the mercury in its motion passed by it.

These experiments he has repeated, and varied several ways; and they have led him to conclude, that the phosphorus of the barometer, known a great while, is not so properly a phosphorus, as the effect of the mercury electrifying the tube of the barometer.

Mr. l'Allamand has put mercury into exhausted tubes, and when these are rubbed, they give much more light than before; there then came out from them on all sides rays of very lively light. Mr. T. has also seen at Leyden, at Mr. Muschenbroeck's, an exhausted globe of glass, which, when rubbed with the hand, seemed all filled with a very bright fire.

An experiment that Mr. l'Allamand has tried, is this: he electrified a tin tube, by means of a glass globe; he then took in his left hand a glass-full of water, in which was dipped the end of a wire; the other end of this wire touched the electrified tin tube: he then touched, with a finger of his right hand, the electrified tube, and drew a spark from it, when at the same instant he felt a most violent shock all over his body. The pain has not been always equally sharp, but he says, that the first time he lost the use of his breath for some moments; and he then felt so intense a pain all along his right arm, that he at first apprehended ill consequences from it; though it soon after went off without inconvenience.

It is to be remarked, that in this experiment he stood simply on the floor, and not on the cakes of resin. It does not succeed with all glasses; and though he has tried several, he has had perfect success with none but those of Bohemia. He has tried English glasses without any effect. That glass with which it best succeeded was a beer-glass.

Mr. Muschenbroeck has repeated his experiment, holding in his hand a hollow bowl exceedingly thin, full of water; and he says he experienced a most terrible pain. He says, the glass must not be at all wet on the outside.

On the Manner of the Seeding of Mosses. By Mr. John Hill, Apothecary.* N° 478, p. 60.

The particular species of moss here described, is the hypnum terrestre, tri-

* John Hill, an ingenious English writer, and a celebrated botanist, was born in 1716, and was bred to the business of an apothecary, which he carried on for some time in St. Martin's Lane. His first publication was a translation of Theophrastus's Tract on Gems, which procured him great reputation. This induced him to undertake a general Natural History in 3 vols. folio. He then became a kind of general writer. He published a Supplement to Chambers's Cyclopedia, and other works. Soon afterwards he obtained the degree of M.D.; and being patronised by the Earl of Bute, continued his botanical labours under the title of the Vegetable System, in 26 thin vols. folio. The king of Sweden conferred on him the order of the Polar Star. Being offended at the treatment he met with in the R.S., of which, on application, he was not admitted a member, he took his revenge by publishing what he termed a Review of the Royal Society, wherein he studiously holds up to ridicule some of the more trifling papers in the early Vols. of the Phil. Trans. He possessed quick and lively parts, with a considerable degree of taste for natural history, and particularly for botany; but his pursuits were so varied, that he did not always allow himself the time necessary for the composition of works of science; hence some of his writings in natural history do not possess all the accuracy that could be desired.

choides, luteovirens, vulgare, majus, capitulis erectis: Raii Synops. ed. 3, p. 48. Hypnum vulgare, sericeum, recurvum, capsulis erectis cuspidatis, Dill. Hist. Musc. 323. It is frequent on old walls, and there is a specimen of it in the 3d vol. of the Hortus siccus of English plants of my collecting.

The head of this moss appears to the naked eye, as at fig. 7, of a pale-brown colour, and smooth surface, and is in part covered with a membranaceous calyptra, resembling in shape an extinguisher, or a funnel inverted. When this calyptra is taken off, and the head placed before the microscope, the surface is seen to be ridged with longitudinal striæ, the basis of the head of a dark orange-colour, and more opaque than the rest; and the top is bounded by an orange-coloured ring, swelling out something beyond the surface of the contiguous parts of the head. A close observation and good glasses have showed, that in this little head there are not wanting the parts essential to the fructification of what are commonly called the more perfect plants.

This ring is truly a monophyllous undulated calyx; and within it arise 16 pyramidal fimbriated stamina: these are of a pale-greenish colour, and are loaded with a white oval farina: the stamina all bend toward each other from their bases, and almost meet in a point at their tops. This is their appearance when the head is nearly ripe, as expressed at fig. 8. And immediately under the arch, formed by these stamina, is placed a slender, cylindrical, hollow pistillum, through which the farina makes its way, and is dispersed among the seeds in the head. The external membrane of the head is a continuation of the outer covering of the stalk, and is strengthened at its basis by 4 or 5 ribs, which soon lose themselves in the striæ.

A longitudinal section of the head shows, that the membrane before mentioned incloses a seed-vessel so large as to fill it every way: in most places they touch; but wherever they do not, a number of very slender, white, and transparent fibres show themselves, which join them together. This seed-vessel is filled with perfect and very beautiful seeds; they are round, transparent while unripe, but afterwards opaque, and of a very beautiful green; which colour they retain even when dried.

The number of seeds in one of these heads is astonishingly great: he often attempted to count them, in such as were full, and out of which few or none had been dislodged by the cutting; and found the number amounted to no less than 13824. Fig. 9 shows a longitudinal section of the head with the seeds, the stamina, and the joining of the capsule with the external membrane of the head.

The stamina, examined alone, afford a most pleasing sight; they are composed of a white transparent substance, of a pyramidal figure, every where covered with a pale-greenish crust; which is the receptacle of a vast quantity of an oval farina, so extremely minute, as to be visible only with the most powerful magnifiers in the double microscope.

The outer membrane of the head becomes separable from the capsule when perfectly ripe and dry; and then, viewed in the double microscope, shows a reticular texture, not visible in it before. When this head is first produced from the plant, the stamina are very slender, and stand erect; the head is scarcely any thicker than the stalk, and the calyptra covers the whole, to shield the tender substance of the farina from external injuries. As the farina afterwards swells in the stamina, the seeds also in the head increase in bulk, and become visible, and are then transparent; but when it is perfectly ripe, the calyptra falls off, and the wind dislodging the farina at times, as it ripens some sooner, some later, it makes its way through the pistillum into the head, and the seeds then become much larger and opaque; to favour the falling of the farina into the pistillum, the stamina, as they ripen, are, by the increase of thickness in the head, thrown farther and farther from each other at their bases, but bend inward at the points, so as to form a kind of arch over the opening of it.

The annual product of these most minute seeds is astonishing. An ingenious gentleman has given, in N° 468 of these Transactions, an account of the wonderful increase of the mallow; one of which he found to yield, in one year, 200,000. But this is much inferior to those of the little plant before us; for, allowing to a root of this 8 branches, and to each branch 6 heads, the produce of this is $6 \times 13824 = 82944$, and $8 \times 82944 = 663552$ seeds, the annual produce of one seed; 13824 of which are contained in a head, whose length is but $\frac{1}{9}$ of an inch; and its diameter but $\frac{1}{27}$ of an inch, and whose weight is but the 13th part of a grain.*

In pl. 4, fig. 7 shows the head of this moss in its natural dimensions, with and without the calyptra. Fig. 8, the same viewed through a powerful magnifier, without its calyptra. Fig. 9, a longitudinal section of the same. Fig. 10, stamina taken off from the head, and viewed by a more powerful magnifier. Fig. 11, a piece of the outer membrane of the head, showing its reticular texture.

Concerning the minute Eels in Paste being Viviparous. By Mr. James Sherwood, Surgeon. N° 478, p. 67.

Examining one day a number of these eels, and viewing a single one, Mr. S. found he had wounded it in the belly; a long slender tube proceeded from the wound, doubled in the form of an intestine, which he then took it for. Next day he communicated this to Mr. Turberville Needham; and having a mind to

* Dr. H.'s observation is correct, so far as relates to the seed of this moss; but if Hedwig's theory of the fructification of the mosses be just, it follows that the organs supposed by Hill to be stamina, are of a very different nature, and form what Hedwig calls the peristoma.

see the viscera, as they then thought, he cut one in two, near the middle; when, to their great surprise, they found this part had shot out from each of the divided ends, and a number of seeming ova issued from them; but they soon found that these were really live eels, included in their proper membranes, though of different degrees of maturity. Some moved but slowly their head and tail, others coiled and uncoiled themselves pretty briskly; and indeed it was pleasant to behold the most mature making many efforts to disengage themselves from their enveloping membranes, frequently varying their position, being sometimes spiral, then like a figure of 8, till at last they were entirely at liberty, and swam about like the parent eel. By this it appears, that what he at first took for the intestine, was now found to be the uterus, which shows in the large eels to be full of dark spots, and are the young eels. These dark spots are also observable in the young ones, as soon as delivered from their parent.

To be sure of this experiment, he repeated the operation on numbers of these eels in the presence of Dr. Parsons, and several other gentlemen, and always found young ones; some so mature, viz. (those towards the middle of the parent) as to disengage themselves before he could get them under the microscope. In cutting one of these eels, he happened to divide a young one exactly in two; but notwithstanding the globular appearances, as in the parent, there issued nothing from the wounded extremity of either part; which must be owing to the immaturity of the young ones.

From this it is plain, that these eels are viviparous; and consequently cannot favour the common opinion among naturalists, that all kinds of animalcules are produced by minute eggs floating in the air, and falling into the different matrices and pabula, that sustain each kind.

Some Observations on the Cancer Major. By Mr. Peter Collinson, F. R. S. N° 478, p. 70.

The cancer major, or largest species of crabs have their chief abode from 20 to 40 fathom water, they herd together in distinct tribes, and have their separate haunts for feeding and breeding, and will not associate with their neighbours. This has been carefully tried, by taking a crab, and marking its shell, and carrying it 2 or 3 miles distance, and leaving it among the same species: this crab has found its way back to its old home, and has been caught by the same fishermen that carried it.

The smallest crab that comes to hand is about the size of a chestnut; the full grown 7 lb. weight; but there has been one caught that weighed 12 lb. The bait is flesh, or pieces of skait, or small shark, of which he eats but little. The fishermen all agree, the crab will live confined in the pot or basket some months, without any food but what is collected from the sea-water, and not decrease in weight. The difference of sex is very conspicuous, and they are very prolific; but he could procure no certain account of their way of coupling, nor in what time they attained to full growth.

Once a year, like the lobster, they lose or cast their shells. Against this extraordinary change, they chuse a close and well-secured retreat in the cavities of rocks, and under great stones: there they creep in, and wait, till by degrees the parts are disengaged; which is effected by withdrawing their legs from their old shells, leaving them, and the upper part of their body-shell behind. In this naked state they make a very odd appearance, being an ill shapen lump of jelly-like substance, which gradually hardens into a shell a size larger than the old one; for this is the way of growth appointed for this animal, and others of the crustaceous species. But what is most surprising, this large species of crab has a power in itself voluntarily to crack and break its own legs or claws, and drop them off. The reproduction of the legs of craw-fish has been mentioned in the History of the Royal Academy of Sciences, with some just remarks on the growth of these creatures' shells; but he knows not of any writer that has taken notice of this strange event of the crab.

Mr. Benjamin Cook, at Newport in the Isle of Wight, F.R.S. informed Mr. C. of this marvellous property in the great crab; but he could not comprehend it, till he saw the experiment tried on two crabs; then he was soon convinced of the truth of the fact; for in a few minutes the legs all dropped off one after another.

This the crab will do in any position; but the easiest method is to lay it on its back, and then take a pair of strong iron pincers, and break the shell, and bruise the flesh of the 3d or 4th joint of its small leg: after it has received the hurt, it bleeds, and gives signs of pain, by moving its leg from side to side; but afterwards holds it quite still, in a direct and natural position, without touching any part of its body, or its other legs, with it. Then, on a sudden, with a gentle crack, the wounded part of the leg drops off at the second joint, or internodium, from its body; just as one sees the neck of a retort separate, where it has been heated by a red-hot iron ring, on the application of cold water. The great legs are cast off in the same manner, but are not so easily laid hold on as the small ones. Those who have not seen this wonderful operation may reasonably conclude, that the leg is cast out of its joint or socket; but it is quite otherwise; for it cracks and breaks off in the smoothest part of the joint, and the rim of the body-shell is no ways assistant to it.

To try what effect increase of pain would have in this work, a small hole was pierced in the great legs, and then a pointed iron was put in to lacerate the inclosed muscle: the consequence was answerable to expectation; symptoms of greater pain ensued, and the leg was cast off with greater violence.

It is really amazing and inconceivable, by what power or contrivance in itself, so wonderful an operation can be performed by the crab, as voluntarily to crack and break so hard a shell, and its muscles, and then cast off its legs. The small diameter of this joint, the disposition of the fibres, and a very small circular fossa, may contribute greatly to accelerate the work; but yet the main spring of action seems beyond the reach of human comprehension. The whole performance is so curious, and so singular a fact in the history of nature, that it may well deserve a nicer consideration, by those that have greater abilities, and more leisure, for such inquiries.

When the leg is dropped off, a mucus or jelly is discharged on the remaining part of the joint next the body, which, as a natural styptic, instantly stops the bleeding, and gradually hardens and grows callous, and forms into a leg in miniature, which by degrees shoots forth, and attains to its natural size, to supply the place of that which was lost.

An experiment was next tried, to see of what great service the mucus or jelly was to the crab. When its legs were all cast off, the ends of 2 or 3 of the stumps were pierced with a pointed iron, so as to break off the jelly that stopped them; on which signs of more intense pain were exhibited, a very large flux of blood ensued, and the creature soon died in great agonies, as was manifest by a tremor of those parts about the mouth, and a frothing like that which attends epileptic fits.

The crabs are naturally very quarrelsome, and with their great legs or claws fight and kill each other: with them they catch hold of their adversary's legs, and whatever they seize, they strongly retain for a long while: there is no escaping their cruel foe, but by voluntarily leaving a part of the leg behind, in token of victory; but the principal end for which this is done, is the saving the life of the conquered; for when they are bitten and bruised, and cannot break off that limb, they soon bleed to death.

The fishermen showed an experiment, to give some idea of the tenacious disposition of this creature, by obliging a crab with its great claw to lay hold of a small one: the silly creature did not distinguish that itself was the aggressor; but exerted its strength, and soon cracked the shell of its own small leg, and it bled freely; but feeling itself wounded, to save its life required a power peculiar to itself to break off that limb in the usual place; which it presently effected, and held fast for a long time the broken part in his great claw: which evidently shows, that this creature retains whatever it lays hold on, and when overcome by its enemy, ransoms its life at the expence of a limb.

A Remarkable Instance of the Happy Effect of Musk, in a very Dangerous Case. By James Parsons, M.D., F.R.S. N° 478, p. 75.

Mr. Darlington, a man of a robust habit of body, was taken ill about the beginning of December last of a rheumatic fever, attended with the loss of the use of his limbs, excessive pain in every part, and swellings in his knees and hands, with all the other symptoms usual in this kind of fever. He also coughed up grumous blood, and had a violent pain in his right side, from a fall against the edge of a table, a few days before he was seized with this fever, which rendered his case the more dangerous.

During the first 10 days he was attended by his apothecary only; who, finding him grow worse, proposed a physician, who was accordingly called in, and ordered a bleeding, with such other remedies as he thought necessary: this, it seems, was the 2d time he had been let blood. But the patient by this time was so averse to taking medicines, that he did not duly follow the method ordered by this gentleman, and at length absolutely refused to take any more, and so discharged the doctor on the 4th day of his attendance.

On the 15th day of his illness he sent for Dr. P., having been formerly concerned for some of his family. His symptoms had increased, and his disease was gaining ground apace. He had him let blood immediately, which was one-third part size, and ordered it to be repeated, and put him under such a regimen as the state of his case then required; but as soon as he was gone, he refused to comply with his prescriptions also. Dr. P. expressed his concern for his obstinate temper, and left him on his 4th visit.

Thus were 18 days passed, and his case becoming more desperate every day. Dr. P. heard no more of him till 5 days after, which was on the 23d day of his disease, when he was intreated to visit him again, and found him most miserably afflicted with 2 of the most dangerous symptoms that can appear at the end of such a dangerous distemper; viz. a long intermission of his pulse every 3d or 4th stroke, and a most fatiguing hickup, which struck him violently about 10 times in a minute.

His case was now deplorable and desperate, through his own folly; and he was then very weak and delirious. However, willing to assist him, and calling to mind the accounts of the musk some time since communicated to the Royal Society,* he was resolved to have recourse to it here. Accordingly he directed a draught, consisting of 1 oz. ½ of strong cinnamon water, 2 drs. of compound piony water, and 15 grs. of musk, with orders to increase the dose of musk to 20 grs., and repeat the draught every 6 hours. He also told his wife to give him a glass of sack, as often as he would have it; and at the same time caused 3

* See Phil. Trans., N° 474.—Orig.

blistering plasters to be laid on his neck and arms, which, among other things, he had refused before.

In 4 hours after the first draught the intermitting pulse was altered to a very calm regular one; but the hickup continued with the same violence, till he had taken the 5th draught; and then returned only once in 6 or 7 minutes. His senses were now restored, and he grew chearful and easy, and said he would take no more of any kind whatever: but being unwilling to cease the exhibition of a medicine which bid so fair for his recovery, Dr. P. gave him a glass of sack, into which the 6th draught was privately poured, which took away his hickup entirely.

The next day he had an appetite to eat, and was indulged by his wife with a large chicken, a great deal of bread, and a pint of beer and ale, which he ate greedily: this overcharged his stomach, and brought on his hickup again, which fatigued him much, before Dr. P. visited him in the afternoon. He then directed a purging draught immediately, which emptied him well, and conquered his hickup, and every other bad symptom.

Next day he found him well, limited his diet for a few days, with directions to repeat his purge once more, after 3 days; and in a fortnight he went abroad.

The blisters might perhaps conduce, in some measure, to do him service; but as the man was so many days ill, and reduced to a condition very little, if at all, better than that of a dying man, he believes the musk, rather than the blisters, was the medicine that restored him: for he had often seen the latter applied in a greater number in vain, even when the symptoms were not so seemingly desperate as in this case.

He took near 105 grains of musk in about 30 hours; but he cannot say he either slept or perspired more than ordinary on it.

Concerning Electrical Fire. By the Rev. Dr. Miles, F.R.S. Nº 478, p. 78.

Mr. Henry Baker having queried, whether that subtil fire which kindles warmed spirit of wine, be resident in the body from which it evidently issues, and be kindled occasionally? or whether it comes from the excited tube pervading instantaneously the body it is applied to? or, lastly, whether there are certain principles in the air, which are thus agitated into an extemporaneous lightning? Dr. Miles here inclines to think the electrical and luminous effluvia to be the same, and not distinct substances. Mr. Hauksbee seems to distinguish them, intimating, that no luminous matter would be communicated from an excited cylinder of wax to his finger, when brought near to the cylinder, though it attracted light bodies; but it is to be observed, that this cylinder of wax was only a coat of wax, of about half an inch thick, on a wooden cylinder of 4 inches diameter: but Dr. M. always found his stick of wax, which consists of nothing else, to emit luminous effluvia very plentifully, and rather in a greater degree than the glass tube.

If we conclude with the English philosophers, that fire is mechanically producible from other bodies, by collision, attrition, &c. or, according to Sir Isaac Newton, by putting the sulphureous particles of bodies into a very strong vibratory motion; by which means they become hot and lucid, i. e. affect us with ideas of light and heat; on this supposition may we not conclude, that the action on the glass tube, when it is rubbed, by putting the parts of it into such a vibration, and consequently agitating violently the sulphureous particles in it, may heat and kindle them? and may it not also be supposed, that when the air is in a due state, nitrous or other particles in the air may contribute to the kindling them? or perhaps, rather that subtil, active, elastic substance, which Sir Isaac Newton supposes to be the cause of the refraction, &c. of light, and which communicates heat to bodies, and is universally diffused; these effluvia being thus agitated and conveyed by a non-electric body intervening, in a due quantity, to the vapour of the warmed spirit, may be supposed to kindle them, without exciting any originally resident fire in the body immediately communicating with them; the luminous effluvia from the finger, or ice, &c. when brought near the inflammable body, being, as far as we can perceive, of the very same kind with those which proceed from the tube; or there is nothing appearing in them which may lead us to suspect they are not the very same, though in a greater quantity than what can come from the part of the tube approached with the end of our finger.

If we conclude, with some of the foreign philosophers, Boerhaave, Homberg, Lemery, Gravesande, &c. that fire is equally diffused throughout the universe by the Creator, pervading the interstices of all bodies, and that there is no fire mechanically produced de novo; then, may we not conclude, that whereas, by attrition of the glass tube, there is produced a very quick and strong vibration of its parts, which must necessarily affect the fire contained in its vacuities, by compression and relaxation; so that, as Boerhaave expresses it, there must be, in the bodies thus agitated, and in the fire contained in its pores, an exceedingly great motion excited, and the surrounding fire, from both these causes, must be agitated, and so much the more violently the nearer it is; may we not conclude, that its force will be hereby sufficiently increased to kindle the spirit to which it is conveyed?

In this, as in the former hypothesis, Dr. M. would not exclude the elastic materia subtilis from being supposed to have an influence on the effluvia. Which ever of the two hypotheses we embrace, Dr. M. inclines to think, that the kindling fire rather proceeds from the excited tube.

Tooting, Feb. 15, 1745-6.

7

An Account of a Book intitled, De quamplurimis Phosphoris nunc primum detectis Commentarius. Auctore Jac. Barthol. Beccario, 4to. Bolognia, 1744. Extracted and translated from the Latin by W. Watson, F.R.S. N° 478, p. 81.

The ingenious author, in the work before us, does not treat expressly on those productions of the chemical art, which we usually call phosphori; but principally of such substances, whether natural or artificial, as imbibe the rays of light in such quantities, and in such a manner, as to appear luminous for a time, even in absolute darkness.

The author divides the phosphori into several kinds, some of which shine of themselves naturally, as the glow-worm and dates, or adventitiously, as the flesh of animals which most probably arises from a degree of putrefaction, sometimes too slight to be obvious to our senses. Other bodies become luminous by attrition, heat, the free access of air, and, lastly, by imbibing and retaining the rays of light. Those bodies that are luminous by attrition are, among others, some diamonds, and the hairs of animals; by heat, several sorts of gems, and mountain crystals; from the free access of air, the phosphori of Kraft and Homberg; from the aspect of light, the Bolognian luminous stone, the preparation by Christian Adolphus Baldwin of chalk dissolved in spirit of nitre, as well as several others discovered by the late Mons. Du Fay, who found, that whatever substances would, by calcination, be converted into a calx, or whose concrete, from a solution in the acid of nitre, would bear fire enough to become red-hot, these bodies were adapted to imbibe and retain light.

The greatest number of phosphori are of the last mentioned kind, and these are principally the subjects of this treatise. Some of these are natural, others artificial; but of these last, the preparation is so slight, as not to change the nature of their constituent parts. The natural phosphori are either fossil, vegetable, or animal. The fossil are, though very different in degree, some sorts of earths, white sand, lime-stones, stalactites, and several other figured stones, iceland crystals, flints, some species of agates, white arsenic; but no sort of metals, metallic or sulphureous bodies, as jet, amber; except the beforementioned arsenic. On the other hand, salts imbibe light, provided they are divested of every metallic principle; otherwise not, though pellucid as possible. For this reason, none of the vitriols will imbibe light; but other salts will, though with a considerable difference as to quantity; for sal gem. and rock-salt imbibe very little, sea-salt, if dry, and in crystals, much more; and in like manner sal ammoniac; more yet, sal catharticum and nitre; weak in the natron of the ancients, and alum; but brightest of all in borax.

In the vegetable kingdom we find very few phosphori; that of dry rotten wood is weak, and not lasting; it appears chiefly on the edges and inequalities of the surface. But this is most remarkable in the rotten wood of the fir-tree, and some others, where in the dark you see shining spots as large as tares; whereas in full light the whole surface appears alike. Some few barks are luminous, but not considerably so; but no fruits, seeds, or their meals. Cotton appears very bright, and the crystals of tartar; but fine loaf sugar appears the most luminous of all, both without and within. Gums and resins retain no light.

There is a vast variety of phosphori in the animal kingdom, such as the bones and teeth; to these may be added the shells of fish, egg-shells, the human calculus, bezoar, and in whatever parts of animals the terrestrial principle is very predominant. But where there is a considerable quantity of oily matter, as in the hoofs, horns, and feathers, no light is manifest.

The author, having gone through the natural phosphori, proposes some queries concerning them; of which the first is, in what and how great a light the object ought to be placed? He tried different phosphori in different degrees of light, and found them imbibe most light from the sun itself, next in quantity, when the sky was clear, and the least in foggy weather. These experiments should be made in the open air, and not in a house with the glass windows shut; because many bodies appear luminous, when the light has come directly to them, which will not have that appearance when the light has passed through the glass. He lastly tried what light they would imbibe from very bright flame, and found that alabaster itself, which is saturated more than any substance by the sun's rays, imbibed exceedingly little. The next query is, how long these bodies should remain in the light to be sufficiently saturated; 4 or 5 seconds were found the utmost length of time required for that purpose. The other query is, how long the received light will continue in these phosphori? it does not last the same time in all; but continues more or less, from 2 seconds to 8, in proportion to the strength of the phosphorus, and the quantity of light received.

We pass now to the phosphori which are produced by art: and first to those which are made by the maceration of plants alone, and without any fire; such as thread, linen cloth, but especially paper. The luminous appearance of this last is greatly increased by heat. This is confirmed by two experiments: the first is, by exposing the paper, spread on an iron grate, to the naked fire, yet not near enough to scorch it, and then laying a warm brick on it to retain the heat; by which means it was observed, that where the paper was not skreened by the iron grate it was most luminous; so that, by the lights and shades, might be distinguished in the dark the image of the iron grate a considerable time. The other experiment is the application of the paper to a plate of warm brass; from which, when in the dark, you might very easily, by its being less luminous, distinguish the margin of the paper, that had not been warmed by the brass.

The author proceeds to take notice of those phosphori which become so by the assistance of fire. But the fire here spoken of is not great enough to dissolve their constituent parts, but only such as may affect the external parts of their texture, and that but gently; so that the process here mentioned is only drying or roasting. For it is not the watery or the saline part in bodies which is torrefied, but the oleaginous, with which many vegetables, and most animals, abound.

The white flesh of animals, such as that of chickens, becomes a phosphorus by roasting, as well as the tendons, and whatever parts of animals become glutinous by boiling, such as carpenter's glue, ising-glass, to these may be added cheese. Bones, though they imbibe light without any preparation, have that property in a much greater degree when burnt, and their luminous appearance is much more lively. But roasting has not this effect on feathers, hoofs, horns, or whites of eggs. The same operation which produces several phosphori from the animal kingdom, gives also several from the vegetable. Thus, by gently toasting gums, as myrrh, gum tragacanth, and others, appear luminous, though different in degrees: and this light is clear, in proportion to the gentle evaporation of their aqueous parts. By this treatment nuts of every kind, pulse, corn, coffee-berries, meal, bread, and wafers, also become phosphori. Turpentine, amber, and some resins, require more fire before they imbibe light; so that you must divest them of their acid, and their light ethereal oil, to make them appear luminous. But here great care must be taken that they boil no longer than from being white they turn yellow; for by proceeding longer, your labour is lost.

It is necessary to be acquainted, that those phosphori which are produced by torrefaction, soon lose their power, which perhaps neither time, nor a thorough dissolution of their parts, can deprive the natural ones of. In general, as long as the phosphori, gained by torrefaction, preserve their power, their light is more sharp and striking, but the natural more weak. But those that are gained by calcination, and Baldwin's Phosphorus, seem to possess both the striking light of those gained by torrefaction, and the weaker light of the natural phosphori: the last they preserve a long time, but the former is lost by degrees much sooner. The well calcined ashes of plants, or rather their terrestrial parts, remaining after the solution of their fixed salts by washing, and neutral salts, continue phosphori after many years. So that, as far as we can judge, the luminating power which is gained by calcination, though not so intense, continues perpetual; whereas that gained by torrefaction always decreases, and in a very little time is no longer visible. Some even, by this method, continue to imbibe light much longer than others. Gum Arabic, which continues longest, lasts 6 days; bread, not one; and coffee, only a few minutes. However, at any time, by a fresh torrefaction, you may recover these languid phosphori; in which property they greatly resemble the Bolognian stone, and other phosphori prepared by art. The phosphori gained by torrefaction, as well as that of Bologna, will not imbibe light, while they are warm; and this last does not appear so luminous when first prepared, as when it has been so some time.

The natural phosphori do not differ only in the beforementioned particulars, but also in the colour of the light itself. The light of the natural generally appears either perfectly bright, or somewhat inclining to yellow; the artificial produces a red, and sometimes a brown light; but there are some exceptions to both these rules. From these different appearances, the author conjectures, that there are two sorts of fire arising from different principles; viz. that in torrefied substances, from a sulphureous, and that of the natural, from a terrestrial principle.

In observing a piece of lapis tutiæ, which was rough and unequal on its convex side, smooth and somewhat polished on the concave; he found, to his surprise, that the rough side was luminous, and the smooth one not. He was very desirous of investigating the cause of this appearance. He remembered that some polished marbles did not imbibe light, or very little, and that at their edges; but, having lost their polish, they admitted and retained it. He therefore conjectures that bodies, according to the disposition of their surfaces for the reflection of the light, either suffer or prevent its entrance into them. If this position holds good in the reflection, why should it not with regard to the refraction? our author produces 2 experiments, which he apprehends not foreign to the present purpose, but is yet making others, for his further satisfaction. He exposed a glass bottle full of well-water to the light, and as soon as possible observed it in the dark. As he expected, it imbibed no light. On pouring into it some oil of tartar, it became turbid and whitish, from the well-water being usually impregnated with calcareous matter. On observing it then in the dark, after having been exposed as before, it retained enough of a pale light to distinguish the shape of the bottle. In a bottle of rain-water he dissolved some talc; which stone, by rubbing, will dissolve in water as salts do, without rendering it opaque; to this solution he added oil of tartar, and this mixture was luminous as the preceding. He therefore concludes, that so long as earthy corpuscles are very small, separate, and agreeing in their surfaces with the water in which they float, they readily transmit the light they receive; for which reason it is impossible they should retain light enough to appear luminous in the dark. But, by the affusion of the saline principle, the earthy corpuscles unite with the water and salt; and from the union of these principles the mixture grows thick, by which the ready transmission of light is prevented; so that if this mixture is without colour, or any thing metallic, the light will be stopped long enough to be visible in the dark. But if, instead of oil of tartar, we add sugar of lead, the mixture will be turbid, but retain no light. In these two experiments the water becomes a phosphorus.

Gems, crystals, and glass, whether whole, or powdered ever so fine, retain no light; so that neither their transparency nor whiteness contribute to their becoming luminous in the dark. Of several diamonds, in all appearance perfectly the same, some were very luminous, others not at all. Of many opaque substances, whether rough, polished, or finely powdered, some were luminous, others not. So that it appears, that not only the external, but the internal texture of bodies also, may conduce sometimes to their being luminous.

From the preceding experiments, the author is led to make some inquiries into the cause of this luminous appearance; and takes notice, that almost all bodies, by a proper treatment, have that power of shining in the dark, which at first was supposed to be the property of one, and afterwards only of a few. How this is brought about is not very easy to solve. If we suppose with some, to which our author in several passages of this work seems not averse, that the light from a luminous body enters and abides in the phosphori, we shall find somewhat new to admire in light itself. It is no new opinion, that this fluid consists of very fine particles, which are continually darted forth from a luminous body, in all directions, with a very great velocity; but it has by nobody been laid down hitherto, that these particles are not dissolved by the violence of their agitation, nor dispersed, nor immediately cease to exist; but subsist still, and adhere to what bodies come in their way, as heat does, and are the causes of odours. If therefore the particles of light are not dissolved as soon as they are emitted from a radiant body, but continue some time, what else is required, but that we allow its atmosphere to every lucid appearance? if the phosphori shine with a borrowed light, but not with their own, and that only when put in motion, and fired by the rays of a shining body, which some experiments seem to confirm, then other new doctrines will arise. There must be then a hidden, a secret principle in bodies, to be lighted up by this most subtle fire. There will be in the universe a certain perpetual fire from these phosphori; the matter of which, though constantly dissipated by burning, does not waste enough to be obvious to our senses.

This work is the result of a great variety of very ingenious observations, and of experiments made with the utmost accuracy; and wherever the author makes any conjecture concerning their causes, he does it with all possible decency, and submission to the judgment of the learned.

Concerning the Electricity of Water. By the Rev. Dr. Miles, F. R. S. Tooting, Feb. 20, 1745-6. N° 478, p. 91.

Reflecting again on Mons. L'Allamand's experiment, Dr. M. resolved to make the following trial: He took his tin tube, which has 2 arms to it, directly opposite to each other, and at that distance from one end of the tube, which is equal to the length of one of the arms; this he suspended by a silk line from the ceiling of the room, letting it hang down of a convenient length. He then took a China basin, holding better than a quart, and having nearly filled the same with water, he stood on the wax-cake, with this basin of water in his hand, so near the pendulous tube, that he could apply the basin to it with convenience; then having suffered himself to be electrified, he held the basin so under the tube, that the lower end dipped about an inch in the water; on this a person approached one end of one of the arms with the spirit of wine in a spoon, and it was immediately kindled with vehemence; and at the same time he received on one of his fingers that held the basin a pungent stroke; and that stroke was given the very instant of time the snap was at the spoon, or any other object that was applied.

He thinks there can be no doubt, but that water is as good a medium of communication to the effluvia, as any substance whatever; for he is certain that all those which came to the spirit, were conveyed to the tube by the water; since the tube dipped in the centre, and was then motionless; so that it never came so near the basin as to receive any effluvia from it.

Description of an Improved Hygroscope. By Mr. Wm. Anderon, F. R. S. Nº 479, p. 95.

On comparing many different hygrometers together, none appeared to come nearer the truth than that recommended by Mr. Boyle, of weighing a piece of sponge in a pair of gold scales. But the difficulty and time Mr. A. found requisite to adjust the weights, and discover the true state of the air, set him on contriving another method, by which at all seasons he might perceive, by inspection only, the most minute alterations with respect to moisture or dryness; and is as follows.

In fig. 3, pl. 4, A represents a thin piece of sponge, so cut as to contain as large a superficies as possible. This hangs by a fine silken thread on the beam B, and is exactly balanced by another threat at D, strung with the smallest lead-shot, at equal distances, and so adjusted as to cause the index E to point at G, in the middle of the graduated arch FGH, when the air is in a middle state between the greatest moisture and the greatest dryness. I shows a little table or shelf, for that part of the silk and shot which is not suspended, to rest on. Hence the effect is evident.

A Letter from ——— to Mr. John Ellicot, F. R. S. of weighing the Strength of Electrical Effluvia. N° 479, p. 96.

Having heard that Mr. Gray gave an account of balls caused to move round each other by means of electrical effluvia, I was very desirous of seeing so delightful a sight. And though I was disappointed in my expectation of a circular motion, yet I found it easy to make two balls act on each other, in a very entertaining manner, for a long time; and that with such a constancy and regularity, as to the effect, that I apprehend we may thence deduce a gauge or standard for measuring electrical powers, and comparing the quantities and strength of the virtue infused into, or remaining in non-electrical bodies, after given times, &c.

This, with a great desire to be able to estimate and compare the effects of experiments with some certainty, and to do something more than amuse myself and friends with the several surprising phenomena which those experiments produce, led me to think of a method, which seems to be quite new, and to promise fair to afford much new light. It is to try or weigh the strength of the electrical effluvia, virtue, or power, by causing it to act on a balance.

I found, the first day, that this method answered even beyond expectation; so that several non-electrical balls, placed successively underneath one of the scales, and then imbued with electrical virtue in the common way, would presently cause that scale to descend 2, 3, 4, or 5 inches, and seem to cleave, for 10 seconds or more, to the several bodies so placed underneath, some having much greater effect than others. Whence it appeared, that there was a sufficient latitude for comparing very different forces, if any such there were. At the next and only opportunity I have had since, I used flat instead of globular bodies; and then I found the effects far more considerable; some of them, whose upper surface was about 3 inches square, having attracted and held down one scale, when there were about 200 grains weight in the other.

Though I am tempted to communicate some things, already observed by this means, with much delight, I reserve them at present for a further examination. Note, that the strings of that scale which is to be acted on, must be long, and non-electrical, and thick; that there may be a ready passage for the electrical virtue to run off, as fast as it is received. Instead of a brass scale-pan, I used a flat piece of cork, filed very smooth and even, especially on the under surface. The other scale needs no alteration, provided the strings be made of silk, as usual, and short enough to keep that scale out of the reach of the electric virtue, which is to act on the former. If the beam were 3 or 4 feet long, the strings of both scales might be of a length, which would make it less troublesome to put in and take out weights.

On the Situation of the ancient Town Delgovicia; and of 2 Men of an Extraordinary Bulk and Weight. By Mr. Tho. Knowlton. N° 479, p. 100.

Within 4 miles of this place (Londisburgh) have lately been discovered many foundations in a ploughed field, which have lain buried for many ages; and without any records or tradition of it. It was discovered by a farmer at Millington, as he formerly tended his sheep on one side of the hill, and on the opposite side had perceived in the corn a difference in colour for some years before; which led him this summer to dig; and happening on the foundations, it encouraged him, with Dr. Burton and myself, to dig likewise in several places; and in one part was discovered a circular foundation 5 feet wide, and the plan within 45 feet diameter; which it seems was a temple dedicated to Diana, said to have been at Goodmanham; but no appearance of it there was ever found. The distance from Goodmanham to Millington, is about 5 miles; and there were likewise many other foundations which had great quantities of Roman pavements within them; by which probably after the dissolution of the temple, it became a Roman station, then called Delgovicia; which has been fixed at Goodmanham, Londisburgh, Hayton, &c.: yet not the least remains ever appearing at any of those places, so as to satisfy an indifferent inquirer; but in this just now discovered, the ruins and foundations are a demonstration of the once grandeur of the place; and doubtless it was the abovementioned Delgovicia. The foundations lay about 18 inches below the surface, and to the depth of 4 or 5 feet within the ground; and are on the Wolds Hills, within 2 miles of Pocklington.

Within a mile and half of Kilham, is a place called Danes Graves, near which it is supposed was fought a great battle, in which vast numbers fell; and so were laid in heaps, and covered with the chalky soil in little tumuli, of the space of 2 or 3 square yards; in which are found great quantities of human bones: and now there is not less than an acre of ground covered over with them, joining close to each other; and it is one of the greatest curiosities of antiquity ever seen.

About 5 miles from thence, in Rudstone church-yard, there stands up, on the N. E. end of the church, a large stone, 30 feet above-ground, and what depth within is not known. Neither is it known on what account this vast obelisk was brought over land, so far as it must have been; because we have no quarries of stone nearer than 20 miles of the place. All the wolds are barren of such materials: it is 5 yards about, and of a parallelopiped form.

In the neighbourhood of Halifax, in Yorkshire, live 2 brothers, named Stoneclift, whose bulk and weight are very extraordinary: the eldest is a married man, and has several children; about 40 years of age. He weighed 35 stone, odd pounds, at 14lb. to the stone; which we may reckon near 500lb. weight. His brother weighs 34 stone odd pounds; and they make between them 70 stone, or

980lb. weight. As one was mounting a horse, the poor creature's back broke under him, and he died on the spot.

Dynamical Principles, or Metaphysical Mechanical Principles. By James Jurin, M. D., F. R. S. Presented March 13, 1746. N° 479, p. 103. *From the Latin.*

When the celebrated Leibnitz published* his new doctrine, by which he determines that the force of a body in motion is to be measured by the square of the velocity, it raised a great controversy in the mathematical world. The same author, in April 1695, published his Specimen Dynamicum, in confirmation of this doctrine; where, among other things, he says, "I arrived at the same true estimation of forces by different ways: one a priori, by a very simple consideration of space, time, and action; another a posteriori, by estimating the force by the effect which it produces in exhausting itself."

It seems he intended the publication of his a priori, which he promised to explain in another place, in May following; for towards the end of his Specimen Dynamicum he adds, "And now, having dispelled error, we shall produce the true and really admirable laws of nature, more distinctly, in the 2d part of this essay, to be published in the month of May." Yet this 2d part never appeared in public. However, to clear this great man from the imputation of not having performed his promise, it has been lately given in the Commercium Literarum between himself and another celebrated mathematician, John Bernoulli.

This author it seems, on seeing the Specimen Dynamicum, wrote to Leibnitz, in June following, applauding some things; but at the same time being so far from approving his estimation of forces, that he even endeavoured to demonstrate, that the forces of moved bodies are not as the squares of their velocities, but simply as the velocities only. But at length, after several letters had passed between them, Bernoulli came over to Leibnitz's opinion, who, being willing to reward the docility of so eminent a disciple, communicates to him his argument a priori, which he had hitherto kept to himself, and at the same time assigns the reason why he did not divulge it sooner. "I would not, says he, honour with this clear light of truth, those who did not receive as they ought those arguments drawn from the affections of heavy or other sensible bodies; and therefore I would not make them public; but reserved them to be communicated to such as had shown themselves proper judges."

Bernoulli therefore, having shown himself to be a proper judge, and having received as he ought those arguments a posteriori, that is, having come over entirely to the opinion of Leibnitz, was thought worthy of the honour to be ad-

* Act. Erud. Lips. 1686.

mitted into these secret recesses of science. "Because, says the author, I see you are on our side, I will freely communicate to you my principle of demonstrating a priori the true estimation of forces; which I have sometimes mentioned as in my possession, but have never yet produced. For communicating to you, is committing a seed to a most fruitful soil, that it may grow up to a large plant."

I cannot but commend the good man for committing his seed to so fruitful a soil; and yet I cannot think him wholly free from meriting some censure. For though he could work no effect on the Papins, Catalans, and other opposers of his doctrine, who seemed to be incapable of conversion, by any demonstrations, however strong; though he might think them unworthy of this clear light of truth; yet why begrudge it to the rest of the learned world? I will not say that it was the part of a good and humane man, and of one who was desirous to increase knowledge, to lay open to all an affair of such moment: but if he had only studied his own glory, before every thing else, he should have acted in this manner; that those detractors might either have been immediately silenced, or condemned by all the world. Finally, as great men are not born for themselves alone, nor for a friend or two; but for all; is it not rather unfair that Bernoulli and his disciples only should enjoy this clear light, while we poor wretches are condemned to live in more than Cimmerian darkness. But it is well for us, now that after 50 years of darkness, that light at length shines out upon all. But behold the argument!

"1. An action performing double, in a single time, is virtually double of an action performing the same in a double time; or the walking of 2 miles in 1 hour, is virtually double of walking 2 miles in 2 hours."

"2. An action performing double in a double time, is formally double of an action performing single in a single time; or walking 2 miles in 2 hours, is formally double of walking 1 mile in 1 hour."

"3. Therefore an action performing double in a single time, is quadruple of an action performing single in a single time; or the walking of 2 miles in one hour is quadruple of the walking of 1 mile in 1 hour."

"4. If for double we had substituted triple, quadruple, quintuple, &c. the action would have come out noncuple, sedecuple, 25ple; and in general it appears that equable equitemporaneous moving actions, are to equal moveable ones, as the squares of the velocities; or, which is the same thing, that in the same or equal bodies, the forces are in the duplicate ratio of the velocities. Q. E. D."

Having read this argument, and, out of regard to the great fame of the author, having considered it with much attention, I must confess I could not discover the least spark of truth in it, or even of common sense. I should have suspected that this had been owing to the weakness of my own eyes, which perhaps might be dazzled by the too great splendour of the light, if a doubt from Bernoulli himself had not relieved me. This ingenious man was so far from acquiescing in this clear light of truth, that he not only made an objection, but even produced a double demonstration.

"I do not see, says he, what can be said by an adversary to the contrary; unless perhaps that the virtual seems to be confounded with the formal action; denying the consequence, that A is quadruple of C, because A is virtually double of B, and B formally double of C."

Having proposed this objection, he adds his demonstrations.

"1. An action performing double in a single time, is virtually double of an action performing the same double in a double time."—"2. An action performing double in a double time, is virtually single of an action performing single in a single time."—"3. Therefore an action performing double in a single time, is double of an action performing single in a single time." Or,

"1. An action performing double in a single time, is formally single of an action performing the same double in a double time."—"2. An action performing double in a double time, is formally double of an action performing single in a single time."—"3. Therefore an action performing double in a single time, is double of an action performing single in a single time."

"You see then the two arguments, which plainly conclude the same thing, but are quite contrary to your conclusion, and depend on that common axiom, that those things which are equal to the same are equal to each other; which however holds only in homogeneous quantities, as here in comparing a virtual action with a virtual, and a formal with a formal, but not one with the other."

Thus Bernoulli, with no less acuteness than modesty. But Leibnitz first endeavours to take off Bernoulli's objection; thus, "I do not well understand, says he, what you mean when you say, a virtual action is confounded with a formal one: for I do not here treat of an action as being either virtual or formal; but one action is double of another, either virtually or formally: virtually, when it is double in estimation, though it be not double in bulk, or congruence; as a ducat is the double of a dollar: but formally, as a dollar is the double of a half dollar. And you must know that what is double formally, is double also in virtue or estimation. Therefore, as the inquiry here is only concerning virtue or estimation, there is no confusion of the different kind of quantities or estimation; for by virtually double, I understand that which is so only virtually; but I call that formally double, which is double both formally and virtually."

He then proceeds, "I might abstain from words used only for the sake of a certain harmony; for as a ducat is the double of a dollar, and a dollar of a half dollar, I conclude that a ducat is the quadruple of a half dollar; so because the walking of 2 miles in 1 hour is the double of 2 miles in 2 hours, and the walking of 2 miles in 2 hours is the double of walking 1 mile in 1 hour, it will follow that the walking of 2 miles in 1 hour is the quadruple of walking 1 mile in 1 hour."

These disturbing words, *virtually* and *formally*, being now removed, which had hitherto fouled *this clear fountain of truth,* Leibnitz not only took off Bernoulli's objection, but brought him over entirely to his side. " Your answer, says he in his next letter, quite satisfies me; for I perceive what you mean by those two terms: and your argumentation appears to me very elegant; so that it ought no longer to be detained from the public; and it will give great weight to the argument a posteriori."

Perhaps Bernoulli would not urge the matter further, as Leibnitz seemed to be in a more than ordinary commotion: " I dare not, says he, promise any thing great; but I hope to be not guilty of an obvious paralogism, in an argumentation which did not escape from me suddenly, but had been considered for several years, and I had boasted of it as a thing of some moment." And yet, that Leibnitz was *guilty of an obvious paralogism,* I believe will soon be shown.

We need not dwell on Leibnitz's examination of both Bernoulli's demonstrations, because they depend on the sense of the words *virtually* and *formally*, understood differently from the meaning of Leibnitz, " I took the terms, says Bernoulli, in a sense different from that in which you now explain them." But Leibnitz, being still in doubt what weight his first demonstration would have with Bernoulli, adds the following to it. " I add another, says he, which, if you examine it to the bottom, comes to the same as the former, and yet it has its own proper weight. Moving actions, I mean equable ones, of the same moveable, are in a ratio compounded of the immediate effects, viz. the spaces run through and the velocities. Now the lengths equably run through, are in a ratio compounded of the times and velocities. Therefore moving actions are in a ratio compounded of a simple ratio of the times, and a duplicate one of the velocities. So that, in the same times, or elements of times, the moving actions of the same moveable are in the duplicate ratio of the velocities; or, if the moveables are different, in a ratio compounded of the simple ratio of the moveables, and the duplicate one of the velocities."

As Bernoulli had said, in his letter of April, that he acquiesced in the former demonstration, without mentioning the latter, Leibnitz asked him in May, " what he thought of the other demonstration, which says he is a little more in the manner of the received form, though they both agree in the root. Bernoulli therefore, when he could no longer avoid opening his mind, in his letter of June thus expresses himself:

" Your other demonstration of the proposition concerning the ratio of moving actions, seems contrived no less ingeniously than the former, and as you express it, more in the received form, though in fact they both coincide. For nothing is more evident than that moving actions ought to be measured by their immediate effects. If therefore the lengths gone through, and the velocities, unless any one will obstinately have the velocity to be rather the cause, are the effects of an immediate action, and indeed the only ones, of which one does not depend on the other, the moving actions will necessarily be in a ratio compounded of the lengths and the velocities: and so, in equal times, in the duplicate ratio of the velocities."

Now in this answer it is plain that Bernoulli approves of the 2d demonstration in appearance, but in reality condemns it, though with the greatest caution and modesty. For he not only hints that the velocity is rather the cause than the effect of an action, but he restrains his assent to this condition, that one of the effects mentioned by Leibnitz, viz. of the velocity and the length gone through, *does not depend on the other, or is not included in the other*. Now as it is very evident, that the length gone through *does* depend on the velocity, and is included in it, it is plain that the demonstration is faulty in the opinion of Bernoulli.

Leibnitz, in his next letter, gave a distinct and copious answer to many other things, but to these tacit objections of Bernoulli he answers lightly, dissembling their force, and as if treating of something else, only just says, "But as I now estimate an action by the compound ratio of its principles, power and time; so I had estimated it a little before by the compound ratio of what it performs; an extensive or material effect, viz. of the length, which I usually call an effect κατ' ἐξοχὴν, and an extensive or formal effect. For it is required that much should be performed and soon. You see now that both the estimations agree together."

By the obscurity of this answer, whether it be affected or natural to Leibnitz, it is easily seen that he would have the velocity to be taken for the effect of an action, which Bernoulli had hinted was rather the cause, but that he dared not name it openly, though he understands it under the name of an intensive or formal effect, which the action performs. Besides, as to Bernoulli's other objection, though the velocity is in the highest degree the effect of an action, as well as the length gone through; yet as one of these effects depends on the other, and is included in it, and certainly the length gone through depends on the velocity, an action ought not to be measured by both those effects; as to this, I say, he observes a profound silence.

The second demonstration therefore seems to be given up by Lebinitz, as well as Bernoulli; and indeed in all their subsequent letters there is not the least mention of it. Besides, that first demonstration, which comes to the same with the other, viz. a true with a false one, it seems not wholly free from exception, either with Bernoulli or with Leibnitz himself. For Bernoulli, though he had declared in April, that it quite satisfied him, that he acquiesced in it, and that it was very elegant, and ought not to be longer withheld from the public; yet in August he knew not what L. meant by the word *action*, on which that whole demonstration depends. "You ought," says he, "to define what you mean by action; otherwise nothing can ever be demonstrated." This was a just admonition, but to no purpose; for in L.'s answer to this, there is not a tittle of that definition, so highly necessary.

But L. himself, in his letter of June, expresses himself thus: "My demonstration a priori, for our estimation of forces, depends on a certain supposition; viz. that an action performs any thing uniformly, in a single time, is double of an action performing the same thing uniformly, in a double time. This supposition ought to have been granted by Catalan and the rest, with whom I have disputed." But what if they will not grant it? why then the demonstration, which depends on this supposition, falls to the ground, at least till that supposition is demonstrated. "But," says L., "I have not yet found out a way of demonstrating this proposition a priori, by the way of congruency; nay not even this, that an action performing the same thing, in a shorter time, is greater; which ought to have been the beginning." Therefore, since that so much boasted demonstration a priori needed another demonstration, which L. had not yet discovered, nor ever after did discover, nor any mortal ever will discover, it is no wonder that this *seed*, though *committed to a most fruitful soil*, did not *grow up to a large plant*. For Bernoulli took a final leave of this *clear light of truth*, when he saw it dwindle away to a mere snuff.

But a gentleman of much higher courage, the learned Chr. Wolf, having attempted to treat the theory of forces in a geometrical manner, communicated it, in the Comment. Acad. Petrop. under the title of Principia Dynamica. "When he had communicated" part of this, "in 1710 to Count Herberstein, Leibnitz, and others, Leibnitz, in a letter 1711, said that it agreed with his, which he had communicated to Bernoulli, Herman, and others, confirming it in these words: I lay down this calculus of pure forces or actions. Let *s* be the space, *t* the time, *v* the velocity, *c* the corpus or body, *e* the effect, *p* the power, *a* the action. Then in equable motion, *tv* will be as *s*, and *e* as *cs*, and *tp* as *a*: and these may be assumed without demonstration. Add, what is to be demonstrated, *ev* as *a*. Hence many other theorems may be demonstrated; for instance, *p* as cv^2. For *tp* is as *ev*; but *e* is as *cs*, and *s* as *tv*; therefore *tp* is as ctv^2, or *p* as cv^2. And in these is contained part of my Dynamics, abstracted from sensible things, though afterwards verified by experiments." "I doubt not, therefore," says Wolf, "but I have here proposed Dynamical principles, which are conformable to the sentiments of Leibnitz."

This indeed is manifest enough, as W.'s theorems exactly agree with the algebraic notations of L. But whether these principles be as conformable to truth as they are to the sentiment of L., may deserve examination. But one thing is particularly deserving of notice in these notations, viz. *a* as *ev*, *which is to be demonstrated*: whence it seems that L. not even then, after 16 years, had not found out a demonstration of the supposition formerly put off to Bernoulli; viz, that an action performing any thing in a single time, is double of an action performing the same thing in a double time: since an action performing any thing in a single time, does it with twice the velocity of an action performing the same thing in a double time. But how W. demonstrates this, we shall examine presently.

DEFINITIONS. *Def.* 1.—I call that, with Leibnitz, *vis viva*, or merely *vis* or *force*, which adheres to local motion. 2. A *pure force*, is that which is not resisted in acting by any contrary force. Hence such a pure force remains unvaried in the whole time of action, and is not in the least exhausted by the effect it produces. 3. A *pure action*, is that which is exercised by a pure moving force. Such as the action of a moveable carried with an equable motion in an unresisting medium. 4. A *uniform action*, is that which is double in a double time, triple in a triple, &c. or in general which is as the time; or such an action as obtains in equable motion. 5. *The effect of a moving force beyond the conflict*, is the translation of a moveable through a space.

AXIOMS. *Axiom* 1.—If two or more equal moveables be moved with equal celerity, their force is the same. 2. The same action is performed by the same force in the same time. That a greater action is performed by the same force in a longer time than in a shorter, and that a greater action is performed in the same time by a greater force than by a less, no one doubts. Therefore the quantity of an action depends on the quantity of force and time. So that if the forces be equal, and the time the same, the action also must be the same. 3. If the same moveable be transferred through the same space, the effect is the same.

THEOREMS. *Theor.* 1.—When unequal bodies are moved with the same velocity, the forces are as the masses. 2. Uniform actions, performed in the same time, are to each other as their forces. 3. Uniform actions, performed with equal forces, are to each other as the times in which they are performed. 4. Uniform actions are in the ratio compounded of the times and forces. 5. Unequal forces perform the same action in times reciprocally proportional to each other. 6. When two equal moveables are carried through unequal spaces, the effects are as the spaces. 7. When two moveables are carried through the same space, the effects are as the masses. 8. When two moveables are carried through any spaces, the effects are in the ratio compounded of the masses and spaces. 9. In equable motions, the effects are in a ratio compounded of the masses, velocities, and times. The demonstrations of these 9 theorems, about which there is no controversy with the Leibnitzians, and which being the same with those given by Wolf, are here omitted.

Theor. 10.—Actions, by which the same effect is produced, are as the velocities. It is on this theorem that the whole matter turns: if it be true, the Leibnitzian doctrine is to be embraced; if not, it is to be rejected: therefore its demonstration must be scrupulously examined. It is divided by Wolf into 3 cases: but as the 2d and 3d depend on the first, we shall need only consider this one. Mr. W. says, "when moveables are equal, and the same effect is produced in different times, the velocities will be reciprocally as the times in which it is produced; that is, a body, which produces an effect in the time $\frac{1}{2}t$, is moved with the velocity $2v$, when another, which produces the effect in the time t, is moved with the single velocity v; and so on. Now it is evident that a uniform action is double, which produces the same effect in half the time, triple which is subtriple, and so on."

But do you say, Mr. Wolf, that this is evident? What if I should deny it? What if I should say that any action, which produces the same effect, is the same, in whatever time it produces it? This is the very supposition of Leibnitz, of which, in his letter to Bernoulli in 1696, he says he has not discovered a method of demonstrating a priori, and in his letter to yourself in 1711, he says is still to be demonstrated. And yet you do not endeavour to demonstrate it, but say it is evident. Now I deny its being evident, and thus your demonstration falls to the ground, and the supposition along with it.

But before substituting a new one, let us consider a little what is understood by action, and what by effect. Wolf, after the example of Leibnitz, has omitted the definition of action. He only distinguishes what is a pure action, viz. that which is free from all impediment; and what is a uniform action, viz. that which increases in proportion to the time: but what he means by action itself is nowhere determined. But till this is done, *nothing can be demonstrated,* as Bernoulli advised Leibnitz long since.

If I might venture to supply this defect, I would ascribe the same definition to action, which Wolf has given of effect; since there seems to be no other difference between action and effect, than that action, if I may so speak, is an effect *in fieri,* and effect an absolute action, or one that is perfected. For in Wolf's example, a vis viva is that which transfers a moveable through a space; therefore the action of a vis viva is the translation of a moveable through a space; and the effect of a vis viva, is also the translation of a moveable through a space; or rather, an effect is a moveable already transferred through the same space.

But generally an action is the preceder of an effect: or rather, an action is that by which any thing is effected; but an effect is the thing itself which is effected. For example, if I write a page, my action will be the writing of a page, and the effect will be a page written. Or, if a workman whiten a wall, his action will be the whitening of a wall, and the effect will be a wall whitened, Or, if a labourer dig a garden, his action is the digging of a garden, and the effect is a garden digged.

Our Theor. 10.—Of equal actions, the effects are equal.

Let any vis viva A perform any action; and let there be supposed any other vis viva B. Now that the vis viva B may perform an action equal to that of the vis viva A, it is necessary that the vis viva B should act exactly as much as the vis viva A has acted. Therefore, after the completion of the action of B, as much will be acted by the force B, as has been acted by the force A; that is, the effect of the vis viva B will be equal to the effect of the vis viva A, the actions of which were equal.

Our Theor. 11.—Actions are in proportion to the effects. For let the effect *e* be produced by the action *a*. Therefore another effect *e*, equal to the first, will, by theor. 10, be produced by another equal action *a*; consequently the effect twice *e* will be produced by the action twice *a*. In like manner it appears that the effect thrice *e* must be produced by the action thrice *a*, &c. And generally, that the effect *ne* (= E) must be produced by the action *na* (= A.) Therefore A : *a* :: E : *e*; that is, the actions are in the ratio of the effects.

Our Theor. 12.—Forces are in the ratio compounded of the masses and velocities.

For, by theor. 4, actions are in the ratio compounded of the times and forces. And, by theor. 11, the actions are in the ratio of the effects. Therefore, the effects are in the ratio compounded of the times and forces. But, by theor. 8, effects are in the ratio compounded of the masses and spaces. Therefore the ratio compounded of the times and forces, is equal to the ratio compounded of the masses and spaces. Therefore the forces are in the ratio compounded of the masses and spaces directly, and of the times reciprocally; that is, in the ratio compounded of the masses and velocities.

Of Two Extraordinary Deers' Horns, found under-ground in different Parts of Yorkshire. By Mr. Tho. Knowlton N° 479, p. 124.

The head and horns here described, were found in a sand-bed, in the river Rye, which runs into the Derwent, in the east-riding, belonging to Ralph Crathorn, esq. They were discovered as he was fishing for salmon; the net happening to hang on 1 or 2 of the antlers, he ordered to pull away; by which some of the antlers were broken off, and discovered it to be part of a deer's horn. At length, with some difficulty, it was dug out pretty entire. Mr. Crathorn supposes, that these wild moors were once inhabited with this kind of deer, not any such now being known to be in this kingdom; and supposes it is, at least, 7 or

800 years since its death: and that by age or poverty destroyed, and by time buried in those sands. These horns had many antlers, and were about 3½ feet in length, from the root to the tip.

Another skull, and horns, of the palmated kind, are here described also: the head and horns weighed together 4 st. 12 lb. or 68 lb.

The length of the skull, from the nose-end to the back-part of the head	1 ft.	10 inches.
The breadth of the forehead	0	11
Length of each horn, from the skull to the tip	5	1
The extent between the horns	6	1
The breadth of the web or palm	2	1

Yet it is evident the horns are not at their full growth, being yet covered with what is called the velvet.

They were found in a peat-moss, at Cowthorp near North Dreighton in Yorkshire, in the year 1744; and were dug up from the depth of 6 feet out of the peat moss,

But what he thinks more extraordinary is, that the late earl of Carlisle's steward, Mr. Joice, in digging the foundation of a house and cellars, found, at the depth of 6 feet, a part of a jaw-bone with teeth, and a horn of a buck, or stag of most exceedingly large dimensions, which lay buried under 2 feet of common soil; then one foot of scalping or sand-bed; then 18 inches of stone; then another vein of sand, 6 inches; then another head of stone; under which lay those before-mentioned jaw-bone, and piece of horn; which, in all appearance, to every one that viewed these stratums, had never been removed.

Dimensions of the Deers' Horns in the Museum of the Royal Society.

Length of the skull	1 foot	4 inches.
Breadth of the forehead	0	9
Length of each horn	5	0
Distance of the extreme tips of the horns	6	0

The Phenomena of Venus, represented in an Orrery made by Mr. James Ferguson, agreeable to the Observations of Seignior Bianchini.* N° 479, p. 127.

In all the common orreries, Venus is represented as having her axis perpendicular to the plane of the ecliptic, and her diurnal motion on it equal to 23 hours

* Mr. Ferguson was a very uncommon genius, especially in mechanical contrivances and structures, for he executed many machines himself in a very neat manner. He had a good taste in astronomy, and generally in natural and experimental philosophy; he also possessed a happy manner of explanation in his lectures, in a clear, easy, and familiar way. Yet his general mathematical knowledge was little or nothing. Of algebra he understood little more than the notation; and he never could demonstrate a proposition in geometry; his constant method being to satisfy himself, as to the truth of any problem, with a very exact construction and measurement by scale and compasses.

of our terrestrial time. Hence, as her annual motion is performed in about 225 of our days, it will contain 234 of hers; consequently, to an eye placed in Venus, the sun will always appear to go through a sign of the zodiac in 19½ of her days; and as her axis has no inclination, she must have a continual equality of her days and nights, without any variation of seasons, and so her animal motion can be of no other use than to keep her from falling down to the sun.

But Bianchini gives a very different account of her; which is, that her axis inclines 75° from the plane of her ecliptic; and that her diurnal motion is performed in 24 days and 8 hours of our time; and this will cause her year, which is equal to almost 225 of our days, to contain only 9¼ of her days; and this odd quarter of a day in Venus will make every 4th year a leap-year to her, as happens to us on earth, by the 6 hours that our year contains above 365 days: and to her the sun will appear always to go through a sign of the zodiac in little more than ¾ of her day, which is equal to 18¾ of our days; and in going round the sun, her north pole constantly leans towards the 20th degree of Aquarius.

Thus, with regard to the absolute length of Venus's year, Bianchini agrees with Cassini and other astronomers: but differs widely in other very remarkable particulars, from which arise so many advantages, as to make that planet incomparably more fit for its inhabitants, than we could possibly conceive it to be by a quick rotation on an axis perpendicular to its annual path.

But, by such a motion as Bianchini describes, these inconveniences are avoided; for there is no place in Venus but what will have the 4 seasons every year, and the heated places will have time to cool; because, to any place over which the sun passes vertically on any given day, he will, on the next day, be 26 degrees from its vertex, even though the place be on the tropic; and if it be on the equator, one day's declination will remove him 37½ degrees from it.

By passing a narrow slip of paper around the terrestrial globe, crossing the

Mr. F. was born in Bamfshire 1710, of poor parents. He first learned to read, by overhearing his father teach his elder brother; and indeed all his knowledge afterwards was of his own acquisition. He soon discovered a peculiar taste for mechanics, which first arose by seeing his father make use of a lever. He pursued this study to considerable length, while he was yet very young; and made a watch in wood-work, from having once seen one. At a fit age he went to service with a farmer; when tending the sheep gave him leisure to make considerable advances in mechanical and astronomical knowledge. He acquired also some skill in drawing; and going to Edinburgh, he there for some time followed the profession of taking portraits in miniature, at a small price: an employment by which he supported his family for several years, both in Scotland and England; while he pursued more serious studies, and the labour of philosophical lectures. Mr. F. was a F.R.S.; he died in 1776, at 66 years of age.

A full account of Mr. F.'s life was printed by himself, being prefixed to his Select Mechanical Exercises. An abstract of which may be seen in Dr. Hutton's Mathematical and Philosophical Dictionary, with a complete list of Mr F.'s separate publications; besides which, his numerous communications to the Royal Society are printed in the Philos. Trans. from vol. xliv. to vol. lvii. inclusive.

equator in 2 opposite points at an angle of 75°, it would, by representing Venus's ecliptic drawn on her globe, serve for the solution of problems concerning her, as the ecliptic on our terrestrial globe does for those relating to our earth.

Things being thus premised in general, Mr. F. proceeds to give a description of the particular phenomena in Venus, confining himself chiefly to what happens in her northern hemisphere; knowing that the same must happen, mutatis mutandis, in the southern.

1. Her axis is inclined 51¼ degrees more than the axis of our earth, and therefore the variation of her seasons will be much greater than of ours. 2. Because her north pole inclines towards Aquarius, and ours to Cancer; her northern parts will have summer in the signs where those of our earth have winter; and vice versa. 3. The artificial day at each of her poles (containing 4⅛ apparent diurnal revolutions of the sun) will be equal to 112½ natural days on our earth. 4. The sun's greatest declination, on each side of her equator, amounts to 75°: therefore her tropics are only 15° from her poles, and her polar circles at the same distance from her equator. Consequently, her tropics are between her polar circles and poles, contrary to what those on our earth are. 6. The sun, in one apparent diurnal revolution from the equator, and any meridian where he crosses it, to the same meridian again, changes his declination at least 14° more on Venus, than on our earth from the equinox to the solstice.

Mr. Ferguson then pursues his speculations on the phenomena of the planet Venus, relative to the more minute particulars; such as the sun's rising and setting, the days and nights, &c.; which being matters of mere curiosity and speculation, it is of no use now to pursue them further; especially also as such phenomena are similar to the like circumstances on our earth, but differing only in their degree.

A Machine for Sounding the Sea at any Depth, or in any Part. Invented by Major William Cock in the Year 1738, *in a Voyage to Georgia.* N° 479, p. 146.

The draught of this machine is exhibited in fig. 1, pl. 5; where AAAA represent a trunk of timber, with a square hollow, through the centre of which passes the square piece of timber BB; having a groove on each side, in which are placed the two pieces of iron CC; the foot of each resting on the pins DD, that pass through the trunk; the upper part of the irons are hooked to an iron pin at E, which passes through the square piece BB; which piece is hollowed for the hooks of the irons CC to pass up and down.

When the weight F touches the ground, the two irons CC sink the trunk down, which unhooks them at E; on which they fall off, and leave the trunk at liberty to float or rise up again to the surface. A machine of these dimensions, loaded with an iron ball F, of 12lb. weight, being let down in water 100 fathoms deep, will go down to the bottom, and the trunk will return in 1 minute and 3 seconds.

An Account, by Mr. J. Breintal, of what he felt after being bitten by a Rattle-Snake. Dated Philadelphia, Feb. 10, 1746. N° 179, p. 147.

Mr. B. walking up a stony hill, his foot slipped, and falling on his knees, he laid his hand on a broad stone to stay himself; and he supposes the snake lay on the opposite side, and might be offended by some motion of the stone, so bit his hand in an instant, without any warning or sight; then slid under the stones, and sounded his rattles.

Mr. B. felt a sort of chilliness when he heard the sound; because he had a constant thought, that if ever he was bitten his life was at an end. Without stop he tore up the stones, resolving to slay his murderer; at last he found him, on which he crushed his head to pieces with a stone; then took him up in his left hand, and ran to his quarters, sucking the wound on his right hand as he went, and spitting out the poison.

This kept it easy; but his tongue and lips became stiff and numb, as if they had been frozen. So getting quickly home, he exclaimed, "I am bitten by a rattle-snake, and there lies my murderer!" casting him down on the threshold.

The first thing applied was a fowl; its belly ripped up, and put on his hand alive, like a gantlet, and there tied fast. This drew out some of the poison; for immediately the fowl swelled, grew black, and stunk. He kept his elbow bent, and his fingers up, to keep the poison from his arm. Thus he walked about, and set some of the company to make a fire on the green; which was done quickly, and there they burnt the snake. Another got some turmerick; this they bruised well, tops and roots; so made a plaster, and bound it round the arm, to keep the poison in the hand: but night came on, or else he believed it had never gone farther than the hand; for this kept the arm secure, till mid-night, or past. Nor all this while had he much pain: his hand grew cold and numb, but did not swell very much; but now puffed up on a sudden, and he grew furious; so he slit his fingers with a razor, which gave some ease. He also slit his hand on the back, and cupped it, and drew out a quart or more of ugly poisonous slimy stuff. But his arm swelled for all they could do: then he got it tied so fast, that all communication might be stopped with the body, that it seemed almost void of feeling; yet would it work, jump, writhe and twist like a snake in the skin, and change colours, and be spotted; and they would move up and down on the arm, which grew painful in the bone.

Thus was it tied 2 days, and all things applied that could be got or thought on. At last, the ashes of white-ash bark, and vinegar, made into a plaster, and laid to the bite, drew out the poison apace.

His tongue and lips swelled that night, but were not very painful, occasioned only, he supposes, by sucking the wound. The swelling of his arm being sunk, till it was at least half gone, they then untied it; but in 2 hours all his right side was turned black, yet swelled but little; nor was there any pain went along with that change of colour. He bled at the mouth soon after, and so continued spitting blood and was feverish 4 days.

The pain raged still in the arm, and the fever more violent; and by turns he was delirious for an hour or two. This happened 3 or 4 times; and 9 days being over, the fever abated, and he began to mend; but his hand and arm were spotted like a snake, and continued so all summer.

In the autumn his arm swelled, gathered, and burst; so away went the poison, spots, and all.

But the most surprising and tormenting were his dreams; for in all sicknesses before, if he could but sleep and dream he was happy so long; being ever in some pleasing scenes of heaven, earth, or air: on the contrary, now if he slept, so sure he dreamed of horrid places, on earth only; and very often rolling among old logs. Sometimes he was a white oak cut in pieces; and frequently his feet would be growing into 2 hickeries. This cast a sort of damp on his waking thoughts, to find his sleeping hours thus disturbed with the operation of that horrid poison.

Microscopical Observations on the Farina fœcundans of the Hollyhock and the Passion-Flower. By R. Badcock, Esq. Dated Kensington, Nov. 6, 1745. N° 479, p. 150.

The first experiment was gathering the bud of a hollyhock so young, that the petala were not yet formed; and stripping off the calix, nothing appeared, but the apices close to the stylus, for the stamina were not yet perceptible: these apices appeared to be a kind of bag; and he could plainly perceive a seam run down the middle of it. He took a fine needle, and carefully opened them; when he found each full of farina, which seemed to lie very regular. This determined him to take notice of the course of the farina in each flower, and he observed the following particulars:

August 24, he took notice of a flower just going to blow, and the petala appeared; the farina was then just burst from its apices. The time of these bursting is as soon as ever the petala blow out enough to be affected by the sun. Aug. 25, The flower opened more, and the farina appeared so thick on the outside of the apices, that they seemed quite covered from sight, without a very narrow inspection. 26. The farina began to decrease visibly, and continued to do so till the 27th. When he perceived some red curled stamina, without any apices, pushing themselves out at the top through the others. These were, within their bend, thick set with a kind of hairs, and in their passage took a good quantity of farina with them, which remained a day longer than that which was contained in the apices. He could not observe the farina to fall on any particular part of the flower, but it seemed rather to be dispersed. When these red stamina appear, the farina is going, and the apices, which contained it, dead.

The flower was kept till it withered, and the stylus, &c. cut off; but in neither experiment was there found any difference, after a month's keeping the farina, except in the colour, which was deeper. Cutting off the stylus, &c. may have a considerable effect on the seed, but seems to have but little on the flower: for though it was cut off as soon as possible, yet the flower blew out the same as if nothing had happened, till about the time that the farina might be supposed to act; then the petala began to look black next the stylus, and dropped off a day sooner than the regular blowing flower.

Not having an opportunity of pursuing this further, for want of flowers and warm weather, he applied to other experiments. He brushed off some dry farina, and putting Thames water to it, found it would not burst, under the space of 7 or 8 minutes, and not till they are soaked in the liquid: for, at the time of acting, they seldom or never lie upon each other, but float off, till they are clear of all encumbrances.

In making experiments on a fresh-blown hollyhock, he observed a lusus naturæ of 2 globules quite smooth and shining, contrary to their nature, which is rough: one of these acted very soon, the other not at all. The whole farina seem to have a strong suction; for he was obliged, in the space of 10 minutes, to apply water 3 times, in order for them to have enough to act in; and he observed that they burst with a greater force, and throw out a much larger pulp, when thrown into a depth of water.

The passion-flower appears on several accounts to be the fittest flower for experiments on the farina of any.

This farina appears, by Mr. Cuff's double reflecting microscope, magn. 6, 5, 4, to be a smooth round globule, of a pretty full yellow, like the appearance, fig. 12, pl. 4, which shows the area of the microscope. These globules, on being more magnified, are found to have some 3 circles, as fig. 13, others 2, others none. Among these many were quite white; but he never observed these act. When the globules, fig. 12, come to be magnified with the first or second magnifier, they appear indented, exactly like fig. 14. All the coloured ones, though differently marked, yet all act alike. And these act in a much less space of time than those of the hollyhock, which are 10 minutes, though fresh; whereas these act instantly, though kept for 24 hours; neither have these any suction or convulsive motion; acting entirely still, and in the first water. Attempting to apply them to the opaque microscope after their action, they stuck round the point like wet skins; but he observed that they burst only once, throwing out all their pellucid matter, which is yellow, at the first discharge. They act no otherwise in oil, but by emitting a matter much thinner than that at bursting; but, having lain in oil for a minute, and put from thence into water, they act instantly, and with a seeming additional force. Being put into malt-spirits, they exhibit a very agreeable appearance: all those which emit, as in oil, lie dead and still; but those which neither burst nor emit, are thrown into so violent an agitation, that they appear like animalcules; sometimes joining ten or a dozen together; on a sudden, an imperceptible force will throw a globule, sometimes, 2 or 3, 3 parts over the area of the microscope; often 2 globules will be whirled round with incredible swiftness, for the space of near a minute, then separated by the same imperceptible swiftness, fly each a different way. They will act thus, till the liquor may be supposed to dry up, when supplying them with liquor will regain their motion; and though you put liquor often to them, yet every time will give them that swiftness. On applying the magnifier, N° 2, he found that it is the white unacting globules that do thus, and imagines that they rise with that spirit which evaporates; and their not being volatile occasions them to stop at top, and continue this motion as long as the liquid has any evaporation; for after a certain time, they lie like the others which have acted. In this liquid they burst, in such a manner, as that the places from whence they burst are perceptible, fig. 16, and the pieces broken off very plain.

On applying aquafortis to this farina, the shape and marks are instantly changed to those marked in fig. 16; whereas on the hollyhock, it has no other effect than burning up their capillary prickles.

Electrical Observations. By the Rev. Henry Miles, D. D., F. R. S. N° 479, p. 158.

On using one of the boxes filled with pitch, wax, &c. for the person to be electrified to stand on, after using it a little while successfully, he wiped the surface of the pitch, &c. with a dry clean cloth, suspecting, from the place it had stood in, some dampness might lodge on it. This done, he set up the box on one side, and held a thread of trial at a proper distance, and found it to attract and repel the same, but on setting it down and standing on it, by no means could it be made appear that he was electrified, or any other person who stood on it afterwards. He then took another box of the same sort, but made use of it without wiping it, and it performed well.

Dr. M. fired common spirit of wine, at the distance of 25 feet, the effluvia being conveyed by 3 persons and 2 laths of deal, tied together thus: the person to be electrified immediately standing on a cake of wax, and holding one end of the lath, another person standing about the middle of the distance of another cake, and supporting the lath, and a third person at the further end, who held the other end of the lath, and fired the spirit; and sometimes held the spoon, while a fourth person fired them by repulsion. In this experiment instead of common thread, he used silver and gold twist, which he thinks much better than the former.

An Extract, by Philip Henry Zollman, Esq., F. R. S. of a Philosophical Account of a new Opinion concerning the Origin of Petrifactions found in the Earth, which has been hitherto ascribed to the universal Deluge; as contained in an Italian Book, entitled, De Crostacei ed altri marini Corpi che se trovano su' Monti, di Anton. Lazzaro Moro, Venice, 1740. *Communicated with several Remarks, by Dr. Balthasar Ehrhart, Physician in Ordinary at Memmingen, and Member of the Acad. Nat. Curios. in High-Dutch at Memmingen,* 1745, *4to.* N° 479, p. 163.

The Italian author has adopted a new system concerning marine petrifaction, the cause of which he refers to fire instead of water, according to the opinion commonly received. The place of his abode has furnished him with particular opportunities of comparing marine petrifactions found in the mountains with the true marine bodies produced by the sea. This place is called San Vito di Tagliamento, 6 hours journey from Venice, under the Bishop of Concordia, belonging to the Patriarch of Aquileia.

The author is a clergyman, who keeps a boarding-school for young men. He has published the book in question at his own expence, in which he shows a great conformity to the principles of Sir Isaac Newton, and other modern philosophers, not very common in Italy, grounding himself on experience, and mathematical proofs.

Having in the first part formed the state of the question, he examines the systems of Burnet and Woodward, almost generally received by the learned, though the former does not make any express mention of petrifaction. He refutes their opinions about the deluge, and of its being the cause of petrifactions. He lays down for a fundamental maxim, that the deluge ought to be believed, according to the scripture, as a miracle, and not to be proved by natural rules; from which he proceeds to another, viz. that whoever lays down for a foundation a principle which does not fit the several phenomena, builds on an erroneous principle.

After having refuted at large Dr. Woodward's opinions, he proceeds to establish his own system, grounded on subterraneous fire, with various arguments of his own, and with the refutation of those of others. He first lays down some general principles, according to Sir Isaac Newton, &c. and then applies to them several instances for supporting his system.

The first is the new island risen out of the sea in the year 1707, near the island Santorini in the Archipelago. The second is a mountain, which rose out of the earth in 1538, near Pozzuolo in the kingdom of Naples, overwhelmed the little town Tripergula, and dried up a navigable lake named Lucrano; being now called the New Mountain, equal in height to a neighbouring old one, called Monte Barbaro. From the circumstances attending those events, he endeavours to prove his new hypothesis.

He calls in aid the several eruptions of the mountains Vesuvius and Etna; and then forms his thesis, viz. "That marine animals and productions, for instance, shells, &c. which are now found in high mountains, were first generated in the sea; but when those mountains were raised, by subterraneous fire, above the surface of the sea, they were petrified so as they now appear." This thesis Moro endeavours to support, by giving the detail of the 12 several strata found in the territories of Modena, when they are digging for wells, mentioned by Woodward, Camerarius, Vallisnieri, and Ramazzini, whose remarks, as well as the newer ones of Whiston and Bourguet, he will not allow to be satisfactory; the greatest difficulty being this, that, from the nature of some of those strata, it seems that the sea has twice covered the plain of Modena, now above some hundreds of feet above the level of the sea; and that from another stratum it may be inferred, that, in the intervals between those overflowings of the sea, the land has been inhabited and cultivated. His thesis he endeavours to support by a remarkable passage from Pliny, book ii, chap. 87. Ingens terrarum portentum L. Marcio, Sex. Julio Coss. in agro mutinensi! namque montes duo inter se concurrerunt, crepitu maximo assultantes, recedentesque; inter eos flamma fumoque in cœlum exeunte, &c.

Dr. Ehrhart compares with this the several strata found in digging in the neighbourhood of Memmingen last year.

Moro touches next on the hypothesis of some, that the sea increases about 1 foot in height in about 2 centuries; and of some others, that it decreases 5 feet in 1 century; as also, how the saltness of the sea may be deduced from his hypothesis.

Further Observations and Experiments on the Passion Flower, and its Farina. By Mr. Badcock. N° 479, p. 166.

After the calix, petala, &c. are stripped off, the flower first presents to view a double row of purple threads: these threads appear as in fig. 2, pl. 5, on which we may plainly perceive a sort of capillary tubes. Here we may be at a loss for a passage for the acting matter of the farina; we must therefore look further. On cutting these threads longitudinally, they appear in many places as this before us, and are often pretty full. The occasion of these appearances, fig. 3, he owns he is not botanist enough to solve, nor will the first magnifier give him satisfaction. At the bottom of these, set round the stem, is a single row of small threads, not exceeding half an inch: these appear to have much broader heads than the long purple threads around them; and being so well secured and fortified from injury, may be of great use and consequence to the flower; yet they appear set in the same manner, though the tubes do not rise so high.

On the top of the flower are 3 stamina placed on the uterus: these are set in a manner described before with tubes, but, on making a longitudinal section, he could not find them carried on in any shape. In the uterus he could not observe any tubes at all; nor any appearance remarkable, till coming to the bottom of the stylus; and then, by degrees, from a smaller to a greater it rises, till the appearance becomes as represented in fig. 4.

On examination, he found the 5 appearances to answer the 5 stamina, on which the apices are set; and from this appearance, growing nearer and nearer to each other by degrees, they join at last all in one in the stalk of the flower.

Concerning an Improvement of the Weather-Cord. By Mr. Wm. Arderon, F. R. S. N° 479, p. 169.

The weather-cord is an hygrometer of a very ancient invention; and, if properly constructed, may be made use of with very good success, to show the various alterations of the atmosphere, in respect to moisture and dryness; but, as commonly made, it never rises or falls sufficiently to point out such minute changes as the curious would be desirous to know. A sense of this defect set him on endeavouring to find out some method of removing it, as follows:

In fig. 5, pl. 5, he only fixed the end of the index AB fast to the silk CE at A, leaving it lying loose on the point D; and in this manner the other end of the index would nearly describe the arch FGH; but then he soon perceived, that the centre of motion, on which the index turned, was changed whenever it moved ever so little; and consequently that the arch struck by the end B must be irregular.

On considering this, he toothed 2 pieces of brass, as 1, 2, and 3, 4, fig. 6, to fit each other so exactly, that on the least motion of the one, the other would move; then fixing the index on the centre C, its motions were rendered much more regular. He placed also a little collar of brass at B, on the cord SR, and to that collar tied the silk, which gave motion to the index, that the cord SR might twist and untwist without any impediment.

If there be no weight placed at bottom, as x in fig. 5, the piece of brass, 1, 2 must be so heavy as to keep the cord SBR at a convenient tightness, and also to counterbalance the end of the index CE, provided it be heavier than the other.

A Description of a Clepsydra or Water Clock. By the Hon. Charles Hamilton, Esq. N° 479, p. 171.

* Fig. 7, pl. 5, represents this machine in perspective. Here an open canal, ee, is supplied with a constant and equal stream, by the siphon d; and has at each end ff, open pipes, of exactly equal bores, which deliver the water that runs along the canal e, alternately into the vessels g1, g2, in such a quantity, as to raise the water from the mouth of the Tantalus s, to the top of the Tantalus t, exactly in an hour. The canal ee is equally poised by the 2 pipes f1, f2, on a centre r; the ends of the canal e, are raised alternately, as the cups zz are depressed, to which they are connected by lines running over the pulleys ll. The cups zz, are fixed at each end of the balance mm, which moves up and down on its centre v.

n1, n2, The edges of 2 wheels or pulleys, moving different ways alternately, and so fitted to the cylinder o, (by oblique teeth both in the cavity of the wheel, and on the cylinder; which, when the wheel n moves one way [i. e. in the direction of the minute hand] meet the teeth of the cylinder, and carry the cylinder with it; and, when n moves the contrary way, slip over those of the cylinder, the teeth no more meeting, but receding from each other; or it may be done by catches or locks, which require a longer description) one or other of these wheels nn, continually moves o in the same direction, with an equal and uninterrupted motion; for the contrivance is such, that the instant one ceases to act, the other begins, and so on.

A fine chain goes twice round each wheel, having at one end a weight, x, always out of water, which equiponderates with y at the other end, when kept floating at the surface of the water in the vessel g, which y must always be. The two cups zz, one at each end of the balance mm, keep it in equilibrio, till one of them is forced down by the weight and impulse of the water, which it receives from the tantalus s t i; each of these cups zz, has likewise a tantalus of its own hh, which empties it after the water has done running from g, and leaves the two cups again in equilibrio; q is a drain to carry off the water.

Fig. 8 shows the front of the clepsydra, with the dial-plate, the hour and minute hands, and the weight and float belonging to n2. The front of the tantalus

* N. B. The letters of reference answer to all the 3 figures; some being seen in one, that do not come in sight in the others.—Orig.

in g2, marked s t i, of which s the mouth, is 18 inches above the bottom of the vessel g, and 18 inches below the top of the tantalus t; i is the issuing leg of the tantalus, which discharges the water out of the vessel g into the cup z, as soon as it runs over the top t, till the water sinks as low as s.

Fig. 9 shows the profile of the clepsydra. And fig. 10 the plan of the same. The case uu incloses the whole machine, except the cistern that supplies the siphon d, which may be placed at any distance from it, as is most convenient, provided the issuing leg d, of the siphon is lengthened out so as to give a constant stream into the canal e. This case uu supports the axis of the cylinder o behind, and the dial plate pp before; in the centre of which turns the axis o, with the index k at its extremity, being the minute hand. The hours may be described by two common wheels, as in ordinary clock-work. For cheap work, chains passing round pulleys would do, instead of wheels with teeth.

The motion of the clepsydra is effected in the following manner: the short leg of the siphon d is placed in a cistern, with its mouth something below the mouth of the waste-pipe; which cistern is supplied with a constant stream, rather more than what runs out at the siphon d, which overplus going off at the waste-pipe, the water always remains at the same height in the cistern, and yet always delivers a constant and equal flow into the canal ee; consequently there is not the least intermission. As the end of the canal e, fixed to the pipe f1, is in the figure the lowest, the water runs all through the pipe f1, into the vessel g1, till it runs over the top of the tantalus t; when it immediately runs out at i into the cup z, at the end of the balance m, and forces it down, the balance m moving on its centre v. When one side of m is brought down, the string which connects it to f1, running over the pulley l, raises the end f1, of the canal e, which turns on its centre, higher than f2; consequently all the water which constantly runs through the siphon d, instantly runs through f2 into g2, till the same operation is performed in that vessel, and so alternately.

As the height the water rises in g in an hour, viz. from s to t, is equal to the circumference of n, the float y rising that height along with the water, lets the weight x act on the pulley n, which carries with it the cylinder o; and, giving a revolution, makes the index k describe an hour on the dial-plate. This revolution is performed by the pulley n1; the next is to be by n2, while n1 goes back, as the water in g1 runs out through the tantalus; for, y must follow the water, as its weight increases out of water.

The axis o always keeps moving the same way; the index k describes the minutes; the tantaluses must be wider than the siphon d, that the vessels gg may be sure to be empty as low as s, before the water returns to them.

Of an Operation made by the high Apparatus, according to M. le Cat's Method, in the Year 1743. *By Claud. Nic. le Cat, M.D., F.R.S. Translated from the French by Philip Henry Zollman, F.R.S.* N° 480, p. 175.

Joseph Bunel, 12 years of age, was afflicted with the stone for 8 or 9 years past. May 17, 1743, being the day appointed to cut him, he prepared to do it by the lateral apparatus, which is the common method. At 5 in the morning the patient was placed in the usual manner for the operation; and endeavouring to enter the probe, in order to cut him laterally, M. le C. found the stone so large, or at least so far advanced in the neck of the bladder, that he had all the trouble in the world to make the instrument enter; nor could he effect it, otherwise than by thrusting it quite on the side. He put his finger into the anus, and was convinced of the reality of the circumstances which the probe had made appear: he immediately thought that the situation of this stone in the neck of the bladder would hinder him from bringing the instruments freely to it; that as this hard body seemed exactly to fill the bladder, it would be impossible to introduce a pair of pincers between it and the inside of the bladder; that its bulk being too considerable, it would cause a mortal laceration by the low apparatus; and lastly, that the patient was in such a case of necessity, as required to have recourse to the high apparatus.

Not having suspected all these particular circumstances, and not being much inclined to make an operation in town, which he had as yet made but once, he had not brought his instruments for cutting by the high apparatus; but seeing it would be a rashness in this case to perform the lateral operation, and not to prefer the high apparatus to it, contrary to all his experiments and principles, he put off the operation to the next day, and sent to Rouen for instruments for cutting by the high apparatus.

That day at noon he gave the patient a very light soup. Next day, the 18th, he cut him by the high apparatus at 8 in the morning. The patient was placed on one of those little beds which the turners make for the country people; the 2 extremities of the bed were raised, especially that which was to support the thighs, and the middle was hollow; the whole covered with alaizes.*

The patient's head was towards the light; his hands and feet were tied across the bed, in the places where they came to lie, when the patient was laid on his back; his legs were stretched and open; his arms brought down below his hips; in short, he was in a very convenient posture. M. le Cat was on the right side of the patient.

In this manner he thrusted an algalie (or hollow catheter) into the bladder, introducing it on the side, as he had done the day before. He injected a syringe-

* These are cloths folded several times, and laid under the patients, to keep them cleanly.—Orig.

full of lukewarm water into the bladder; which occasioned a protuberance not immediately above the pubis, but 3 or 4 fingers breadth higher; which plainly proved, that the stone took up the whole exterior and inferior region of the bladder. He opened the teguments, and pushed his instrument into the bladder close to the pubis; but finding there only one membrane and the stone, he was obliged to bring the edge of his knife upwards, and then the instrument really entered, and abundance of water and urine came forth.

He turned again his instrument, to support the bladder with the projecting part, which is on the back of it. He slipped over it the straight suspensor (a catheter that opens with a bow), and dilated the bladder with the incision-knife, towards the pubis, and introduced the lateral suspensors. He put his finger into the bladder, and having felt a stone which was above the first, he pulled out this upper stone with the forceps, which broke between the lateral suspensors. He put his finger in again, and felt another; which he took hold of, and pulled out, taking it for a third stone, though it was but a fragment of the first, which had escaped the forceps.

This fragment being taken out, he put his finger in again, and felt distinctly that the bladder was parted into two chambers, like a gourd. In the upper hindermost chamber, which he had opened, the injection was lodged, and the stone which he had taken out. In the lower chamber he felt the great stone, which went as far as the neck of the bladder, the top of which was surrounded by the bladder like a neck, the opening of which did not admit more than the tip of a finger, with which he felt that the partitions of the bladder were closely united, and adhered to the surface of this stone.

These melancholy discoveries made him very uneasy; this particular structure of the side of the bottom of the bladder making him sensible of the same impossibility of introducing his instruments, which he had met with towards the perinæum. He tried to thrust his finger by force between the stone and the bladder by setting the nail strongly against the stone, and loosening the bladder from it, which seemed to adhere to it, dilating at the same time the bladder with all the force his finger was capable of. He at last introduced his finger to a certain length, with which he loosened the adherent parts from the stone all along, as far as his finger could reach; then he tried to pass in the forceps, afterwards the different scoops, but all in vain. He was for an instant believing that he could not get it out. This frightful idea made him redouble his endeavours.

He began to dilate again with the forefinger of his left hand, and, with the same finger, and the thumb of his right hand in the anus, he violently thrust the stone upwards towards the belly, after a long and painful labour, both for the patient and the operator. He introduced on his finger, which was between the stone and the bladder, the small scoop of the double crochet quite beyond the stone, and then he began to pull, having wrapped some linen round the instrument which hurt him; but the pubis, under which the stone was, caused an obstacle, not to be overcome. He therefore ordered an assistant to push through the anus the stone towards the belly; and he on his side having discovered the end of the stone under the pubis, put in the fore-finger of his right hand, and pressed as hard as possible the stone with it towards the spoon, which at the same time he strongly pulled with his left hand. These 3 forces being united, made at last the stone give way, and brought it forth to the joy of the spectators, as well as the patient. The upper side of the stone, or that which answered to the pubis, was pretty even, and seemed to give a passage to the urine; the under or hindmost side, which lay towards the prostates and the entrance of the bladder, kept the shape of these parts; its substance was crumbling, of a reddish colour, and like the membrane of the bladder, having a fungous poile on it, with which it seemed to have been incorporated, and of which a sort of covering yet remained on its surface: this covering being taken off, and the stone a little dried, this whole appeared to be pierced with porosities, which seemed to have been the receptacles of the foresaid poile. There was no mark on this side of any urine having passed.

A pledget charged with digestives, a rag dipped in an embrocation, and an emollient cataplasm, were laid on the wound; the patient was laid on his belly, his breast being supported by one bolster, and his head by another. The same day, an hour after, he was let blood, 3 porringers full. He found himself a little out of order after bleeding, and had several nauseas. He had a simple cordial made for him with 5 oz. of balm-water, 1 oz. of syrup of barberries, and 1 oz. of syrup of clove-July flowers; of which he took several spoonfuls a day, when his spirits seemed to fail him.

He sweated much all the afternoon; which made the Dr. defer bathing him till half an hour after 7; his pulse was short, quick, and strong: the first hour he was in the bath made no alteration; he was sick at heart, and spit up a white froth; at last he fell asleep in the bath for at least an hour: his pulse seemed to be slower, and on coming out of the bath he found himself very well. He was dressed as before at 10 o'clock, and laid on his belly. He slept best part of the night, which hindered his being blooded at 3 or 4 o'clock, as had been ordered.

May 19th. At 8 o'clock in the morning he was in a strong fever. He had 2 porringers of blood taken from his arm: the bleeding made his pulse short, low, and quick, and made him sick at heart. His pulse grew strong again, and the fever came on him. He designed to bathe him yet that morning; which should have been done if he had been blooded earlier; but it was late; and besides he had had no stool. He therefore thought it necessary to give him an emollient and anodyne clyster at 10 o'clock. He was afterwards dressed; and the bathing deferred till 4 in the afternoon: but the clyster not being come from him, and complaining of a pain in his stomach, he was ordered to be put in the bath from half an hour past 11, till half an hour past 1. He found himself very well in the bath; his clyster there came from him, and he had a good stool. He was put to bed again at a quarter past 1: there he had another stool, and found himself very well; yet he was still very feverish. He slept from half past 4 till half past 6; after which his fever was almost half gone.

He was put into the bath again at half past 7 in the evening. He desired it himself; partly on account of the ease which it had hitherto given him; partly because the situation of lying on his back, and sitting in the bath, was an agreeable change from that of lying on his belly; which he was obliged to do in his bed. His pulse was very well in the bath; his look and eyes were more clear than in the preceding bathings; and indeed he neither had any pain in his belly, at his stomach, in his breast, nor in his head; the wound only smarted now and then; he could not bear being touched about it; however his belly was very flat, and he began to be hungry. He staid an hour and half in the bath, and slept good part of the night and morning.

May 20th, he did not wake till 8 o'clock: he had hardly any fever at all, and he found himself much better than the day before. M. le Cat ordered another clyster to be given him, on account of the benefit which he had received from it the day before, and because he had not been at stool since. It made him sick at stomach again, and did not come from him.

He was put into the bath at 9 o'clock. Part of his clyster came from him there an hour and half after. At every bathing he was ordered a spoonful of cordial at going in, and towards the middle of the bath a mess of broth, or 2 half messes. He came out at the end of 2 hours. After this 4th bathing he found himself almost without any fever; and was so well, that he teized them for something to eat. This determined M. le Cat to let the suppuration quietly fix itself; which seemed already to begin at the wound of the teguments. He made water this day once or twice through the penis.

May 21st—In the morning, he was without a fever; and the distemper which he complained of most, was hunger. M. le Cat then set out for Roüen. The patient continued to grow better every day. M. le Cat returned the 10th day, to prescribe him a less strict diet. The cicatrix of the bladder had formed itself on the 20th. That day no urine at all passed any more through the wound of the belly, a large bandage was laid over the wound, and he was sometimes put on his back; especially after he began to make water through the penis. The wound of the teguments was entirely cicatrised on the 40th day; and he was so well, that he came to see M. le Cat at Roüen on the 50th day. He was grown so fat, that M. le Cat hardly knew him again.

Of an Hygrometer made of a Deal Rod. By Mr. William Arderon, F.R.S. N° 480, p. 184.

Mr. A. perceived common deal expanded itself very much laterally, or across the grain: and this he imagined, if properly used, might show the different degrees of moisture or dryness in the air. These thoughts set him on searching the Philos. Trans. to see if any ingenious person had recorded his opinion on this subject: and he found in N° 127, that an anonymous author had made several attempts to construct hygrometers of deal boards; and again, N° 129, that Mr. J. Coniers had added some improvements to it; but as the method taken by these two gentlemen seemed liable to some objections, he determined to make a trial on a plan and form entirely different from theirs; and has been so fortunate as to succeed greatly beyond his expectations.

His way was thus: he procured a piece of coarse deal board; mostly of sap. From this he sawed 7 pieces across the grain, 10 inches long and an inch broad; and as the board was just an inch in thickness, he consequently obtained 7 parallelopipeds of an inch square each.

These 7 pieces of deal he joined together, lengthwise, with strong glue; which made a square rod of 70 inches long. He found it necessary to place these small pieces in such a manner, when he glued them together, in respect to their grain, as is represented in the 2 figures following, to prevent their forming themselves into a sort of curve; which they naturally do, if they are placed all the same way; and he found himself obliged to fix the rod in such a number of brackets as appear in the drawings, in order to keep it straight.

He placed this rod at first perpendicular to the horizon, between 2 pieces of wood of the same thickness, and nailed against the ceiling of his room; but then he had one side only exposed to the air: however it acted tolerably well, which encouraged him to try to make it more perfect; as you will find delineated, fig. 1 and 2, pl. 6.

Both these deal rods were placed against the ceiling of the room with brackets, and were buttoned down into square mortises in each bracket, with small pieces of deal, that fitted their tops exactly. Thus all their 4 sides became exposed to the air; and the only difference between them was, the increasing the effect of their variation by 2 different methods.

To the rod at fig. 1, he added 2 levers: the first of which ABD had its shorter end AB but 3 inches in length, and its longer BD 12; consequently the end D moved through 4 times the space that the end A did. The second lever EFG he fixed to act with the former. The shorter end EF of this lever was 3 inches, and the longer end FG, 45 inches; by which the effect of the other was increased 15 times, and that of the deal rod 60 times. So that if the rod lengthens but $\frac{1}{15}$ of an inch, the point of the lever G moves 6 inches; and if the rod lengthens but 1 inch, the point G moves 60.

To this hygrometer he fixed a small index, such as is common in Mr. Haukesbee's barometers, to slip up and down on a wire, as is represented at K.

Fig. 2 represents another method he employed to increase the power of the deal rod. This may be fixed in a much smaller compass, and yet is no less capable of showing the minute differences in the moisture or dryness of the air, than the other The deal rod in this was managed and fastened in the same manner as shown before. He also applied a lever ABD to the top, exactly of the same dimensions as in the other; but, instead of a second lever, he placed a graduated circle, with an index, like that of the minute-hand of a clock. This he fixed to a small axis, which was moved one way by a silken thread wrapped twice or thrice round it, one end being tied to the longer end of the lever at D, and the other way by the gravity of the weight W. And here, if the length of the index RS be 15 times as long as the semidiameter of the axis which the silk turns on, it is evident that the sensibility of the rod's alteration will be increased 60 times, &c.

The deal rod is strongly nailed down at N, both in the first and second draught; but, in all other parts, they have free liberty of contraction or dilatation.

He observed, that heat and cold have a considerable power of lengthening and shortening the deal rod, as well as the moisture and dryness of the air; and this at first sight would induce one to imagine, that it would be rendered almost useless; but it is really far otherwise; for, by placing it near a thermometer, it is easily rectified with respect to its expansion or contraction, by heat or cold, at the same time that it truly shows the various degrees of moisture or dryness in the air. In short, it is an instrument made very easily, of materials to be got almost every where, and of little cost. It is capable of being serviceable either by sea or land, and may be placed in any direction.

Concerning the Farina fœcundans of the Yew-Tree. By R. Badcock, Esq. N° 480, p. 189.

March 3d, strolling round his garden in search of objects, Mr. B. observed a yew-tree in blossom, but having at that time only the bud, fig. 3, pl. 6, which, taken out of its case, may be seen with its umbilicus *a*, at fig. 4. That every flower has its farina, has been justly advanced; and as it was his design to observe that, he took a branch into the house; but making but a slight observation at that time, he laid it on half a sheet of brown paper in a warm drawer; and, to his great surprise, coming to examine it a few hours after, he found the whole number of the buds blown out into full flower, and such a quantity of farina on the paper, that it seemed more like a paper of brimstone than any thing else.

He then no longer neglected a thorough examination, which he began and completed in the following manner:

Fig. 5 is a separate view of its partitions, before it is near blowing. Every 5 of these go to a flower, and divide properly for blowing, some small time before they burst: there are sometimes 6 to a flower, though seldom: they open at the bottom aa, and immediately, letting drop the farina, turn themselves up; so that the top, which now appears the head, will when blown be the centre of the flower. (See an explanation of this at fig. 6, where the whole division makes the flower, and the strokes the division of the petala). a is the stem of the flower: now the bottom opening discovers this stem; and the easy transition of turning very near inside-out, makes a complete flower, fig. 7, the shape of which is seldom exact in any 2; though there are near 12 flowers formed by the bud. (See the back part of one, fig. 8, in which those ridges should show its division on the back.) We come now to the farina, fig. 9, which matches the rest of the flower, as to irregularity; there scarcely being 2 alike; and, when viewed opaquely, has a great resemblance to the small pieces observed in a paper of gum Arabic; it is opaquely of a clear white; but, when laid on paper in a quantity, appears like flower of brimstone, only paler. Its action is as various as its shape. (See several forms, fig. 10.) It seems to be only fixed in one particular, which is, that though there are ever so many different shapes, when dry, water once put to them, makes them all round, before any action begins: a proof that there is a suction.

This flower has neither apices, stamina, nor stylus; which is the reason why so much farina is shed.

Of a Bristle that was lodged in a Gentleman's Foot, and caused a violent Inflammation. By Mr. Arderon, F. R. S. N° 480, p. 192.

John Wood, Esq. of the city of Norwich, being afflicted with great pain, and a violent inflammation, in one of his feet, applied to Mr. Castil, an eminent surgeon, for his assistance; who, on strict examination perceived a few short hairs sticking out, not far above the setting on of the tittle toe. Their thickness, and particular manner of standing out, put him upon taking hold of them with his forceps; when, to his great surprise, he extracted a large hog's bristle, of near 4 inches in length.

The gentleman had immediate ease, and got well in a few days, without any other assistance. But he had no guess how the bristle got into his foot; unless by some accident it slipped into his stocking, and from thence worked its way in.

Had Mr. Wood deferred his application to a surgeon, till the bristle had been entirely buried in his foot, how miserable might he have been? What dreadful operations in surgery might he not have undergone, without the least probability of having his malady found out, or obtaining a cure for it? Probably many desperate cases in surgery may arise from such accidents as this.

Observations on the Spina Ventosa. By the late Claudius Amyand, Esq. F.R.S. N° 480, p. 193.

What practitioners generally understand by the spina ventosa, is a caries in the bone, from the extravasation of some sharp juices within it relaxing the tone of the fibres, and swelling and increasing its bulk beyond the natural bounds.

In this case, the humour, or extravasated juices, pent in the bone, works its way out of it, through the external cortex, or into the joints, or both. By detention it acquires an acrimony; and, like vinegar, and other acrid juices, it not only relaxes the tone of the bony tubes, by mollifying them, but also, like a caustic, it tears and lacerates them. At this time the bone swells, tumefies, and spreads; and the sap flowing, running out through the lacerated tubes, overspreads the surface, and adds to the tumefaction, as the liquid matter, forming a callus, is indurated there. So that, when this happens at or near the joints, the bones in contact are knit together, and the cariosity is incrusted and covered with an exostosis, in as many places as the matter confined within the bone, on breaking its cortex, will work its way out at. And thus this distemper may be considered differently, as it happens to be in its different stages.

The alteration the bone suffers from the extravasated matter lodged within the substance or cavities of the bone, in the first stage of a spina ventosa, becomes the occasion of some exfoliation or detachment from it. As that matter acquires a greater acrimony, the texture of the bone being relaxed, and the lamellæ made soft and yielding, the bone is enlarged in its dimensions; and, in the last stage of it, in which the bone is carious, the corrosive matter destroys the continuity, as it makes its way through the cortex, and into the joints. At this time imposthumations appear in as many places as the matter can make its way out at. The callous matter lodged under the periosteum, gradually ossifying, covers the bone more or less with exostoses; and the joints are stiffened, by the extravasation and induration of the sap flowing out of the bone there.

The imposthumations that happen in the bone towards the centre of long bones, are always attended with additional mischief, as the working out of the matter there meets with a greater resistance from their lamellæ, which lie close, and are compact; the exfoliations made in the first stage, and, as it were, in the beginning of the spina ventosa there, being frequently confined and locked in by the cortex of the bone, or some callous expansion on its surface. In the last stage of this distemper in this place, the bone is usually perforated with large holes, tubu, lous cavities, and fistular openings, and the main bone rotten, at a time that the exfoliations inclosed preserve their primitive state and solidity. The patient then cannot survive it long: for, as a hectic fever and diarrhœa are fed by the continual absorption of some of the matter into the blood, so the body is drained by the large discharge from such wounds. The amputation of the limb is then the usual remedy; but a better may be hoped for, before it comes to this pass, as will appear from the following observations.

Observation 1.—One Thomas Pentney, a shepherd in Norfolk, aged about 22, was admitted into St. George's Hospital, towards the beginning of August 1739, and committed to Mr. Middleton's care. Five years before, on the crisis of a fever, he had felt a great pain in the os humeri of his right arm, which continued several months; during which, the dimensions of this bone were so increased, that, towards the end of the year, it was half as large again as is usual in the natural state. About this time the bone imposthumated; and the matter being discharged by the breaking of the integuments on the outside of the arm, the patient was eased, so as to have been able to attend his flock as before.

When he came to the hospital 4 years after this, he had above 12 holes through the integuments on the outside of the arm, answering to, and corresponding with, as many leading into the medullary cavity of the bone; some of which were large enough to admit the finger. These imposthumations had been most vexatious of late. There was an anchylosis at the elbow; and, for 2 years last past, he neither could bend his arm, nor use it in pronation and supination. The whole bone felt thick and unequal up to the shoulder, where the articulation was free. The patient now did not complain much. The discharge from the wounds was moderate; his rest, stomach, and pulse, as well as could be expected; and therefore he was determined to forego any thing, rather than submit to the amputation that was proposod.

In consultation with Mr. Pawlet, Mr. Wilkie, Mr. Middleton, and Mr. Hawkins, it was agreed to make an incision from the deltoid muscle down to the elbow, thereby to lay the distempered bone bare, so far as it appeared to be affected; and, with the exfoliative trepan, to make a fair opening into the medullary cavity of the bone, by taking off so much of it as was perforated in the external part of the arm, and so to make way for the application of the actual cautery, if that was found necessary.

The first 2 operations were performed with ease: for, as in the incision the periosteum was readily detached from the bone, so the bone in view, being nearly of a cartilaginous nature, and making little resistance, was soon removed, by the repeated application of the exfoliative trepan. In the course of the operation, Mr. Middleton found a bone loose, which hitherto had lain concealed under the cortex of the bone; which, being taken out entire, measured above 7 inches in length, and more than 2 in circumference; the centre forming a tubular channel, wherein the medulla of the bone had formerly been inclosed. This was an exfoliation fairly separated from the surface of all the medullary cavity of the bone, but improperly called an exfoliation, as the thickness of the bone throughout was as thick as a shilling: it was more substantial in some places than in others, and opened here and there, so as not to be a complete tube. But what perhaps may be thought to deserve more notice is, that this loose bone, though it had for many years been soaking in the matter lodged in the bone (which, at times, had worked its way out, by perforating the surface of the os humeri externally), yet this exfoliation was in nowise altered, or tainted with cariosity, as the main bone was; but in every respect so sound, as to give ground to hope the posterior part of the os humeri might be so; but it happened otherwise.

On the removal of this exfoliation, the whole internal surface of the bone was found of a substance like a cartilage: it was bare in some places, and covered with flesh in others. The flesh was fungous in the lower extremity of the bone that was carious; firmer towards its upper end, where it was sound, and callous towards the middle, which was degenerated into a cartilaginous substance.

The removal of this exfoliation having laid open the whole bone, in the inferior part of it was found a sinus leading into the articulation with the cubit, and a cariosity in the bone there; and, in the upper end, where the bone appeared now sound, but formerly was distempered, several foramina, which were filled up with flesh, under cover of the deltoid muscle.

At this time the surface of the wound was very large, and the discharge from it greater than the patient could support. His stomach hitherto had been good; but that being defective, and a hectic fever with a diarrhoea attending, the amputation of the limb was concluded on. It was taken off near the articulation with the scapula, where the bone was sound. The patient did not survive it long; for the diarrhoea increasing, he died spent, within 4 days after the operation.

The humerus next to the shoulder-joint did not, after the patient was dead, appear so sound as it had during life: for the matter proceeding from the bone, which was found, in 2 or 3 places of it, under the deltoid and pectoral muscles, showed that this part of the bone was not in that sound state it appeared in, when the amputation was made.

Observation 2.—Nov. 5, 1739, Mr. Johnson, about 26 years of age, having complained for 12 months past, of a swelling in the bone of his right arm, which seemingly had been the cause of several imposthumations he had had thereabouts since, then applied to Mr. A. for cure. He could assign no cause for this swelling, saving his taking cold, as he imagines, after having exercised himself by flinging heavy stones to a great distance: for that soon after he was seized with a fever, and a great swelling from the neck to the finger's end of this arm; which settling towards the middle of the os humeri, where several fistular openings now were, and coming to suppuration thereabout in a short time after, a great quantity of matter was discharged by incision in Nottingham; when the surgeon had told him, that the bone was bare; and soon after had cut another opening in the hind part of his arm, where there was another gathering, and the bone also laid bare, and dressed both wounds, as expecting the bone to scale off; but that meeting with no cure, but, on the contrary, the wounds breaking open as often as they had been healed up, and that matter was still gathering in new places, and the swelling in the bone increased, had determined him to look for a cure in London.

At this time there were 5 or 6 fistular openings leading to the bone distilling a sanous matter on the sides of the tendon of the deltoid muscle, and the hind part of the arm, where the bone was principally enlarged; though it was very remarkably increased in bulk the whole way down to the elbow. Mr. A. could not with his probe discover the state the bone was in; but, being satisfied it was carious, and that this distemper was a spina ventosa, he proposed for the cure the laying open all the bone in the anterior part of the arm; which the patient readily submitted to.

This distemper was found to be a spina ventosa, or cariosity in the body of the os humeri, by which above 4 inches of the solid bone had been destroyed; all which was cased in by an exostosis, or callous expansion; saving in a few places, where the matter flowing from the medullary cavity of the bone had preserved an opening.

This spina ventosa was treated nearly in the same manner as the above-mentioned, and the cure performed as follows. It was entered on the 7th of November, by making an incision to the bone on the external part of the arm, about 6 inches long, and 1 broad, beginning it above the place in the bone where the deltoid muscle is inserted; but on the side of it, almost down to the supinatores radii; and then, by destroying with the lapis infernalis all the flesh growing on the exostosis or callous expansion encompassing round, and as it were incasing the carious bone, which the next day being scraped off, the fistular opening, leading into the medullary cavity, then came in view, and the probe going a great way into it, he immediately proceeded to trepan the bone, and enlarge that fistular opening into it with the exfoliative trepan, perforating through the callous expansion or exostosis, which was spread externally almost ¼ of an inch upon it, quite into the medullary cavity. The next day he applied this instrument above and below the preceding perforation; and, by cutting and paring off the angles between them and the sides of the perforations, with an instrument the engravers make use of, then made a fair opening into the medullary cavity of the bone, and a convenient one too; for the discharge of the matter hitherto confined within it, which whilst pent in had occasioned the cariosity, and the progress of it, now found to have destroyed above 4 inches of its body; and also for the removal of the fragments and loose bones confined in the medullary cavity, and the application of the necessary means, as well to stop the progress of the evil, as to promote the casting off of the morbid bones.

But the work was far from being finished, as, on the extracting the forementioned loose bones, it appeared that they had a fungus sprouting, as well from the circumference of the medullary cavity above and below that part of the bone which had been operated on, as from the callous expansion over its outward surface, occasioning a greater discharge of sanies than the patient's strength could support; that the opening in that cavity was not yet sufficient to discharge all the matter deposited in it, as well through a fistular opening in the internal part of the bone answering to the axilla, as another somewhat lower over the large vessels that run on the surface of the bone internally, that, being sheltered above and below, they could not come at them without enlarging further the opening in the bone externally. This laid them under the necessity of enlarging this opening in its upper and lower part, so as to bring in view the forementioned. These were enlarged with a terebellum, without any hazard of wounding the large blood-vessels, which were sheltered by the callous expansion lining the carious bone on this side: and having thus rendered easy the discharge from all these cavities, they had nothing to struggle with then but the running; which from this time became daily less; that from the internal part of the arm, by the matter having a more easy vent for itself; and that from the fungus on the bone by a solution of the lapis infernalis it was dressed with. Mr. Singleton, surgeon in Aldersgate-street, being consulted on the case, on the 18th day from the first operation, was of opinion, that the amputation was hardly practicable; the sinus into the medullary cavity showing that the cariosity was up to the head of the bone; but that, if it was practicable, he thought the patient had a far better chance for his life, by pursuing the cure in the way he was in. At this time they had but in part subdued the fungus that was continually sprouting up from the sinuous vacuity in the upper part of the bone, where the medulla was all wasted, as well as from that which was yet growing without the bone from the callous expansion by which the carious bone was covered: but this was so effectually overcome and conquered by the repeated application of the actual cautery, and by it the bone so dried up, that in less than 2 months from its use all the morbid bone had cast off.

The quantity of the morbid and carious bone taken out at divers times, being about 4 inches of the solid, was effectually repaired, by the matter flowing as well from the circumference of the callous expansion about it, which all along had steadied the patient's arm so, that he could pull his stockings on and off, as from the matter flowing from the ends of the os humeri into the cavity formed round this encasing or encircling bone. If they had been lingering in their proceedings, it is likely the great discharge would have exhausted the strength that was necessary to carry on this cure, before they could have got through so many that were unavoidable; and that the flesh growing from the edges of the external wound would have rendered more painful and difficult the several operations on the bone. By this proceeding, this difficult cure was ascertained in less than a month, and entirely finished in 6; the patient having now the power of his arm as complete as ever. Nor was the limb at all disfigured or shortened; the expanded encircling bone attached to the ends of the os humeri preventing this; so that the only appearing defect was, that the bone about the wound was thicker than usual; but that strengthens it, and supplies a defect in the anterior part of the arm, where there is a considerable hollowness.

In favour of the young surgeons, Mr. A. closes this account with a few remarks, by way of inference.

1. That it is highly probable, a suppurated phlegmon in the marrow, on the crisis of a fever, was the original cause of the spina ventosa in these 2 cases, and that if the bone had been denudated, and the opening through it enlarged, when the matter first made its way through the integuments, that the progress of the evil had been prevented in both, and the cure brought about in the last case with far greater ease.

2. That a large opening is always more advantageous than many smaller, seemingly equal to it. And this appears plain in the 2 cases mentioned, inasmuch as the matter which was discharging through the many large foramina in the bones corresponding with the medullary cavity in them, have not prevented the progress of the evil; and therefore it may be concluded, that as a large opening in the bone, by giving a free vent to the matter, will afford the nearest prospect of a cure in the spina ventosa of all bones, so that must be the work of the surgeon, when that distemper breaks out towards the centre of long bones.

3. That, in the spina ventosa affecting long bones towards their centre, the application of the trepan, or of any other instrument as shall take away a considerable portion of the substance, is particularly necessary, were it only to make way for the removal of such exfoliations as are detached from the inner cavity of the bone in the first stage of the distemper; which, in these 2 cases were concealed and shut in; and in many others may be wedged and locked in by the induration of the callous matter on the surface of the bone.

4. That in a spina ventosa, in the centre of long bones, though the discharge attending it is not great, if any of their joints are made stiff by an anchylosis, viz. by a callous expansion that shall solder together the bones in contact, the only resource will be the immediate amputation of the limb; forasmuch that, if that is delayed till the patient labours under a hectic fever, colliquative sweats, a diarrhœa,

or such symptoms as denote a reflux of the matter pent up in the bone into the mass of blood; the operation then will afford very little hopes of success: whereas the spina ventosa that affects the extremities of long bones only, and that which appears in scrophulous cases in the bones of the carpus and tarsus, when the discharge is not great, are best cured by lenient means, and the most pacific methods.

5. That in that stage of the spina ventosa, in which the bone is carnified, that is, turned into flesh, with a painful fungus shooting out, as well from the callous matter spread over the carious bone changed into flesh, as from the carious bone itself degenerated, that in this case, as there can be no hopes of restoring it to itself, the removal of the bone so degenerated, is the only method to be pursued; as that will make way for the application of the actual cautery, in which the cure principally consists: and if this does not succeed, we must proceed to amputation. This was the case of Mr. Coreho in St. Mary Axe, whose thumb Mr. Sainthill took off the 26th of October, 1739: the last bone of which, affected with a spina ventosa about 18 years, was so swelled out, and changed into flesh, that not the least part of this bone, as a bone, was found, but only its cartilaginous covering, in the articulation with the second internode; all the bone itself being nothing else but a lump of flesh.

Extract of a Letter from Mr. John Henry Winkler, Græc. et Lat. Litt. Prof. publ. Ordin. at Leipsic. Concerning the Effects of Electricity on Himself and his Wife. N° 480, p. 211.

When Mr. W. heard of Mr. Muschenbroek's experiment,* he tried the same; but he found great convulsions by it in his body. It put his blood into great agitation; so that he was afraid of an ardent fever; and was obliged to use refrigerating medicines. He felt a heaviness in his head, as if a stone was lying on it. It gave him twice a bleeding at the nose. His wife, who had only received the electrical flash twice, found herself so weak after it, that she could hardly walk. A week after, she received only once the electrical flash; a few minutes after it she bled at the nose.

A Catalogue of 50 Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1744, pursuant to the Direction of Sir Hans Sloane, Bart., P.R.S. By Jos. Miller. N° 480, p. 213.

[This is the 23d presentation of this kind, making up the number of 1150 different plants.]

* That with the gun-barrel suspended as the iron bar. See Transactions, N° 476.—Orig.

On Extracting a large Stone by an Aperture in the Urethra. By Mr. George Howell, Surgeon, at Haverfordwest. N° 480, p. 215.

John Cod, of Roch, in Pembrokeshire, aged 40, was from his infancy troubled with nephritic complaints, and frequent obstructions of urine. He, at 25 years of age, had a total suppression of urine for some days; when, on a stone rushing out of the bladder into the urethra, he had immediate ease from his pain; and was therefore negligent concerning its extraction from thence.

Five years after, he married; the stone remaining in the urethra; which was then, he imagines, about the size of a walnut. In 4 years his wife brought him 2 children. About that time he sent for Mr. H. and wanted him to dissolve the stone; he having just then read some treatises speaking largely of the powers of lithontriptic medicines. He told him, that admitting the power of the dissolvents of the stone might be very considerable, when the stone was in the bladder, and might be surrounded by the medicated urine, very little was to be expected in the posture the stone then lay; but if he was willing to have it cut out, he would undertake the operation. This he would by no means consent to; and so they parted at that time. He heard no more from him for 6 years.

In August last he had a message from his family, desiring his assistance, believing him dying; he having had a total suppression of urine for 3 or 4 days. Just as the servant was coming with the message, his urine had forced a passage through the superior part, or dorsum penis, and had flowed out plentifully.

On coming to him, Mr. H. proposed cutting out the stone; his answer was, he would submit to whatever he should think necessary; he being now convinced, that if he were not speedily relieved, his death would be inevitable.

The whole quantity of urine, contained in the bladder, had made its way through the new opening in the back of the penis; and therefore at present the inflammation of the penis being very violent, he deferred the operation, and only applied a fomentation and cataplasm, to take off that inflammation.

He returned in 2 days; and, by an incision into the urethra near 3 inches long, he extracted the stone, which had been there 15 years. It weighed 2¼ oz. and measured 6 inches round, in the direction it lay in the penis. About 50 small stones lay between this great one and the bulb of the urethra. These came forth from this incision. After this, he put in 5 pins, and made the twisted suture, and then introduced a leaden canula.

The urine passed regularly 2 days; when an inflammation came on, which tore out all the stitches. He tried once more, and took the greatest hold possible; but the urethra was so tender, that all endeavours to unite the lips of the wound were fruitless. The man was glad to compound for life, even with the loss of the penis; if that were necessary, which the stone had increased to an enormous bulk. But, after attending and dressing him about 2 months, the orifice, through which the stone was extracted, was reduced to ½ an inch in length from the frænum backwards. But although he introduced a leaden pipe near 3 inches long into the urethra, the urine never came through the natural orifice, but still continued to pass through the aperture.

The passage in the back of the penis, made by the urine before the operation, healed up kindly. He afterwards much rejoiced that the incision could not be healed, because he had voided several large stones that way; which he most probably must have been cut for again, had that been closed. For several years his urine did not drop, but by the introduction of a knitting needle, or such-like instrument, which would move the stone: and this operation generally took up 2 hours every morning.

It might easily be seen which way the stone lay in the penis, by observing some holes in it, which the patient had made with the knitting needle, hoping to push through it in time. These holes were in the fore-part, or against the natural orifice. The other end was found quite polished, by the friction of the small stones againt it. The superior part was like the glans penis: the stone being placed on a table, and observed at a small distance, it seemed not unlike a glans cut off, only larger.

A fortnight after the operation, his wife was delivered of a 3d child. She has the character of a very virtuous woman; but how the semen in coitu could pass by this stone, when the urine could not, but with such difficulty, Mr. H. did not pretend to determine.

Abstract of a Letter from Robert Southwell, Esq. to Mr. Henry Oldenburg, dated Kinsaile, Sept. 19, 1661, concerning some extraordinary Echos; lately communicated to the Royal Society by the Rev. Henry Miles, D. D., F. R. S. N° 480, p. 219.

The best whispering place I ever saw was that at Gloucester: but in Italy, in the way to Naples, 2 days from Rome, I saw, in an inn, a room with a square vault, where a whisper could be easily heard at the opposite corner, but not at all in the side corner that was near to you.

I saw another, in the way from Paris to Lyons, in the porch of a common inn, which had a round vault; but neither of these were comparable to that of Gloucester; only the difference between these last 2 was, that to this, holding your mouth to the side of the wall, several could hear you on the other side; the voice being more diffused. But to the former, it being a square room, and you whispering in the corner, it was only audible in the opposite corner; and not to any distance from thence as to distinction of the words. And this property was common to each corner of the room, and not confined to one.

As to echos, there is one at Bruxelles that answers 15 times: but when at

Milan, I went 2 miles from thence to a nobleman's palace. The building is of some length in the front, and has 2 wings jetting forward; so that it wants only one side of an oblong figure. About 100 paces before the house, there runs a small brook, and that very slowly; over which you pass from the house into the garden. We carried some pistols with us, and, firing one of them, I heard 56 reiterations of the noise. The first 20 were with some distinction; but then, as the noise seemed to fly away, and answer at a greater distance, the repetition was so doubled, that you could hardly count them all, seeming as if the principal sound was saluted in its passage by reports on this and that side at the same time. Some of our company reckoned above 60 reiterations when a louder pistol was discharged.

Part of a Letter from Mr. J. Durant, dated Newcastle, Feb. 9, 1673-4, to Robert Boyle, Esq. F. R. S. concerning a Coal Mine taking Fire near Newcastle upon Tyne; also of the Blue Well; and of a Subterraneous Cavern in Weredale; lately communicated by the same. N° 480, p. 221.

The fire at first was occasioned by a candle, negligently placed by a pitman, as he was working in a pit about 30 years ago. It was so small at first noticing, that half-a-crown reward was denied to one, who for that price would have engaged to extinguish it; now it has wasted land and mine, and grown so furious, as no hopes of its ceasing are conceived, before the failure of its fuel.

The grounds where it began belong to a village called Benwell, about a quarter of a mile northward from the river Tyne; whence, by a slow progress, and frequent deviations east and west, it marched northward; sometimes preying on the coals nearer the surface of the earth, and then subverting houses and grounds lying over it; sometimes on the deeper mines, and was conspicuous only by its smoke and fire in the night. Now it rages, and has already caused great devastation, in grounds belonging to a village called Fenham, near a mile northward from the place where it first was kindled.

Its eruptions at present are in many places, and various depths. I have, both last winter and this, in frosty nights, for then it burns most furiously, occasionally riding by, in near 20 places, seen its flames and pillars of smoke. That ever it has ejected stones, or the like, I cannot, by information or observation, affirm; the concreted salts we have from it being always found either candying the supercrescent furze, or impacted in the surface of the earth, at its eruptions.

There is a stream near this town, which on its banks in the summer time, as also when evaporated over the fire, leaves behind it a blue powder. Its head is thence called the Blue Well. There are some subterraneal grottos or caverns in Weredale, about 20 miles south-west of this place; where, by a little hole creeping into the side of a vast mountain, is entered a spacious cavity, chambered with walls and pillars of decident lapidescent waters; the hollowness in some places being pervious further than any yet has adventured to discover; the darkness of these caverns requiring the help of candles, which are often extinguished by the dropping water.

A Letter from James Parsons, M. D., F. R. S. serving to introduce a Remark from John Milner, Esq. concerning the Burying of the Cows, dead of the present reigning Distemper, in Lime or not. N° 480, p. 224.

When the means for preventing the infection among the cattle were under consideration, burying them was thought the most effectual method to obstruct its progress; and, by way of improvement to this project, the addition of lime was imagined necessary, for the more speedy destruction of the distempered carcases. But some doubts arising, whether the lime might not exalt the putrid particles, and help to spread the infection, it was the opinion of several of the learned, that it was most safe, on that account, to bury them without it.

This difference will probably be decided by the inclosed account of cattle buried both with and without lime, written by John Milner, Esq. one of the justices appointed to inspect into the affair, and one who has the good of mankind at heart as much as any person whatever. I hope it will serve to prevent the practice of burying them with lime for the future, as this accidental fact makes it more than probable, that malignant particles may be sent up, and spread through the air.

N. B. The cattle were buried 10 feet deep with lime; 8 feet deep without lime.

Mr. Stallwood, a farmer at Hackney, informed the justices, to whom the care of the distempered cattle was committed, that he had buried 13 cows very deep, with the quantity of lime appointed by the justices; and observing his dogs to scratch and tear up the ground with their feet, to get at the cows' flesh, (the lime fermenting, and causing a foam, as he called it, or strong scent of meat to arise, which made the dogs so eager to come at it) he beat them off several times; but the dogs always returning as soon as he was gone, for some time he hired boys to keep them off. But that he had buried several other cows in another place, with their hides cut and slashed, without any lime (being ordered by the justices so to do) and the dogs never attempted to scratch or tear up the ground there, though it lay open to them equally with the other ground, and they often ran over it.

N. B. Two bushels of lime to each cow was the allowance.

1

Critical Observations concerning the Oenanthe Aquatica, succo viroso crocante of Lobel; by Mr. W. Watson, Apothecary, F. R. S. occasioned by an Extract of a Letter from Mr. George Howell, Surgeon, at Haverfordwest, to the Author, giving an Account of the Poisonous Effects of this Plant to some French Prisoners at Pembroke.* N° 480, p. 227.

Extract of Mr. Howell's Letter.—" Eleven French prisoners had the liberty of walking in and about the town of Pembroke; 3 of them, being in the fields a little before noon, found and dug up a large quantity of a plant with its roots, which they took to be wild celery, to eat with their bread and butter for dinner. After washing it, while yet in the fields, they all 3 ate, or rather tasted, of the roots.

" As they were entering the town, without any previous notice of sickness at the stomach or disorder in the head, one of them was seized with convulsions. The other 2 ran home, and sent a surgeon to him. The surgeon endeavoured first to bleed, and then vomit him; but those endeavours were fruitless, and he died presently.

" Ignorant yet of the cause of their comrade's death, and of their own danger, they gave of these roots to the other 8 prisoners, who all ate some of them with their dinner.

" A few minutes after, the remaining 2, who gathered the plants, were seized in the same manner as the first; of which one died: the other was bled, and a vomit with great difficulty forced down, on account of his jaws being, as it were, locked together. This operated, and he recovered; but was some time much affected with a dizziness in his head, though not sick, or in the least disordered in his stomach. The other 8 being bled and vomited immediately, were soon well.

" There were in these men none of those comatose symptoms mentioned to have happened to the Dutch soldiers, who were poisoned by eating the cicuta major.†

" After I had done examining, I ordered some of the herb and root to be brought. I found it to be the oenanthe aquatica cicutæ facie of Lobel, which grows in great plenty all over this country, is called by the inhabitants five-fingered root, and is much used by them in cataplasms for the fellon, or worst kind of whitflow. The Frenchmen ate only the root, and none of the leaves or stalk."

The poisonous effects of this plant, in the instance beforementioned, exactly square with those mentioned of the same plant, in N° 238 of the Phil. Trans.‡

* *Oenanthe crocata.* Linn. † Philos. Trans. N° 473, Vol. ix. p. 38 of these Abridgments.
‡ Vol. iv. p. 242 of these Abridgments.

where 8 young lads, near Clonmel in Ireland, where this plant is called tahow, mistook its roots for those of sium aquaticum, or water parsnip, and ate plentifully of them. About 4 or 5 hours after, going home, the eldest, almost of man's stature, without the least previous disorder or complaint, fell down backwards, and died convulsed. Four more died in the same manner before morning, not one of them having spoken a word from the moment the venomous particles had attacked the genus nervosum. Of the other 3, one ran stark mad, but came to himself next morning. The hair and nails of another fell off. One of them only escaped without any harm, who ran home above 2 miles, and drank warm milk, which caused a diaphoresis. A Dutchman likewise was poisoned with the leaves of this plant, boiled in his pottage; which he took for smallage, and to which its leaves have great resemblance.

Dr. Allen, in his Synopsis Medicinæ, mentions an instance of 4 children, who ate of these roots. They indeed were in great agonies before they fell into convulsions. In their fits they vomited, which was encouraged by large draughts of oil and warm water; and by other proper care they all did well. He takes notice likewise of a pig dying in convulsions, from eating some of these roots, which it had grubbed up.

Stalpart van der Wiel, in his observations, takes notice of the deadly effects to 2 persons, who had eaten these roots, mistaking them for Macedonian parsley. These men also, soon after eating these roots, were troubled with violent heats in the throat and stomach, attended with a vertigo, sickness at the stomach, and purging. One of them bled at the nose, the other was violently convulsed. Both of them died, one in 2 hours, the other in 3.

It is very remarkable, that neither the French prisoners, who were killed at Pembroke, nor those before cited in the Phil. Trans. felt any heat or disorder in their stomach, before the attack of the convulsive paroxysms; whereas those mentioned by Dr. Allen, and Stalpart van der Wiel, were in great agonies, from the violent heat in their stomach and throat, before they were attacked by convulsions.

The same variety of symptoms we meet with in Wepfer, with regard to those people who were poisoned by the cicuta aquatica; where some of them, who had eaten the roots of this plant at the same time, stood and assisted their friends, till they died of convulsions, without feeling themselves anywise disordered, and afterwards, in their turns, died in the same manner. Others were violently affected by it, as soon almost as they had eaten it. Confer Wepfer's history with the *German ephemerides. Linnæus mentions, in the †Flora Lapponica, the great

* Ephemerid. Natur. Curiosor. Dec. 2. Ann. 6. Obs. 116.—Orig.
† See Flor. Lappon. p. 72.—Orig.

slaughter, and miserable manner, in which the horned cattle died, from eating this plant at Tornœa. This author also, in his Flora Suecica, acquaints us, notwithstanding Rivinus and Mappus have asserted, that the horned cattle not only eat this plant without detriment, but are very fond of it, that 3 oxen were killed by eating its roots. He was fully convinced that they were the roots of the cicuta aquatica; because, soon after this accident, the country people brought him some of them, desiring to know to what plant they belonged. He thereupon planted them in the academical garden, and was fully satisfied what they were.

Wepfer has confounded his cicuta aquatica, in the history of it,* with the poisonous oenanthe of Lobel; where he says, that Lobel has described the cicuta aquatica under the name of oenanthe cicutæ facie, succo viroso crocante; and mentions, that it is not very frequent, but in the northern parts of England by the sides of rivers, and in watery places; he adds, that Lobel has not been exact in his description. To which Mr. W. answers, that Lobel's description of the oenanthe is very exact, for the time he lived; and it is very evident that Wepfer never saw this oenanthe; which plant, Mr. W. believes, is not found in Germany. Wepfer likewise, in the Ephemerides Naturæ Curiosorum,† is under the same mistake: and tells you that Stalpart van der Wiel differs from him; and calls the plant, mentioned in his observations, oenanthe, as Lobel does; and though Stalpart has given figures of the plant accurate enough for a common observer to distinguish the plants by, and though 9 years elapsed between the publication of his book de Cicuta and his observations in the Ephemerides, he was still in the same error, and believed the oenanthe of Lobel, and his cicuta aquatica, as well as that of Gesner, to be the same poisonous plant. The accurate Hoffman also, when treating of vegetable poisons, makes no mention of this difference.

Neither the roots of the oenanthe of Lobel, nor those of the cicuta of Wepfer, have any flavour in them disagreeable enough to deter those who taste them from eating. They both occasion violent convulsions and death, if not timely prevented. The intention of cure seems in both to be the same, viz. first, by emptying the stomach and intestines as soon as possible, and then by causing the patient to swallow large quantities of oleaginous fluids. But it is to be observed, that causing the patient to swallow any quantity is attended with great difficulty, after he is attacked by the poison; because of the jaws being, as it were, locked together by the violence of the spasm. After the stomach is freed from this pernicious vegetable, the symptoms have generally diminished by degrees, and the patient recovered.

* Cicutæ aquat. historia et noxæ, p. 15.—Orig.

† Ephemerid. Nat. curios. Dec. 11. Ann. vi. Obs. 116.—Orig.

Threlkeld, in his Synopsis Plantarum, mentions, that he has seen great plenty of this oenanthe in Cumberland, where the country people call it dead tongue, and use it, when boiled like a poultice, to the galled backs of their horses.

Neither the German botanists,* nor Haller in his Enumeratio Stirpium Helvetiæ, mentions this plant as growing among them. Mr. W. believes therefore, it is seldom met with but in Holland, England, and in some parts of France; for Morrison mentions it growing in Bretagne near the mouth of the river Loire. This plant was communicated to Matthiolus by a professor of physic at Padua, (see Matth. p. 628). Linnæus, in the Flora Suecica, says, that he received it from a correspondent, who gathered it in Scania.

Lobel, and after him John Bauhin and others, take notice of this plant's growing in the northern parts of England. It grows also in the western and southern parts, by the sides of rivers, large waters, and sometimes by ponds. It grows near Bath. Dr. Allen mentions its growing within 3 miles of Bridgewater. Its being produced in Wales, is the occasion of this paper. Mr. W. had seen it very frequently by the sides of the river Thames, both above and below London. He had found it likewise by the side of a large pond near the road, in the town of Dulwich, not far north of the college; likewise by the sides of a large water near the mills, ¼ a mile south-east of Dartford in Kent.

Lobel is the first who has given a small figure and a tolerable description of this oenanthe, in his Adversaria Plantarum.† He has likewise represented it in the 730th of his Icones. This seems likewise to be the plant described by Valerius Cordus, ‡ under the denomination of olsenichium; and by Dodonæus, under that of apium sylvestre, sive thysselinum; § where the description, place of growth, and form of the roots, agree exactly with the plant under consideration; though his figure is execrably bad. This bad figure is copied, and the description translated by Gerard || in his Herbal, without making any mention of Dodonæus. This figure is likewise copied in Parkinson's Theatre of Plants. John Bauhin, Matthiolus, Gerard, Parkinson, and Morrison, have given figures of this oenanthe; but these representations give scarcely any other idea of the plant, than that it is an umbelliferous one, with roots divided like those of asphodel. Of these, however, Morrison's ¶ is the best; and his description, in his book de Umbelliferis, is very exact and copious. Mr. Ray's description is taken from Lobel. Mr. W. has at the bottom of the page * recited the various synonyma, under which this plant is mentioned among authors.

* Unless the olsenichium of Valerius Cordus, and thysselinum of Dodonæus, hereafter mentioned, are other names of the plant in question.—Orig.

† Adversaria plant. nov. 326.—Orig. ‡ Valer. cord. p. 149.—Orig. § Dodon. Pempt. 687.—Orig. || Gerard. Emac. 1020.—Orig. ¶ Morrison Umbel.—Orig.

* Oenanthe, de qua hic agitur, synonyma.—Oenanthe tertia Matthioli, p. 629; oenanthe, succo

As it appears, from what has been said, that the oenanthe of Lobel, and cicuta aquatica of Wepfer, had not been sufficiently distinguished by medical writers hitherto, Mr. W. hoped he should stand excused for making a few observations on this last. This, though a plant frequently met with on the continent, and very well described by botanical writers, is seldom found near London; but it grows in many parts of England by the sides of large standing pools, and near the banks of fens. He was informed by Robert More, esq. an excellent botanist, and a very worthy member of the R. S., that it grows plentifully in many parts of Shropshire. He had received it from Dr. Wilmer, who gathered it by the sides of the river Colne, not far from Uxbridge. It is mentioned by Mr. Ray as growing near Brereton mere in Cheshire, and in several other places. It is mentioned by Gesner *; and Wepfer, in his history of it, has given 4 tables of different parts sufficiently accurate. It is figured and described by John Bauhin †. Lobel's Icon. 208, relates to this plant. Dodonæus's figure, which is not a bad one for the time, is copied both by Gerard and Parkinson. Morrison has given 2 figures of it, one in his general history, the other in his book de Umbelliferis, though under different names. But the most elegant and descriptive figures are those of the Hortus Eystettensis and Rivinus. As the synonyma of this plant are very many, and very different, Mr. W. has inserted them at the bottom of the page. ‡

Though the medical writers have not sufficiently distinguished these plants, the botanists have. These, indeed, in their turns, have been as negligent, when writing concerning their uses.

The instances mentioned in these papers are but too sufficient testimonies of the malignant properties of this plant; but Mr. Miller informed Mr. W. further, that not many years since, a whole family were poisoned with it at Battersea. As this plant is frequent so near us, and as its appearance and smell are so like smallage and celery, we are greatly interested that the knowledge of it be extended as much as possible. As he found no good representation of it among authors, and as a good figure conveys a stronger idea to the generality of readers than the most accurate description, he procured that admirable artist Mr.

viroso, cicutæ facie Lobellii. J. B. III. p. 193; oenanthe, chærophylli foliis. C. B. P. 162; filipendula, cicutæ facie. Ger. Emac. 1057; oenanthe, cicutæ facie Lobellii Park. 894; oenanthe maxima succo viroso, cicutæ facie. Morris. Hist. Sect. 9, tab. 9; oenanthe, foliis omnibus multifidis obtusis, fere æqualibus. Hort. Cliff. 99. Royen. 107.

The oenanthe of Lobel is called in English wild parsley by Gerard and Hemlock Dropwort.—Orig. It is the oenanthe crocata Linn.

* Gesner, Hort. 254.—Orig. † J. Bauhin, iii. p. 175.—Orig.

‡ The cicuta aquatica of Wepfer is called in English long-leaved water cresses by G. Gerard, very injudiciously, and water parsnip with narrow leaves by Parkinson; but much better by Mr. Ray, long-leaved water-hemlock.—Orig. It is the cicuta virosa Linn.

Ehret to draw not only this plant, but also the cicuta aquatica of Wepfer; that they might be the more easily known from all plants, and distinguished from each other by their being both seen at one view.

P.S. Mr. W. was informed by Mr. Ehret, that, in drawing the oenanthe, which he has executed with his usual elegance and accuracy, he was obliged to have a quantity of it placed before him on a table; when, the room being small, its effluvia caused in him a universal uneasiness, with a vertigo; so that he was constrained to have it removed, and never after placed before him but a small piece at a time.

There is something in the formation of the root of the cicuta aquatica before-mentioned, deserving particular notice. This plant generally grows either near the sides of large stagnant waters, or in shallow rivers of slow streams. Towards the end of autumn, or the beginning of winter, the root for the succeeding summer is formed out of the lower part of the stalk. Out of the crown of this root are then seen the rudiments of the leaves of next year; and from the sides of this grow the crowns of several smaller roots. This root, in its whole length, is divided transversely into a number of large unequal cells. Corresponding with the partitions, which divide these cells, the surface of the root is marked circularly with little round depressions. So great a part of this root is occupied by the cells, that it becomes specifically lighter than water; so that in winter, on the increase of water in the rivers and pools, this root, as well that part intended for the succeeding summer, as that which furnished the plant the preceding, is buoyed up. The old root then rots, and floats on the surface of the water with the new one all the winter; and in rivers these are frequently carried to very great distances from the places of their growth. In the spring the old root is washed away; and the new one on its coming near the soil, sends out from the circles before mentioned, particularly from those nearest the bottom, a great number of long slender white fibres, by which this root becomes again fixed to the soil, propagates its species, and remains thus, until by the rotting of these fibres it is again weighed up. The old root decaying, and being washed from the new, is the cause of that truncated appearance observed in the root of the figures of Dodonæus, Parkinson, and Morrison, who have exhibited this plant in a flowering state. This provision of cells in the root seems to be given to this plant by nature, that as great part of its root is apt to perish in winter, vegetation might not be prevented, nor the root destroyed, unless the whole number of cells are spoiled, which very rarely happens.

Explanation of fig. 11, pl. 6, exhibiting the oenanthe cicutæ facie.* a, its tuberose roots surrounding the stalk. b, b, b, b, a leaf taken from near the

* Oenanthe crocata Linn.

bottom of the stalk. c, a branch with the umbels of flowers in different states. d, an interior view of the flower of its natural size. e, a posterior view of the same. f, the anterior appearance of the flower through a microscope. g, the posterior view of the same. h, a view of the rudiments of the fruit after the decay of the flower. i, the same magnified.

Explanation of fig. 12, pl. 6, representing the cicuta aquatica of Wepfer.*—a, a branch of this plant with its umbels of flowers in different states. b, the appearance of the bottom of the stem, growing from the crown of the old root. c, an interior view of the flower of its natural size. d, an interior view of the same magnified. e, a posterior view of the flower magnified. f, the vasculum seminale, and seed. g, the same magnified.

Explanation of fig. 1, pl. 7, representing the root of the cicuta aquatica in winter. a, the rudiments of the leaves. b, the old rotten root not yet separated from the new one of the preceding summer. c, a longitudinal section of the root exhibiting the cells.

A Letter to Mr. Benj. Robins, F.R.S. showing that the Electricity of Glass disturbs the Mariners' Compass, and also Nice Balances. N° 480, p. 242.

While so many gentlemen are labouring to find out the uses of electricity, it has been my fortune to discover one of the inconveniencies attending that property in glass.

Having lately had occasion to compare together two compasses of a different make, the one having a bare needle, as usual, and the other a chart, in the manner that mariners' compasses are commonly made, I happened to wipe off with my finger some dust, which lay on the glass of the former; and thus put the needle, which was before at rest, into a violent disorderly motion, partly horizontal, and partly vertical or dipping. After several repetitions of the same thing, I found that the glass, by so slight a touch, was at that time excited to electricity, so far as to disturb the needle extremely.

The same glass being rubbed a very little more with a finger, or a bit of muslin, or of paper, would attract either end of the needle, so as to hold it to the glass, for several minutes, far out of the due direction, according to what part of the glass was most excited. And when the needle had for some time adhered to the glass, and afterwards dropped loose, and made vibrations, those vibrations would not be bissected, as usual, by that point where the needle should rest, but either be made all on one side, or be very unequally divided, by means of some remains of electrical virtue in that part of the glass which had attracted the needle; till, at length, after 15 minutes or more, all the electricity being

* Cicuta virosa Linn.

evaporated, the magnetical power took place. The cure for this inconvenience, is to moisten the surface of the glass: even a wet finger will do it immediately and effectually.

I need not suggest, that the same quantity of friction will not at all times have the same effect on these glasses, any more than it will on the electrical tubes; but I have reason to believe that glass sometimes becomes in some degree attractive without any friction at all; and may possibly be excited by great concussions in the air, such as thunder, or the discharge of great ordnance, &c. and if so, may thereby disturb the compass.

I must however observe, that the mariners' compass is much less dangerously moved by wiping or exciting the glass than the other; because the excited parts of the glass attracts that part of the chart which lies nearest, just underneath, without giving it so much verticity, as it does to the other sort of compass with a bare needle. And further, that the deeper, or the farther distant the needle hangs below the glass, the less disturbance it is likely to receive, by wiping, rubbing, or otherwise exciting the cover.

I shall make no further reflections on these facts than to observe, first, that all the minute, irregular, reciprocating variations, which have been observed in the directions of dipping and horizontal needles, mentioned in some of the Transactions, as N° 425, may probably have been caused by the glasses which covered the instruments. And, secondly, that the flat pieces of glass, often placed under the scales of an essay-balance, are likewise very capable of attracting, and making even the lighter scale preponderate, where the whole matter weighed is so very small. I have not tried this last, but I remember, that Mr. Ellicot some years ago suspected, if he did not find it certain, that such pieces of glass disturbed his balance, and had given him a vast deal of trouble, on a supposition, that the beam itself was defective.

Concerning some new Electrical Experiments lately made at Paris. By Mr. Turberville Needham. N° 481, p. 247.

These are the curious experiments of M. le Monnier. His electrifying glass is an oblong spheroid; the diameter from pole to pole is 4 or 5 inches longer than that at the equator, which is about 12 inches. Each of these poles is terminated in a stem, or portion of a hollow cylinder, about 3 inches in length, and 1 in diameter, spirally embossed on the outside into a large male screw. This is what they use here instead of our tubes, and with surprising effects, such as greatly surpass what have yet been seen in England. The electrifying spheroid is turned by means of a wheel about 4 feet in diameter, with the same motion, and exactly in the same manner, as the spindle is turned round by the spinning-wheel: allowing a due proportion to the frame, on which the glass spheroid is mounted, that it may answer to the wheel that turns it.

The whole machine is mounted on a floor of boards, wheel, frame, glass, &c. and employs 2 men, the one to turn the wheel, the other to sit behind the glass spheroid, and apply the concave of each hand to its lower convex surface; for it is by this friction that the electricity is excited.

When the electrifying glass has been some little time in motion, the person who desires to be electrified, applies the extremities of the nails of one hand, and stands not upon cakes of wax, as in England, but within the area of a square drawer or box about 5 inches deep, and filled with 5 parts pitch, 4 of resin, and 1 of bees-wax: they are not mixed, but disposed in the following manner; the pitch is placed next to the sides of the box, and rises almost to a level with them, the resin in the middle is level with the pitch, and the wax forms a thin surface, covering both to a level with the box itself; probably, however, this is in itself very indifferent, and that any one body of the electrics per se would answer equally.

Exper. 1.—The person electrified by this machine not only emits fire from all parts of his body, on the touch of another, with more vigour, and in a much more sensible manner, than when electrified by a common tube; but fires also spirits of wine with such ease, that when the spirits have been once but simply set on fire by a match or paper lighted, and the flame has been instantly blown out, they will, with that small degree of heat they have acquired, take fire on his touch 10 or 20 times successively, without failing once.

Exper. 2.—If the person electrified holds a sword in one hand, the chamber being darkened, a continual flame issues out at the point, in smell and colour resembling the fumes of phosphorus, and nearly as strong as that of an enameller's lamp: with this difference, that when any other of the company applies a hand, even to the point, where the concentrated rays begin to diverge, it burns not, nor is any otherwise sensible to the feeling, than as a continual blast of wind.

Exper. 3.—This is performed with a square bar of iron, about 4 feet in length, and half an inch in thickness; to one extremity of which is adapted, by the help of a screw, another piece of iron beat flat, like the end of one of the legs of a pair of tongs. This flat piece of iron being screwed in, the bar is placed parallel to the horizon on a wooden stand, and the stand within the area of the drawer or box, upon the pitch, resin, and bees-wax, as above. The operator orders the bar to be electrified by repeated revolutions of the glass spheroid, as above; and places one finger on the middle of the bar, to prevent the communication of the electricity from one end to the other, till he has covered the flat piece of iron with as much saw-dust as it will carry. Some of the company, in the mean while, takes up on the point of a knife likewise, a quantity of saw-dust, and holds it under the flat piece of iron, at about an inch distance.

Exper. 4.—The most surprising of all, is Mr. Muschenbroeck's experiment, improved by M. le Monnier. A musket-barrel open at both ends, is suspended parallel to the horizon, by silken threads within reach: and at the breech end, about 3 inches from the extremity, is hung, by a ring of iron worked into the barrel itself, a small iron chain about half a foot in length. A glass phial, resembling in size and shape a common vinegar-cruet, is then prepared, full of water and well corked, with an iron wire running through the cork almost to the bottom, and emerging 2 or 3 inches above it, out of the top of the phial. The head of this wire is bent, to catch in the lowest link of the chain; and is to be there suspended, when it has been electrified. From the mouth of the barrel, which is pointed in a line parallel to the equatorial plane of the revolving spheroid, comes a long iron wire, inserted into the barrel itself, as far as one-third of its length, and thence proceeding till it touches the glass spheroid; to a contact with which it is determined by one of the silken loops mentioned above. Every thing being thus disposed, the gun-barrel is to be electrified by repeated revolutions of the glass spheroid; which is to be in a continual contact with the long wire that proceeds from it. The phial is, at the same time, to be electrified by the operator, who takes hold of the body of the bottle, and applies to the electrifying spheroid the bent extremity of that wire, which passes from near the bottom of the phial through the cork, as described above. When the phial is sufficiently electrified, which will be done in 8 or 10 revolutions of the spheroid; the person who has courage enough to suffer the experiment, grasps the bottom of the electrified phial with one hand, and with the other touches the gun-barrel. At that instant, a great part of the nervous system receives a shock so violent, that it would force the strongest man to quit his hold, and turn him half-round.

Exper. 5.—When the phial has been sufficiently electrified as above, the whole company join hands; the operator at one extremity of the line grasps the bottom of the electrified phial, and the person at the other extremity touches the wire, which rises above the cork. At that instant, the whole company receives a shock, resembling that in the experiment of the gun-barrel, but not so strong; for it seems not at all to extend beyond the elbows. This is the experiment, which Abbè Nollet performed on 180 of the guards, before the king, who were all so sensible of it at the same instant, that the surprize caused them all to spring up at once.

Exper. 6.—Another experiment with the electrified phial consists, first, in placing a wire fixed in a pedestal, erect in a basin of water, the head of which wire is bent, and rises 3 or 4 inches above the level of the water; and then, in touching the surface of the water with one hand, and the standing wire with the wire of the electrified phial, which is grasped by the other hand, as in the preceding experiments. The effect of this is much more violent than that of the last experiment, and exceeds even the shock of the gun-barrel; so that here the utmost precaution must be used, not to electrify the phial too much.

Exper. 7.—If the electrified phial be held in the hand, and the chamber be darkened; the wire inserted in it is perceived to emit a stream of fire at its extremity without any discontinuance; but when suspended by a silken thread, the fiery eruption instantly ceases.

Exper. 8.—If the non-electrified phial be placed on a glass salver, it acquires from the revolution of the spheroid no electricity, though its wire be in contact with it all the time; unless the finger of some one in the company be approached very near to the phial itself; but in that case it receives it visibly from the finger; insomuch that if the chamber be darkened, you see the electrical fire streaming out of the finger, and entering into the water, through the body of the glass phial, which is thereby immediately impregnated with it; and this, though the hand should be placed even under the glass salver itself.

Exper. 9.—If the electrified phial be placed on a table, and any light body be suspended by a silver thread, within the distance of about 2 inches from the phial; the phial will attract that light body to it with force, if any of the company touch the wire of the phial; but if the phial itself be touched, it will repel it with a force equal to its attraction in the former case.

Exper. 10.—This experiment consists in the communication of the electrical fire from the glass spheroid to many persons at once, as in England from a tube; with this only difference, that the company here do not join hands, but are united to each other by taking hold of iron chains, which surprisingly increase the force of the communicated electricity: for it is to be observed, that whenever the communication is carried on by a metallic medium, the effects are much more sensible.

Exper. 11.—This experiment is no other than what has been frequently tried in England, the attraction of leaf-gold by a hollow wooden globe, to which electricity is communicated, by a packthread of a very great length suspending it; after it has been conducted over silken threads crossing the chamber at several distances, in a sort of spiral, consisting of as many turns as the place will admit.

At the grand convent of the Carthusians in Paris, the whole community formed a line of 900 toises, by means of iron wires of a proportionable length, between every 2; and consequently far exceeding the line of the 180 of the guards above mentioned. The effect was, that when the 2 extremities of this long line met in contact with the electrified phial, the whole company, at the same instant of time, gave a sudden spring, and all equally felt the shock, that was the consequence of the experiment.

Another phenomenon was the result of a late experiment of Abbè Nollet's. He fixed, at the two extremities of a brass ruler, 2 small birds, a sparrow and a chaffinch: this ruler had a handle or pedestal fastened to its middle, for the convenience of holding it. When both the gun-barrel and the phial had been sufficiently electrified, as in the 4th experiment, he applied the head of the sparrow to the suspended phial, and the head of the chaffinch to the barrel. The consequence, on the first trial, was, that they were both instantaneously struck lifeless, as it were, and motionless, for a time only, and they recovered some few minutes after; but on a second trial, the sparrow was struck dead, and on examination found livid without, as if killed by a flash of lightning, most of the blood-vessels within the body being burst by the shock. The chaffinch revived, as before.

The Path of the Comet which appeared in March and April 1742, from the Observations made at the Jesuits' Observatory at Pekin. Communicated by Mr. James Hodgson, F.R.S. and Master of the Mathematical School Christ's Hospital. N° 481, p. 264. *From the Latin.*

True time of observation.				Right Asc.		Declination.			Constellations by which the Comet passed.
	d.	h.	min.	°	′	°	′		
March	2	4	30 m.	281	55	6	0	S	At the foot of Antinous.
	4	4	0 m.	283	30	5	15	N	Near the Serpent's tail.
	5	4	45 m.	283	33	10	50		Below the tail of Aquila.
	7	4	0 m.	284	48	22	40		Between Anser and Cerberus.
	11	2	30 m.	288	1	44	57		Betw. the wing of Cyg. & Lyra.
	12	4	30 m.	289	6	50	3		In the northern wing of Cygnus.
	13	3	15 m.	290	11	54	15		
	14	4	0 m.	291	40	58	50		Between Cygnus and the belly of Draco.
	15	3	15 m.	293	12	62	36		
	16	4	0 m.	295	0	66	0		In the belly of Draco.
	17	4	30 m.	297	10	69	11		

True time of observation.				Right Asc.		Declination.		Constellations by which the Comet passed.
	d.	h.	min.	°	′	°	′	
March	18	4	0 m.	299	34	71	50	Between Draco and Cepheus.
	19	4	0 m.	302	39	74	23	At the knee of Cepheus.
		8	20 a.	304	38	75	40	
	22	9	0 a.	319	56	81	0	Between the feet of Cepheus, and afterwards in the same in the vicinity of the north pole.
	23	9	45 a.	327	25	82	14	
	24	10	15 a.	336	22	83	12	
	27	9	0 a.	21	24	84	26	
	28	8	40 a.	26	28	84	20	
	29	1	30 m.	30	34	84	13	
	30	2	0 m.	38	13	83	45	
	31	2	50 m	45	3	83	29	
April	1	2	50 m.	50	51	83	0	
	2	3	12 m.	55	55	82	27	

From the observations of March 2 and 4, it appears that the comet came to the equator March 3, about 6 in the morning, in 282° 30′ right ascension, with 84° 30′ inclination of its path to the equator; therefore its longitude was 13° 35′ of ♑, with 22° 54′ north lat. Hence it may be collected, that the comet's path, which appeared to be in a great circle, met the ecliptic in 9° 19′ of ♑ and ♋, with 80° of inclination; and the colure of the equinoxes at the distance of 5° 37′$\frac{1}{4}$ from the poles of the world, toward the equinoctial points, with an inclination of 77° 33′$\frac{1}{4}$; and the colure of the solstices at the distance of 23° 57$\frac{1}{3}$ from the poles of the world, towards the solstitial points, with an inclination of 13° 38′, equal to the greatest elongation of its orbit, from the same colure in the adverse part, and to the distance of the poles of the orbit from the equinoctial points.

Of the various Genera and Species of Music among the Ancients, with some Observations concerning their Scale. By John Chrtstoph. Pepusch, Music. D., and F.R.S.* N° 481, p. 266.

In compliance with your request, I here send you some of my thoughts on the various genera and species of the Greek music. What were these, and how far the doctrine of the ancients in this respect is reconcileable with the true nature of musical sounds, are questions which have not a little perplexed the learned. That musical intervals are founded on certain ratios expressible in numbers, is an old discovery. It is well known that all musical ratios may be analysed into the prime numbers 2, 3, and 5; and that all intervals may be found from the octave, 5th and 3d major; which respectively correspond to those numbers. These are the musician's elements, from the various combinations of which, all the agreeable variety of relations of sounds result. This system is so well founded on experience, that we may look upon it as the standard of truth. Every interval that occurs in music is good or bad, as it approaches to, or deviates from, what it ought to be on these principles.

The doctrine of some of the ancients seems different. Whoever looks into the numbers given by Ptolemy, will not only find th primes 2, 3, and 5, but 7, 11, &c. introduced. Nay he seems to think all 4ths good, provided their component intervals may be expressed by superparticular ratios. But these are justly exploded conceits; and it seems not improbable that the contradictions of different numerical hypotheses, even in the age of Aristoxenus, and their inconsistency with experience, might lead him to reject numbers altogether. It is pity he did: had he made a proper use of them, we should have had a clearer insight into the music of his times. However, what remains of the writings of this great musician, joined to observation and experience, has enabled Dr. P. to throw some light on the obscure subject of the ancient species of music.

By the manner in which Euclid and others find the notes of their scale, it

* Dr. Pepusch was one of the greatest theoretic or scientific musicians among the moderns. He was a Prussian by birth; and in 1680, when not quite 15 years of age, he was chosen to teach music to the Prince Royal of Prussia. He afterwards came to England, and was engaged at Drury-lane theatre; though the popularity of Handel kept Pepusch in the 2d rank; yet his talents and judgment were so much respected, that he taught music to professors of music themselves. The university of Oxford honoured Dr. P. with the degree of Dr. of music, and the R. S. elected him one of their members. He married a Tuscan lady, an eminent opera singer, who had acquired by her profession a fortune of £10,000. Dr. P. died in 1752, being 85 years of age, and was buried in the Charterhouse.

must have been composed of tones major, and limmas. Hence the 7 intervals of one octave would be thus expressed in numbers, $\frac{9}{8}$, $\frac{256}{243}$, $\frac{9}{8}$, $\frac{9}{8}$, $\frac{256}{243}$, $\frac{9}{8}$, $\frac{9}{8}$. Some modern authors have from this inferred the imperfection of the Greek music. They allege that we here find the ditonus, or an interval equal to 2 tones major, expressed by $\frac{81}{64}$, instead of the true 3d major expressed by $\frac{5}{4}$. As there can be no question of the beauty and elegance of the latter, the former therefore must be out of tune, and out of tune by a whole comma, which is very shocking to the ear. In like manner the trihemitone of the ancients falls short of the 3d minor by a comma; which is also the deficiency of their hemitone or limma, from the true semitone major, so essential to good melody. These errors would make their scale appear much out of tune to us; and indeed it appeared out of tune to them; since they expressly tell us that the intervals less than the diatessaron or 4th, as also the intervals between the 5th and octave, were dissonant and disagreeable to the ear. Their scale, which has been called by some the scala maxima, was not intended to form the voice to sing accurately, but was designed to represent the system of their modes and tones, and to give the true 4ths and 5ths of every key a composer might choose. Now if, instead of tones major and limmas, we take the tones major and minor, with the semitone major, as the moderns contend we should, we shall have a good scale indeed, but a scale adapted only to the concinnous constitution of one key; and whenever we proceed from that into another, we find some 4th or 5th erroneous by a comma. This the ancients did not admit of. If, to diminish such errors, we introduce a temperature, we shall have nothing in tune but the octave. We see then that the scale of the ancients was not destitute of reason; and that no good argument against the accuracy of their practice can from thence be formed.

It was usual among the Greeks to consider a descending, as well as an ascending scale, the former proceeding from acute to grave, precisely by the same intervals as the latter did from grave to acute. The first sound in each was the proslambanomenos. The not distinguishing these two scales has led several learned moderns to suppose, that the Greeks, in some centuries, took the proslambanomenos to be the lowest note in their system, and in other centuries to be the highest. But the truth is, that the proslambanomenos was the lowest, or highest note, according as they considered the ascending or descending scale. The distinction of these is conducive to the variety and perfection of melody; but Dr. P. never yet met with above one piece of music, where the composer appeared to have any intelligence of this kind. The composition is about 150, or more, years old, for 4 voices; and the words are, Vobis datum est noscere mysterium regni Dei, cæteris autem in parabolis; ut videntes non videant, et audientes non intelligant. By the choice of the words, the author seems to allude to his having performed something not commonly understood.

The following is an octave only of the ascending and descending scales of the diatonic genus of the ancients, with the names of their several sounds, as also the corresponding modern letters.

	Ascending.		Descending.	
A		Proslambanomenos		*g*
	$\frac{9}{8}$		$\frac{8}{9}$	
B		hypate hypaton		*f*
	$\frac{4}{3}\frac{5}{4}\frac{6}{5}$		$\frac{3}{4}\frac{4}{5}\frac{5}{6}$	
C		parhypate hypaton		*e*
	$\frac{9}{8}$		$\frac{8}{9}$	
D		lychanos hypaton		*d*
	$\frac{9}{8}$		$\frac{8}{9}$	
E		hypate meson		*c*
	$\frac{4}{3}\frac{5}{4}\frac{6}{5}$		$\frac{3}{4}\frac{4}{5}\frac{5}{6}$	
F		parhypate meson		*b*
	$\frac{9}{8}$		$\frac{8}{9}$	
G		lychanos meson		*a*
	$\frac{9}{8}$		$\frac{8}{9}$	
a		mese		G

Where it appears that the same Greek names serve for the sounds in the ascending and descending scales.

In the octave here given, 4 sounds, viz. the proslambanomenos, hypate hypaton, hypate meson, and mese, were called stabiles, from their remaining fixed throughout all the genera and species. The other 4 sounds being the parhypate hypaton, lychanos hypaton, parhypate meson, and the lychanos meson, were called mobiles, because they varied according to the different species and varieties of music.

By genus and species was understood a division of the diatessaron, containing 4 sounds, into 3 intervals. The Greeks constituted 3 genera, known by the names of enharmonic, chromatic, and diatonic. The chromatic was subdivided into 3 species, and the diatonic into 2. The 3 chromatic species were the chromaticum molle, the sesquialterum, and the toniæum. The 2 diatonic species were the diatonicum molle, and the intensum; so that they had 6 species in all. Some of these are in use among the moderns, but others are as yet unknown in theory or practice.

The diatonicum intensum was composed of 2 tones, and a semitone: but, to speak exactly, it consists of a semitone major, a tone minor, and a tone major. This is in daily practice; and we find it accurately defined by Didymus, in Ptolemy's Harmonics published by Dr. Wallis. The next species is the diatonicum molle, as yet undiscovered by any modern author. Its component intervals are, the semitone major, an interval composed of 2 semitones minor, and the complement of these 2 to the 4th, being an interval equal to a tone major, and an enharmonic diesis. The 3d species is the chromaticum toniæum. Its component intervals are, a semitone major, succeeded by another semitone major; and lastly the complement of these 2 to the 4th, commonly called a superfluous tone. The 4th species is the chromaticum sesquialterum, which is constituted by the progression of a semitone major, a semitone minor, and a third minor. This is mentioned by Ptolemy, as the chromatic of Didymus. Examples among the moderns are frequent. The 5th species is the chromaticum molle. Its intervals are two subsequent semitones minor, and the complements of these 2 to the 4th; that is, an interval compounded of a 3d minor, and an enharmonic diesis. This species is never met with among the moderns. The 6th and last species is the enharmonic. Salinas and others have determined this accurately. Its intervals are, the semitone minor, the enharmonica, diesis and the third major.

Examples of 4 of these species may be found in modern practice. But he does not know of any theorist who ever yet determined what the chromaticum toniæum of the ancients was: nor have any of them perceived the analogy between the chromaticum sesquialterum and our modern chromatic. The enharmonic, so much admired by the ancients, has been little in use among our musicians as yet. As to the diatonicum intensum, it is too obvious to be mistaken.

Aristoxenus and others often mention the tone is divided into 4 parts, and the semitone into 2; thus making 10 divisions or dieses in the 4th. And this is true, if we consider these sounds in one tension; that is, either ascending or descending; but, accurately speaking, when we consider all the dieses or divisions of the 4th, both ascending and descending, we shall find 13; 5 to each tone, and 3 to the semitone major. But then it is to be observed, that some of these divisions will be less than the enharmonic diesis; for if we divide the semitone major into the semitone minor, and enharmonic diesis, ascending, for instance, E, *E, F, and then divide in like manner descending, F, ♭F, E, we shall have the semitone major divided into 3 parts thus, E, ♭F, *E, F; where the interval between ♭F and *E is less than the enharmonic diesis between E and ♭F, and between *E and F, as is easily proved.

Now, if we suppose these small intervals equal, by increasing the least division, and diminishing the true enharmonic diesis, we shall then have a 4th divided into 13 equal parts; and consequently the octave divided into 3 such equal parts; which gives us the celebrated temperature of Huygens, the most perfect of all.

From this it appears, that the division of the octave into 31 parts, was necessarily implied in the doctrine of the ancients. The first of the moderns who mentioned such a division was Don Vincentino, in his book L'Antica Musica ridotta alla moderna Prattica, printed at Rome, 1555, folio. An instrument had been made according to his notion; which was condemned by Zarlino and Salinas, without sufficient reason. But Mr. Huygens, having more accurately examined the matter, found it to be the best temperature that could be contrived. Though neither this great mathematician, nor Zarlino, Salinas, nor even Don Vincentino, seems to have had a distinct notion of all these 31 intervals, nor of their names, nor of their necessity to the perfection of music.

In Huygens's temperature the tones are all equal; but in a true and accurate practice of singing they are not so. And the tone divided in every species must be the tone minor; for the division of the tone major is harsh and inelegant. So that, in the division of the 4th, it is to be observed, that in every species, the tone major must either be an undivided interval, or make part of one.

It may perhaps be wondered how the foregoing doctrine can be found in the writings of the ancients, since the distinction of tones into major and minor is no where mentioned in them. But it is to be observed, that though the terms do not occur, yet the thing itself was not unknown to them. They have not indeed expressed themselves fully; yet, from the whole of their writings come to our hands, the doctrine before laid down may be well supported.

Observations on the Precipices or Cliffs on the North-east Sea Coast of the County of Norfolk. By Mr. Wm. Arderon, F. R. S. N° 481, p. 275.

These dreadful heights are equally dangerous to come near above or below; as they are so frequently tumbling down, and as often washed away by the billows; and though they are 20, 30, and in some places 40 yards, or more, in perpendicular altitude, yet the sea has gained on the land at least 110 yards in less than 20 years time for some miles on this coast.

The various strata, which make up this long chain of mountainous cliffs, must be very entertaining to every one, who takes a pleasure in looking into the many changes which the earth undoubtedly has undergone, since its first creation. Vegetable mould, oaz, sands of various kinds and colours, clays, loams, flints, marls, chalk, pebbles, &c. are here to be seen at one view beautifully interspered; and frequently the same kind many times repeated; as if at one time dry land had been the surface; then the sea; after morassy ground; then the sea, and so on, till these cliffs were raised to the height they are now found.

This is demonstrated by the * roots and trunks of trees, which are to be seen at low water in several places on this coast near Hasborough and Walket; bones of animals are often found here also.

Among the many strata found in these cliffs, there is one of a dark grey colour, that sweats out a yellow sulphureous matter; it seems to be that sort of

* Dr. Hook, in his posthumous works says, the like are to be seen on the coasts of Cumberland and Pembrokeshire.—Orig.

earth from which vitriol is made; but this is of such a caustic nature, that if but a small piece of it be held to the tongue, in a moment it causes as sharp and excruciating a pain, as if a red-hot iron had been held to it.

The marl, or rather chalk pits, at Cantley White House, about 3 miles from Norwich, are made in the side of a long chain of hills, which runs along the side of the river Yar, and about a furlong or two now and then distant from it. These hills seem to have been formerly the boundaries to an arm of the sea, which made Norwich a famous sea port. This some of our ancient histories make mention of as an undoubted truth, though now considered as a mere fable, as no vestiges of it remain above ground at this day.*

In the abovementioned marl pits he discovered a stratum of shells, of about 2 feet thick, running nearly parallel to the horizon. He examined carefully this stratum, where he found a great many kinds of shells; but none which had withstood time's all-devouring teeth, so as to bear the handling; excepting the common wilk, some of which were very perfect. Among the variety of things he noticed in this stratum was a piece of coal, which he picked out from among the shells. This must have lain here as long as they, and been brought from some other county, as nothing of its kind is to be found here, but what is brought from distant parts. These shells lie 14 yards above the surface of the river, and nearly 6 beneath the top of the hill, and he believes 34 yards above the surface of the sea at Yarmouth. And it is very remarkable, that in these marl pits, 6 or 7 yards lower than the abovementioned stratum of shells, are found a vast quantity of stags' horns lying in all directions. Several I took out with my own hands; so much so, that the workmen, which are employed here, say, that they scarcely work a day, but they find more or less of them. But none are found entire.

These horns have been very large ones; some of the spines measuring 12 inches and upwards in length. Many of them are more than 2¼ inches in diameter, and several of them above 12 inches from spine to spine.

Another curiosity was the entire skeleton of a man, which was found in the same stratum with the abovementioned horns, as one of the workmen assured me; he said, he took the pains to lay it altogether on the grass, as regularly as he was able; but his curiosity being then satisfied, he left it to be ground to pieces by the carts and waggons that came thither for the marl; so careless were these poor ignorant people of so valuable a specimen of the human race.

Helmet stones and belemnites are found here in abundance, at all depths, and in every different stratum, which shows that the fish which produced these fossils

* Verstegan says, that many places which were sea became dry land, at the breaking of the German Ocean through the Isthmus which once joined England to France. Verst. p. 117.—Orig.

have been very plentiful; and so they must have been all over the country, as the like are to be found in every place wherever the earth is broken open, or a pit is digged.

About a mile south of a little country town called Kisick, and near 2 furlongs from Hartford-bridge, or 3 miles south-west of Norwich, is a pit, in which the country people dig a particular sort of clay to lay on their sandy lands. Among this clay lie a great many knots, lumps, or nodules, of a bluer kind of earth, not widely differing from that which is found in Harwich cliff; these, when digged up, are soft; but when they have been for some time exposed to the open air, they become almost as hard as flint. In and upon these lumps are the impressions of the cornu ammonis, or snake-stones, in a beautiful manner, from 1 inch to 5 or 6 in diameter, and several have part of the shells on them, of a yellowish white. Many other shells are found in these lumps; as the pectunculus, helmet stones, belemnites, common cockle, turbos, &c. but these are most of them very small.

But still more curious than all the rest are certain lumps of petrified crystallized matter, of a very odd form, such as he had never seen or ever read of. They appear to have been originally lumps of blue clay, cracked by some subterraneous heat, or other unknown cause, into which the water has insinuated, and their contained salts have crystallized in the cracks. When these lumps are taken up and become dry, the clay part falls from out the exterior cells; and then they may be thought grossly to represent a honey-comb.

On a Polypus at the Heart, and a Scirrhous Tumour of the Uterus. By Peter Templeman, M.D. N° 481, p. 285.

Ann Hicks was brought to the workhouse of St. Andrew's, Holborn, on Saturday, Nov. 15, 1746. Her complaints were, a difficulty of breathing, from a cold she had caught about a fortnight before, with a violent pain and palpitation of her heart. The pulse was scarcely perceptible. The surgeon, Mr. Tait, being present, Dr. T. ordered him to open a vein; but to keep his finger on the pulse, and if it did not rise on her losing a little blood, immediately to desist. On losing 1 oz. or 2 of blood, the pulse grew more languid, and he accordingly desisted. Dr. T. then ordered a large blister to be applied to her neck, and gave her oily medicines with the volatile salts. He did not visit at the workhouse again till the Wednesday following, when he found her much easier in her breath, but the pain and palpitation of the heart continuing. As the oily medicines had occasioned a violent purging, he ordered her the elixir asthmaticum in cinnamon water. Her pulse was still so little discernible, that though he thought it intermitted, yet he could not be positive. She, however, died on Friday, and her body was opened on Saturday.

On exposing the body naked on the table, there appeared a very large and hard swelling in the hypogastric region, which was supposed at first view to be a child, and the more so, as the woman had never made any complaint of an uneasiness in those parts. Having opened the body, it was found to be a swelling of the uterus, which was greatly enlarged, and extremely hard. Besides the whole body of the uterus being thus enlarged and hardened, there were 2 large protuberances distinct from each other, that grew prominent out of the upper surface of the uterus, and were each of them of the size of a large egg. There was likewise a third protuberance on the opposite side, but much smaller than the other two; and another that seemed to be but just budding. The operators cut down directly through one of the large protuberances into the very body of the uterus, and found nothing but a solid mass of a cartilaginous substance. The texture indeed of the protuberance was somewhat laxer than of the body of the uterus. They then introduced a probe from the os tincæ, to examine if there was any cavity in the uterus; and found a small one reaching to the fundus, and barely large enough to admit the probe. The ovaria and Fallopian tubes were in their natural state; except a small deviation of the Fallopian tube on the right side.

Dr. T. knows no writer that has taken notice of a similar appearance in the uterus, but Ruysch, in his Thesaurus Anatomicus Decimus, N° 106. He has not given any plate or description of the dimensions of his enlarged uterus; but this, which Dr. T. has described, was in its greatest breadth 4¾ inches; its length from the os tincæ 6 inches; thickness 3¼ inches; and its weight, including the ovaria, Fallopian tubes, &c. 2 lb. 12 oz. Avoirdupois.

Whatever Ruysch had observed of that kind were in old women. This woman Dr. T. considered to have been between 30 and 40. He does not mention having seen more than one small additional scirrhus; whereas in this there were 3 or 4. And lastly he does not mention any thing of the hard cartilaginous substance of the uterus.

There were several remarkable adhesions in the abdomen and thorax; as of the omentum to the peritonæum, of the lungs to the pleura and diaphragm, of the pericardium to the pleura. The liver and spleen appeared in their natural state; the kidneys were enlarged beyond their usual size; the coronary veins of the heart were much distended with blood, and the lungs inflamed to a degree of mortification. On examining the cavities of the heart, they found in the right ventricle a polypose concretion, of a fleshy fibrous substance, that adhered to the ventricle, and in separating it from thence was rent into 2 pieces.

On the Communication of Electricity. By Mons. le Monnier, the younger, M. D. F. R. S. and Memb. of the R. Acad. of Sciences at Paris. N° 481, p. 290.

The author of this memoir proposes to examine these 3 questions, viz. how is this electric virtue to be communicated to such bodies as have it not, and which are not capable of acquiring it by bare friction only? How is the electric matter propagated? And, lastly, in what proportion is it distributed?

As to the first, the author observes, that this electric virtue is no other way to be communicated, but by the near approach of a body already actually possessed of the same; that the rule laid down by Mons. du Fay, "That bodies never receive electricity by communication, unless they are supported by bodies electric in their own nature," does not always take place, and that it is subject to great exceptions. For, first, in the Leyden experiment, the phial filled with water is strongly electrified by communication, even when carried in the hand, which is not a body electric by nature. 2dly. All bodies that are electrified by means of a phial of water fitted to a wire, and which has already received a great degree of virtue by communication; all such bodies, placed in any curve line, connecting the exterior wire, and that part of the bottle which is below the surface of the water, acquire electricity, without being placed on resin, silk, glass, or the like.

Thus one may give a violent concussion in both the arms to 200 men all at once, who holding each other by the hand, so form the curve just mentioned, when the first holds the bottle, and the last touches the wire with the end of his finger; and this, whether these persons actually touch each other's hands, or whether they are connected by iron chains, that either dip in water, or drag on the ground; whether they are all mounted on cakes of resin, or whether they only stand on the floor; in all which cases the experiment equally succeeds. Electricity has in this manner been carried through a wire of the length of 2000 toises, that is to say, of about a Paris league, or near 2¼ English miles, though part of the wire dragged on wet grass, went over charmil hedges or palisades, and over ground newly ploughed up.

3dly. The water of the basin in the Thuilleries, whose surface is about an acre, has been electrified in the following manner: there was stretched round half the circumference of the basin an iron chain, which was entirely out of the water: the two extremities of this chain answered to those of one of the diameters of the octagon; an observer, placed at one of these extremities, held the chain with his left hand, and dipped his right at the same time into the water of the basin; while another observer, at the opposite side of the basin, held the other end of the chain in his right hand, and a phial well electrified in his left; he then caused the wire of his phial to touch an iron rod, fixed upright in a piece of cork that floated near the edge of the basin; at that instant both observers felt a violent shock in both their arms. This same fact was again confirmed by experiments made on 2 basins at the same time, that it might distinctly appear that the electrical effluvia did really pass along the superficies of the water.

4thly. It has been confirmed, by repeated comparisons, that a bar of iron placed in the abovementioned curve, does not at all acquire more electricity, when it is suspended in silken lines, than when it is held in the bare hand. Whence it appears that, in this case, the contiguous non-electric bodies do neither partake of, nor in any way absorb the electricity that has been communicated.

Besides many strong exceptions to the rule laid down by Mons. du Fay, the author adds another yet stronger, and indeed directly contrary to that rule; which is, that the same phial of water, fitted with its wire, receives either no virtue at all, or at least none that is sensible, so long as it is either placed on a stand of glass that is very dry, or that it is suspended by a silken thread, while its wire rests on the globe; and that, to make it receive the virtue, the part of the phial which is below the surface of the water, must communicate with some body that is not electric; as is evident, when it is touched, while it rests on the stand of glass, with the finger, for it then instantly becomes electric; and the same also happens when it is touched with a piece of metal; but not when touched with a dry tube of glass.

The electrical rests produce here on the bottle an effect so contrary to M. du Fay's rule, that if one places a phial, perfectly well electrified, and which throws out the pencil of fire copiously, on a dry stand of glass, or on a line of silk; its light immediately goes out, and its electricity is as it were laid to sleep. We may then securely approach the finger to its wire, and no electrical sparks will come from it. The author has even drawn out of it entirely both the wire and the cork, and has kept it half an hour in his pocket, without destroying the electricity. But in this case we must only touch the wire, and not the phial itself; for by touching the 2 at the same time, we return to the Leyden experiment; but on touching the phial only, the electricity revives in the wire, and the pencil of fire displays itself again, provided we have not staid too long: but when the wire only is touched, the body of the bottle becomes strongly electric, and draws to it, from a considerable distance, any light substances.

This last case gives room to an experiment that looks at first like magic: there was hung up a little tinkling bell by a silver wire, at the height of 8 or 9 feet, and there was placed on a glass stand well dried, a phial newly electrified; the centre of the bell, and that of the phial, were nearly in the same horizontal line; but the bell was 6 or 7 inches from the surface of the phial. Every thing being in this state, the bell remained quite still when the stand was very dry; but the instant we either approach a finger, or any other non-electric body, to the wire of the phial, the bell leaps to it: and we might begin again, and repeat the experiment 20 times together, without having any occasion to new-electrify the phial.

With regard to the propagation of electricity, the velocity with which the electrical matter is conveyed, has been found too great to be yet determined with any exactness. The author made an experiment with an iron wire of 950 toises in length, and he was not able to observe, that there passed so much as a quarter of a second of time, between the wire's receiving the electricity at one end, and his feeling the shock in both his arms at the other; which infers a velocity at least 30 times as great as that with which sounds are propagated.

In seeking what might be the force which shot forward the electric matter, with so much rapidity, through the length of the wire, he at first thought it might be performed by the explosion of the spark of fire, which is perceived when the electrified phial is brought into contact with the wire conducting the electric matter; but the following experiment soon convinced him he was mistaken. He disposed horizontally a wire folded in 2, on lines of silk; the whole length of this wire was of 1319 feet, and the 2 parallel halves were about 6 feet distant from each other. The electricity was then communicated by means of a phial; and it preserved itself in the wire for several minutes, by reason of the silken lines on which this was supported: a finger was then brought to one of its extremities, to take away the virtue; and in the same instant it ceased also at the other extremity of the wire: so that in this case the matter in question returned to the finger, that is, marched backward, with the same velocity with which it was before shot forward: the electric matter therefore now came towards the explosive spark, for this spark appeared on the finger as soon as it approached the end of the wire to take away its electricity, and therefore it is not this spark which shoots forward the electric matter with so great a velocity.

The last part of the memoir concerns the proportion in which the electric matter is communicated to bodies of the same nature. And here the author first establishes, that it is not communicated to homogeneous bodies, in proportion to their masses or quantities of matter, but rather in proportion to their surfaces. Yet all bodies having equal surfaces do not receive equal quantities of electricity: those receive the most, whose surfaces are extended the most in length. Thus a square sheet of lead receives a much less quantity of electricity, than a long strip of the same metal with a surface equal to that of the square sheet: insomuch that the only way to increase in any body its faculty of receiving the electric virtue, is to continually increase its length.

Of a very Large Stone, found in the Colon of a Horse; and of Several Stones taken from the Intestines of a Mare. By Edward Bailey, M.D. of Havant in Hampshire. N° 481, p. 296.

The horse in which the large stone was found, belonging to a miller, had been fed with bran only for several years. He was observed to be in pain sometimes, but never so bad as to be hindered from his work, till the day of his death; when he was taken on the road with symptoms of violent pain, and wanted to lie down: however, the carter drove him home; but, as soon as he had unharnessed him, the poor creature was seized with a great shaking, and dropped down dead immediately. On stripping him, the man observing a swelling in his belly, opened it, and found in the colon a very large stone, but presently broke it in pieces.

Dr. B. did not hear of this stone till the latter end of last summer, when a gentleman showed him a fragment of it; which excited his curiosity to go to the mill where the horse died, to inquire for the remains. He found several pieces of it, weighing in all 1 lb. 6¼ oz. Troy. Some of them had been kept dry in the mill, but the greatest part laid abroad mixed with rubbish; which, though exposed to the weather above 12 months, was not much altered, being only a little more brittle than the rest, and somewhat mouldered on the outside. About a fortnight since, another piece of the same stone was brought to him, weighing about 8 oz. Troy, containing near half the nucleus and the innermost laminæ cohering together.

From all these fragments, and the description of the stone given by those who saw it, before it was broken in pieces, it appears to have been of a spheroidal figure, about 16 inches in circumference, consisting of a nucleus and several laminæ or shells involving each other; some of them are parted, but the rest adhere so close together, that they cannot be separated without breaking. All the laminæ are composed of transverse striæ, with their points converging like rays towards the centre of the nucleus. They are of a brown colour, and shine like resin. The nucleus is of an oval figure, and differs but little in its composition from the rest of the stone, having no other extraneous matter in it besides a few pieces of straw, and small sticks, like the twigs of a broom; some of them appear intermixed with the striæ throughout the body of the stone. The external surface of the stone, and those laminæ that have been exposed to the air, look of an ash-colour, are pretty even, but not very smooth, having many small holes in them.

About the beginning of last July, 5 large stones were found lying near each other in the intestines of a mare, which belonged to a carrier, who had worked her several years in his team. She was in good case, and always appeared to be sound and healthy; till one morning, being at grass, she was found lying on the ground in a great agony of pain, with which she was continually tortured for about 6 hours, without any relief from various remedies which were applied: at last she got up, and ran about the field like a mad creature, till she died.

Dr. B. had seen only 2 of the stones which were taken from her; one of a triangular shape, the other oblong, a little depressed in the middle, bearing some resemblance to a horse-bean. They are both similar in substance, and seem to be of the bezoar-kind, being of a closer texture than that above described, are of an olive-colour, and finely polished. The other three, as he was informed, were of the same colour and texture, and one of them larger than either of these, which measured one way round 12 inches, and the other way 11.

Experiments.—These 2 stones, being sawed asunder, looked like polished marble; and were found to contain a piece of an iron nail in the middle. The triangular stone weighed 75 grs. above $1\frac{1}{4}$ oz. Troy. The other, being the larger, weighed but $2\frac{1}{4}$ grs. short of 16 oz. Troy.

A fragment of the smaller stone, which in the air weighed $103\frac{6}{10}$ grs. in water weighed $42\frac{6}{10}$ grs.; so that its specific gravity is to water, as 170 to 100. A fragment of the larger stone, which weighed in air $83\frac{7}{10}$ gr. weighed in water of the same degree of warmth, $34\frac{7}{10}$ gr. so that the specific gravity of this is the same as of the other.

A quantity of the larger stone, weighing 4 oz. being distilled in a coated retort, yielded $37\frac{4}{10}$ gr. above 20 oz. of a strong alkaline spirit, of a brown colour, such as is drawn from hartshorn; leaving a black coal weighing $74\frac{1}{10}$ gr. short of 2 oz.; $36\frac{7}{10}$ gr. being converted into air, and otherwise lost in collecting the produce of the distillation; a small quantity of black oil adhered to the neck of the receiver, and a few drops of this oil appeared in the spirit, when it was first poured off; but after standing some time, it fell to the bottom in the form of a black sediment. The black coal, calcined under a muffle in a very strong fire, lost only 22 grains, and became a white insipid earth.

A fragment of the great stone, which had lain exposed to the air and weather above 12 months, as mentioned above, weighing in the air 58 gr. weighed in water $24\frac{4}{10}$ gr. after it had stood a considerable time, that the water might enter its cavities: so that this stone, though seeming of a loose texture, came out not much inferior to the other in specific gravity; this being to water as 165 to 100.

A portion of this stone, in a strong open fire, lost in calcination just half its weight; becoming, as the former, a white insipid earth; which, being infused in boiling water, made no alteration in its colour, taste, nor smell.

Three oz. of this stone distilled, produced $24\frac{1}{8}$ gr. more than $1\frac{1}{4}$ oz. of the like alkaline spirit as the former yielded, and left a black coal, weighing $16\frac{1}{10}$ gr. above $1\frac{1}{8}$ oz.

From this chemical analysis, it appears that these stones are compounded chiefly of earth, a large quantity of volatile alkaline salt and water, some oil, and a small quantity of air. From hence it also appears, that the component principles of these stones bear a nearer resemblance to those of hartshorn, than that of the calculus humanus: for, according to Dr. Hales's account, in his Vegetable Statics, experiment N° 51, 241 grains of deer's-horn, being distilled, left a calx weighing 128 gr.; viz. above half its weight; which shows that horn contains much about the same quantity of earth as these stones do: whereas the calculus humanus, when distilled, affords but a small quantity of earth, spirit, or oil; the greatest part of it being converted into air.

Half an oz. of one of the stones being powdered, and infused in 4 oz. of boiling water, made it smell strongly of horse-dung, and gave it a disagreeable taste, while it was hot; but, when the infusion was cold, it lost its taste and smell, and the water, after standing some time, became, without being filtered, as pale and clear as before; nor did the mixture of it with oil of tartar, oil of vitriol, nor aquafortis, produce any alteration in it. This experiment, being repeated several times, by infusing boiling water on the same powder, was attended with the same effects, though near 2 months passed between the first and last infusions. The powder, after the first infusion, appeared like mud of two different colours and consistencies; the upper part being softer, and of a lighter colour, the under of a dark-brown, feeling hard like sand. This difference in the colour and texture of the sediment remained in all the infusions.

A small piece of the same, and another of the largest stone, being let fall into boiling water, sunk immediately, and continued at the bottom, without rising at all, though the water was kept boiling a considerable time: which shows, that these stones are specifically heavier than the stone found in the stomach of a horse, which Mr. Watson gives an account of in the Philos. Trans. N° 475; and also that their constituent principles are more firmly united together, than those of that stone; 2 pieces of which, being let fall into water almost boiling, immediately sunk, but rose again, and continued alternately rising and sinking a considerable time: and, as that gentleman observes, the powder of that stone being infused in boiling water, the infusion, when cold and filtered, was of a light-brown colour; whereas the colour of the water was not changed in either of the abovementioned infusions; neither did any ebullition ensue on the mixture of them with oil of tartar, vitriol, &c.

Dr. B. tried to dissolve these stones, by digesting small pieces of them in the strongest acid and alkaline menstrua, viz. spirit of salt, sulphur, oil of vitriol, aquafortis, and capital soap-lees, &c. and at the same time he tried the effects of those menstrua on several stones, which were given him by some of his patients, who voided them. They were softened by some of them, but not totally dissolved by any, except the oil of vitriol and aquafortis: nor did the aquafortis cause any ebullition in dissolving them, as it did in the solution of the calculus humanus, which was attended with a brisk ebullition, and hissing noise, arising from the eruption of the air bubbles from it: which confirms what he observed above, that these stones contain but a very small quantity of air, and that their saline and oily particles are so closely combined with earth, as not to be extracted without a strong fire.

On the Petrifactions of Lough-Neagh in Ireland. By Mr. James Simon, of Dublin. N° 481, p. 305.

Most of the ancient writers, who have treated of Ireland, have mentioned the peculiar qualities of Lough-neagh, of turning wood into stone; some of them* have gone so far as to say, that it would turn that part of the wood which was in the mud into iron; the part in the water into stone; while the part above water remained wood.

Some later writers, particularly Wm. Molyneux, Francis Nevil, and Edward Smyth, and from them the late Dr. Woodward,† and others,‡ seem rather to think, that this petrifying quality does not lie so much in the lake itself, as in the ground near or about it.

Mr. Edward Smyth,§ who enlarges most on this subject, and seems to have led the others, and drawn them into his opinion, tells us, "That no experiment or observation yet made, which he had heard of, could prove that this lough has really the quality of petrifying wood, or that the water does any way help or promote the petrification." He there gives an example of a gentleman of worth and credit, "who had fixed two stakes of holly in two different places of the lough, near that place where the upper-bann enters into it, and that the parts of the stakes which had been washed by the water for about 19 years, yet remained there without any alteration, or the least advance to petrification."

Another reason for his doubting of this quality is, "That though it is reported that the water has this virtue, especially where the black-water discharges itself into the lake, yet that as it seems evident, from the nature of liquid bodies, that any virtue received in one part must necessarily be diffused through the whole, at least in some degree; therefore, says he, there is good reason to believe, that the water is wholly destitute of this petrifying quality:" but a few lines lower he tells you, "That he had sufficient ground to conjecture, that other wood as well as holly had been petrified about this lough; because some fishermen, being tenants to a gentleman from whom he had this account, told him, that they had found buried, in the mud of this lough, large trees, with all their branches and roots petrified; and some of that size, that they believed they could scarcely be drawn by a team of oxen; that they had broken off several branches as thick as a man's leg, and many thicker, but could not move the great trunk."

He supposes Mr. Smyth, or the gentleman his friend, saw these branches, and was thereby convinced of their real petrification, as he was by the bulk of those trees of their being oak, and not holly; "because, says he, no other tree in that

* Boetius Hist. Gem. et Lap.—Orig. † Catal. of English Fossils, part 2, p. 19.—Orig.
‡ Sir James Ware's Antiq. by Walt. Harris, p. 227. Edit. 1745, folio.—Orig.
§ Afterwards Bishop of Down. See Phil. Trans. N° 174.—Orig.

country, these excepted, grows to that vast size; at least it is certain that holly never does."

But how Mr. Smyth came to be convinced, that these trees were oak, and not holly, and yet was not convinced of the petrific quality in some parts of the lough, though these trees were found petrified in its mud, is amazing: for if a team of oxen could scarcely draw them from thence, it must be as hard to draw them from any adjacent ground (where they must have grown, lain, and be petrified) into the mud of the lake, where they were afterwards found: for it must be supposed, that either these trees grew on the banks of the lake, and, through age, or any other accident, fell into the water or mud, and were there petrified; or that, with great labour and expence, they were brought into it from some adjacent ground, after their actual petrification, which is hardly to be supposed.

Mr. Smyth tells you further, that "Two gentlemen of the north of Ireland where this lough lies, had told him, that they had seen the same body, partly wood, and partly stone; but the only reason for thinking so, being the diversity of colours, which might well enough proceed from several degrees of petrification, we may properly think them deceived; for they made no experiment on that part which they reputed wood. The bark is never found petrified, as he was informed by a diligent inquirer; but often somewhat rotten about the stone, answerable to the bark."

Mr. Smyth contradicts himself no less in his last supposition, than he did in the first. His friends assured him, that they had seen one or more of the Lough-neagh stones partly wood and partly stone; but they were deceived, he says: the diversity of colours, by which they judged one part of the stone by its colour to be wood, and the other part likewise, by its colour different from the other, to be stone, were no more than different degrees of petrification. What are we to understand by these different degrees of petrification? by this something rotten about the stone often found? if not, that some part of the wood was actually turned into stone, some other part in a degree less petrified, and some other part not petrified at all, as these gentlemen assured him: the diversity of colours, seeing and feeling, was enough to convince them, and to determine the point.

"The earth, says the great Robert Boyle* harbours different kinds of petrescent liquors, and many of them impregnated with one sort of mineral or other." There are no springs, no waters, but are more or less impregnated with such mineral and saline particles; which appears from the most limpid; which, after evaporation, still in the residuum, gives some particles of salt, with some stony and mineral ones.

Mr. S. has found by experience, that petrifying springs are generally impreg-

* R. Boyle, of the Origin and Virtues of Gems.—Orig.

nated, some with calcareous and particles of other stones, and others with ferruginecus and vitriolic particles. Those of the stony or calcareous kind, when they drop on wood, or other vegetables, act on them for the most part by incrustation, having different degrees and periods for their respective incrustations and coalitions, which yet adhere close to each other: they seldom turn the wood into stone; but, sticking to the wood, plants, &c. coagulate on it, and by degrees cover it with a crust of a whitish substance of different thickness, by which the wood is immerged or wrapped in a stony coat, which, if it be broken before the wood be rotten, you find it in the heart of the stone or incrustation, as is seen in those petrifications at Maudling meadows in Gloucestershire, at Hermitage near Dublin, and many other places: or, if the wood be rotten, you will find a cavity in the stone, which very often is filled by a subsequent incrustation or petrification; the stony particles then taking the place of the rotten wood.

Sometimes indeed, these waters, permeating the pores of the wood either longitudinally or transversely, insinuate themselves into them, fill them up with their stony particles, swell, and, by their burning or corroding quality proceeding from the lime-stone, destroy the wood, and assume the shape of the plant, the place of which they have taken.

These petrifications generally ferment with acids and spirit of vitriol, and by calcination may be reduced to lime.

Ferrugineous or metallic petrifying waters mostly act by insinuating their finest particles through the pores and vessels of the wood, or other vegetables, without increasing their bulk, or altering their texture, though they greatly increase their specific gravity: and such is the petrified wood found in or on the shores of Lough-Neagh; for it does not show any outward addition or coalition of forcing matter adhering to, or covering it (except in some places, where a thin slimy substance, taken notice of hereafter, is sometimes observed), but preserve the grain and vestigia of wood; all the alteration is in the weight and closeness, by the mineral particles pervading and filling the pores of the wood: these stones, or rather wood-stones, do not make the least effervescence with spirit or oil of vitriol, nor aquafortis; which shows that they are impregnated with metalline particles, or stony ones, different from the calcareous kind; and may be the reason why the petrified wood, mentioned by N. Grew,* made no ebullition, at which it seems he was surprised.† These stones he could not reduce into lime by the most intense fire, nor, with proper ingredients, procure a vitrification or fusion.‡

* Reg. Soc. Mus. p. 270.—Orig. † This contradicts an observation of Mr. John Beaumont, (Phil. Trans. N° 129), That mostly mineral stones will stir with acids; whereas all those that I have tried, whether English or Irish, did not at all stir with acids.—Orig. ‡ Stones of the calcareous kind turn to lime by calcination, and ferment with acids; but other kinds, such as slate, fire-stone, free-stone, rag, grill, &c. will do neither, as experience has hitherto testified.—Orig.

Though mines have not perhaps been discovered near the Lough, there is reason to believe that there are such in its neighbourhood, from the great quantity of iron-stones found on its shores, and places adjacent to it, and from the yellowish ochre and clay to be met with in many places near it. Of these iron-stones, which are very ponderous, outwardly of an ocherish yellow colour, and inwardly of a reddish brown, he calcined many, and found the powder of all to yield strongly to the magnet. Gerald Boate* mentions an iron mine, in the county of Tyrone, not far from the Lough, and such others at the foot of Slew-Gallen mountains.

That mines are generated and found in the bowels of hills and mountains, is obvious to any that have the least knowledge of metallurgy; and that springs also proceed from mountains, is no less obvious; therefore should a spring happen in the bowels of any of these mountains to run through a vein of mineral of any kind whatever, it will wash and dilute some parts of such mineral, impregnate itself with the unctuous, saline, and metallic particles of such mines, and convey them along with its water; and if in its way, whether under-ground, or at its issuing out of the cliffs of a mountain, of the sides of a river, or of the lake in question; or whether it rises under water, in the middle of such a river or lake in any particular place, and in its course meets with wood, vegetables, or any other lax bodies (lodged in the mud or gravel), whose pores, by the natural heat of the mineral streams, or any other accident, being open and duly prepared, these metallic moleculæ and saline particles will penetrate through, insinuate and lodge themselves in the pores and vessels of such wood, &c. fill them up, and, by degrees, turn them into stone;† "There being some of these lapidescent juices of so fine a substance, yet of so petrifying a virtue, that they will penetrate and petrify bodies of very different kinds, and yet scarcely, if at all, visibly increase their bulk, or change their shape and colour."

That such springs there are, hidden under the water or mud of this lake, will appear probable, from what has been said, and perhaps evident, from the accounts since received, that in the great frost of 1740, the lake was frozen over so as to bear men on horseback, yet several circular spaces continued unfrozen. But how the several attempts, made, as mentioned, by Messrs. Molineux, Nevil, and Smyth, to procure wood half-petrified (by fixing stakes of holly in the lake, which received no alteration) proved unsuccessful, the reason I think is plain, because they were not fixed in the proper place, viz. the course or vein of the spring, where nothing but chance could have directed them. This petrified wood is often found in different places on the shores of the lough, but generally in greater plenty when the water has been disturbed by great storms; which makes

* Nat. Hist. of Ireland, Dub. 1726.—Orig. † Rob. Boyle of Gems, p. 124, 8vo.—Orig.

it impossible to fix on the particular place where the petrifying juice most prevails; except a tree, or any large piece, should be found so fixed as to resist the force of the waves.

That this petrific quality is in some peculiar parts of the lake, he has endeavoured to prove; that it is or may be in some peculiar places of the adjacent ground, he grants; though as yet he could not procure any of those stones found in the ground, with wood continuous. Such as he has seen, are of the white whetstone kind, and seem to be holly or ash, petrified by some strong nitrous and stony particles; for, in a solution of it in aquafortis and oil of vitriol, it leaves no tincture, but the liquor growing muddy, like pipe-water after great rains, and therefore shows that they are not so strongly impregnated with metalline particles, as those stones found in or on the shores of the lake.

Mineral streams or exhalations, being highly saturated with stony and mineral particles, are often found to have a petrifying virtue, as is seen at the bath called Green Pillars* in the city of Buda in Hungary. If such streams should, in certain places, find or force their way through the sand or pores of the earth, they may operate on wood, &c. buried in the ground, permeate its vessels, and by degrees turn it into stone; and such is the most probable, if not the only reason, that can be assigned for those petrifications of wood found in sand, as mentioned by Boyle and Plot.

He received last summer, 1745, from a friend about 30 of these stones, found on the shores of the lake, some in the water, some in the mud, some in the sand, and others in a yellowish clay. That they were petrified in the lake is probable, but whether in the water, mud, sand, or clay, is no matter; for certain it is (to use Mr. Smyth's own words), that they were not brought hither from any distance, such as 2, 4, 6, 8 miles, after being dug out of the ground, and then thrown and dispersed on the shores of the lake: and besides, the difference in the colour of these stones, those found in the lake, and those found in the ground somewhat distant from it, is such that they cannot well be mistaken for each other. Those found in the ground are white, and of a looser texture; those found in or on the shores of the lake are black, closer, and heavier. That these last were petrified by a mineral spring, appears from the few following observations.—They do not ferment with acids, spirit and oil of vitriol. The solution of this stone in aquafortis gives a beautiful red tincture, and in oil of vitriol leaves a tincture of a brown dark red. The woody part of these stones in aquafortis also gives a red tincture, though somewhat paler; and, when taken out of the liquor, shows red spots in its pores, which he takes to be particles of iron and sulphur: these spots, when the wood began to dry, became black; and the wood, when dry, turned of the colour of a deep red Jesuit's-bark.

* Philos. Trans. N° 59.—Orig.

In some of these stones, several curious veins, of a red and bluish colour, are very remarkable, being intermixed with black and white striæ. Having broken some of them, he found in the inside a kind of white, and several clusters of small white and black angular crystals, which through the microscope appear transparent, and of different shapes, but mostly hexagonal. He discovered such crystals in some of the woody part of these stones.

One piece of a white stone he calcined in a crucible for 24 hours, but could neither reduce it to coal nor lime. The powder yielded faintly to the magnet. This stone was found in the ground at some distance from the lake. One piece of a black stone, found in the lake, he likewise calcined for 24 hours, and could not reduce it to coal or lime: the powder yielded briskly to the magnet. He calcined one piece of another stone, about one inch thick, for about 4 hours, in an intense fire, till it grew as red as it could be, when he took it out of the crucible. He observed several veins, not discernible before, of a ferruginous matter, about $\frac{1}{10}$ of an inch thick, and when reduced to powder, it applied strongly to the magnet.

In other stones he found some veins of wood, about 1 and 2 inches thick, no way petrified, though the stones were every way so outwardly. Some of that woody part he also burnt in a crucible; it emitted a bluish flame, as if impregnated with sulphur, and had the strong smell of burning charcoal. When burnt to a coal, and reduced into powder, it faintly yielded to the magnet.

He calcined another of these stones, weighing 1 oz. 13 dwts. $12\frac{1}{2}$ gr.; after burning 4 hours it weighed only 1 oz. 10 dwts. $8\frac{1}{2}$ gr. and lost 3 dwts. 4 gr.; which proceeds probably from unpetrified veins of wood in the heart of the stone, which were destroyed by the fire, as in the crucible it emitted now and then a bluish flame, like brandy when burning. This stone, when taken out of the crucible, and cooled, had the colour of iron, when heated in, and cooled from the forge.

Part of another stone, which, by visible veins of ore, appeared to contain a good deal of iron, he likewise calcined for 4 hours; the powder yielded most surprisingly to the magnet; so that it appears, that the opinion of Nennius, Boetius, and other ancient writers, was not absolutely destitute of foundation.

The white wood-stones are generally found in the ground at 2, 4, 6, and 8 miles distance from the lake, and sometimes very deep in the earth. The black ones are always found in the water, or on the shores of the lough; sometimes at the mouths of rivers or rivulets that empty themselves into it; but those with wood continuous have not yet been found above 20 yards distance from the water of the lake; that is, where the water reaches in the winter, or at other times.

Some of these stones are outwardly covered with a thin white substance, which has run through the pores of that part of the stone that was exposed to the air, and not covered by the water, mud, or clay; and on some others it is rather an incrustation of that white substance, which he takes to be the slimy, unctuous, saline parts of the petrescent juices that filled the outward pores of the stone, or coagulated on it. This white part scraped, and put into a crucible in a violent fire, could not be reduced to lime, though it grew red as coal. This powder calcined appeared through the microscope quadrangular, like grains of salt; which made him suspect, that these petrifications contain, besides metalline, a great deal of saline particles, whose sides being strongly attracted to each other, and closely joined, hinders the fire from expanding the pores of these stones, and their being reduced to lime. This black stone, when broken, appears through the microscope very beautiful, and like cloth of silver, the pores and vessels of the wood being filled with white minute crystals.

Of these stones Mr. S. had some with wood outwardly continuous; others with wood inwardly; one, the least part is of stone, the rest wood; another vice versa; another entirely wood, except a thin coat of stone on one side, which appears to be the very bark; one stone which at one end distinctly shows the annual ringlets of the wood; one that shows the wood, before it was petrified, had been bent, and partly broken, the fissure being filled with a sparry matter, and appears plainly from the present appearance and position of the fibres of the stone. Some of these stones strike fire with a steel, and others, by a strong collision, emit a train of sparks. Some of these stones show the grain of holly, ash, and fir. He had only one piece of oak petrified, easily distinguished by its grain; it shows the very knots of the wood where young twigs were cut; and has a hole made through it before it was petrified.

As for these stones being fit for sharpening or setting of razors, &c. the black ones are rather too hard, and the white ones too soft. The whetstones or hones, vulgarly so called, which are sold for Lough-Neagh stones, are none of these, but of a soft gritty kind, and found near Drogheda.

When these stones with wood continuous are taken out of the water, mud, or clay, the woody part dries, cracks, and falls away; which is the reason why few can be well preserved; and besides, every body, unwilling to trust their eyes, will touch and scrape the wood, and thus destroy the most curious part of the stone.

On the same Subject. By the Right Rev. Dr. George Berkeley, Lord Bishop of Cloyne.* N° 481, p. 325.

Mr. Simon seems to put it out of doubt, that there is a petrifying quality both in the lake and adjacent earth. What he remarks on the unfrozen spots in the lake is curious, and furnishes a sufficient answer to those who would deny any petrifying virtue to be in the water, from experiments not succeeding in some

* Bishop Berkeley was celebrated for his religious, moral, and metaphysical writings, of which an account is to be found in numerous biographical publications. He also wrote a Treatise on the Virtues of Tar Water, which he recommended with much enthusiasm in scorbutic and other cachectic disorders. He was born at Kilcrin, in Ireland, 1684, and died at Oxford in 1753.

parts of it; since nothing but chance could have directed to the proper places, which probably were those unfrozen parts.

Stones have been thought by some to be organized vegetables, and to be produced from seed. To Bp. B. it seems, that stones are vegetables unorganized. Other vegetables are nourished and grow by a solution of salt attracted into their tubes or vessels. And stones grow by the accretion of salts, which often shoot into angular and regular figures. This appears in the formation of crystals on the alps: and that stones are formed by the simple attraction and accretion of salts, appears in the tartar on the inside of a claret-vessel, and especially in the formation of a stone in the human body.

The air is in many places impregnated with such salts. He had seen at Agrigentum, in Sicily, the pillars of stone in an ancient temple corroded and consumed by the air, while the shells which entered the composition of the stone remained entire and untouched. He has elsewhere observed marble to be consumed in the same manner; and it is common to see softer kinds of stone moulder and dissolve merely by the air acting as a menstruum. Therefore the air may be presumed to contain many such salts, or stony particles.

Air, acting as a menstruum in the cavities of the earth, may become saturated (in like manner as above-ground) with such salts as, ascending in vapours or exhalations, may petrify wood, whether lying in the ground adjacent, or in the bottom of the lake. This is confirmed by the author's own remark on the bath called the Green Pillars in Hungary. The insinuating of such salts into the wood seems also confirmed by the author's having observed minute hexagonal crystals in the woody part of the petrifactions of Lough-Neagh.

A petrifying quality shows itself in all parts of this terraqueous globe, in water, earth, and sand; in Tartary for instance, and Afric, in the bodies of most sorts of animals, it is even known that a child has been petrified in the mother's womb. Osteocolla grows in the land, and coral in the sea. Grottoes, springs, lakes, and rivers, are in many parts remarkable for this same quality. No man therefore can question the possibility of such a thing as petrified wood; though perhaps the petrifying quality might not be originally in the earth or water, but in the vapour or steam impregnated with saline or stony particles. Perhaps the petrifaction of wood may receive some light from considering amber, which is dug up in the king of Prussia's dominions.

The bishop adds another remark, which may be useful for the better understanding of the nature of stone. In the vulgar definition, it is said to be a fossil incapable of fusion. He had nevertheless known stone to be melted, and when cold to become stone again. Such is that stuff, by the natives called sciara, which runs down in liquid burning torrents from the craters of mount Etna, and which, when cold and hard, is hewed and employed at Catania, and other places adjacent. It probably contains mineral and metallic particles; being a ponderous, hard, grey, stone, used for the most part in the basements and coins of buildings.

Hence it should seem not impossible for stone to be cast or run into the shape of columns,* vases, statues, or relievos; which experiment may perhaps, some time or other, be attempted by the curious; who, following where nature has shown the way, may (possibly by the aid of certain salts and minerals) arrive at a method for melting and running stone, both to their own profit, and that of the public.

Observations on a sort of Libella † *or Ephemeron. By Mr. Peter Collinson, F.R.S.* N° 481, p. 329.

Walking by the river's side at Winchester, Mr. C. was told, that now was the time of year that the May flies, a species of libella, came up out of the waters, and were seen for a few days, and then disappeared. This excited his curiosity, having never seen this insect.

May 26, 1744, he was first shown it by the name of May fly, on account of its annual appearance in that month. It lies all the year, except a few days, in the bottom or sides of the river, near the likeness of the nymph of the small common libellas; but when it is mature, it rises up to the surface of the water, and splits open its case; then, with great agility, up springs the new animal, with a slender body, with 4 blackish-veined transparent shining wings, with 4 black spots in the upper wings; the under wings much smaller than the upper ones; with 3 long hairs in its tail. See pl. 7, fig. 2, 3, 4; where fig. 2 represents the back of this insect; and fig. 3 and 4 side-views of the same.

The husk or exuviæ that it leaves behind floats innumerable on the water. It seemed to him a species of ephemeron; and he imagined it was the same insect described by Goedart and Swammerdam; but a few days convinced him otherwise, for he soon found these had a longer duration than theirs.

The next business, after this animal is disengaged from the water, is flying about to find a proper place to fix on, as trees, bushes, &c. to wait for its approaching change, which is effected in 2 or 3 days. The first hint he received of this wonderful operation, was seeing their exuviæ hanging on a hedge. He

* To confirm what the bishop says, I remember when I was in the college in France, that I went to see a relation of mine, a frier, at Fontevrand, where he showed me in their church two pillars of stone, about 60 feet high, all of one solid piece, which he said had been run. J. S.—Orig.

† I take this to be the musca tripilis mentioned in Moufet, Insect. Theat. p. 64, and may properly be called musca, libellæ affinis, cauda tripili. C. M:—Orig.

This insect is the *ephemera vulgata*, Linn. or common May-fly. When Mr. Collinson speaks of having never seen it, he must surely mean that he had never seen it in its early or nascent state.

then collected a great many, and put in boxes; and by strictly observing them, he could tell when they were ready to put off their old cloaths, though but so lately put on.

He had the pleasure to show his friends one that he held on his finger all the while it performed this great work: it was surprising to see how easily the back part of the fly split open, and produced the new birth, which he could not perceive partakes of any thing from its parent, but leaves head, body, wings, legs, and even its three-haired tail behind, or the cases of them. After it has reposed itself awhile, it flies with great briskness to seek its mate.

In the new fly a remarkable difference is seen in their sexes, which he did not so easily perceive in their first state, being then male and female much of a size, but now the male was much the smaller, and the hairs in their tail much the longer.

He was very careful to see if he could find them engendering, conceiving it to be much after the manner of the same species: but all that he could discover was, that the males separated, and kept under the cover of the trees, remote from the river. Hither the females resorted, and mixed with them in their flight, great numbers together, with a very brisk motion, of darting or striking at each other, when they met, with great vigour, like as house-flies will do in a sunny room. This they continued to do for many hours, and this seemed to be their way of coition; which must be quick and soon performed, as they are of so short duration. He tried several ways to make further discoveries, but all proved ineffectual.

When the females were impregnated, they soon left the company of the males, and sought the rivers, and kept constantly playing up and down on the waters. It was very plainly seen, every time they darted down, that they ejected a cluster of eggs, which seemed a pale bluish speck, like a small drop of milk, as they were sinking to the bottom of the river; and then, by the elasticity of their tails, they spring up again, and then dart down again. Thus they continue, till they have exhausted their stock of eggs, and spent their strength, being so weak that they can rise no more, but fall a prey to the fish.

But by much the greater numbers perish on the waters, which are covered with them. This is the end of the females; but the males never resort to the river that he could perceive; but after they have done their office, drop down, languish, and die, under the trees and bushes.

He observed that this species of libella abounded most with females; which was very necessary, considering the many enemies they have in their short appearance; for both birds and fish are very fond of them, and doubtless under the water they are a food for small aquatic insects.

What is further remarkable in this surprising creature is, that in a life of 3 or

4 days, it eats nothing, seems to have no apparatus for that purpose, but brings up with it out of the water sufficient support to enable it to shed its skin, and perform the principal ends of life with great vivacity.

They appear at 6 o'clock in the evening. On the 26th of May he perceived a few; but the 27th, 28th, 29th, and 30th, it was a sight very surprising and entertaining, to see the rivers teeming with innumerable pretty nimble flying animals, and almost every thing nearly covered with them; when he looked up, the air was full of them, as high as he could discern; and seemed so thick, and always in motion, the like it seems when one looks up and sees the snow coming down: and yet this wonderful appearance, in 3 or 4 days after the last of May, totally disappeared.

On a Stone taken out of the Bladder of a Dog; which being cut asunder had a Piece of Dog-grass in its Centre. By Mr. Wm. Fidge, Surgeon at Portsmouth. N° 482, p. 335.

The stone above alluded to was taken out of the bladder of a very large mastiff, about 5 years old, belonging to the porter of his majesty's dock-yard at Portsmouth. The dog died in about 3 days after receiving a kick from some one endeavouring to part him from another mastiff he was fighting with.

When Mr. F. had opened the abdomen, he found it filled with bloody urine; and having before heard that his death was supposed to be occasioned by the kick, he immediately thought the bladder must be the part hurt; which, when he had cleansed the abdomen, he examined, and found this large stone, with the bladder contracted close to it on every side, and rent at the bottom about $\frac{3}{4}$ of an inch; so that what urine came to the bladder was discharged into the abdomen; which was plainly the cause of the dog's death.

When first taken out it weighed 10 oz. $2\frac{1}{8}$ drs. After it was cut asunder, he found it formed on what seemed to be a piece of dog-grass.

He did not find the least particle of gravel or sand, either in the kidneys or ureters; but all the bones (except the ribs and cranium) were more or less carious.

An Uncommon Dropsy from the Want of a Kidney; and the Description of a large Saccus that contained the Water. By Samuel Glass, Surgeon at Oxford. N° 482, p. 337.

Mary Nix, at Hampton-Poyle, a small village in Oxfordshire, had been remarkable all her life for the preternatural size of her belly. After her death, Mr. G. had the curiosity, with some learned gentlemen of the university, to inspect her body. Her mother was then present, and informed them that this her daughter was born dropsical; that she herself had been ill of the same disease for some time before and during her pregnancy; but on the birth of this child she was freed from that disorder. The child, though born dropsical, proved otherwise healthy; and notwithstanding the disease continually increased as she grew up, she lived to be near 23 years of age.

She was a tall well-proportioned woman, except with regard to the enormous size of her belly; and for one of so unwieldy a bulk, healthy, brisk, and active. Her appetite was always good, and she was never more than ordinarily thirsty; had no remarkable difficulty of breathing, not even when she lay supine, nor did her thighs or legs ever swell. Her menses, which appeared at the usual time of life, continued regular, till within 8 months of her death. The only complaint was now and then a pain in making water; and the quantity she made was commonly about 4 or 5 oz.

On the suppression of her catamenia, there succeeded a dyspnœa, loss of appetite, emaciation of the superior parts, and a tumefaction of one of her legs with ulcerations. These symptoms gradually increased till her death.

On taking the dimensions of her body before dissection, they found the circumference of her abdomen to be just 6 feet 4 inches, and from the xyphoid cartilage to the os pubis it measured 4 feet and ½ an inch. The cutaneous vessels, distributed on the abdomen, were remarkably large, and distended with blood, and the spurious ribs were pressed greatly outwards and upwards.

The thorax being laid open, they observed that the diaphragm was forcibly protruded into that cavity. The base of the heart lay under the right clavicle, and its apex on the most convex part of the diaphragm; which convexity advanced as high up as the 3d superior rib. The lungs were surprisingly small, scarcely exceeding in magnitude those of a new-born child. The right lobe slightly adhered to the pleura, the left was free, and both were in a sound state. Within the pericardium was found, as usual, a small quantity of liquor, but none in the cavity of the thorax.

They next perforated the abdomen in the most convenient depending part, and evacuated from thence a surprising quantity of water, which was lightly tinged of a coffee-colour, limpid as urine, and not in the least fetid. This water was carefully measured, and found to be not above a pint less than 30 gallons wine measure, which must weigh, according to the common calculation, near 240lb.

They afterwards made an incision into the abdomen along the linea alba. The integuments on the epigastric region were very thin; the abdominal muscles much extenuated; and above the umbilicus the tunica cellulosa contained no fat; but from the navel to the os pubis, the panniculus adiposus was ½ an inch thick. On dilating the incision, the large membranous bag that contained the water presented itself to view, adhering transversely about 10 inches to the anterior part of the peritonæum.

This adhesion being separated, they had a full view of this wonderful reservoir, which was of an enormous size, and had almost occupied the whole cavity of the abdomen. In figure, colour, thickness, number, magnitude, and distribution of blood-vessels, it very much resembled the uterus of a cow at the end of gestation. The whole inside was scabrous, and looked as if parboiled; and here and there was observed a small quantity of a coffee-coloured sediment. On the left interior part was discovered the orifice of a duct, which opened obliquely into the cavity of the saccus, and would easily admit of a large goose-quill. From this opening the tube advanced about 12 inches between the membranes of the bag obliquely upward, and toward the right, from whence it was inflected downwards, and passed between the duplicature of the ligamentum latum uteri, to be inserted into the bladder of the urine. The saccus was connected to the ligamentum suspensorium hepatis, to a considerable part of the mesocolon, to the peritonæum on the right side in 2 or 3 different places, to the same membrane the whole length of the spine, and to the ligamentum latum uteri on the right side of the body.

The liver was sound, but less than in a natural state; and its convex part adhered closely to the diaphragm. The stomach, spleen, omentum, small intestines, and the upper part of the colon, were thrust very high up into the left hypochondrium. The convolutions of the lower part of the same intestine were entirely obliterated; and that, with the rectum, formed one continued straight tube, from the left hypochondrium down to the anus. The left kidney, with its emulgent vessels and ureter, were in their natural state and situation. The uterus, tuba Fallopiana, and ovarium, on the same side, had nothing preternatural; but on the right side, the Fallopian tube and ovary were disposed in a very extraordinary manner. The tube, by means of the adhesion of the ligamentum latum uteri to the saccus, was extended to 3 times its ordinary length. The ovary was likewise, by the same cause, rendered very preternatural, being no less than $5\frac{3}{4}$ inches long, 1 inch broad, $\frac{2}{10}$ of an inch thick, and 2 inches and $\frac{1}{8}$ distant from the uterus. The bladder of urine was very small, but appeared to be sound.

They then made an accurate search for the right kidney; but to their great surprise, found no such viscus, nor any thing analogous to it, unless the saccus that contained the water already described, may be esteemed such: and what seemed to favour this opinion, was the disposition of the emulgent vessels on the right side, which were propagated from the aorta and vena cava to this saccus, in the same manner as to the kidney on the opposite side; and after having run

12 or 14 inches between the membranes of the bag without any ramifications, were distributed all over it in the manner before mentioned.

The following queries are added:

Was not the saccus originally a mishapen kidney, and the duct a ureter? Was not the water contained in the saccus prevented from growing putrid, by being continually drained off through the duct into the bladder of urine, and by being afresh supplied by the emulgent artery; and more being secreted than was evacuated, the quantity thereby continually increased? Was not this the reason why the patient had never any anasarcous swellings of her thighs or legs, nor any thirst, or other signs of a confirmed dropsy? Were not the lungs prevented from growing by the great diminution of the cavity of the thorax, and the pressure they sustained from the distended abdomen? And might not their never having occupied a larger space than they did at birth, be the reason she never laboured under any difficulty of breathing? Was not the bladder of urine likewise by the superincumbent weight, prevented from dilating itself; and that the reason why the water was often made, and always in so small a quantity?

An Explanation of an ancient Inscription discovered at Rutchester, the last Station in England, on the Roman Wall, 1744. By John Taylor, LL.D., Chancellor of Lincoln, and Register of the University of Cambridge. N° 482, p. 344.

The inscription is,

IMP. CÆS. M. AVRELIO.
SEVERO. ANTONINO.
AV. PIO. FELICI .X. PARTHIC.
i.e. MAX. BRIT. MAX. GERM.
Augusto. MAX. PÒNTIFICI. MAXIM.
TRIB. POTEST. XVIII. IMP. II.
COS. IIII. PROCOS. P.P. CO-I. I.
FIDA. VARHVL. CREO⊕ANO
NNANA. FECIT. SVB. CVRA. TCO
LEG. XX. GR.

according to the copy given him by Dr. Hunter of Durham, who copied the inscription.

This inscription addressed to Caracalla has nothing in it very singular, except the title of the cohort that dedicated it, namely,

FIDA. VARDVL. CRE^{O}⊕ANONNANA.

concerning which Dr. T. offers these few conjectures.

The Varduli were a people of Hispania citerior, mentioned by Pomponius Mela, and others; and are recorded now, in no less than 3 inscriptions, to have served in Britain as auxiliaries. The 2 others are printed in Horsley's Britannia Romana, Northumb. N° xciv. Durh. xxvi. We find troops of several nations to have been here on the same occasion, as Cohors prima, secunda, &c. Batavorum, Dacorum, Nerviorum, Tungrorum, Delmatarum, Thracum, &c.

This cohort of the Vardul is entitled FIDA, a very common appellation; and moreover CREO⊕ANONNANA; the last letters of which he separates, and reads without inserting a singe letter ANTONINIANA, thus, ANTONNANA. Nothing is so frequent in inscriptions, as this compendiary way of writing ANTONINVS, and its derivatives.

On this hint therefore he is persuaded, that when the stone is next inspected, these little apices will appear, which are easily overlooked, when this brief manner of writing is not expected or attended to.

In regard to the appellation Antoniniana, it is observable of the ancient militia, that several of their cohorts and legions, as well Roman as provincial, complimented themselves with the imperial surname, of which Dr. T. produces some instances.

But the imperatorial addition ANTONINIANA is perhaps the most frequent of any, as the name of Antoninus was assumed by a long series of emperors; as in several instances here produced.

What remains to be accounted for, is CREO⊕. Which letters if any one should compare with Horsley's Durham inscription, N° xxvi., where mention is made of the same Varduli, he will find a very strong resemblance; and be apt to conclude, that what explains the one, will bid very fair to explain the other.

CR then he takes to be distinct marks, and expressive of Civium Romanorum. And of this he finds little room to doubt, when he observed the same marks applied to several corps, who were as strictly provincial as our Varduli, such as Afri, Asturienses, &c.

The freedom of the city had been for some time before this a regular reward for the fidelity of the provinces, or any other military or civil merit. The famous oration of the emperor Claudius, or the act for incorporating the people of Vienne in Gaul (a large fragment of which is preserved in Gruter, p. DII.) is a remarkable instance of what is advanced. QVID. ERGO. NON. ITALICVS. SENATOR. PROVINCIALI. POTIOR? EST. IAM. VOBIS. CVM. HANC. PARTEM. CENSURÆ. MEÆ. APPROBARE. CŒPERO. QVID. DE. EA. RE. SENTIAM. REBVS. OSTENDAM. SED. NE. PROVINCIALES. QVIDEM. SI. MODO. ORNARE. CVRIAM. POTERINT. REFICIENDOS. PVTO. For so the last words must be emended, as they have been restored by Reinesius and Grævius.

Afterwards, probably a little before the date of our inscription, which is near the end of the reign of Caracalla, came the general constitution of that emperor; the memory of which being fresh, might probably occasion the insertion of the words Civium Romanorum in this monument. In orbe Romano qui sunt, ex Constitutione Imperatoris Antonini Cives Romani effecti sunt, are the words of Ulpian, in the first book of the Digest. Tit. de Statu Hominum, Law XVII. That the Antoninus there mentioned was our Caracalla, is abundantly made good by Baron Spanheim, in his comment on that text. To which may be added the words of Prudentius, produced by the same learned writer on the occasion.

What remains, EO∞, Dr. T. restores EQ∞, according to the model of the Durham inscription produced above:

COH. VARDVL. C. R. EQ. ∞

For the difference in stones is so minute in this particular; that he pronounces it may as well be one as the other. And he reads the whole title of the cohort thus:

CO-IOrs Ima FIDA VARDVLorum Civium Romanorum EQuitate Milliaria ANTONINIANA.

concerning which he offers the following.

The frequent mention of equestrian cohorts, or, to speak more adequately, of Cohortes Equitatæ, in old inscriptions, has been a great cheque on several antiquaries, who have been taught to consider the cohorts as appropriated to the foot service, as the alæ and turmæ were to the horse. Mr. Horsley in particular, p. 94, imagines the cohors prima Claudia equitata, which he met with in the Notitia, was intended to intimate that this cohort had been promoted from the foot to the horse service. But when that gentleman was led, by the mark or monagram in the Durham inscription referred to in these papers, to consider that corps as consisting of 1000 horse, his difficulty is increased to that degree, that he knows not what to affirm on it. Now of all this there is a very easy solution. The auxiliary or provincial cohorts were either entirely or purely foot, like the legionary and ordinary cohorts; or else they had a mixture of both kinds of militia, and consisted of horse and foot together.

This latter sort, as they could not properly be ranked under either denomination of horse or foot, for they were made up of both, seem to have appropriated to themselves the distinguishing title of COHORTES EQUITATÆ, corps of infantry with a mixture of horse. And of this term, so very significant, and so little understood, he finds frequent mention.

Nor have we these testimonies only, but also a full and decisive proof of this denomination, and, what is yet behind, of their number also, in a writer very well versed in military affairs, Hyginus, who wrote a Treatise de Castrametatione, in the time of Trajan. From him we are informed, that these troops were called milliariæ, as consisting of 1000 private men, part horse and part foot. The proportion of the former of these to the latter, was nearly as 1 to 3, viz. 240 to 760, instead of 250 to 750. Which little difference was possibly occasioned by the necessity of dividing them into centuriæ and turmæ.

For the mark ⊗ he accounts thus: the usual note of a thousand is either I between cc's, thus CIↃ; or else X, thus CXↃ. The former figure, when closed at the top, exactly resembles an ancient M, thus ⊕; and the latter, when shut up, a figure of eight inclined ∞. Both which marks have been long used to express a thousand. The latter is the mark before us, the X between cc's, but closed in on all sides thus ⊗, if this be in reality the figure on the stone. For Mr. Gordon in his Iter Septentrionale copying an inscription, having the mark of four thousands, gave us the thousand inclosed on all sides, the very mark in our inscription; but on Mr. Horsley's inspection it turned out to be the second figure, the thousand inclosed only at both ends thus ∞.

The last part therefore of the inscription is to be thus understood:

COHors I^ma FIDA VARDVLorum Civium ROMANORUM EQUitata MIlliaria ANTONINIANA FECIT SVB CVRA T. CO........ Legati, Tribuni, or Centurionis LEGionis XX^mæ Genio ROMæ.

Which last words are to be applied to the emperor, and contain a compliment at that time not unusual.

Abstract of the Rev. Mr. Gould's Account of English Ants. By the Rev. Henry Miles, D. D., F. R. S. N° 482, p. 351.

This treatise on English ants contains, 1. Their different species and mechanism. 2. Their manner of government, and a description of their several queens. 3. The production of their eggs, and process of the young. 4. The incessant labours of the workers, or common ants; with many other curiosities observable in these surprising insects.

Five species of ants have occurred to the observation of our author. 1. The hill ant, vulgarly called the horse ant. 2. The jet ant. 3. The red ant. 4. The common yellow ant. 5. The small black ant.

Having described the size and colour of these, he proceeds to describe the structure and nice mechanism of ants with great accuracy; observing, that besides the viscera, there is in the body of ants a bag of corroding spirituous liquor, which they can eject to a considerable distance at pleasure. This particular has also been observed by other writers.

He says he has met with a ligament in the red ant, which unites the breast and body, consisting of 2 lobes somewhat round; but in other ants there appears but one lobe, which rises higher, and is broader, than the lobes in the red. It is this species of red ants which he has observed to have a sting, of the same contexture with that of a bee, in miniature; in other ants he has met with no sting; but they bite, or make a small incision, with their saws, ejecting some of the aforesaid corroding liquor, &c. The red ants, which are furnished with a sting, he observes live more open, &c. and are more bold than any of the others, and therefore such a weapon is serviceable to them.

The jet ants have a peculiarly disagreeable smell, which he imagines may be a great preservative to them against an enemy; and that the spirit which all ants eject is very strong, affecting at a small distance in the same manner as spirits of hartshorn.

Chap. 2 treats of their colonies, cells, &c. Here the author observes, that though they unite in colonies, in such places and situations as are most agreeable to their different natures, &c. yet their residence is not so limited as to admit no variation; however, the several species never so intermix, as to associate and breed together, though they will live near and good neighbours with each other.

Their architecture is adjusted with remarkable curiosity and art; the whole structure being divided into a number and variety of cells, all communicating with each other by little subterraneous channels, which are circular and smooth; but as for the incrustation, most virtuosi have mentioned, in the apartments of ants, our ingenious author observes, that after the most careful observation he could never find any composition in their structures; the cells being formed in the mold itself, without any addition of glue, straws, &c. He acknowledges it may be otherwise in hotter climates, where sand is more apt to crumble.

Their works are all carried on by the assistance of their double saws, and the hooks at their extremity. The process and manner of their work may easily be observed, if you deposit some ants, with a lump of moist earth under a glass.

A colony, from the latter end of August to the beginning of June, is usually composed of a large female, and various companies of workers. And besides these, in the latter end of June, all July, and part of August, of a number of winged ants commonly known by the name of ant flies. The government has been universally taken for a republic or commonwealth; and has been treated as a body consisting of males and females; the former being those which make their appearance with wings in the summer. But as, in the economy of bees, the generality of them have no distinction of sex, but make it their whole employment to provide for the young laid by their queen, so the same character is found to be maintained in the constitution of ants. The common ants therefore, which usually present themselves to our view, are, he says, like the common bees, of neither sex, but seem entirely destined to take care of, and educate the young, which the queen deposits in the cells.

In every perfect colony there is at least one queen; who, in the space of 7 or 8 months, gives birth to a family, amounting, at a moderate computation, to 4 or 5000; except the red queens, who are not so prolific. The yellow ants being the most frequent, he gives a very particular and curious description of their queen; which is perhaps 5 times larger than any of her subjects; and in her front she has 3 eyes, in a triangular form, which are smaller than the 2 common ones on each side her head. The queen of the jets, he says, he never had the pleasure of seeing.

He has beautifully represented the obedience and respect the queen commands, in whatever apartment she condescends to be present. A universal gladness spreads itself through the whole cell, expressed by particular acts of joy and exultation; they have a particular way, it seems, of skipping, leaping, and standing on their hind legs, and prancing with the others; which frolics they make use of both to congratulate each other when they meet, and to show their regard for the queen. Some walk gently over her, others dance around her, and all endeavour to exert their loyalty and affection. However romantic, says the author, this description may seem, it may easily be proved, by placing a queen, with her retinue, under a glass; for in a few moments you will be convinced of the honour they pay, and esteem they have for her.

In October, ants and their queens begin to retire downwards; and in the depth of winter are to be found in the remotest apartments, incircled close with a cluster of attendants, and as it were benumbed.

The author has been the more circumstantial in the time and manner in which the queen lays the eggs, &c. to remove a mistake of Sir Edmond King's, who, not aware of there being a superior female, gave into the old opinion, that the small ants were the females, and supplied the colony with young; after a just description of the sperm or eggs, Sir Edmond observes, that he found that substance among the common ants, and that he gave the more credit to that opinion, because of the great care and tenderness with which they treat it. But our author does not allow this reason to be conclusive, inasmuch as the same is to be met with in the constitution of bees; adding, that having at all times of the year observed the common ants, he could never discern any alteration in their bodies but what was occasioned by food, or some accident.

The queen, he says, lays three different sorts of eggs, male, female, and neutral; the first two in the spring, the last in July and part of August.

The queen having furnished the eggs, the common ants brood over them in little clusters, perhaps by way of incubation; and remove them to different parts of the colony, for the better advantage of moisture, and a just degree of heat and cold. The time of continuance in the egg-state is rather uncertain; but he says they seem to disengage themselves from the membranes that inclose the eggs in the same manner as silk-worms do.

The process of ant vermicles is remarkable, and worth observation. The female eggs put on the form of worms some time in February, at latest; the male by the latter end of March; the neutral by September. The first summer they grow very sparingly, the succeeding winter they seem at a stand; in the beginning of April of the 2d year they visibly augment every day; and in 6 weeks, or by the end of May, the male and female attain their greatest proportions, and are ready for another change. This long continuance of ants in a vermicular state he thinks a great curiosity, hardly to be met with in any other class of insects, the female ant continuing above a year and quarter, the workers a year, the males somewhat more.

In treating of the transmutation of ant vermicles to nymphs or aurelias, he says, the vermicles weave in the manner of silk-worms, and in a few days infold themselves in a soft silken kind of tissue; they henceforth assume, and, while confined in this monument, continue the character of aurelias. These are the small bodies which abound in the settlements in the summer months, and are vulgarly reputed ant-eggs; but their size and visible transmutation, show the mistake.

He takes notice of a remarkable variation in the aurelias of the red ants. When the worms arrive at their period of transmutation, they do not infold themselves in a tissue or shell, like the others, but lie motionless, and to outward appearance insensible; in a few days they look whiter than ordinary, and in this manner gradually put on the form of ants. Thus Providence is tied down to no particular laws, but can by a surprising variety accomplish the same ends.

In the 7th chap. he treats of the transformation of the several aurelias to flies and common ants, with a description of their structure, duration, and other curiosities relating to the change. But the just progress of ants'-eggs, vermicles, nymphs, &c. cannot, he says, be precisely stated; because they will not arrive at maturity under glasses, as Swammerdam before him had observed.

As soon as the ant-nymphs, surrounded with a tissue, are tending to life, the workers give them air, by an aperture in the head part of the covering; which aperture they gradually enlarge; and after a day or two take out the young, and expose it to the freer access of the sun-beams, which are of great force in promoting its maturity.

He observes, that philosophers have usually confounded the 2 different sorts of ant flies, the large and small, considering them all as males; though there be so wide and manifest a variance in the colour, size, &c. that the naked eye may easily distinguish it. On the contrary therefore he presumes they are of different sexes; the small ones he takes to be males, and the large females; and thinks it highly probable, that some of these females afterwards give birth to new colonies, and entitle themselves to the dignity of queens; there being many strong experimental reasons to support so uncommon a curiosity; which he also recites, and answers the chief objection against it, taken from the number of these ant flies;

the principal thing of which his answer consists is, that the most obvious use of them is for the sustenance of other animals.

In the close of this chapter he annexes a few remarkable curiosities resulting from the change. The casting of their wings is an instance, he says, peculiar to the large ant-flies; these being to other insects their highest decorations; and the want of them lessens their beauty, and shortens their lives. On the reverse, a large ant-fly gains by the loss, and is afterwards promoted to a throne, and drops those external ornaments, as emblems of too much levity for a sovereign.

Chap. 6 treats of the incessant labours of the workers, the true method of collecting their provisions, and inquires into the truth of the opinion of laying up corn, &c. against winter, &c.

The feeding the young is the most laborious exercise belonging to the working ants, and a part of their industry the most uninterrupted of any. The juices of most sorts of fruit, insects, and honey, or any other delicious liquid, are the repast which they nurture them with. These juices they extract, and first convey into their own alvus, and afterwards infuse into the bodies of the vermicles; which aliment may probably undergo some refinement in the repositories of the ants, and, being there meliorated, is properly tempered for the delicate structure of the worms.

It has been a dispute among the inquisitive on this subject, whether ants have magazines of corn, and lay up a stock of provisions against winter. The generality of writers hold the affirmative; referring to Solomon, Pliny, Virgil, Horace, Aldrovand, Swammerdam, &c. Here it may be observed, in justice to Swammerdam, that, in his Biblia Naturæ, he expressly says, that he never at any time observed them to get together any food against winter; and is of opinion that during the severity of the winter they eat nothing, as is common with many insects, and some species of bees.

Our author, with great deference to the writers who have held the affirmative, and with extreme decency, differs from them, offering a handsome apology for himself. He suggests, that in warmer regions they may not undergo the chill they do with us; and therefore may not pass the winter in a state of numbness. That if this be the case, a store of food must be necessary to them, which is not to our northern ants, which live as it were entranced. He adds, that on the most impartial examination of authors, the opinion seems rather to be supported by its antiquity, than reduced to a clear demonstration. He tells us, that as on the most exact and frequent examination of numerous settlements in the winter, he could never trace out any reservoirs of corn, or other aliment; not even in those of the hill ants, which are the largest, and proportionably strong; so, to put this matter beyond all reasonable doubt, he had recourse to experiments;

which, had the supposition been true, could not probably fail of succeeding. At the beginning of spring, he placed in several flower pots, and other conveniencies, different colonies of yellow small black ants, &c. with their respective queens, attendants, and vermicles; in which position they continued summer, autumn, and winter, and carried on their operations as in other settlements, nourished their young, and brought them to perfection; whence he concludes, that they would have laid up provisions had it been their custom; but, on carefully examining some of these pots, he found no appearance of magazines of corn, or any collected food; and that on his having frequently observed their excursions from, and return to their colonies, he could never find that they ever returned with any wheat, corn, or any other vegetable seed; though they would with eagerness attack a pot of honey, or a jar of sweatmeats, &c.

Whether they lay in their food against winter, can only be determined by examining into the fact: this our author has done with great diligence, and has discovered, with respect to our English ants, that they eat not at all in the winter, and have no stores laid in of any sort of food. The opinion therefore of their laying in magazines against winter, seems to have been grafted on the scriptures, rather than found in them; and this from a conclusion naturally enough made, from observing their wonderful labour and industry in gathering their food in the summer; supposing that this must be to provide against winter. And after all, great part of their labour, which may have been bestowed in other services, might easily be mistaken, by less accurate observers, for carrying in food.

A Remark on Father Hardouin's Amendment of a Passage in Pliny's Natural History, Lib. 2, § 74, Edit. Paris. folio, 1723. By Martin Folkes, Esq. Pr. R. S. N° 482, p. 365.

Father Hardouin's reading of the passage is, "Vasaque horoscopa non ubique eadem sunt usui, in trecentis stadiis, aut ut longissime, in quingentis, mutantibus semet umbris solis. Itaque umbilici (quem gnomonem appellant) umbra in Ægypto meridiano tempore, æquinoctii die, paulo plusquam dimidiam gnomonis mensuram efficit. In urbe Roma nona pars gnomonis deest umbræ. In oppido Ancone superest quinta. Decima in parte Italiæ, quæ Venetia appellatur, eisdem horis umbra gnomoni par fit."

The geographical reader cannot but observe here immediately, that something is faulty in this passage as it stands; since the equinoctial shadow of the gnomon being made shorter at Ancona than at Rome, the latitude of Ancona must consequently be less than that of Rome; whereas it is known to be considerably greater; Ancona standing on the Adriatic, about 2 degrees to the north of that capital.

But on turning to Hardouin's observations on this passage, the text is found to have been altered by him in a very remarkable manner from all the former printed editions. And though he acquaints us he had met with some variation in the manuscripts, yet he appeals to none; nay, he even says expressly, that his amendment was purely made on conjecture; whence we may safely conclude, that it stands unsupported by any various reading or authority whatever. He also acknowledges that all the printed editions conspire in another reading, which Mr. F. has found to be true in several he has had occasion to look into, with this only variation, that whereas the first edition in 1469, and several of the following ones, print the word quinta at length, and xxx only in figures; some of the latter ones, and that by the Elzevirs particularly in 1635, print both the words at length. And the plain meaning of the passage is only this, that the length of the shadow of a gnomon, or upright style, at noon, on the day of the equinox, is in Egypt little more than half the height of the gnomon; that the same at Rome wants a 9th part of that height; that at Ancona the height of the gnomon exceeds the length of its shadow by a 35th part, or is in proportion to it as 35 is to 34; and that in the part of Italy called Venetia, the length of the shadow, and the height of the gnomon, are equal to each other.

The particulars here mentioned are respectively true, in the 4 following latitudes, 26° 34′, 41° 38′, 44° 10′, and 45°. The first of which is the latitude of the middle parts of Egypt, and the last that of several places in the territories of Venice, the city itself standing, according to Manfredi's table, in the latitude of 45° 33′, and Padua in that of 45° 28′.

It therefore appears on the whole, that this text needed no correction; and for the observation, that 35 parts were too many for a gnomon to be divided into, it will be found to have very little weight, when it is considered, that the ancients made use of very large gnomons on these occasions; that one of the obelisks now standing at Rome, that of St. John's Lateran, is in height 108 English feet, without the pedestal; and that the other, still buried under the Campo Marzo, which was formerly used for this very purpose, wanted but little of the same height. The 35th part therefore of the height of such a stone, did not fall short of 3 English feet; a much less quantity than which would easily discover itself in the shadow, whose length, notwithstanding all difficulties arising from the penumbra, might certainly be determined to less than half a foot.

It may be just added to this remark, from the description given by Pliny of this gnomon, lib. 36, § xiv, of the obelisks, that there was laid down, from the foot of the obelisk northward, a level pavement of stone, equal in breadth to the breadth of the obelisk itself, and equal in length to its shadow at noon on the shortest day; viz. that its length was to the height of the obelisk nearly as 22 to 10; and that into this pavement there were properly let in parallel rulers of

7

brass, whose distances from the point, directly under the apex of the obelisk, were respectively equal to the lengths of the shadow at noon, on the several days of the year; as the same lengths decreased from the shortest day to the longest, and again increased from the longest day to the shortest.

After which the author mentions in a passage, greatly corrupted, and therefore now almost unintelligible, that one Manilius, or Manlius, had added to the top of the obelisk a gilded ball, the use of which was to make the shadow of the extremity the more observable, as the middle part of the shadow of that globe could readily be estimated, whereas the shadow of an apex would, at so great a distance, be entirely imperceptible.

Concerning Spelter, Melting Iron with Pit-coal; and on a Burning Well at Broseley. By the Rev. Mr. Mason, Woodwardian Professor at Cambridge, and F. R. S. N° 482, p. 370.

What spelter is, or what uses are already made of it, Mr. M. professes not to know: but he believes it was never yet applied to so large a work as the cylinder of a fire-engine, till Mr. Ford, of Colebrook Dale in Shropshire, did it with success; it ran easier, and cast as true as brass, and bored full as well, or better, when it had been warmed a little; while cold, it is as brittle as glass, but the warmth of his hand soon made it so pliant, that he could wrap a shaving of it round his finger like a bit of paper. This metal never rusts, and therefore works better than iron, the rust of which, on the least intermission of working, resists the motion of the piston.

Several attempts have been made to run iron ore with pit-coal, he thinks it has not succeeded anywhere, as we have had no account of its being practised; but Mr. Ford, from iron ore and coal, both got in the same dale, makes iron brittle or tough, as he pleases; there being cannon thus cast so soft as to bear turning like wrought iron.

At Broseley, about a mile from the forementioned place, in the year 1711, a well was found, which burned with great violence, of which some account is given in Philos. Trans. N° 334; but it has been many years lost. The poor man, in whose land it was, missing the profit he used to have by showing it, applied his utmost endeavours to recover it; but all in vain, till May last; when attending to a rumbling noise under the ground, like what the former well made, though in a lower situation, and about 30 yards nearer to the river, he happened to hit on it again.

The well for 4 or 5 feet deep is 6 or 7 feet wide; within that is another less hole, of like depth, dug in the clay; in the bottom of which is placed a cylindric earthen vessel, of about 4 or 5 inches diameter at the mouth, having the bottom taken off, and the sides well fixed in the clay rammed close about it.

Within the pot is a brown water, thick as puddle, continually forced up with a violent motion, beyond that of boiling water, and a rumbling hollow noise, rising and falling by fits 5 or 6 inches; but there was no appearance of any vapour rising; which perhaps might have been visible, had not the sun shone so bright.

On putting down a candle at the end of a stick, at about a quarter of a yard distance, it took fire, darting and flashing in a violent manner, for about half a yard high, much in the manner of spirits in a lamp, but with a greater agitation. The man said, that a tea-kettle had been made to boil in 9 minutes time, and that he had left it burning 48 hours together, without any sensible diminution.

It was extinguished by putting a wet mop on it, which must be kept there a small time; otherwise it would not go out. On the removal of the mop, there succeeded a sulphureous smoke, lasting about a minute; and yet the water was very cold to the touch.

The well lies about 30 yards from the Severn; which, in that place, and for some miles above and below, runs in a vale full 100 yards perpendicular below the level of the country on either side, which inclines down to the country at an angle of 20 or 30 degrees from the horizon; but somewhat more or less in different places, according as the place is more or less rocky.

The country consists of rock, stone, earth, and clay, unequally mixed; and as the river, which is very rapid, washes away the soft and loose parts, the next successively slip into the channel; so as, by degrees, and in time, to affect the whole slope of the land: and as the inferior strata yield coal and iron-ore, their fermentation may produce this vapour, and force it to ascend with violence through the chinks of the earth, and give the water the great motion it has. This might be obstructed in one place by the forementioned subsiding of the sloping bank, and might afterwards find a vent in another; in like manner as it happened at Scarborough Spa, a few years since.

On the Effect of Electricity on Vegetables. By Mr. John Browning, of Bristol. N° 482, p. 373.

Having an operator at Bristol, with a good electrifying machine, Mr. B. was desirous to electrise a tree, and therefore sent him the following for that purpose, viz. laurustinus, leucoium majus flore pleno ferrugineo, and stœchas citrina cretica. These were not chosen with any design; but only convenient, being the least plants he had.

He promised himself the pleasure of seeing their leaves erected when electrised, but was disappointed; neither did the leaves flag on their being touched. However, he was agreeably recompensed, by a stream of fine purple blue coloured light, much resembling an amethyst, that issued from the extremity of each leaf upwards, of an inch in length, when the finger, or any other non-electric, approached near it. This colour he attributed to the watery particles in the earth, having often observed the very same colour issuing from the long leg of a syphon. On putting his finger on the gun-barrel to stop the electricity, the leaves of each tree had a trembling motion, which remained for some little time, and immediately ceased on withdrawing his finger from the barrel, and admitting the electricity.

The stœchas plant has a very long hoary leaf, and bears its blossom on a very small, slender, and almost naked stem, rising near a foot above the body of the plant. This stem had a motion given it, when any non-electric was brought within about 2 inches of its summit, much like the vibration of the pendulum of a clock; which vibrating motion was parallel with the breech of the gun, quite contrary to the same kind of motion he had before observed in a needle, hanging perpendicularly by a thread at the end of the gun; the needle always vibrating in the direction of the gun. The motion of the plant and needle always continued as long as the glass globe was excited.

He tried whether electricity could be propagated without mutual contact, by suspending another gun in silk cords, about 2 inches from contact; and found the electricity was nearly as strong in the second gun as in the first. At the distance of between 3 and 4 inches it was much abated, and so it gradually diminished, as the distance increased to near 6 inches, where it would scarcely attract a thread of trial.

Mr. Baker, since receiving the above account, had an opportunity of electrifying a myrtle-tree, of between 2 and 3 feet in height, growing in a pot; who found, that whenever the hand, or other non-electric body, was brought near the leaves, streams of fine purple fire issued from them, with a considerably cold air; and that the leaves were attracted at some distance, and moved vigorously towards a non-electric body.

Transit of Mercury over the Sun, Nov. 5, 1743, seen at the Observatory at Giesen. By Christian Lewis Gersten, Math. Prof. and F. R. S. N° 482, p. 376. *From the Latin.*

The sun being much obscured by flying clouds, the following observations only could be taken at intervals between them.—At $9^h\ 5^m\ 55^s$, true time corrected, Mercury was first seen on the sun's disk, being just wholly entered, but yet adhering to the edge. At $1^h\ 35^m\ 45^s$ afternoon, Mercury seemed to touch the sun's inner edge; at $1^h\ 35^m\ 50^s$ it certainly either touched or went a little beyond it. So that the interval between the two internal contacts was $4^h\ 29^m\ 50^s$; but the duration of the central transit he makes $4^h\ 33^m$. The sun's diameter by observation was 32′ 30″. The latitude of Giesen is 50° 30′. The angle of Mercury's apparent path with the ecliptic, was 8° 26′. The planet's latitude at the ingress 10′ 57″, at the conjunction 9′ 10″, at the egress 6′ 59″. The planet's horary motion 5′ 56″; and the inclination of its orbit 7′ 5″.

Observations on so much of Mons. le Monnier the younger's Memoir, lately presented to the Royal Society, as relates to the Communicating the Electric Virtue to Non-electrics. By Wm. Watson, F. R. S.* N° 482, p. 388.

One of the questions proposed to be examined is, in what manner the electric virtue is to be communicated to such bodies as yet have it not, and which are not capable of acquiring it by bare friction only?" M. le Monnier observes on this, "That no other manner is known, by which the electric virtue may be communicated, besides the near approach of a body actually possessed of the same: that the rule laid down by M. du Fay, That bodies never receive electricity by communication, unless they are supported by bodies electric in their own nature, does not always take place; and that it is liable to great exceptions: for, first, in the Leyden experiment, the phial filled with water is strongly electrified by communication, even when carried in the hand, which is not a body electric by nature."

To this Mr. Watson answered, that M. du Fay's rule is confirmed by all the experiments yet made public, and even by that of Leyden quoted by our author, or what is usually called that of Muschenbroeck. For, in this experiment, is not the non-electric water contained in and supported by the glass phial, which is electric in its own nature? Its being carried in the hand is no more than its being placed on any other non-electric body, and therefore is no proof against the general position. It is well known, that if the phial be made non-electric by wetting its outside, so as not to leave some inches perfectly dry, between its mouth and that part which is wetted, the water and phial part with the electricity as fast as they receive it, unless it is stopped by another electric per se.

Secondly, Our author mentions, "That all bodies which are electrified by means of a phial of water fitted to a wire, and which has already received a great deal of virtue by communication; all bodies, he says, placed in any curve line, connecting the exterior wire and that part of the bottle, which is below the surface of the water, acquire electricity without being placed on resin, silk, glass, or the like: that thus a violent concussion may be given to 200 men all at once; who holding each other by the hand form the curve just mentioned, when the first holds the bottle, and the last touches the wire with the end of his finger; and this equally, whether they are all mounted on cakes of resin, or stand on the floor: that the electricity has in this manner been carried through a wire of the

* See Phil. Trans., N° 481.—Orig.

length of 2000 toises, or near 2¼ English miles; part of which wire dragged on wet grass, went over hedges, palisados, and over land newly ploughed up."

The experiments in the 2d argument do nowise invalidate M. du Fay's rule; for their success depends on keeping whatever forms the curve line mentioned by our author, whether it consists of men or wire, in a non-electric state: and if whatever forms this curve line acquires any degree of electricity more than its original quantity, which it is well known may be done by being placed on originally electrics, the effect of the shock is proportionably lessened. Thus, if a man, standing on electrics per se, apply his hand to the phial of water, suspended by a wire to the electrified gun-barrel as usual, this person will acquire electricity, which will be sufficiently perceptible in him, by his attracting light substances held near his body, or by his firing inflammable ones, when properly presented to him; if a person then thus electrified, by applying one of his hands to the phial, touch the electrified gun-barrel with a finger of his other, let the phial be ever so strongly electrified, he feels but a slight stroke; and this stroke is greater or less, as the difference of the accumulation of electricity in the body of the man, and that of the water in the phial. Thus we know from experiment, that though a considerable quantity of the electricity, in impregnating the phial of water with it, pervades the glass, yet the loss of it in this way, is not equal to what comes in by the wire: therefore we will, for the sake of a more easy method of explanation, suppose that the phial, when electrified in the most perfect manner, contains a quantity of electricity equal to 10; that the man's body, by standing on wax, and touching the phial with one of his hands during its electrification, contains a quantity equal to 7: on his touching the gun-barrel with a finger of his other hand, he will receive a small stroke only equal to 3, the difference of the electricity of the water and that of his body: and if he touch the gun-barrel again, without removing his foot from the originally electric, the stroke will be scarcely perceptible, on account of his body being nearly of the same degree of electricity with the water in the phial. So that here we see that the violence of the shock, to be felt by whatever forms the curve line, depends on its being, in the most perfect manner, free from any degree of electricity more than the original quantity; which is contrary to the opinion of our author.

Thirdly, M. Monnier tells us, "That the water of the basin of the Thuilleries, whose surface is about an acre, has been electrified in the following manner: There was stretched round half the circumference of the basin an iron chain, which was entirely out of the water; the two extremities of this chain answered to those of one of the diameters of the octagon: an observer, placed at one of hose extremities, held the chain with his left hand, and dipped his right at the same time into the water of the basin; while another observer, at the opposite side of the basin, held the other end of the chain in his right hand, and a phial well-electrified in his left. He then caused the wire of his phial to touch an iron rod, fixed upright in a piece of cork that floated near the edge of the basin. At that instant both observers felt a violent shock in both their arms. The same fact was again confirmed by experiments made on two basins at the same time, that it might appear distinctly, that the electrical effluvia did really pass along the superficies of the water."

The water of the basin in this experiment was no more electrified than the wire which dragged along the ground, &c. was in the former. When Mr. W. was first informed, without being acquainted how, that an acre of water had been electrified, he was amazed, and told the gentleman who acquainted him, that if his idea of electricity was in the least true, such an effect could not be produced, without electrifying the whole terraqueous globe from a larger mass of matter. And indeed when he heard M. le Monnier's paper read, he easily saw the deception: so that, instead of electrifying the whole quantity of water contained in the basin, the electricity passed only through so much of it as formed a line between the iron rod fastened in the floating cork, and the hand of that observer which was dipped in the water.

These experiments still more and more establish the account Mr. W. lately laid down, of the electricity's always describing the shortest circuit between the electrified water and the gun-barrel; or, which is the same thing, the wire of the electrified phial. And this operation respects neither fluids nor solids, as such, but only as they are non-electric matter. Thus this circuit, in the preceding experiment between the phial and the wire, consisting of the 2 observers, the iron chain, the line of water, and the iron rod in the floating cork.

4thly, M. le Monnier mentions, "That it has been confirmed, by repeated comparisons, that a bar of iron, placed in the abovementioned curve, does not at all acquire more electricity when it is suspended in silken lines, than when it is held in the bare hand: whence it appears to him, that in this case the contiguous non-electric bodies do neither partake of, nor absorb in any way, the electricity which has been communicated."

The curve line beforementioned, let it consist of whatever non-electrics it will, unless the whole of it be properly supported, the communicated electricity cannot be accumulated: so that the suspending one part in silk lines cannot be supposed to produce any effect.

This gentleman further observes, "That the phial of water fitted to its wire does not receive the least degree of electricity, if its wire, suspended by a silk line, be applied to the globe in motion, or if that phial be placed on a dry glass stand." This M. le Monnier takes to be directly contrary to M. du Fay's rule; especially as the phial cannot be replete with electricity, unless, while it is exciting, some non-electric body touch the phial below the water.

That the phial of water receives no degree of electricity in this case, is not strictly true: it receives as much as any other mass of matter of the same bulk would, under the same circumstances. For we find that we cannot highly electrify the water, unless the electricity from the globe be directed through the water and phial to the non-electric in contact; in which passage a great quantity is accumulated, by its not pervading the glass so fast as it is furnished by the wire; and therefore we find that when the water will contain no more, the surcharge runs off by the wire: so that this experiment, any more than those which precede, does not contradict M. du Fay's opinion; the thinness of the glass permitting it, not wholly, but partially, to stop the electricity.

On the Perpendicular Ascent of Eels. By Mr. Wm. Arderon, F.R.S. N° 482, p. 395.

On reading, some years before, what Dr. Plot, in his History of Staffordshire, relates concerning the passage of eels across meadows, in the night-time, from pond to pond, Mr. A. could hardly forbear thinking that the gentleman must have been deceived; but what Mr. A. has lately seen gives him great reason to believe that account to be strictly true.

On June 12, 1745, while viewing the flood-gates belonging to the water-works in the city of Norwich, he beheld a great number of eels sliding up them and the posts adjacent, notwithstanding they all stood perpendicular to the horizon, and 5 or 6 feet above the surface of the pool below the water-works. They ascended these posts and gates, till they came into the dam above. And, what makes the matter appear still more strange, they slid up with the utmost facility and readiness; though many of the boards and posts were quite dry, and as smooth as a common plane had left them.

He observed, that at first they thrust their heads, and about half their bodies, out of the water, and held them up against the wood-work for some time: probably till they found the glutinous matter, which is constantly about their bodies, become sufficiently thick or viscid, by being exposed to the air, to sustain their weight: then they would begin to ascend directly upwards, with as much seeming ease, as if they had been sliding along the level ground; and thus they continued to do, till they had got into the dam above.

On those Fossil Figured Stones called Belemnites. By Mr. Emanuel Mendez Da Costa. N° 482, p. 397.

The belemnites is a fossil of different magnitudes and colours, ever regular in shape; which is either cylindric, conic, or approaching to them. Numbers of them have, on one side only, a chap or seam running their whole length; others have it in part; and in others it is not at all to be observed: it consists of a talcy matter, with an intermixture of spar or crystal, disposed in striæ from or near its centre to its circumference, and is made up of crusts inclosing each other, the innermost being as regular as the outermost. Sometimes, though seldom, in comparison to the numbers of the belemnites, in the centre is a cavity, always conical, whatever the external shape of the belemnites be. This conic cavity is at different times empty, or else filled, either with a solid body of mineral matter, crystal, stone, pyrites, &c. or with a regular jointed conic body, called by lithologists the alveolus of the belemnites; which, though constantly regular and jointed, is yet composed of various mineral or metallic substances.

The alveolus, though not fully proved such, yet seems, by the assent of most of the present naturalists, to be a body of marine origin; a shell the nearest related to the nautilus kind: it is concamerated, and even in some is discovered another great characteristic of the nautilus kind, viz. the gut or siphunculus. Therefore, taking this body for granted to be of marine origin, it remains to discuss, whether this body became accidentally lodged in the belemnites? or, whether the belemnites itself is also of marine origin, and a part dependent on its alveolus?

Various have been the opinions of lithologists concerning the origin of the belemnites; some have even asserted them of the vegetable kingdom; others, that they are teeth or horns of fish, appendages of shells, bodies cast in shells of the tubuli kind, or the very shells themselves, spines of echini, or a kind of straight nautilus. The last 3 opinions are what he endeavours to confute, as they seem somewhat probable, and are now the most prevailing; and to prove the belemnites to be a natural fossil or lapis sui generis.

That the belemnites are not teeth or horns of fish, is shown in the letter Dr. J. Woodward wrote on that subject to Mr. Bourguet, of Switzerland. But a further argument against their being teeth, which that learned naturalist has not touched on, is, that no belemnites have their natural varnish or polish, which always covers the teeth of all animals; whereas the greatest part of those fossil bodies, which we know to be such, as the bufonitæ, glossopetræ, &c. are found with that same varnish or polish. As for their owing their form to being moulded in shells, it will appear contradictory to reason, when we consider, 1st, that their constitution is always as regular as their figure; and, 2dly, that their inner layer or nucleus is as equally regular as the outer crust or whole body; which particular could never have happened, had they been moulded in shells; as is evident, by the turbinitiæ, conchitæ, and other bodies, which owe their figures to that cause. That the belemnites are not spines of echini, let us first consider, that no kinds hitherto discovered have ever been found with spines analogous to these bodies; nor indeed has any marine shell whatever such a texture.

He is sensible there are some species of fossil echini; as, the most common conoid or pileated echini, the common echini galeati, the echini clypeati, and some kinds of the echini ovarii, &c. which, though we are certain that they have been marine shells, yet those particular species are not known in the sea: but then several other species of that same genus are. The case of this is quite different, since not one single species of such a genus has ever been found.

The excessive size and thickness of numbers of belemnites described by authors, viz. of near 2 feet in length, and above 2 inches in diameter in the thickest part, others of 3 feet long, and others as thick and long as a man's arm; not to enumerate those only under a foot in length, and of proportionable thicknesses, concludes echini of a vast size, to have a number of such spines to move.

The varieties of the belemnites, how can they quadrate to the spines of one genus of echini only? Solid belemnites, belemnites with a single crust, or like a tube, with a conic cavity only; that empty, or otherwise filled with a solid mass, or with a regular jointed body, as the alveolus, belemnites of various magnitudes and thicknesses, &c. can all these varieties be imagined to belong to one genus of shells, which we suppose to exist, to maintain a favourite system?

The species of echini discovered are numerous; and the spines of all those agree in having a hollow axis, which runs proportionably from their basis to their apex, quite different from the belemnites: and for their constitution, a foreign naturalist, a member of the Royal Society, Mr. Klein of Dantzic, who has professedly written on this subject, could only find of 2 kinds, viz. those of a porous constitution, which he observed only to belong to one genus, and those of a solid shattery substance, like a talcy spar not striated; which is the most general, and is exactly the same constitution as all the fossil spines, or lapides judaici are.

Further, the lapides judaici have, at some times, been found adhering to their papillæ or tubercles, and with fragments of their shells; whereas no naturalist has ever found fossil either the shells, or the fragments of such a genus of echinus; nor even any remains proportionable to such large spines. In whatever manner the greater part of such shells may have perished (which is unlikely, if we consider their texture and strength), some must have escaped, when the spines are found in such excessive numbers every where, and always perfect and regular; whereas the fossil spines, or lapides judaici, as they are called, as also the echini or shells, and all the fossil bodies of marine origin, are found broken and shattered in all kinds of manners.

As for their being shells of the tubuli kind, his reasons against it are: Were the belemnites such, they must be all tubular, more or less; or otherwise must have suffered some degree of petrifaction to fill up their cavities. The unreasonableness of that argument is demonstrated by all belemnitæ being of one and the same texture and constitution; though numbers are solid, and numbers are tubular, in different degrees. Now one kind of petrifaction, or any other change in the earth, which they might have undergone, could never have given so regular a texture and substance, and cause such different effects as solidity and tubularity. And if, on the other hand, we allow it to be inconsistent, as it is, to form the idea of a shell of the tubulus kind, by a solid body, without that body having suffered some change in the earth, while buried in it, we must either deny all solid belemnites to be such tubuli, and run to subterfuges, by owning them to be natural fossils; or else allow a great inconsistency, to uphold a wrong system.

That the belemnites are not a tubular case, which is part of, and covers a shell of the nautilus kind, as is its alveolus. The variety of circumstances already alleged of the belemnites serve to demonstrate the improbability also of this opinion, as it has done of the other two. The numbers of belemnites of all kinds, so plentiful every where, and the consideration of how few are furnished with alveoli.

Numbers indeed have conic cavities; but that those cavities never did contain alveoli, is evident; that the sides of the said cavities are even, and without any circular or other impressions, which a belemnites that has ever contained an alveolus must have; that body being in close contact to all parts of the investient belemnites, must consequently impress it with its concamerations; which impressions must be therefore found on the sides of the cavities of all belemnites which ever contained them.

As for asserting, that all the alveoli, which are now found loose, were originally lodged in belemnites; it cannot be, without inferring also that all belemnites, which are now devoid of alveoli, contained such formerly; which, by some external or other agent, have been forced out and loosened from them.

To consider such an agent, we must also conclude its force to have been exceedingly great, to loosen out the nucleus of a body in close contact with all its investing parts; and strengthened further to it by ridges and grooves; such a force must have compressed, shattered, and otherwise broken and destroyed the belemnites that contained them; which is contrary to observation. Further, forcing out the alveolus might perhaps easily have happened to the conic belemnites; which has a basis of a larger diameter than the middle, where the alveolus is lodged; but we cannot conceive the same by the cylindric, fusiform, and other belemnites, of which the 2 ends or extremes terminate pointed; while the middle, where the alveolus is lodged, is thick and swelled.

To force an alveolus out of such shaped belemnites, it is evident that the narrow ends of the said belemnites must be quite forced open, broken, and shattered, before a broader and more capacious body could be forced through, especially to such a brittle shattery fossil as the belemnites is. The evident facts to the contrary of this are also too common to insist on, since all these belemnites are ever found regular, perfect, and entire.

Further, let us consider the alveoli which are now found in belemnites, they are very seldom if ever found as mere shells, but ever differently changed or petrified. They are moulded of stone, pyrites, crystal, &c. Now it can never be argued, that the contained bodies can ever be so differently changed or petrified in their covers or shells, and those covers or shells which admitted such different petrifying particles to undergo no change or petrification whatever.

Another proof against this opinion, is the diverse forms of alveoli now discovered by naturalists, as conic, cylindric, curved, spiral at the apex, &c. whereas all belemnites, which have cavities, have none but conic ones.

These cylindric, &c. alveoli are now found in Pomerellia in Poland, in the marble of the island of Oeland, in the Baltic sea, belonging to Sweden, and in the marble of Sweden; in Gothland in masses of building stone; in Ingria, in several parts of Prussia, &c. and are commonly of an immense size, to several feet in length, and proportionably thick, yet not perfect. For such alveoli, which are only nuclei, we must suppose immense large belemnites; and such we have never heard of, so with probability we may conclude none such to exist.

The very view of a belemnites sufficiently evinces its mineral origin, and shows it evidently composed of 2 fossil substances, a talc, and a spar, or bastard crystal; of which the former is the basis, and from which principle he attributes its striated texture. Most of the talcy bodies are of a fibrous nature, and several are composed of crusts inclosing each other, in the same manner as the septa of the ludus helmontii, some of the asbestos kind, the hæmatites crusts, &c. Of the stalactites tribe there are several, which so entirely approach the texture and constitution of the belemnites, that were their shapes a little more regular, the most experienced lithologist might easily be deceived. And he remembers when abroad, to have seen such, of a prodigious size, which, though he was then somewhat conversant in the fossil study, he could not help taking for belemnites. He does not therefore wonder, that Petrus Assaltus, in notis ad Metallothecam Mercati, p. 282, and Langius, Hist. Lap. Figurat. Helvetiæ, p. 133, should judge them a native figured fossil, formed in the earth, of the stalactites kind, if that term for the belemnites might with propriety be used.

The cavities of stalactites in some measure illustrate, and are adequate to the cavities of belemnites; they are placed in as various positions, and are only different from them by not being exactly conic. As for the regular figure of the belemnites being excepted against, he believes few fossilists will argument that, when we see as perfect regular figures in the fossil kingdom, as in any other parts of the creation; as witness the salts and crystals of all kinds; the rhomboid, hexagonal, columnar, and other selenites; the cubic, octangular, dodecaedral, and other pyrites; the quadrangular pyramids of tin, the rhombs of iron, cubes of lead, and infinite other native fossils, which are far more perfect figures than the belemnites are. Chymical and other trials and tests demonstrate a greater certainty of its mineral origin. As for that marine body the alveolus, he thinks it is of the nautilus kind; and which, at the concretion or formation of the belemnites, became accidentally lodged in its cavity, in the same manner as all other marine bodies became lodged in the various fossil substances they are now found in.

On the Indian Poison, sent over by Don Antonio De Ullöa of Seville, F.R.S., and mentioned by M. De La Condamine, Member of the Royal Academy of Sciences at Paris, in his late Account of the River of the Amazons; being a Letter from Richard Brocklesby, M.D., F.R.S.* N° 482, p. 408.

On hearing lately part of Don Antonio de Ulloa's † letter to the president, Dr. B. was suspicious M. de la Condamine had taken some facts there on the authority

* Dr. Brocklesby was distinguished not only for his skill and humanity in the exercise of his profession, and for his medical publications, but also for being the associate and friend of 2 memorable political characters, Wilkes and Burke, as well as of the celebrated critical and moral writer Dr. Samuel Johnson. He was born at Minehead in Somersetshire, 1722, and three years after was taken over to Ireland, where he received his grammatical education, after which he went to study physic, first at Edinburgh and next at Leyden, at which last university he took his degree of M. D. in 1745. He afterwards came to London, and in 1751 was admitted a licentiate of the College of Physicians. In 1754 he obtained an honorary degree from Dublin, and a similar honour was conferred upon him the same year by the university of Cambridge. In 1756 he was admitted a fellow of the London College of Physicians, and in 1758 he was appointed physician to the army, and served in that capacity in Germany, during the greater part of the 7 years war. He was afterwards made physician to the hospitals for British forces. On the conclusion of the war he returned to London, and acquired considerable reputation and emolument by his practice. He died in 1797, at the advanced age of 75.

Besides his inaugural dissertation, and the Harveian oration which he pronounced before the College of Physicians, he wrote an Essay on the Mortality among horned Cattle, a Dissert. on the Music of the Ancients, and several medical papers inserted in the Phil. Trans.; but his principal work is that which is entitled Œconomical and Medical Observations in 1 vol. 8vo. This book contains many excellent practical remarks concerning the history and treatment of various disorders, with some useful hints relative to the management of hospitals, being the result of his experience while he served as physician to the army.

Dr. B. was respectable for his professional abilities, but still more so for his truly benevolent disposition. He not only gave advice gratis to the poor, but granted annuities to several widows who had been left necessitous; not to mention his liberal offers to Dr. Johnson, and his liberal donation to his friend Mr. Burke. The reader will participate with the biographer in emotions of the most pleasing kind, on the mention of such rare instances of exalted friendship and benevolence.

† This gentleman was one of those sent by the king of Spain to attend and assist the French astronomers of the Royal Academy of Sciences, in their late measure of a degree of latitude near the equator. He was taken prisoner at Cape Breton in his return home, and brought into England; where his papers all relating to the measure of the degree, and other astronomical and philosophical observations, were by favour of the lords commissioners of the Admiralty restored to him, to be published in his own country. An abstract of the same was however, by their lordships' leave, communicated to the Royal Society, by their president, who was entrusted with the perusal of the same: and the author himself, who is a gentleman of great merit, was soon after unanimously chosen a Fellow of

of others, or else had been himself a little too much addicted to that general bias of mankind, the love of prodigy and wonder.

To be better satisfied, Dr. B. dissolved, in a certain quantity of fair water, as much of the Indian poison as could be suspended, and let it stand to clear 24 hours; and having made a superficial incision with a lancet into the nose of a young cat, a few drops were sprinkled on the wound. The creature at first discovered no marks of injury received; yet in half an hour she seemed, by mewing more than before, to be sensible of some pain. Thus she remained about 20 minutes; when at length she shivered, was sleepy, soon became convulsed, and in about half an hour her limbs were flaccid, and her belly swelled. These symptoms continued till in a short time she expired.

Some time passed ere he sat down to inquire what visible effects had been produced on the body. He then separated the head from its trunk, and carefully examined the brain, and particularly the origin of the nerves; but could not discover any preternatural appearance in any of these parts. He then opened the thorax, and, with some surprize, found the pulsation of the heart as regular, as if the animal were in perfect health. This appearance continued above 2 hours after the cat's head was off; but afterwards languished, and was much weaker.

He then opened one ventricle of the heart, in which the blood was somewhat coagulated. This may be thought to be partly owing to the medicine; for soon after it had produced convulsions in the creature, he had a mind to see what bleeding would do, and with that view cut off the tail; but contrary to expectation, the arteries that supply it with blood bled very little; and on cutting off the head, the carotids and both vertebrals did not pour out above half a common spoonful.

But as it might be questioned by some, from the continuance of the heart's pulsation, whether the cat might not possibly, if let alone, have recovered, he poured a few drops of the same solution as before into a superficial wound of a young dog, weighing 12 lb. The creature in less than an hour shivered, became sleepy, was very cold, and so stupid, that he suffered himself to be often burnt by the hot ashes beneath the grate, where he lay for warmth. In this comatose way he continued near 4 hours, and then shook off his stupor, and was much better. He left him all night, and found him next morning quite well, and as hungry as ever. On this Dr. B. made an incision at that time into one of the crural veins, and poured a few drops of the solution into it: in less than 10 minutes the dog gave signs of great pain, soon shivered, grew cold, was convulsed, and in less than 20 minutes died.

the Society. Some time after his return he procured the above-mentioned specimen of this Indian poison, which he sent over, together with some books, as a present to his friend the President of the Society. De Ulloa was born in 1716. He returned to South America in 1759, and was for some time governor of Lousiana. He died in 1795, at 79 years of age.

On opening him, nothing uncommon was found, nor was the blood in this creature's heart so thick as in the former. The crural vein did not bleed from a large orifice, after the poison was infused, though it was likely to do it before. But as some authors have said, that birds in particular are instantly deprived of life, if the least particle of certain poisons are infused into the blood, he had a mind to try one experiment, and to this end infused a few drops of the solution into a cuticular wound of a small bird. This occasioned hanging of the feathers, and a stupor, in less than 10 minutes, and killed him in somewhat more than 15.

He gave about 2 drs. of sugar to another bird of the same kind, and shortly afterwards poured a little of the solution into its mouth; but two drops had scarcely touched his tongue before the creature was convulsed, and he could hardly lay him down before all motion was taken away. He gave these 2 birds to 2 cats; and the cats made so uncommon a noise the whole night, that they disturbed the family's rest.

From these experiments we find that the supposed specific is of no manner of use, even when the poison is only taken at the mouth; and from them it may appear probable, that the poison is nearly on the same footing with white arsenic in the cure of the tooth-ach.

On the Moon's Motion. By Mr. Richard Dunthorne. N° 482, p. 412.

In the preface to his lunar tables, Mr. D. hinted, that one use of publishing those tables would be, the assisting of persons desirous further to rectify the lunar astronomy, by enabling them more readily to compare the Newtonian theory with observations. After publishing those tables he spent some time in that comparison. As the motion of every secondary planet must partake of the errors in the theory of its primary, he thought proper, before he undertook the examination of the lunar numbers, to compare those of the sun with observations. He compared several sets of Mr. Flamsteed's observations, after the method he himself teaches, in Prolegom. Hist. Cœlest. p. 133, et seq. which, for many reasons, he thinks the best method hitherto used; and determined the mean motion of the sun at Greenwich, the last day of December at noon, Anno 1700, o.s. ♑ 20° 43′ 40″; of its apogee, ♋ 7° 30′ 0″; and the greatest equation of the sun's centre, 1° 55′ 40″; which he is fully persuaded are very near the truth.

The theory of the sun being thus settled, he proceeded to examine the elements of the lunar astronomy. He began with observations of lunar eclipses about the equinoxes, when the apogee of the moon was in the sun's quadratures; because at those times he could conceive the moon's motion affected with no inequality, besides the annual one, called by Newton the first equation, and the elliptic one, called prosthaphæresis: from a comparison of such observations he obtained the moon's mean longitude, which came out 1′, at least, greater than in the tables, and very nearly as Newton has it in the last edition of his Principia.

He went on to examine the place and motion of the apogee, and theory of the increase and decrease of the eccentricity, as well as the greatest and least eccentricities themselves; all which agreed so well with the tables, about the sun's mean distances, that he dare not venture to make any alteration in them: indeed the 6th equation does not so well account for the variation of the motion of the apogee, and change of the eccentricity, according to the greater or less distance of the sun from the earth; and therefore he set himself to compute what change this difference of the sun's action on the lunar orbit would introduce in the moon's place in every situation of the sun and lunar orbit; and found, after many tedious computations, that the sun being in apogee, this change, where greatest, would amount to about 4′, and to 4′ 16″, when the sun is in perigee. In other distances of the sun from the earth, this greatest change is proportional to the difference of the cubes of the mean and present distances; and in every situation of the moon, and of her orbit, the present is to the greatest equation, nearly as the sine of the excess of the moon's mean anomaly above twice the annual argument to radius. It increases the moon's longitude, when the sun is in his $\left\{\begin{array}{l}\text{Apogeon}\\ \text{Perigeon}\end{array}\right\}$ Semicircle, and that excess $\left\{\begin{array}{c}\text{less}\\ \text{greater}\end{array}\right\}$ than 180°; and diminishes it when otherwise.*

In fine, he compared the theory of the moon, as to her longitude, with several observations, as well in the octants and semi-octants, as in the syzygies and quadratures, and found such an agreement when the above corrections were made, as seemed rather to be wished than hoped for, considering the many inequalities with which the sun's action disturbs the motion of the moon, and the defects to which the best observations are liable.

He compared 100 observed longitudes of the moon with the tables; viz. 25 eclipses of the moon, all, except the first, taken from Flamsteed's Historia Cœlestis, the Philosophical Transactions, and the Memoirs of the Royal Academy of Sciences; the 2 great eclipses of the sun in 1706 and 1715; 25 select places of the moon from Flamsteed's Historia Cœlestis, and 48 of those longitudes of the moon computed from Flamsteed's Observations by Dr. Halley (as he supposes) printed in the first edition of the Historia Cœlestis. The difference between the observed and computed places, run from 0 to 4′, sometimes in excess, sometimes in defect.

Several observed latitudes of the moon, which he compared with the tables, show them to be very near the truth, both in the motion of the nodes, and also in the quantity and variation of the inclination.

* If this equation be increased and diminished in a direct ratio of the moon's horizontal parallax, it will become more exact. And if it were always diminished by a fourth or perhaps a third part, it would agree better with observations.—Orig.

Concerning the Discoveries of the Russians on the North-East Coast of Asia. By Mr. Leonard Euler, Prof. Mathem. and Member of the Imperial Society at Petersburg. Dated Berlin, Dec.* 10, 1746. N° 482, p. 421.

1st. The new land, which Behring fell in with, at the distance of 50 German miles from Kamschatka towards the east, was followed by him, and coasted a

* Leonard Euler, a most profound mathematician, and a general scholar, was born at Basil, April 15, 1707. His father, being a clergyman, intended his son, and had him educated, for the same profession; but, his powerful attachment to the mathematics proving too strong for all opposition, he was suffered to pursue those studies to which he had fully and effectually devoted himself, under the assistance of professor John Bernoulli. At 16 years of age, he took his master of arts degree; on which occasion he delivered a Latin discourse, highly applauded, on a comparison between the philosophy of Newton and the Cartesian system.

In 1727 he carried the 2d prize of the Paris Academy of Sciences, for the answer to a prize-question on the masting of ships; which led him to a more general study of naval architecture, which he afterwards enriched with so many valuable discoveries. Euler's merit would have given him an easy admission to honourable preferment either in the magistracy or university of his native city, had not both civil and academical honours been there distributed by lot. The lot being against him in a certain promotion, he quitted the country, and went to Petersburg, where he was made joint professor with his countrymen Hermann and Daniel Bernoulli, who had been invited there a short time before.

Here a noble emulation between him and the Bernoullis, &c. had the happiest effect in improving and extending the sciences. Here he enriched the academical collection with numerous and valuable memoirs in every one of its volumes, to the time of his death, and even left behind him a multitude of new memoirs, in order to continue his annual contributions for many years longer. Here he carried to new degrees of perfection the integral calculus, invented the calculation by sines, reduced analytical operations to a greater simplicity, and thus was enabled to throw new light on every part of mathematical science.

In 1730, Euler was promoted to the chair of natural philosophy; and in 1733 he succeeded his friend Dan. Bernoulli in that of mathematics. Here the violent and laborious efforts, his emulation induced him to make, at length threw him into a fever, that endangered his life, and from which he escaped not without the loss of one eye entirely, and which some time afterwards brought on total blindness many years before his death; though still without abating his ardour for scientific improvements.

In 1730, the Academy of Sciences at Paris adjudged the prize to his memoir on the Nature and Properties of Fire; and in 1740 he shared equally with Maclaurin and D. Bernoulli the prize given by the same Academy, for the important subject of the tides in the sea; our author's solution of this question being esteemed a masterpiece of analysis and geometry.

In 1741, Euler was invited to Berlin, to direct and assist the academy that was there rising into fame. And here his labours will appear more especially astonishing, when it is considered, that while he was enriching the Academy of Berlin with a profusion of memoirs, on the deepest parts of mathematical science; he still continued his philosophical contributions to the Petersburg Academy, whose memoirs display the amazing fecundity of his genius, and which had granted him a pension in 1742. It was with great difficulty that, in 1766, he could obtain leave from the King of Prussia to return to Petersburg, where he wished to pass the remainder of his days. But soon after his return it was, that by an attack from another violent disorder, he was totally deprived of the sight of his other eye.

great way, though uncertain how far: from whence alone it will appear, that an abatement must be made in the distance of about 30 degrees, which has been supposed to be between the last known head-land of California towards the west, and the farthest extremity of this new discovered land towards the east.

2dly, Capt. Behring having had the opportunity of observing an eclipse of the moon at Kamschatka, concluded from the same, that that place lay much farther off to the east, than is expressed in any map; and that, to represent it truly, it ought to be transferred into the other hemisphere, as its longitude is more than 180 degrees east from the isle of Ferro. For this reason, Captain Behring's new land will be considerably approached to the last known part of California, and will not indeed appear to be many degrees from it.

What we have therefore still to hope is only, that in this unknown district there may be found some strait, by which the Pacific Sea may freely communicate with Hudson's Bay; but if it shall appear that there is no such passage, it must then be concluded, that whatever further progress may happen to be made through Hudson's Bay, the opening at last must only be into the Frozen Sea, from whence there could be no passing into the Pacific Ocean, but by the neighbourhood of Kamschatka; and this way would doubtless be too long, and too dangerous, to be mastered in the course of one summer.

It is much doubted whether the Russians will ever publish the particulars of their discoveries, either such as have been made from Kamschatka towards America, or such as have been made on the northern coasts of Asia. And indeed it

About this time Euler was honoured by the Academy of Sciences at Paris with the title of one of its foreign members; after which, the academical prize was adjudged to 3 of his memoirs, concerning the inequalities in the planetary motions. The two prize questions proposed by the same academy for 1770 and 1772, were designed to obtain from the labours of astronomers a more perfect theory of the moon. Euler, though blind, was a competitor for these prizes, and he carried them both. He afterward revised his whole theory, and pursued his researches till he had constructed the new tables, which appeared, with the great work, in 1772. On this occasion it is impossible to observe without admiration, such immense calculations on the one hand, and on the other such ingenious methods employed to abridge them, and to facilitate their application to the real motion of the moon. But this admiration will become astonishment, when we consider at what period, and in what circumstances, all this was effected. It was at a time when our author was totally blind, and consequently obliged to arrange all computations by the sole powers of his memory and his genius: it was when he was embarrassed in his affairs by a dreadful fire, that had consumed great part of his substance, and forced him to quit a ruined house, every corner of which was known to him by habit, which in some measure supplied the want of sight. It was in these circumstances that Euler composed a work which alone was sufficient to render his name immortal.

Neither the infirmities of old age, nor the loss of sight, could quell the ardour of Euler's genius; he continued his profound researches till the day of his death, which happened suddenly, by an apoplexy, in Sept. 1783, at 76 years of age.—Besides his voluminous separate publications, his memoirs were the chief support of several of the Academies of Europe, and he left moreover an immense collection of unpublished papers behind him.

is but very much in general that Mr. E. knows the success of this last expedition. What he did know, was communicated to him by order of the Court, from the College of Admiralty, for him to make use of it in the geography of Russia, which he was at that time charged with.

They passed along in small vessels, coasting between Nova Zemla and the Continent, at divers times, in the middle of summer, when those waters are open. The first expedition was from the river Oby; and at the approach of winter the vessels sheltered themselves by going up the Jeniska; whence the next summer they returned to sea, in order to advance farther eastward; which they did to the mouth of the Lena, into which they again retired for the winter-season.

The 3d expedition was from this river, to the farthest north-east Cape of Asia. But here they lost several of their boats, and a great part of their crew, so as to be disabled from proceeding, and from making the whole tour, so as to arrive at Kamschatka. It was however thought, that a further attempt was then unnecessary, as Capt. Behring had already gone round that Cape, sailing northward from Kamschatka.

The Russians have not attempted the passage round Nova Zemla; but as they have passed between that land and the coast of Asia, and as the Dutch formerly discovered the northern coasts of Nova Zemla, we may now be well assured, that that country is really an island.

Observations made on the Bansticle, or Pricklebag, alias Prickleback, and also on Fish in general. By Mr. Wm. Arderon, F. R. S. N° 482, p. 424.

About the beginning of April Mr. A. took a bansticle out of the river, full of spawn, and put it into a glass jar, at the bottom of which he had placed a small quantity of sand, as he always did in every vessel with fish; and about the 20th of May it buried its spawn in the sand. He was in hopes this spawn would have produced a young brood, but was disappointed; which he imputed to its being frequently disturbed by the pouring in of fresh water.

For some days after he had catched this bansticle, it refused to eat any thing he offered it, as is common with all fish he had yet kept; but frequently giving it fresh water, and coming often to it, it became so familar as to eat small worms he now and then threw into the jar, and from that time grew so tame as to take them out of his hand; nay it became so bold at last, that when its belly was full, or it did not like what he offered, it would set up its prickles, and with its utmost strength make a stroke at his fingers, if he put them into the water to it.

This fish was of so unsociable a disposition, that it would suffer no other fish to live in the jar with it, and so audacious, as to attack whatever he put in, though 10 times its own size. One day, for the sake of diversion, he put a small ruff into the jar to it, which the bansticle immediately assaulted and put to flight, having in the conflict torn off a good part of its tail, and would probably have killed it, had he not separated them very soon.

The endeavours these pricklebacks use, and the ability they have, to get from place to place, are extraordinary; for though the largest of them scarcely measures above 2 inches in length, he has seen some of them leap out of the water a foot high perpendicularly, and even much farther in an oblique direction, when they wanted to get over boards or stones, or some other obstacle to their passage. It is scarcely to be conceived what damage these little fish do, and how greatly detrimental they are to the increase of all the fish in general among whom they inhabit. For it is with the utmost industry, sagacity and greediness, that they seek out and destroy the spawn of all sorts of fish; and indeed all the young fry, that come in their way, are pursued by them with the utmost eagerness, and swallowed down without distinction, provided they are not too large. In proof of what is here asserted, the bansticle beforementioned in the glass jar, did, on the 4th of May, devour, in 5 hours time, 74 young dace, which were about a quarter of an inch long, and the thickness of a horse-hair. Two days after it swallowed 62, and would doubtless have eaten as many every day, could he have procured them for it.

Nature has furnished this little fish with a kind of breast-plate or armour, to be its defence against any outward injury: she has likewise bestowed on it several offensive weapons or spines, placed on its sides and back, which it immediately erects on the least appearance of danger, or when it attacks some other fish. The sharpness of these prickles guards it well enough from larger animals, that might otherwise prey upon it; but neither these, nor all the endeavours it can use, are able to free it from an enemy that torments it even to death; viz. a kind of louse, of an oval figure, having eight legs, and a very transparent body, which is able either to swim or crawl, and sticks on it so fast, sucking and plaguing it all the while, that it makes it almost mad. One remarkable particular in this louse is, that its little fibrillous fins are always in motion, whether the creature be swimming about, or fixed on the fish.

All fish regulate their times of eating and abstinence by the temperature of the air, and the quarter from whence the wind blows; and would those persons who are lovers of angling take the pains to keep a few small fish in glasses, they might at any time easily foretel, from their taking or refusing food, what sport is to be expected, and often save themselves many a weary step taken to no purpose.

He always observed, among the fish he kept in jars, that such as have lived a while together, contract so great an affection for each other, that if they are separated they become melancholy and sullen, and are a long time before they forget the loss. About Christmas he put 2 ruffs into a jar of water, where they lived together till April; when he gave one of them away. After this separation, the fish that remained was so affected, that for 3 weeks it would eat nothing he could give it; so that fearing it would pine to death, he sent it to the gentleman on whom he had bestowed its companion; and on being put together again, it eat immediately, and recovered its former briskness.

A Supposition how the White Matter is produced, which Floats about in the Air in Autumn. By the same. N° 482, p. 428.

Mr. Arderon having lately a large spider in his hand, by chance he let it fall, and it hung by its thread, as they very commonly do. On holding his hand very still it readily ascended up it again, and thus by giving it a shake, and then holding his hand still, the spider ascended and descended a great many times. He thought at first it had spun a new thread at every descent, and was desirous to have measured how long a one he could cause it thus to spin; but on a stricter examination he very plainly perceived, that whenever it ascended, it wound its thread with its feet into a sort of coil, and when it descended only ravelled it out again.

The manner how they perform this is diverting enough: but as spiders may be had almost in every place, and the experiment is so easily tried, he only adds, that as these coils of thread are exactly like those floating in the air towards the end of summer, he thinks it is not improbable those are made in the same manner, when spiders have a mind to direct their course in the same direction their threads lie.

Remarks on the Precious Stone called the Turquoise. By Cromwell Mortimer, Sec. R. S. &c. N° 482, p. 429.

This stone has received its modern name of Turchesia, and Turquoise, from its being most commonly brought from Turkey into various parts of Europe. De Boodt* says, the colour of this gem is a variegation of green, white, and blue; and that there are two sorts of it, the oriental, from the East-Indies and Persia, and the occidental, from Spain, Germany, Bohemia, Silesia, &c.; that in Persia, where it is found in greatest plenty, it adheres to black stones, as if it were an excrement or a transudation from them. A stone of this sort is seldom found to exceed a walnut in size; and he mentions one in the great duke's museum, on which the head of Julius Cæsar is engraved, as a very extraordinary sample; he adds, that he never saw one larger than a hazel-nut; that some of the oriental ones have the faculty of preserving their colour perpetually, which are called stones of the old rock; and that others lose their colour gradually, and

* Gemmar. et Lap. Hist.—Orig.

are called of the new rock. He then gives an instance of a turquoise which had lost its colour on being laid by some time after its owner's death, which recovered its beautiful colour on our author's wearing it on his finger in a ring.

Cæsius, in his treatise de Mineralibus, p. 601, says, this stone is called Turcoïs by Mylius, in his Basilica Chemica; by Albertus Magnus, in his treatise of minerals; and by Rueius, in his treatise of gems; but Turca, by Caussinus de Lapillis symbolicis. De Boodt, and Dr. Woodward,* with other modern writers, take it for the callais of Pliny. Salmasius, in his Plinian. Exercit. p. 142, says, many have mistaken the modern turquoise for the cyanus, but that the cyanus was transparent like the sapphire; whereas the turquoise is a sort of jasper.

Dr. Woodward, in his letter to Sir Jo. Hoskyns,† says, that the turcoïs, or callaïs of Pliny, is nothing else but fossil ivory tinged with copper. It is not denied, that some stones sold for turquoise, and possibly all that the Doctor saw were certainly such; but probably those which the authors call of the old rock, and in which the colour is permanent, are real mineral stones: this sample now produced seems to show this, both from the form and size; its shape shows it not to be part of any animal bone; but its botryoïd form is a demonstration that it is the product of fire, which had once melted this substance; and that when it cooled, its surface was formed into bubbles and blisters, in the same manner as the hæmatitis botryoïdes or bloodstone, whose surface consists of knobs, resembling a bunch of grapes.

That the elephas ὀρυκτὸς, or ebur fossile of Theophrastus, said‡ to be of various colours, thinks to be tinctured with copper, and to be what Dr. Woodward calls the turquoise; indeed he suspects it to be what De Boodt calls of the new rock, and says, is liable to lose its colour, which it recovers again from the effluvia of the person who wears it. He therefore, for distinction sake, thinks all these stones of the ivory origin should be called pseudo-turchesiæ, or bastard turquoise; and the other sort, of which this is one, the true or real turquoise; for, by examination in the chemical way, he found it to be a very rich copper ore; some of it pounded and dissolved in spirit of hartshorn gives a deep blue; in aquafortis a fine green; and an iron wire put into it was in 1 hour's time incrusted with copper; some of it calcined, without any flux in a crucible, run to a slag, or half vitrified substance; whereas the same heat, had it been ivory or bone, would have reduced it to a white ash like bone-ashes; for he exposed it to such a fire as vitrified the tile that covered it. Its hardness and consistence to an engraver's tool seems to be the same as common white marble; its colour is not mended by heat, but it grows brittle when red-hot.

* Method of Fossils. Letters, p. 17.—Orig. † Ibid. p. 16.—Orig.

‡ See Theophrastus's Hist. of Stones, translated by John Hill, Lond. 1746, 8vo, p. 94.—Orig.

This specimen, shown to the Society, was about 12 inches long, 5 inches broad, and in some places near 2 inches thick; rough on the under side, as though broken off from the rock it had been affixed to; and the upper side was composed of smooth polished knobs, in form like the botryoïd iron ore.

Sir Hans Sloane, in his noble museum, had several specimens of these oriental turquoises, all botryoïd; especially a mass from China, about 3 inches long, 2 broad, and near an inch thick; all which seem to be copper ores; and he had likewise samples of turquoises from Spain, and the south of France; which were all small, and seemed really to be pieces of ivory tinged with copper.

Of a Curious Echinites. By Mr. Henry Baker, F. R. S. N° 482, p. 432.

Mr. Baker showed the Society a very extraordinary echinites, the like to which he had never seen in any museum, or found described by any author. For the echinitæ usually met with, are made up either of chalk or flint, or some stony, chalky, or sparry matter, formed within the shell of the echinites, and thence taking their figure as in a mould: which shell is often broken off and gone, but at other times remains impregnated with talcy or sparry particles; whereas the present subject is composed of a transparent crystalline substance, which has received its general figure by having been circumscribed within the shell of some echinus, and shows linear ridges and divisions correspondent to the lines and plates found in this kind of echinus.

Were this all, it would be a very uncommon production, as these bodies have been very rarely known to be formed of crystal;* but it is rendered much more curious and extraordinary by having exact rows and series of little cells, all of the same regular figure, though lessening gradually in size, as they ascend from the base upwards. See fig. 5, pl. 7.

This body having been formed within the shell of an echinus, one would expect, as is the case in all other echinitæ usually known, that its figure should be exactly answerable to the mould in which it was formed; but Mr. Baker observes, that the echinus's shell is perfectly smooth internally, having no rising parts correspondent to these cells or cavities; and therefore, as it could not receive its configuration from thence, it must be owing to the natural shooting of the crystalline matter, or to some other cause, which he does not pretend to know.†

The configuration seems however in some measure to correspond with the nature of the shell in which it was formed; as to the number of the rows of cells, these being ranged by fives, as the papillæ, indentings, lines, or other marks on the recent shells of echini constantly are; these rows are 20 in number,

* Sir Hans Sloane had a mass, which was formed within an echinus, the shell being broken off; it was one half or side crystal, the other side of a substance like chalk, but much harder.—Orig.

† Perhaps to some cells or membranes belonging to the body of the echinus. C. M.—Orig.

viz. 5 double ranks of large and extremely regular cells, as at aa, &c. between which lie 5 other double rows of smaller and less distinct cellulæ, shown at bb, &c. These cells, which are hexagonal, and of which those in every row lie alternately to those of the next, by which means they fill up the whole space, decrease in their size gradually, as they approach nearer to the top; all the rows at last almost concentring at the apex, leaving only a small space or vacuity, where, in the shells themselves of this kind of echinus, there is an aperture. The smooth part at A is formed of a pebbly stone, bearing the same marks as are usually found in the impression of these echinitæ dug up in gravel pits: which proves, that this must have received its general figure from one of those shells, whatever has been the cause of this remarkable configuration of a crystalline part.

This curious echinite was found in a marl pit at Baborough, about 3 miles west of the city of Norwich, and was presented to Mr. Baker by Mr. Wm. Arderon, F. R. S.

On Birds of Passage. By Mr. Mark Catesby, F. R. S. N° 483, p. 435.

The places to which birds of passage retreat when they leave us, are first of all to be inquired after, and then it will be proper to examine by what route, and in what manner they convey themselves to such places.

The reports of their lying torpid in caverns and hollow trees, and of their resting in the same state at the bottom of deep waters, are so ill attested, and absurd in themselves, that the bare mention of them is more than they deserve. Of much the like stamp is a late hypothesis, which sends them above our atmosphere for a passage to their retreat; which seems as remote from reason, as the ethereal region is from the aërial; through which last region he cannot conceive any obstruction to their passage, when, by the approach of our winter, they find a want of food, and at the same time are directed by instinct to resort to some other parts of the globe, where they may find a fresh supply, for the want of food seems to be the chief, if not the only reason of their migration. And though tit-mice and other small birds abide here the whole winter, and subsist on insects, which they find torpid, or in a state of mutation, in the crevices of the barks of trees, and other their winter recesses, yet most birds of passage, having tender bills, are incapacitated for this work; but then the length of their wings enables them to prey on numberless flying insects, with which the air is stored during the warm months; and it is observable, that not only swallows, but most other summer birds of passage, feed on the wing, on such insects as are seen no more when cold weather commences.

The various conjectures concerning the places to which birds of passage retire, are occasioned by the want of ocular testimony to bring the matter to some certainty. But if the immenseness of the globe be considered, and the vast tracts of land which still remain unknown to us, it is no wonder that we are yet unacquainted with the retreat of these itinerant birds. On this head Mr. C. agrees in the general opinion of their passing to other countries by the common natural way of flying, with this additional conjecture, that the places to which they retire, lie probably in the same latitude in the southern hemisphere, as the places from whence they depart; where the seasons reverting, they may enjoy the like agreeable temperature of air.

It may be objected, that places of the same latitude in the southern hemisphere may be divided by too wide a tract of sea for them to pass over. But why then may not some other parts of the southern hemisphere serve their turn? This seems more reasonable, than that they should remain on our side of the northern tropic; within a few degrees of which, at the winter solstice, it is so cold, as frequently to produce snow; which, by dispersing such insects as birds that feed on the wing, and particularly the swallow kinds, subsist on, must make them perish inevitably, were they not to change their quarters for those more favourable climes, where a continuance of warm weather affords their natural and proper food. This their sagacity dictates to them, and is the apparent cause of their periodically leaving us at the approach of winter, before flies are so dissipated by cold and winds as to be found no longer in the air, though they may with other insects be met with in holes and hidden recesses, and serve to subsist other birds of passage.

What Mr. C. infers from hence is, that as swallows cannot continue and subsist so long in cold seasons as other birds of passage, they are necessitated to visit us somewhat later, and to depart sooner; for though nightingales, and other birds of passage, are not often seen or observed after they cease singing, yet he has frequently noticed them in their solitary coverts a month after the departure of swallows. From these reasons he concludes, that birds of passage, particularly swallows, are necessitated to pass the tropic of Cancer; but how far more south, or to what part of the southern hemisphere they go, remains unknown.

The manner of their journeying to their southern abode may vary, as the different structure of their bodies enables them to support themselves in the air: those birds with short wings, such as the red-start, blackcap, &c. though they are incapable of such long flights, and with so much celerity, yet he thinks they may pass in like manner, but by gradual and slower movements. Swallows and cuckows may probably perform their flight in half the time; yet there seems no necessity for a precipitate passage, because every day's passage affords them increase of warmth, and a continuance of food a longer time than is necessary for their passage, were it to the same latitude south as that from whence they go.

As Providence in many instances has guided defenceless animals to make use of the most necessary means for their security, why may not these, and other itinerant birds, perform their long journeys in the night time, to conceal themselves from rapacious birds, and other dangers that day-light exposes them to; which nocturnal travelling of birds of passage he believes more than barely probable, from the following observations, which may serve in some degree to confirm it.

Lying on the deck of a sloop on the north side of Cuba, Mr. C. and the company with him, heard 3 nights successively flights of rice-birds, their notes being plainly distinguishable from others, passing over their heads northerly, which is their direct way from Cuba, and the southern continent of America, whence they go to Carolina annually at the time that rice begins to ripen; and, after growing fat with it, return south back again.

The flight of birds of passage over the seas has by some been considered as a circumstance equally wonderful with other stories concerning them; and especially in regard to those with short wings, among which quails seem, by their structure, little adapted for long flights, nor are they ever seen to continue on the wing for any length of time; and yet their ability for such flights cannot be doubted, from the testimony of many. Bellonius in particular reports, that he saw them in great flights passing over and repassing the Mediterranean sea, at the seasons and times they visit and retire from us. The same sagacity that instructs them to change climates, may also reasonably be thought to direct them, and other birds of passage, to the narrowest part of our channel, to evade the danger of passing a wide sea; though, by the many instances of birds driven hundreds of miles from any land, there seems not that necessity for their finding the straits of Calais, as the shortest passage to our island, they being not unable to perform much longer flights.

There are also winter birds of passage, which arrive here in autumn at the time the summer birds depart, and go away in the spring season, when summer birds return. These however are but few, there being only 4 sorts known, viz. the fieldfare, redwing, woodcock, and snipe; which last two he has often known to continue the summer here, and breed; so that the fieldfare and redwing seem to be the only birds of passage that constantly and unanimously leave us at the approach of summer, retiring to more northern parts of the continent, where they breed, and remain the summer, and at the return of winter are driven southerly from those frigid climates in search of food, which there the ice and snow deprives them of. There are many others, particularly of the duck and wading kind, that breed, and make their summer abode in desolate fenny parts of our island. When the severity of our winters deprives them of their liquid sustenance, necessity obliges them to retire towards the sea in numerous flights, where in open brackish waters they find relief, and at the approach of the spring they retire to their summer recesses. But these cannot be included among those usually termed birds of passage.

Besides the different kinds of swallows, there is but one kind of European bird that subsists in like manner by catching its food on the wing, that is, the caprimulgus, or goat-sucker, the capacious structure of whose mouth and gullet is formed to receive insects of the larger kinds, as scarabæi, grillotalpæ, &c. These are also birds of passage.

We have made it pretty evident, that summer birds of passage come to, and depart from us, at certain seasons of the year, merely for the sake of a more agreeable degree of warmth, and a greater plenty of food; both which advantages they procure by an alternate change of climate; but the migration of winter birds of passage, and particularly of fieldfares and redwings, is much more difficult to be accounted for; there being no such apparent necessity, either on the score of food or climate, for their departure from us.

The reason of their coming here in winter is probably for the sake of food, and a more suitable climate than that they leave; but in some severe cold seasons, and when there is a scarcity of berries, they subsist here with difficulty, and are even famished sometimes for want of sufficient food; yet what appears most unaccountable is, that such as have continued with us a whole winter in penury, and should, one would imagine, rejoice at our approaching spring, and build their nests, and couple, on the contrary all depart; as if that mild and pleasant temperature, which delights and cherishes most other creatures, were disagreeable to them. We know the places of their summer retirement to be Sweden, and some other countries in that latitude; but, as they would find those countries too cold for their reception, and probably destitute of provision, were they to hasten directly thither when they depart from hence, they journey gradually, and prolong their passage through the more moderate countries of Germany and Poland, by which means they arrive not at those northern regions, adapted by Providence for their summer abode, and the breeding of their young, till the severity of the cold is so much abated as to render it pleasing to them, and food may be there found. When they visit us again in winter, their return back is after the same manner.

The winter food of these birds being berries, and particularly haws, as a greater abundance of them grow in this island than can be supposed in the more northern regions, that may possibly be one great allurement to bring them over hither; but the principal reason inducing them to travel southward, is probably the rigour and severity of the cold in those frigid climes, which nature therefore directs them to desert for such as are more temperate.

A Swedish gentleman informed Mr. C. that observing the use we make of quick-set hedges in England, he sent some plants of the white thorn over to his own country for the same purposes; before which time he assured me there were none of them in Sweden, which he mentions in support of what was said above.

The coming of these birds to us may then pretty well be accounted for from the reasons aforesaid; but the cause of their departure from hence at the time they leave us, is one of those secrets in nature which are not yet discovered; for, should it be suggested that they do not leave us till the haws and berries are all gone, and they are under a necessity to seek for food elsewhere, this would amount to little, unless it could be shown that the northern regions, to which they journey, can afford them a fresh supply; which it is almost certain they cannot. And therefore, when first they go from us, they must either alter their diet, or be in much distress; but as it is evident that their food in the summer time must be of a different kind from what they eat in winter, it is most likely they change their diet; and then one would imagine they should find subsistence here in greater plenty, and much sooner, than in the colder countries to which they fly.

In short, all that can be said to be known of the matter, ends in this observation, that Providence has created a great variety of birds, ando ther animals, with constitutions and inclinations adapted to their different degrees of heat and cold in the several climates of the world, by which no country is without inhabitants, and has given them appetites for the productions of such countries, whose temperature is suited to their nature, as well as knowledge and abilities to seek and find them out. From which we may infer, that the birds we have been mentioning could no more subsist in the sultry climes of the Molucca isles, than birds of paradise could in the frigid regions of Sweden or Lapland.

Besides the migratory birds already mentioned, which breed and remain the whole summer, there are other birds that arrive periodically at certain places for the sake of some sort of grain, or other food, which may be supposed their own country is destitute of; these birds, after no long continuance, depart, and are no more seen till that time 12 months after; at which time they return, and so continue repeating these annual visits, as has been already observed of the rice-bird, and blue-wing of Carolina. Though the secret ways by which instinct guides birds, and other irrational creatures, are little known to us, yet the causes of some of their actions are apparent.

Analogous to the lucrative searches of man through distant regions, birds take distant flights in quest of food, or what else is agreeable to their nature; and when they discover some new grain, or pleasing food, they return and acquaint their community, and joining in numerous flights, make annual excursions to solace on this their exotic food.

Since the discovery of America there have been introduced from Europe several sorts of grain, which were never before known in that part of the world, and which not before some length of time were found out, and coveted by some of these migratory birds. No wonder that this grain should not be immediately known to birds of distant regions; for above half a century passed from the time of cultivating wheat, rice, and barley, in Virginia and Carolina, before those grains were found out and frequented by these foreign birds, of which one has only lately made its first appearance in Virginia, as Dr. Mitchel informed him; viz. that being in his garden a bird flew over his head which appeared with uncommon lustre, and surprised him the more, not having seen the like bird before. Mentioning this to some of his neighbours, he was told by them, what afterwards was confirmed to him by his own observation, viz. that these exotic birds had only within these few years appeared in Virginia, and had never been observed there before.

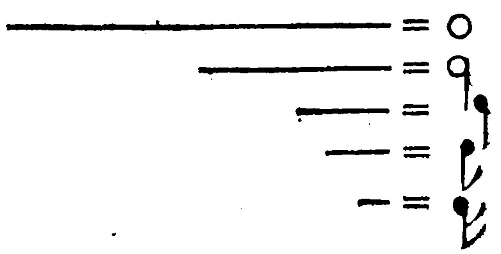
They arrive annually at the time that wheat, the fields of which they most frequent, is at a certain degree of maturity; and have constantly every year, from their first appearance, arrived about the same time in numerous flights. So that they have acquired the name of wheat-birds.

Of a Machine to write down Extempore Voluntaries, or other Pieces of Music. By the Rev. Mr. Creed. N° 483, p. 445.

The following is a demonstration of the possibility of making a machine that shall write extempore voluntaries, or other pieces of music, as fast as any master shall be able to play them on an organ, harpsichord, &c. and that in a character more natural and intelligible, and more expressive of all the varieties those instruments are capable of exhibiting, than the character now in use.

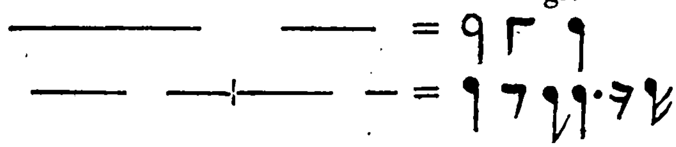
Maxim 1.—All the varieties those instruments afford fall under these 3 heads: 1st, the various durations of sounds, commonly called minims, crotchets, &c. 2dly, the various durations of silence, commonly called rests. 3dly, the various degrees of acuteness or gravity in musical sounds, as A re, B mi, &c.

Maxim 2.—Straight lines, whose lengths are geometrically proportioned to the various durations of musical sounds, will naturally and intelligibly represent those durations. Ex. gr.



The first line, being 2 inches, represents a semibreve.
The second is 1 inch, and denotes a minim.
The third is half an inch, and signifies a crotchet.
The fourth is a quarter, and answers to a quaver.
The fifth is an eighth, and denotes a semiquaver.

Maxim 3.—The quantity of the blank intervals, or discontinuity of the lines, will exactly represent the duration of silence or rests. Ex. gr.



Maxim. 4.—The different degrees of musical sounds, as gamut, A re, B mi, &c. may be represented by the different situations of those black lines on the red ones or faint ones. See fig. 6, pl. 7.

Problem.—To make a Machine to Write Music in the aforesaid Character, as fast as it can be Played on the Organ or Harpsichord, to which the Machine is fixed.

Suppose that a cylinder may be made by the application of a circulating, not a vibrating pendulum, to move equally on its axis the quantity of 1 inch in a second of time, which is about the duration of a minim in allegros. As the cylinder a, fig. 7, and to move under the keys of an organ, as b, c, d, and nail points under the heads of the keys; it is manifest that if an organist play a minim on c, that is, if he press down c for the space of a second, the nail will make a scratch on the cylinder of 1 inch in length, which is the mark for a minim.

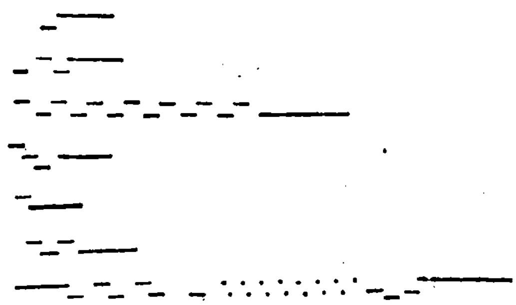
Again, if he rest a crotchet, that is, if he cease playing for the space of half a second, the cylinder will have moved under the nails half an inch without any scratch; but if the organist next press down d for the space of half a second, the nail under d will make a scratch on the cylinder half an inch long, which is the mark for a crotchet. It will likewise be differently situated from the scratch that was made by c, and consequently distinguished from it as much as the notes now in use are from each other by their different situation in the lines, in fig. 6. These 3 instances include all that can be performed on an organ, &c.

Therefore it is already demonstrated, that whatever is played on the organ during one revolution of the cylinder a (fig. 7), will be inscribed on it in intelligible characters. He proceeds to show how this operation may be continued for a long time.

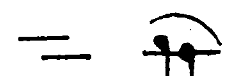
In fig. 8, aa, b, c, d, are the same as in fig. 7. Let x be a long scroll of paper wound about such a cylinder as z. Let eeee be the same scroll brought over the cylinder aa, to be wound on the cylinder yy, as fast as the motion of aa, which is determined by a pendulum, will permit. It is manifest, that whatever is played on the organ during the winding up of yy, will be written on the scroll by the pencils b, c, d, &c.

All the graces in music, being only a swift succession of sounds of minute duration, will be expressed by the pencils by small hatches geometrically proportioned to those durations. Ex. gr.

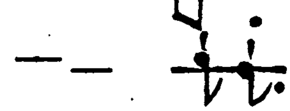
A single beat
A double beat
A shake
A turn
A single backfall
A double backfall
A shake and turn



If a line commence exactly over or under the termination of another, it is an indication of a slur; as



So a small interval indicates the contrary; as



Flat or sharp notes are implied by their situation on the red or faint lines; the natural notes being always drawn between them, viz. in the spaces. (See fig. 6.)

The scroll may be prepared before-hand with red lines to fall under their respective pencils. It is the surest way to rule them after; though it is possible to contrive that they may be ruled the same instant the music is writing. The places of the bars may be noted by two supernumerary pencils, with a communication to the hand or foot of a person beating time.

Grave music from brisk, slow from fast, &c. will be better distinguished by this machine, than in the ordinary way by the words adagio, allegro, grave, presto, &c. for by these words we only know in general this must be slow or fast, but not to what degree, that being left to the imagination of the performer; but here it is known exactly how many notes must be played in a second of time; viz. as many as are contained in 1 inch of the scroll.

Lastly, whereas, in the ordinary way of writing music, you have either no character for graces, or such as do not denote the time and manner of their performance, here you have the minutest particles of sound that compose the most transient graces mathematically delineated.

The Figure of the Mustela Fossilis. Communicated from Dr. Gronovius at Leyden. N° 483, p. 451.

Mustela fossilis, sive cobites cœrulescens, lineis quinque nigris longitudinalibus. Arted. Ichthyol. gen. xi. 3.* See fig. 1, pl. 8.

This fish was kept alive in a jar of water a year wanting 9 days, without changing the water, and without any other food than what the water afforded. These fish are dug out of the sands near Wesel in Holland.

On the Belluga-Stone. By Mr. Peter Collinson, F.R.S. N° 483, p. 451.

These stones of the belluga † were collected by Dr. Cook at Astracan. The calculus of the belluga is found of various shapes and sizes; it is mostly of a flatted oval figure, sometimes roundish, globular with unequal depressions, and of a yellowish white colour externally, and a smooth polished surface. It differs in magnitude, as it does in figure, from the bulk of a pigeon's egg up to 4 or 5 times that size.

They are mostly compact, ponderous and solid, not very friable, but requiring a pretty smart blow of a hammer to break them. They yield easily to the saw; but this defaces their internal texture, which is very remarkably elegant and regular. The stones consist of concentric coats firmly adhering to each other, formed about a nucleus, which appeared to be quite a heterogeneous substance, from its colour, hardness, and texture.

But another obvious circumstance in its structure renders the belluga stone different from most others, which is its radiated appearance. It seems composed of an infinite number of shining rays, regularly diverging from the central nucleus to the circumference, representing both in colour and form the flakes of a pure white terra foliata tartari, or (excepting the colour, which is yellowish) the striated spicula of antimony.

This stone is found in the fish called the belluga, a species of sturgeon, the acipenser tuberculis carens artedii, part iii. p. 92. It is commonly called lapis bellugæ, by the Russians kamen belluga, which signifies the same thing.

Of this fish several authors have given the following account. In shape it is not much unlike a sturgeon,‡ only its snout is proportionably shorter and thicker; the skin on the back is light grey, but under the belly it is white, and without

* Willoughby, Hist. Pisc. p. 124, tab. G. 3, 4. Raii Syn. Pisc. p. 69.—Orig. *Cobitis fossilis,* Linn.

† The fish in which the concretion here described is found, is the *acipenser huso* of Linn. or isinglass sturgeon, sometimes called beluga or belluga. The stone, according to Dr. Pallas, is found in the kidneys of the fish.

The beluga here mentioned must by no means be confounded with another animal of the same name, which belongs to the whale tribe, and is the *delphinus leucas,* Linn. Gmel.

‡ Vide Crull's History of Russia.—Orig.

scales:* its flesh is whiter than veal; whence the name belluga, or the white fish; and affords a much more delicious dish † than sturgeon. Of its row or spawn is made the cavlar; and some are found so large as to yield from 156 to 200 weight of it. They are found in greatest plenty, and especially those of the largest size,‡ in the river Volga, about the city of Astracan.§ Stralenberg says, he saw one caught in this river 56 feet long and 18 feet thick; and takes them to be the largest river fish in the world. They are likewise found in other rivers, as the Don, and those that flow into the Baltic and Caspian seas.

Mr. C. is not certainly informed, neither do authors agree, in what part of the fish this stone is found; Stralenbergh says in the head and stomach; some || say in the air bladder; others in a particular bag near the anus or inferior gut; others again in still different parts. It is found in both sexes, but oftenest in the male, and of all ages; but is very rare and scarce, for in a thousand fish it often happens that not one stone is found.

Hence it would appear that these stones are preternatural to the fish; perhaps morbid productions, just as the stone in the human bladder, notwithstanding its curious and regular form; probably the food of the fish, the situation of the parts in which it is generated, and many other circumstances, may contribute to this uniformity of appearance.

A little of this stone scraped, and laid on a hot iron, gave a faint urinous smell, and calcined into a light, greyish, insipid earth. Had it been a real animal substance, or a constituent part of the animal, its smell would probably have at once discovered it.

The natives about the Volga very much esteem this stone for its virtues, being in great reputation to promote delivery. The common people take from 10 grs. to 30 40, or even 60, scraped fine in a little water, 2, 3, or 4 times in 24 hours, when the case is dangerous. It is also highly commended as a diuretic and lithontriptic; and this not only among the common people, but among such as are more capable of informing themselves of its effects.

An Occultation of Cor Leonis by the Moon, on Thursday, March 12, 1747, observed in Surrey-Street, Strand, London, with a Reflecting Telescope, made by Mr. Short, F. R. S. which magnified about 100 *times. Communicated to the Royal Society by J. Bevis, M. D.* N° 483, p. 455.

Apparent Time.

1747, March 12^d 8^h 24^m 19^s The star immerged into the dark limb.

* Stralenbergh's History of Siberia.—Orig. † Crull's History of Russia.—Orig.
‡ Stralenberg, ibid.—Orig. § Crull's History of Russia.—Orig.
|| Dr. Cook's Letter.—Orig.

1747, March	9^d	27^h	4^m	It emerged from the enlightened limb a small matter to the west of the moon's zenith.
		44	4½	The moon's preceding limb passed the meridian in the transitory.
		44	21	The star passed the meridian.

Mr. Short, another gentleman, and Dr. B. agreed to a single second in the immersion, with different telescopes; but the Doctor saw and pronounced the emersion 2 or 3 seconds before them.—There had been an exact observation of the sun's transit at noon; and the clock gained about half a second a day.

Surrey-street is accounted 27 seconds in time west of the Royal Observatory at Greenwich.

An Observation of an Uncommon Gleam of Light proceeding from the Sun. By Mr. Peter Collinson, F. R. S. N° 483, p. 456.

March 8, 1746-7, near 8 o'clock in the morning, as Mr. C. was riding within 3 miles of Brentwood in Essex, there appeared a singular phenomenon in the heavens. The morning was fine and clear, the sun shone bright, no cloud to be seen, but the air a little hazy: where the phenomenon appeared, which was a bright cloudy spot, seemed a very small portion of a rainbow, only the colours very faint. It was in a horizontal direction north of the sun, and from it projected a long luminous ray, which terminated in a point. It continued very strong for more than half an hour after he saw it, and then vanished away by degrees.

On the Property of New Flannel Sparkling in the Dark. By Mr. Benjamin Cook, F.R.S. N° 483, p. 457.

Since reading the Trans. N° 476, with respect to the sparkling lady, who could communicate a kind of electrical fire to her garments, Mr. C. can give an instance nearly like it, of a lady who was surprised at such an appearance from a flannel petticoat, which she happened to shake in the dark. But at last it was found, that new flannel, after some time wearing, would acquire this property; but that it lost it by washing.

On Windsor Loam. By Mr. John Hill, Apothecary. N° 483, p. 458.

An accident calling Mr. Hill to Hedgerley, the place where there is dug an earth commonly called Windsor loam, and famous not only in England, but many other parts of the world, he went to the pits, and informed himself of the present condition of them: and as there appears too much probability that this earth will be exhausted and lost entirely to the world, in a few years, he presumes it may not be unacceptable to have an account of the pits of it, and whatever else relates to it, taken on the spot.

This earth itself is a coarse harsh loam, composed of a very large shining sand, of extreme hardness, and a fine soft tenacious clay: its value is its remarkable quality of standing the force of the most violent fires without running to a glass; which makes it extremely useful to all who have occasion for such fires, and is the reason of its being sent not only into all parts of England, but to Holland, Germany, and many other parts of the world. It is used for making the bricks employed in building the wind-furnaces for melting iron, for coating over the insides of assay furnaces, used by the workers on metals, and on many occasions of like kind at the glass-houses, both in England and other nations.

The place where it is dug is Hedgerley, a small village about 22 miles from London, surrounded with hills, under one of which this loam lies. The pits are about a quarter of a mile south-west from the town, and 5 miles north of Windsor: they extend over 4 acres of ground, situated on the descent of a hill; and were intended to have been carried over much more ground by the person who now works them; but on trials the loam is found not to extend as was imagined.

They dig, before they come at this, a very good common brick-clay, a tile-clay, and a potter's earth, a kind of clay of a firmer texture, and deeper colour, than either of those; but the strata of these are seldom pure or regular, and at the boundaries of the stratum of loam a pure hard sand, evidently the same with that in the composition of the loam, but left loose, from there not having been clay in the way to bring it into the condition of the perfect mass. They have already worked the stratum so far as to find it bounded east and west by beds of this sand, and northward by chalk, and are therefore afraid it will be soon exhausted; at least, whatever they get hereafter must be procured with more labour and expence, as they have no where to search for it but higher up in the hill; from whence it must be fetched at greater depths, and much more expence; and this increasing difficulty of procuring it has been the reason of its rising in its price to that it is now sold at, which is 5 shillings a bushel in London; but which is not to be wondered at, since on the spot the quantity that makes a thousand bricks, which used to cost 1s. 8d. now costs 10s. the digging, and will every year cost more and more, unless a new stratum of it should be discovered somewhere thereabouts, which their many unsuccessful trials make them at present despair of.

It is to be observed, that this valuable earth forms but a single stratum, and that does not rise and dip with the elevation and descent of the hill, as the strata of the earth, stone, &c. in hills usually do, but seems to be even and flat at its bottom; for the higher up the hill they open their pits, the deeper in proportion they find the stratum of loam lie.

It is worthy observation, that this hill appears from this not to have been formed as the hills and mountains on the earth in general have been, by a disruption and elevation of the strata by violence from within the earth; for in that case this stratum of loam must have been elevated with them, and would have been as near the surface, or nearly so, in one part of the hill as in another, and need have been dug for no deeper from the top than from any other part; whereas, on the contrary, it appears to lie flat and level underneath the whole mass of earth, which makes the hill, and was probably the surface, on the first settling of the terrestrial and other matter from among the waters of the deluge.

The earth, which makes the hill, seems to have been a prodigious mass of matter, rolled along by the irresistible force of that immense body of water, and afterwards lodged upon it.

That this might be the case, the immense force of that vast quantity of water, and the ease with which heavy bodies are moved in water, may serve to make probable; and what the more favours the conjecture is, that the earth which makes the hill is not disposed in such regular pure strata, as the earths settled regularly from the waters always are, but seems evidently a mixed mass, made by the jumbling together of various kinds of clay, &c. which are in some parts of it found pure, though not in whole strata; and in others irregularly blended in different proportions one with another; which, as the principal matters that compose it are of very different colours, viz. a red and a white clay, is the more apparent. And this is further confirmed by there being none of those common extraneous nodules found lodged in it, which are so frequent in the strata of clay formed by subsidence; such as the ludus Helmontii, pyritæ, &c. These have settled with, and lodged themselves almost every where among those strata; but it is no wonder there are none of them here, if this hill has been formed as above; since in the rolling it along, they must naturally have been left behind: and he supposes that the frequency of these bodies in almost all our little clay-pits, and the entire absence of them in the vast quantities of clay that have been dug here, will be esteemed, by all who have looked deeply into these studies, one great argument of the truth of this system; which may also extend perhaps to many other hills as well as this.

As the workmen are now obliged to dig this loam at 26 feet deep, instead of about 14, at which depth they long found it, and must hereafter, as they are obliged to ascend the hill, dig it at 38 or 40 feet, the price of it will probably deprive us of it before the vein is exhausted.

It would be a matter worthy consideration, whether, from examining the parts it is composed of, a succedaneum might not be found for it, by an artificial mixture of similar substances. In order to attempt this, Mr. H., by means of water, disunited its parts, and procured them separate; and on comparing them with the various earths and sands from different parts of England, which he at times procured, he thinks that he can exactly match the sand with one from Hampstead heath, and the clay with one from a pit near the lower end of High-gate: the proportions may be easily learned, by accurate observation of the quantities of each, where disunited; and a succedaneum on these principles easily made.

It is evident, that the only reason why it endures the fire so much better than other clays, is the extreme hardness and great quantity of the sand it contains: and as he imagines it easy to throw a sand of equal hardness, and in equal quantity, into an artificial loam, he sees no reason to doubt of making it equally useful.

On the Relief found in the Stone from the Use of Alicant Soap and Lime-Water. By Mr. Rob. Lucas. N° 483, p. 463.

Dr. Morgan advised Mr. L. to drink a pint of lime-water every day. Colonel Morgan and his lady advised him to take 4 pills of alicant soap morning and evening; on which he resolved to add the soap-pills to the use of the lime-water; only, instead of the quantity proposed, he took between 20 and 30 a day, amounting to near an ounce; which he thought he might safely do, well knowing that Mrs. Stephens's prescription amounted to almost 3 oz. of soap, besides other ingredients.

He used with great success stone-lime newly calcined; but by those experiments it should seem, that the dissolving power of lime water made of oyster-shells, is almost double to that of lime-stone. There are two good qualities attending these remedies: the first is, that they are cheap, easily come at, and prepared by one's self. 2dly, that they may be safely used for a long time, without danger to health; for a quart of lime-water, and an oz. of soap, had never given him the least nausea, or lowness of spirits, or abatement of appetite, and he was never better in health than at the above date (1747).

His motive for being so particular in this affair, is a desire to be instrumental of giving ease to others in so unhappy a condition; being firmly persuaded, that what has already so far relieved him, will dissolve stones of greater magnitude than he supposes his to be.

On the Figures of some very Extraordinary Calculous Concretions formed in the Kidney of a Woman. By Mr. Charles Lucas at Dublin. N° 483, p. 465.

These calculous concretions were formed in the left kidney of Mary Anne Mac Mahon, otherwise England, taken out after her death, in the 30th year of her age. They were of very irregular and various forms, mostly resembling many rough pebbles rudely united and cemented together.

On the Formation of Pebbles. By Mr. Wm. Arderon, F.R.S. N° 483, p. 467.

In all strata of pebbles, that Mr. A. examined, there are some which are broken, and whose pieces lie together, or very near each other; but as bodies of such hardness could not be broken without some considerable force or violence, their situation implies that they suffered such force or violence as broke their parts asunder, in or near the place where they at present lie.

Others again have had pieces broken from them, though not the least fragment of those pieces can now be found: whence we must conclude, that whatever might be the cause of their fracture, they must either have been broken at some place distant from where they now lie, or the pieces broken from them must at some time or other have been removed to some distant place.

Several of these pieces of broken pebbles have their edges and corners so very sharp, that it seems as if they had never been removed from the place where they received the damage. Others have their sides and corners so blunted, rounded, and worn away, that one cannot help imagining they must have been very roughly tossed backwards and forwards against other hard bodies, and that too with great violence, or for a very long continuance; since without a great deal of friction such hard bodies could scarcely have been reduced to the forms they are now found in.

Among these strata of pebbles are several fragments of various kinds of marble, various kinds of sand-stone, and various kinds of gypsum, though this part of the kingdom affords no such thing; most of which have attained the hardness of the very hardest of our pebbles, as it should seem, by lying among them.

Such pebbles as are found here in strata near the surface of the earth, are much more brittle, and break easier without comparison, than those which lie in deeper strata: for if the first of these fall, but with their own weight, on any other stone, from the height of 3 or 4 feet, they will break very frequently into ten or a dozen pieces; whereas such as are found deep in the earth will endure being thrown against each other with all the force one can give, and that too 20 times perhaps, before the least splinter of them can be broken off.

On the Distances between Asia and America. By Arthur Dobbs, Esq. of Castle-Dobbs in Ireland. N° 483, p. 471.

Professor Euler,* swayed by the opinion of captain Behring, seems still to believe that the last land he discovered is joined to California, which country is now known to be part of the continent of America, and not an island; in which fact of its being continuous to California, Mr. D. differs still in opinion

* See Phil. Trans., N° 482, or Vol. ix, p. 320 of these Abridgments.—Orig.

from him; for if that were a fact to be depended on, he would candidly own that there could be no passage from the north-west of Hudson's Bay to the western ocean of America, without sailing near 70° of longitude; the distance of the north-east cape of Asia from the north-west of Hudson's bay, in a parallel almost as far north as the polar circle, before the passage can be made to the Pacific Ocean; which might therefore be very reasonably called an impracticable passage, as it could not possibly be made in one summer, if at all.

Now Behring fixes his north-east cape 126° 7′ east longitude from Tobolski; and Tobolski is 86° east from Fero; so the cape is 212° 7′ east of Fero, or about 194° east from London. By captain Middleton's observation of Jupiter's satellite at Churchill river in Hudson's Bay, that river is 95° west from London; which, added to 194°, makes 289°; consequently the north-east cape of Asia is 71° distant from Churchill, to complete 360°; which, in the latitude of 65°, computing 8 leagues to a degree of longitude, of which 20 make a degree of latitude, the distance between that cape and Hudson's Bay would be 568 such leagues.

From the known longitude of the north cape of Japan in 40° lat., which is pretty exactly known, from the observations made by the jesuits at Peking, and is about 150° east from London, and from the best computed longitude of California in 40° north lat., it lies in 130° long. west from London, making together 280°, leaves 80° for the distance of California from Japan; allowing 17 leagues to a degree of longitude in 40° north lat., the distance would be about 1360 leagues: by the same calculation California must be at least 7 or 800 such leagues from the north-east cape of Asia; so that in so great a space there may be very great countries or islands,* without supposing the new discovered country continuous to California, and might well allow of an open channel or sea, from 50 to 100 leagues wide, between the discovered coast and California.

By the account given to professor Euler, Behring sailed southwardly to the Isles of Japan, and from thence sailed eastwardly 50 German miles, about 250 English miles; which makes about 80 leagues, of 20 to a degree. At that distance from Japan he discovered land, which he coasted north-west; still approaching towards the north-east cape, without going ashore, till he came to the entrance of a great river; where sending his boats and men ashore, they never returned, being either lost, killed, or detained by the natives, which made his discovery incomplete; his ship being stranded, and he afterwards died in an uninhabited island.

As no latitudes nor longitudes are fixed by this account, he probably sailed

* The Japanese, in their maps of the world printed in Japan, have laid down in this very tract two islands as large as Ireland, with the names to them, as appears in that map bought by Dr. Kempfer in Japan in 1686; now in Sir Hans Sloane's museum.—Orig.

from Kamschatka south-east, perhaps more southerly than to 50° latitude; and there found land north-east from Japan; otherwise, by coasting it north-west he could never approach the north-east cape, which is at least 40° longitude east of Japan; and if he made land 80 leagues east of Japan, he must have sailed north-east to make the north-east cape. There is therefore reason to believe this coast was part of that he saw in his first voyage, where he lost his anchor; and is the coast Gama discovered, and the Dutch afterwards called the Company's Land, east of the straits of Uzicez, which is at least 7 or 800 leagues west of any known land of America, and above 1000 near the latitude of Japan; so that, if I should allow 700 leagues for countries or islands east of his new-discovered coast, there might still be a passage of 100 leagues for the southern or Pacific Ocean to communicate with Hudson's Bay, and to cause such great tides and currents, as are found on the north-west of Hudson's Bay; as also a free passage for the whales, which are seen in all the openings north-west of that bay, and are caught there in numbers by the Eskemaux savages; for as these do not go in by Hudson's Strait from our Atlantic Ocean, it cannot be presumed that they should go up by Japan towards the north-east cape, and from thence go 70°, or above 660 leagues, to Hudson's Bay, and be there in the month of June, and, after staying till September, return again the same way to the southern ocean, to pass the winter. Now as Behring only coasted at a distance, he could not possibly know whether it was a continent, or great island; the last of which seems the most probable.

On the Chinese Chronology and Astronomy. By the Rev. Mr. G. Costard. N° 483, p. 476.

The affectation of some nations, in carrying up their histories to so immoderate a height, plainly show those accounts to be fictitious and without foundation. This was the case of the Babylonian and Egyptian accounts; and probably it will be found to be the same with any other people that make the like pretensions. The only people in later times that have been thought to contradict this opinion, are the Chinese, of whose history the world has been taught to entertain very extraordinary conceptions. But that even they will be no exception to this surmise, but on the contrary a strong confirmation of it, will, Mr. C. thinks, appear from what he now offers.

Mr. C. then enters on a desultory discourse to show the improbability of the very remote æras of the Chinese historians, as well as of skill in their astronomers, to make any just and accurate calculations of the celestial motions; notwithstanding what has been said by the Jesuit missionaries on those heads.

From what has been here offered, Mr. C. thinks, it is pretty evident, that how ingenious soever the Chinese may be in works of art, their talents do not lie towards mathematics and astronomy; for were not this the case, must it not be surprising, that having, as they say, so long a series of observations in the one science, and of professors in the other, they should never have been able to get beyond the first elements of either?

It is not his design, he says, to enter into any controversy with the learned fathers of the Society of Jesus; the world has been frequently indebted to them for their philosophical labours; and will be so again, when they shall have considered the Chinese history with proper accuracy, and told us in what manner they have been able to preserve accounts and observations of so ancient a date. Public libraries, it is allowed, they have none; nor does it appear they ever had. Where then could things so useless, as the generality must have thought astronomical observations, be reposited? When intrusted to private hands, they must have run great risk of being destroyed by wars, by fires, and in popular commotions; which must frequently have happened in so long a course of years.

Of a new Mirror, which Burns at 66 Feet Distance, invented by M. de Buffon, F. R. S. and Member of the Royal Academy of Sciences at Paris. By Mr. Turberville Needham. N° 483, p. 493.

This instrument may be considered as Archimedes revived; and the credit of antiquity, in this point, is in some measure re-established. This machine consists of 140 small plain mirrors, each of about 4 by 3 inches square; they are fixed at about a quarter of an inch distance from each other, on a large wooden frame about 6 feet square, strengthened with many cross bars of wood for mounting the mirrors. Each of them has 3 moveable screws, which the operator commands from behind, so contrived, that the mirror can be inclined to any angle in any direction that meets the sun; and by this means the solar image of each mirror is made to coincide with all the rest.

They tried the experiment one morning with 24 only; for no more were then ready for the purpose: the effect was, that in very few seconds of time, a combustible matter prepared with pitch and tow, and daubed on a deal-board, was set on fire, and burned vigorously at the distance of 66 French feet. Judge now of the effect 140 will produce; and whether the invention may not be improved to the height of all that has been advanced of Archimedes by the ancients. The only difficulty they found was, to make the solar images of the mirrors coincide; but this is owing to the yet imperfection of their method of mounting, which may be easily improved.

On the same Mirror Burning at 150 *Feet distance. By the Marquis Nicolini, F. R. S.* N° 483, p. 495.

The affair of Archimedes setting the Roman fleet on fire by means of burning glasses, has been considered as a thing impossible and romantic. Descartes positively denied the fact, which had been believed for so many ages; and our modern philosophers, after many trials, and various reasonings, have been of the same opinion. But M. de Buffon, being asked if it might be possible to invent a phaometer, or machine for measuring the intensity of light, has discovered by trial, that light was able to produce great effects in a focus at a great distance, by using a great number of disks, which would reflect so many images of the sun, and throw them all into one place. He put together therefore a sort of polyedron, consisting of 168 small mirrors, or flat pieces of looking-glass, each 6 inches square; by means of which, with the faint rays of the sun, in the month of March, he set on fire some boards of beech wood at 150 feet distance. By increasing the numbers of mirrors, he hopes to be able to do the same at 900 feet distance.

His machine has besides, the conveniency of burning downwards or horizontally, at pleasure; and it burns either in its greater focus, or in any nearer interval, which our commonly known burning-glasses will not, their focus being fixed and determined.

Perhaps this machine may afford a manner of measuring either light, or the different degrees of heat of burning bodies. The difficulty is to find the method of marking the degrees, and of fixing a point of comparison; for the point of kindling will not determine it; because that chiefly depends on the greater or less degree of inflammability of different combustible bodies.

Mr. Maupertuis, in a letter to the president, dated at Potzdam, May 20, 1747, says, that his friend Buffon has recovered the burning-glasses of Archimedes; that with 168 plane glasses, each 6 inches square, he has melted a silver plate, at the distance of 60 feet, and fired pitched boards at 150. Each speculum is moveable, so as, by the help of 3 screws, to be set to a proper inclination for directing the rays towards any given point.—Orig.

On a Pirorganon, or Electrical Machine. By Jo. Henry Winkler, Prof. Lips., F. R. S. N° 483, p. 497.

This is an electrical machine, contrived to make several sparks and crackles, from several different parts, and which sparks may exhibit certain imaginary figures and shapes; such as, a winged wheel, also Charles's wain, &c. But the machine being extremely complex, and the uses merely fanciful and whimsical, it is not deemed of any use to represent or describe it more particularly.

On Gems or Precious Stones; more particularly such as the Ancients used to engrave on. By Robert Dingley, Esq. N° 483, p. 502.

Gems or precious stones, of all species, are sometimes found of regular shapes, and with a natural polish; and sometimes of irregular shapes, and with a rough coat. The first sort may be considered as of the pebble kind; and they are said to be found near the beds of rivers, after great rains; the others are found in mines, and in the clefts of rocks.

The gems of the first sort were what the ancients most usually engraved on: these are commonly called intaglios; and they are mostly of a long oval figure, inclining to a point at each end, convex as well on the engraved face as on the others, with a ridge running from end to end on the outer side, which is thus as it were divided into two faces; both which are also, though not so distinctly, parted from the upper face, by another ridge running quite round the oval.

The stone most commonly found engraved is the beryl; the next is the plasm or prime emerald; and then the hyacinth or jacinth. The chrysolite is sometimes, but rarely, found engraved; as are also, but that very seldom, the crystal, or oriental pebble, the garnet, and the amethyst.

Of the beryl there are three species; the red, inclining to orange-colour, transparent and lively; the yellow, of an ochre-colour; and the white, commonly called the chalcedon, of the colour of sheer milk. These last two have less life than the first.

The plasm or prime emerald is green, nearly of the colour of stagnated water; sometimes tolerably clear, but for the most part full of black and white specks, and rather opaque.—The jacinth is of a deep tawny red, like very old Port wine, but lively and transparent.—The chrysolite is of a light green grass colour, and is supposed to have been the beryl of the ancients, transparent, but not lively.—The crystal, or oriental pebble, is harder and more lively than the common rock crystal; is of a silverish hue, and but very little inferior to the white sapphire.—The garnet is of the same colour as the jacinth, but more inclining to the purple, and not so lively.—The amethyst is of a deep purple, transparent and lively.

There were some other species of stones engraved on by the Romans; but rarely before the latter times of the empire, when the art itself was greatly on the decline. All the beforementioned sorts of stones are said to have been of the produce of Egypt, or of the East Indies; and to have been brought from the borders of the Nile, or of the Ganges.

Here follows a general table of what are usually called precious stones.

The beryl is red, yellow, or white.—The plasm is green.—The jacinth of a deep tawny red.—The chrysolite of a light grass green.—The crystal, or oriental pebble, of a silverish white.—The garnet of a deep red claret-colour.—The amethyst, purple.—The diamond, white.—The ruby, red or crimson-coloured.—The emerald, of a deep green.—The aqua marina, of a bluish sea-green, like sea-water.—The topaz, of a ripe citron yellow.—The sapphire, of a deep sky-blue, or of a silver white.—The cornelian, red or white.—The opal, white and changeable.—The vermillion-stone, is more tawny than the jacinth.

All these stones are more or less transparent; the following are all opaque:

The cat's-eye, brown.—The red jasper, called also thick cornelian, is of the colour of red ochre.—The jet, black.—Agates, are of various sorts.—The blood-stone, is green, veined or spotted with red and white.—The onyx, consists of different parallel strata, mostly white and black.—The sardonyx, of several shades of brown and white.—The agat onyx, of two or more strata of white, either opaque or transparent.—Alabaster, different strata of white and yellow, like the agate-onyx, but all opaque.—The toad's-eye, black.—The turquoise, of a yellowish blue inclining to green.—Lapis lazuli, is of a fine deep blue.

Of most of the species beforementioned there are some of an inferior class and beauty. These are commonly called by jewellers occidental stones; they are mostly the produce of Europe, and found in mines or stone quarries; and are so named, in opposition to those of a higher class, which are always accounted oriental, and supposed to be only produced in the more eastern parts of our continent.

The onyx, sardonyx, agate-onyx, alabaster of two colours or strata, as also certain shells of different coats, were frequently engraved by the ancients in relief; and these sorts of engravings are commonly called cameos. They also sometimes ingrafted a head, or some other figure in relief of gold, on a blood-stone. Besides which, there are some antiques, mostly cornelians, that are covered with a stratum of white. This stratum has by some been considered as natural, but it was really a sort of coat of enamel that was laid on. This was used only in the times of the lower empire.

The stones esteemed the best for engraving on, were the onyx and sardonyx; and next to them the beryl and the jacinth. The ancients engraved most of their stones, except the onyx and the sardonyx, just as they were found: their natural polish excelling all that can be done by art; but the beauty of the several species of onyxes could only be discovered by cutting.

The merit both of intaglios and cameos depends on their condition, on the goodness of the workmanship, and on the beauty of their polish. The antique gems of Greek work are the most esteemed, and next to them the Roman ones, in the times of the higher empire.

A New Method for a Mural Astronomical Quadrant, free from many inconveniencies to which former ones are subject. By Christian Lewis Gersten, F.R.S. N° 483, p. 507. *From the Latin.*

The great usefulness of arches, firmly fixed to walls in the plane of the meridian, is well known to all who are the least conversant with astronomical observations. Hence it is that few observatories can be accounted well furnished without one. Yet it is commonly found that there is no wall so solid and firm, and no bond of iron or other metal so strong, as to keep this instrument perfectly true with respect to the earth's axis. Mr. G. has therefore devised this new contrivance for a mural arch, furnished with a telescope and micrometer, to be constructed so as to answer the following purposes, viz.

1. That at any time it may appear if the plane of the instrument be vertical. 2. Whether a perpendicular passes exactly through the centre of the quadrant and the beginning of the divisions on the limb. 3. To correct the aberration of the plane of the quadrant from the vertical line, without altering the position of the beginning of the divisions on the limb, with respect to the perpendicular. 4. And again, to correct the aberration of the beginning of those divisions, from the perpendicular, without changing the proper position of the plane of the quadrant with respect to the vertical line. 5. In like manner, to correct the deviation of the plane of the quadrant from the plane of the meridian, without altering the perpendicular situation of the plane of the qnadrant, and of the beginning of the divisions. 6. That it may be quite free from the variation in the expansion of the metals by heat and cold. 7. That the instrument may easily be rectified, viz. that it may easily appear whether the line passing from the object, through the intersection of the threads in the tube to the eye, be exactly parallel to the line passing through the centre of the quadrant and the division shown by the rule; and to set it easily right when there is occasion; a division otherwise very laborious and difficult.

To obtain all these requisites, Mr. G. then gives a very long and minute description of the several parts and contrivances of this instrument, with a vast multitude of references to a great number of plates and figures; in which it is not now profitable to follow him, as we apprehend we are possessed of better mural quadrants in this country, the description of which will hereafter occur.

The whole quadrant itself is recommended to be of solid metal, by which means, Mr. G. says, when it expands or contracts by heat or cold, it will always remain similar to itself; and the free expansion or contraction will not be hindered by its suspension.

Two Observations relating to Morbid Anatomy. By Albert Haller, Professor of Physic at Gottingen, and F. R. S. N° 483, p. 527. *An Abstract from the Latin.*

In the first of these observations it is stated that in a woman 40 years of age, the vena cava, between the origin of the left renal vein and the iliac veins, was discovered to be contracted in so great a degree as scarcely to allow the blood to pass through it. A portion of coagulated polypose blood was found within its contracted cavity. On the other hand, the right spermatic vein was dilated to such an extent, as to supply the place of the vena cava, transmitting the blood, diverted from its natural channel, to the vein of the ureter (ureteris venæ) which in the sound state is a small vein, and arises from the right iliac. A similar instance of a constriction or obliteration of the cavity of the vena cava is recorded in Johan. Rhodii Mantiss. Anat. Obs. xxi. From both cases it is evident, that an obstruction may occur in the larger blood-vessels; and that when, by reason of such obstruction, the blood is diverted from its natural channel, it rushes with impetus into the smaller canals, which consequently become dilated.

The second observation relates to the appearances noticed in the dissection of a woman, who was reputed to be 100 years old. The muscles, glands, nerves, and cellular membrane, were found to be very compact and hard. There was a very great enlargement of the aorta at its origin from the heart.

1. The arterious valves of the heart were partly indurated, partly interspersed with stony concretions (petrosis tumoribus) much the same as in one of Cowper's figures (Myolog. Reform. tab. xi). The other valves were nearly in a sound state.

2. The inner coat of the aorta, not only at its origin from the heart and in the thorax, but in the abdomen also, had the appearance of being ulcerated, and was beset with a number of loose squamæ, which were either of an osseous or of a stony compactness; and heaps of small tophaceous concretions blocked up the mouths of most of the vessels which branched off from the aorta.

3. In like manner a number of osseous crustæ were found on the internal coat of the hypogastric and iliac arteries, and of the arteries which go to the pelvis, &c. And in all the arteries of the body were found hard coagulated portions of blood of a round figure, but of a smaller diameter than their containing canals.

4. The gall-bladder was full of bile, scarcely bitter, and moreover contained 20 small angular calculi, by one of which the mouth of the ductus cysticus was completely obstructed. Professor H. remarks that he had generally found the bile to have a sweet taste, when concretions were formed in it.

Concerning the Property of Water Efts in slipping off their Skins as Serpents do. By Mr. David Erskine Baker.*† N° 483, p. 529.

This animal is to be found in the spring, and during the whole summer season, in most ditches and shallow standing waters throughout England, being unknown to very few.

It has long been known, that most of the serpent kind put off, or, as we commonly term it, cast their skins, at certain periodical times; though we are very little acquainted with the manner of their performing this work, since it is

* The species of newt here intended is the *lacerta aquatica* of Linnæus, or common water-newt.

† Son of Mr. Henry Baker before noticed. Vol. 8, p. 426.

commonly done in their retiring places, where we can seldom get a sight of them; nor should we indeed know that their skins are changed at all, did we not often find the skins they have cast off. But from this little lizard, which Mr. B. has more than once carefully attended during the whole operation, a reasonable guess may be formed as to most other kinds; and as it is a creature easy to procure, may be kept in a jar of water for many months, and the intervals between the periods are so short, for they shed their skins every 2 or 3 weeks, it is in every one's power to see with his own eyes what is here described.

A day or two before the skin is to be changed, the animal appears more sluggish than usual, takes no notice of the worms you give it, which at other times it devours greedily; the skin in some places appears loose from the body, and its colour not so lively as it did before; and thus it continues till the great work of putting off the old skin is to be performed. It begins this operation by loosening with its fore feet the skin about its jaws, which, when open, are wider than any part of its own body, and pushes it backward gently and gradually both above and below the head, till it is able to slip out first one leg, and then the other; which when it has done, it proceeds to thrust the skin backward as far as these legs can reach; it is then obliged to rub its body against pebbles, gravel, or whatever else it can meet with, till more than half its body is freed from the skin, which appears doubled back, and covering the hinder part of the body and the tail. When the business is thus far done, the animal, turning its head round to meet its tail, takes hold of the skin with its mouth, and setting its feet on it, by degrees pulls it quite off, the hind legs being drawn out as the fore ones were before.

If the skin be then examined, it will be found with its inside outwards, but not having the least hole or breach; that part which covered the hind legs seeming like gloves that are turned without pulling out the tips of the fingers, though entirely perfect and unbroken. The coverings of the fore legs remain within the skin. They do not however put off the coverings of their eyes along with the skin, as some snakes are found to do; for the skin of this little creature has always two holes at the places where the eyes have been.

This operation sometimes takes up near half an hour, after which it appears full of life and vigour, as well as very sleek and beautiful.

When the skin is come off, if it be not taken away soon, it is very common for the creature to swallow it whole, as it does all its other food; and if it take it in by the head-part first, as frequently is the case, the tail-part, being filled with air and water, becomes like a blown bladder, and proves so unmannageable that it is very diverting to see the pains it costs to discharge the air and water, and reduce it to a fit condition to be got down its throat.

Many creatures of very different kinds put off their skins and shells at certain periods. All serpents are supposed to do so; the skins of several kinds being often found whole. Crabs, lobsters, cray-fish, shrimps, and probably most or all of the crustaceous fishes, cast their shells from time to time; and if we may guess of the rest by the fresh water shrimp, which Mr. B. had kept several times and observed, their shells are put off without any other breach than one, longitudinally, in the middle of the belly part, through which the body, tail, and claws are drawn out, and the shell left in a manner whole.

Of the insect tribe, every caterpiller has 3 or 4 skins, before its change into the aurelia state, in which the place of creeping out is a little below the head. The spider throws off the skin or shell 3 or 4 times, getting out of it by a rupture underneath, and leaving every claw, and even the horny covering of his forceps entire. Even the little mite casts its skin also at several short periods, and nearly in the same manner.

Pl. 8, fig. 2, represents the lizard; and fig. 3 the same in the act of drawing off the skin with its mouth.

A Remark by the Editor.—Wm. Oliver the viper-catcher, mentioned in N° 443 of these Transactions, made a present to the Royal Society of a female viper big with young, which was kept alive in common green moss, in a box with a glass cover. She brought forth several young ones, which slipped off their skins, and the outer membrane of their eyes along with them, in 6 weeks after their birth; and they shed them again 2 months after: but being then put into spirits of wine to preserve them, they were killed; but may still be seen in the museum of the Society. They first loosen the skin about the mouth, and so slip it off backwards, by wriggling themselves through the entanglement of the moss: for some of the skins were torn, and parts of it stuck in the moss. C. M.—Orig.

An Improvement of the Celestial Globe. By Mr. James Ferguson. N° 483, p. 535.

The paper may be seen at large in Mr. F.'s Astronomy, art. 401, and the fig. of the globe in pl. 3 of that work.

The Case of a young Child, at Houghton in Huntingdonshire, born with all its Bones Displaced. By Mr. Edward Davis, Surgeon at Huntingdon. N° 483, p. 539.

Mr. D. being desired to see this child, found both the radius and ulna of the right arm, with the bones of the carpus and metacarpus, also the fore-finger and little finger of the same hand, all dislocated. The radius and ulna of the left arm were dislocated, and receded from each other; likewise the fore-finger and little-finger of the same hand. The os femoris of the right leg was dislocated very oddly, and laid downwards, so that one might feel the end of it: the patella laid high up the thigh; and the tibia and fibula at their union with the os femoris were also dislocated, and receded very much from each other. The right leg, the tibia and fibula, at their union with the os calcis, also the os calcis, and the tarsal and metatarsal bones, likewise most of the toes. Of the left leg, the fibula, with some of the metatarsal bones, and some of the toes. The head likewise was very curious: the lambdoidal suture ossified all round, and rose with a prominency half an inch high: the occipital bone had several risings, which felt like several exostosises; and the 2 protuberant sides of the occipital bone enlarged to a prodigious degree, and united with each other, but with a dent between them which felt like a suture. They were enlarged to about 6 inches long, and 3 broad: it was all ossified; the midwife and nurse said it was soft at first: the rest of the head appeared very well.

This child was 7 days old: he had reduced the dislocated bones, though some with great difficulty; for the ends of the bones and cartilages seemed to be all ossifying; and there seemed to be a universal anchylosis coming on. He could not reduce the right foot well; it was all ossified, with the bones displaced, and the extensor pedis pollicis longus was contracted, and had drawn the foot almost round. The jaw-bone was also dislocated, which the midwife could easily put in its place, and the chin-stay supported it pretty well, only apt to slip out on one side. The midwife and nurse said they could for the first 2 days, put all the bones in their places with ease, but they continually fell out again.

The mother received a fall a fortnight before delivery, and she fancied the bones were displaced with the fall, though she did not hurt herself: but whether it was from thence, or from some vice in the fluids, he would not determine. If it were not for several exostosises and anchylosises in several parts, he should have imagined the child (though so young) was rickety; but for the above reason it could not be that. The child seemed lusty and strong, but he thought would soon be otherwise; the woman was lusty, and walked out about her business, though but a week before delivered; and she had 6 children besides, all very healthy.

On the Situation of the Ancient Roman Station of Delgovitia in Yorkshire. By John Burton, of York, M.D. N° 483, p. 541.

The learned antiquarians have hitherto been greatly at a loss to find the place where the Delgovitia of the Romans really stood; some supposing it at one place, and some at another.

Mr. Francis Drake, in his excellent History and Antiquities of York, has given every thing which has hitherto been written in support of the claim made by each place to the honour of rising out of the ruins of that ancient town, with his reasons for fixing that station at Londesburgh; all which need not now be repeated.

There are 3 places where the site of Delgovitia has been fixed at; viz.

Weighton, Godmanham, and Londesburgh. But Dr. Burton dissents from Mr. Drake, and thinks that Delgovitia was not at any of those three places, for several reasons which he thinks make against that supposition. But he rather thinks its situation near the town of Millington. For this place is best adapted by nature for the defence of the country: here also are found the remains of old military works. All these works inclose 4185 acres of ground; whence it is evident here must have been a large army. We see in several places where their tumuli or barrows were represented by little green hills.

Having shown the fortifications and out-works of the camp, he next endeavoured to prove the part within these on which Delgovitia stood. About half a mile north-east of Millington, on the south side of a gently sloping hill, were found several stone foundations of buildings of different sizes, and of different shapes; among which were found several fragments of Roman pavements, Roman tiles, flues, and two Roman coins. These are all proofs of the buildings having been Roman. There was likewise dug up a piece of a large stone pillar, of about 6 feet in length, but of no regular order; which notwithstanding might yet be Roman; for we cannot suppose those military people so well skilled in architecture as the artists at Rome.

If Delgovitia, as Cambden hints, be derived from the British word delgwe, which signifies statues or heathen gods, this place may lay claim to a title on that account, much sooner than either Weighton or Godmanham; for here was dug up a circular foundation resembling a temple in all appearance; being 45 feet diameter within, and the foundation was near 5 feet thick.

Near to this circular building, but south of it, were the foundations of two oblong square buildings, but with a strait entrance, not 2 feet wide, in which probably they put in the fuel and fire for their sacrifices; there being evident marks of burning on the stones, being almost burnt through; also, in digging in the middle of these two buildings, were found about half a yard thick of ashes, with some few small pieces of wood, fuel, and pieces of brute bones, chiefly burnt, and a great part of a horn of a large deer. East of these were laid open the foundations of another square building, where were found various pavements, coins, &c.

From what has been said, he thinks there is nothing wanting now to prove this to have been the Delgovitia, but to reconcile the distance as mentioned in the Itinerary. This he does pretty nearly, by an actual measurement.

He thinks it is evident that neither Weighton, Godmanham, nor Londesburgh, stand where Delgovitia was. He has in the first place shown the probability of this place near Millington being the station, from the known prudence of the Romans, because one set of men could defend the whole 4 passes; which could not have been done, had they been placed at Weighton, Godmanham, or Londesburgh. 2. He was shown that from the very situation and nature of the country, there required but little art to make their camp at that time almost impregnable; the valleys in general being from 60 to 90 yards deep, and their sides very steep. 3. That from this camp and Londesburgh they might see the whole country from the Humber on the south-east, up the vale of York on the west towards the north-west side; so that no army could surprise them that way. 4. That they could always have a sufficient quantity of provisions, and never want water, even in the hottest summers. And, 5. That there has been a Roman station here, as is evident from the Roman pavement, coins, tiles, and foundations of the ruins; and if the Romans had a station at Weighton, Godmanham, or Londesburgh, they would scarcely have had one so near the other.

All these things concur in proving this to be the site of Delgovitia; and there is or can be no argument brought against it; except that, by the Itinerary, the distance from Eboracum by Derventio, is set down at 20 M. P. and by our measure the distance from York to the circular foundation, in the camp, is only 17¼ miles, and 55 yards; so that there is above 2¼ measured miles difference. In answer to this, he says, may not the Itinerary be as wrong here as in some other places, which is very evident in several instances? and as it is wrong in some others, doubtless it may be so in this: besides, the Romans might calculate from the centre of York; and this mensuration only goes from the bar at Walmgate to the circular foundation in the Roman camp. But supposing the Itinerary to be exactly right, yet when the difference between the Roman Mil. Pass. and our miles is calculated, he thinks it will end all disputes on that score.

An Appendix to the foregoing Paper. By Mr. Fr. Drake, F. R. S. N° 483, p. 553.

Time, which subverts and destroys the greatest works of mankind, has an equal property of bringing things to light. The Delgovitia of the Romans in this country, so long sought after by Cambden, and other writers, is at length discovered so far, that there is no need of any more conjecture about it.

Being informed, in the year 1745, of some Roman curiosities found in a field near Millington, on the Wolds, Dr. Burton of York and myself set out to survey them. On our coming to the place, an intelligent countryman and his father conducted us to a large plain field, on the south side of Millington wood, where they showed several foundations of buildings under ground, on the very stones of which the apparent marks of fire may be traced. Two bases of pillars, of an irregular order, and a large piece of a column, were also discovered; several pieces of tesselated pavements, Roman bricks, tiles, &c. were dug up. The father said that, about 40 years before, he saw the foundations of a circular building, about 15 yards diameter, dug up in this place; which must have been the vestiges of some circus or temple; that it had been the custom for the inhabitants of their village, time out of mind, to dig for stones in this ground when they wanted; and which they must often do, in a country almost clear of such materials. The church of Millington itself seems to have been built out of the ruins of this ancient Roman station.

That this was really the Delgovitia so long sought after, is beyond contradiction. The distance from York coincides very justly with the Itinerary; 19 or 20 Italian miles agrees pretty well with our present computation; and at the same time points out the true military way from the Humber to York. Instead of forcing a road through the vale, the Romans very wisely chose to mount the hills as soon as possible; and therefore directed their stratum from York to the ford, over the river Derwent at Stainfordburg; and from thence in a direct line to Garrowby hill; which I take to be corrupted from Barrowby, many of those tumuli or barrows being near this place. On the top of this mountain, though the road turns up it by an easy ascent, begins a series of such enormous works for fortification, as the like is not to be met with in the whole island.

This road on the summit of the hill in a straight line points directly for Sureby or Burlington bay, the Sinus Salutaris of Ptolemy. But another road to the right takes a different course, and comes down to the ruins beforementioned. Thence the road leads directly to Londesburg, the place which I once thought the station sought for; it passes through Lord Burlington's park, where more of it was laid open last year than I had before seen, in widening the large and noble canal in that inclosure. This place was before a morass, and the Romans were obliged to force a way through it, which is 8 yards broad, and laid with stone edgeways to a great depth. The road passed up the hill on the other side this marshy place, and divided into two branches on the top of it; one way pointing through Weighton to Brough on the Humber, and the other by the east end of Godmondham directly for Beverley; which now I am convinced also was the Petvaria of Ptolemy. From which last station it must have gone out directly for Patrington or Spurnhead; one of which was certainly the Roman Prætorium, mentioned as the last stage in the first itinerary route of Antoninus.

This sea-port must be very commodious to touch at, either going or returning from Gaul, or the Belgic coasts, and bringing military stores, &c. from thence, either to York or Malton; to which last place the Camolodunum of Ptolemy, another road branches out, apparently from the conjunction on the top of Garrowby hill, and leads directly to it. But to return to our Delgovitia.

The situation of this place is admirable, and the stupendous works about it, thrown up for a defence to this station, and the several grand roads near it, are not to be described. The town itself was placed on a declivity of a hill, almost full south; and very near its ruins arise some rapid springs of excellent water; and so copious as, when joined in one stream, turns a mill; from which probably the name of Millington has proceeded. There was also lately discovered a well above a mile E. b. S. from these springs, dug through the solid rock, 26 yards deep, which must have been a Roman work.

To the south-west there are no ramparts thrown up; but to the east, north-east, and due north, the whole country is full of them. The vales are all guarded by small encampments at their angles; the vestiges of the barracks, now visible, are called by the country people the camps. These were to prevent any sudden surprise that way. On the hills, from vale to vale, some of which are from 60 to 90 yards deep, and prodigiously steep, are thrown up works, as ramparts, 12 yards broad, and proportionably high, which join in right angles with the vallies, and serve as a strong barrier everywhere.

Of a Plica Polonica. By Mr. Joseph Ames, F. R. S. and Secr. of the Soc. Antiquar. N° 483, p. 556.

June the 22d, 1746, in the morning, Mrs. Hannah Coomes, a neat old woman, whose matted hair, or plica polonica, as it is called, Mr. A. showed the Society, came and gave him the following information.

That she was born in the hay market, in the parish of Whitechapel, and baptised at Aldgate, in June, 1645. Her mother, having such sort of hair, used to comb her's much to prevent it, till sometimes the blood came; when she was about 14 years old, she perceived it to grow thick just about the back part of her head, and at length grew to this matted long substance he now saw it, of 109 inches long. She said she had had 4 husbands; the first Nicholas Woodcock, to whom she was married when about 28 years old, and had 4 children by him; all died young; but she observed nothing of their hair growing so.

Of some Clay Moulds, or Concaves of Ancient Roman Coins, found in Shropshire. By Mr. Henry Baker, F. R. S. N° 483, p. 557.

Four, of the five in his possession, were found in digging sand, at a place called Ryton near Condover, 5 miles from Wroxalter, i. e. Uriconium, in Shropshire, about a mile from the Watling-street Road: these are all of the size of a Roman Denarius, and little more than the thickness of our halfpenny. They are made of a smooth pot, or rather brick clay, that seems to have been well cleansed from sand or dirt, and well beaten or kneaded, to render it fit for taking a fair impression. Great numbers of these were found, but for want of care most of them were broken in pieces. The fifth, which is twice as thick as any of the rest, was found at Wroxalter; the clay it is made of differs but little from the former; and the impression on it is also of the size of a denarius, and of the same time with the rest.

Some years ago, the earl of Winchelsea had several impressions or moulds of this sort, all joined together side by side, on one flat piece of clay, as if for the making many casts at once; they were all of the emperor Severus: and he had seen, in the earl of Pembroke's most valuable collection, a clay mould impressed on both sides: among these also is one of the sides bearing the head of the same emperor, and the other side a known reverse of his. Four of the above 5 are also of Severus or his wife Julia, and the other is a reverse of Caracalla, his son and immediate successor; so that all that we know of may be said to be of the same time very nearly.

They are seemingly intended for the coinage of money; though it is very difficult to conceive in what manner they could be employed to that purpose; especially N° 2, which has an impression on both sides; unless we should suppose they coined two pieces at the same time, by the help of three moulds, of which this was to be the middle one.

If, by disposing these into some sort of iron frame or case (as our letter-founders do the brass moulds for casting their types) the melted metal could be poured into them, it would certainly be a very easy method of coining, as such moulds require little time or expence to make, and therefore might be supplied by new ones, as often as they happened to break.

These moulds seem to have been burnt or baked sufficiently to make them hard, but not so as to render them porous like bricks, by which they would have lost their smooth and even surface; which in these is plainly so close, that whatever metal should be formed in them would have no appearances like the sand-holes, by which counterfeit coins or medals are usually detected.

A Commodious Disposition of Equations for Exhibiting the Relations of Goniometrical Lines. By Wm. Jones, Esq. F.R.S.* N° 483, p. 560.

THEOREM.—In a circle whose radius is r, let there be two arcs, A the greater, a the less, each in the first quadrant; put s, t, f, and v, for the sine, tangent,

* Wm. Jones, an excellent mathematician, and the friend of Sir I. Newton, was born 1675, in the Isle of Anglesy, North Wales, where his father was a farmer; which profession not agreeing with our author's studious disposition, he came up to London, where he soon got into a merchant's counting-house; and so gained the esteem of his master, that he gave him the command of a ship for a West-India voyage. On his return he set up a mathematical school, and published his book on Navigation in 1702: and afterwards, on the death of the merchant, Mr. Jones married his widow. The son of lord Macclesfield being his pupil, and having rendered some essential service to the Macclesfield family, Mr. Jones was rewarded with the office of secretary to the chancellor, as well as that of a deputy teller of the exchequer. Mr. Jones died July 3, 1749, at 74 years of age, being one of the vice-presidents of the Royal Society. At the time of his death, he left his widow with child,

secant, and versed sine of an arc; s', t', $ſ'$, the sine, tangent, secant of the complement, and v', the versed sine of the supplement of that arc; let $z = \frac{1}{2}\overline{A+a}$, $x = \frac{1}{2}\overline{A-a}$; or if z and x be put for the arcs, it will be $A = z + x$, $a = z - x$. Then will the terms in any column of the following table, be proportional to their corresponding ones in any other column.

A Table of the Relations of Goniometrical Lines.

$2s,z$	$\frac{s,2z}{s,\overline{A+a}}$	$\frac{v,2z}{v,\overline{A+a}}$	$s,A+s,a$	$\frac{s',a-s',A}{v,A-v,a}$
$2s,x$	$s,A-s,a$	$\frac{s',a-s',A}{v,A-v,a}$	$\frac{s,2x}{s,\overline{A-a}}$	$\frac{v,2x}{v,\overline{A-a}}$
$2s',z$	$\frac{v',2z}{v',\overline{A+a}}$	$\frac{s,2z}{s,\overline{A+a}}$	$s',A+s',a$	$s,A-s,a$
$2s',x$	$s',A+s',a$	$s,A+s,a$	$\frac{v',2x}{v',\overline{A-a}}$	$\frac{s,2x}{s,\overline{A-a}}$
r	s',x	s,z	s',z	s,x
$ſ,z$	r	t,z		
t,z	s,z	$ſ,z-s',z$		
$ſ',z$	t',z	r		
$t,z+t',z$	$ſ',z$	$ſ,z$		
$ſ,x$			r	t,x
t,x			s,x	$ſ,x-s'x$

Hence almost an infinite number of theorems may easily be derived; some of which are the following, given here as examples of the use of the table.

I. $s,z \times s,x = \frac{1}{2}r \times \overline{s',a - s',A} = \frac{1}{2}r \times \overline{s',z-x} - \overline{s',z+x} = \frac{s,z}{ſ,x}rr = \frac{s,x}{ſ',z}rr.$

$s',z \times s',x = \frac{1}{2}r \times \overline{s',a + s',A} = \frac{1}{2}r \times \overline{s',z-x} + \overline{s',z+x} = \frac{s',z}{ſ,x}rr = \frac{s'x}{ſ,z}rr.$

of a son, who being born afterwards, was the late Sir Wm. Jones, chief judge in India, so justly celebrated for his eminent talents and learning.

In all the works of our author, a remarkable neatness, brevity, and accuracy, every where prevails. He seemed to delight in a very short and comprehensive mode of expression and arrangement; in so much, that sometimes what he had contrived to express in two or three pages, would occupy a little volume in the usual style of writing. It has been said that Mr. Jones possessed the best mathematical library in England, there being scarcely a book of that kind but what was there to be found. He had collected also a great quantity of manuscript papers and letters of former mathematicians, which have often proved useful to writers of their lives, &c.

Many other curious particulars relating to Mr. Jones, with a catalogue of his works, may be seen in Dr. Hutton's Dictionary of Mathematics, under the article Jones, and in the introduction prefixed to the same author's work on Logarithms, p. 121.

II. If A, B, C, be any three angles; $Z = A + B, X = A - B, H = \frac{1}{2}\overline{A + B + C}$.

Then $\frac{1}{2}r \times \overline{v,C - v,X} = s,\frac{1}{2}\overline{C + X} \times s,\frac{1}{2}\overline{C - X} = s,\frac{1}{2}\overline{A + C - B} \times s,\frac{1}{2}\overline{B + C - A} = s,\overline{H - B} \times s\overline{H - A}$.

And $\frac{1}{2}r \times \overline{v,Z - v,C} = s,\frac{1}{2}\overline{Z + C} \times s,\frac{1}{2}\overline{Z - C} = s,\frac{1}{2}\overline{A + B + C} \times s,\frac{1}{2}\overline{A + B - C} = s,H \times s,\overline{H - C}$.

III. $\frac{ss,z}{\grave{s}\grave{s},z} = \frac{tt,z}{rr} = \frac{rr}{\grave{t}\grave{t},z} = \frac{v,2z}{\grave{v},2z} = \frac{t,z}{\grave{t},z}$; or $\frac{ss,\frac{1}{2}z}{\grave{s}\grave{s},\frac{1}{2}z} = \frac{tt,\frac{1}{2}z}{rr} = \frac{rr}{\grave{t}\grave{t},\frac{1}{2}z} = \frac{v,z}{\grave{v},z} = \frac{t,\frac{1}{2}z}{\grave{t},\frac{1}{2}z}$.

IV. $\frac{1}{2}r = \frac{ss,z}{v,2z} = \frac{ss,\frac{1}{2}z}{v,z} = \frac{\grave{s}\grave{s},z}{\grave{v},2z} = \frac{\grave{s}\grave{s},\frac{1}{2}z}{\grave{v},z}$; and $s,z = \frac{2ss,\frac{1}{2}z}{t,\frac{1}{2}z} = \frac{2\grave{s}\grave{s},\frac{1}{2}z}{\grave{t},\frac{1}{2}z}$.

V. $\frac{s,z}{v,z} = \frac{r}{t,\frac{1}{2}z} = \frac{\grave{t},\frac{1}{2}z}{r} = \frac{\grave{v},z}{s,z}$.

VI. $\frac{t,z}{t,x} = \frac{s,A + s,a}{s,A - s,a} = \frac{\grave{t},x}{\grave{t},z}$; and $\frac{rr}{t,z \times t,x} = \frac{\grave{t},z}{t,x} = \frac{\grave{t},x}{t,z} = \frac{\grave{s},a + \grave{s},A}{\grave{s},a - \grave{s},A} = \frac{\grave{t},z \times \grave{t},x}{rr}$.

VII. $\frac{s,A}{s,a} = \frac{t,z + t,x}{t,z - t,x} = \frac{s,\overline{z + x}}{s,\overline{z - x}}$; if z and x are two arcs, then $A = z + x, a = z - x$.

VIII. $s,\overline{z \pm x} = \frac{s,z \times \grave{s},x \pm \grave{s},z \times s,x}{r} = \frac{t,z \pm t,x}{\int,z \times \int,x}$.

IX. $\grave{s},\overline{z \pm x} = \frac{\grave{s},z \times \grave{s},x \mp s,z \times s,z}{r} = \frac{rr \mp t,z \times t,x}{\int,z \times \int,x}r$.

X. $t,\overline{z \pm x} = \frac{t,z \pm t,x}{rr \pm t,z \times t,x}rr$; and $\grave{t},\overline{z \pm x} = \frac{rr \mp t,z \times t,x}{t,z \pm t,x}$.

XI. $\int,\overline{z \pm x} = \frac{\int,z \times \int,x}{rr \mp t,z \times t,x}r$; and $\grave{\int},\overline{z + x} = \frac{\int,z \times \int,x}{t,z \pm t,x}$.

XII. In three equidifferent arcs A, z, a, where z $(= \frac{1}{2}\overline{A + a})$ is the mean arc; and x $(= \frac{1}{2}\overline{A - a})$ their com. diff.; put $p = \frac{\grave{s},x}{r}$, $q = \frac{s,x}{r}$; $P = 2p \times s,z$, $Q = 2q \times s,z$.

Then $s,A = P - s,a = Q + s,a$; And $s,a = P - s,A = s,A - Q$.

XIII. Let $d = v,A - v,a = \grave{s},a - \grave{s},A$; then $ss,A - ss,a = \overline{2\grave{s},A + d} \times d = \overline{2\grave{s},a - d} \times d$.

XIV. Let A, B, C, &c. be the sines, a, b, c, &c. the co-sines, $\grave{a}$, $\grave{b}$, $\grave{c}$, &c. the tangents, of the arcs, α, β, γ, &c. whose number is n; the radius being r; put s for the product of the n co-sines, s', s'', s''', &c. for the sum of the products made of every sine, every two, three, &c. sines, by the other ($n-1$, $n-2$, $n-3$, &c.) co-sines, where the co-sine noted by $n-n$ is unity,

Then the sine of $\overline{\alpha + \beta + \gamma + \delta, \text{\&c.}} = \overline{s' - s''' + s^{v} - s^{vii}, \text{\&c.}} \times \frac{1}{r^{n-1}}$.

And the co-sine of $\overline{\alpha + \beta + \gamma + \delta, \text{\&c.}} = \overline{s - s'' + s^{iv} - s^{vi}\ \text{\&c.}} \times \frac{1}{r^{n-1}}$.

XV. Also putting T' for the sum of the tangents of the arcs, α, β, γ, &c. T'', T''', T^{iv}, &c. for the sum of the products of every two, three, four, &c. tangents;

Note.—When an arc is terminated in the second, third, or fourth quadrant, some of the signs (+ and −) of the terms in the preceding theorems, will, by the known rules, become contrary to what they now are.

and $A = T'$

$B = AT'' - T^{iv}$

$C = BT'' - AT^{iv} + T^{vi}$

$D = CT'' - BT^{iv} + AT^{vi} - T^{viii}$

$E = DT'' - CT^{iv} + BT^{vi} - AT^{viii} + T^{x}$. Put $R = \frac{1}{rr}$.

&c. &c.

Then the tangent of $\overline{\alpha + \beta + \gamma + \delta, \&c.} = A + BR + CR^2 + DR^3 + ER^4$; &c.

XVI. Hence, the sine, tangent, and secant, of any arc a, being represented by s, t, $\int$, the co-sine, co-tangent, and co-secant, by $s^{\backprime}$, $t^{\backprime}$, $\int^{\backprime}$; those of the arc na are expressed as in the following theorems.

Putting $n' = n.\frac{n-1}{2}$; $n'' = n'.\frac{n-2}{3}$; $n''' = n''.\frac{n-3}{4}$; $n^{iv} = n'''.\frac{n-4}{5}$; &c.

Sine of $na = \overline{nA - n''AP + n^{iv}BP - n^{vi}CP + n^{viii}DP, \&c.} \times \frac{s^{\backprime\, n-1}}{r^{n-1}}$;

where $P = \frac{ss}{s^{\backprime}s^{\backprime}}$; $A = s$; $B = AP$; $C = BP$; $D = CP$; &c.

Or $= \overline{ns - \frac{n-1}{2}.\frac{n-2}{3}AP + \frac{n-3}{4}.\frac{n-4}{5}BP - \frac{n-5}{6}.\frac{n-6}{7}CP, \&c.}\ \frac{s^{\backprime\, n-1}}{r^{n-1}}$;

where A, B, C, &c. stand for the respective preceding terms.

Or $= ns + \frac{1+n}{2}.\frac{1-n}{3}AQ + \frac{3+n}{4}.\frac{3-n}{5}BQ + \frac{5+n}{6}.\frac{5-n}{7}CQ + \frac{7+n}{8}.\frac{7-n}{9}DQ$, &c.

where $Q = \frac{ss}{rr}$; A, B, C, &c. stand as before.

XVII. Co-sine of $na = \overline{1 - n'P + n'''P^2 - n^{v}P^3 + n^{vii}P^4, \&c.} \times \frac{s^{\backprime\, n}}{r^{n-1}}$, where $P = \frac{ss}{s^{\backprime}s^{\backprime}}$.

Or $= r + \frac{0+n}{1}.\frac{0-n}{2}AQ + \frac{2+a}{3}.\frac{2-n}{4}BQ + \frac{4+n}{5}.\frac{4-n}{6}CQ + \frac{6+n}{7}.\frac{6-n}{8}DQ$, &c.

where $Q = \frac{ss}{r}$; and A, B, C, &c. stand for the respective preceding terms.

Or put $M = \overline{\frac{2s^{\backprime}}{r}}\Big|^{n} \times r$; $N = \frac{rr}{4s^{\backprime}s^{\backprime}}$; $A = \frac{1}{2}$; $B = AN$; $C = BN$; $D = CN$, &c.; $p = n$; $p^{\backprime} = n - 1$; $p^{\backprime\backprime} = n - 2$, &c.

And $a' = p$; $b' = p.\frac{1}{2}p'''$; $c' = p.\frac{1}{2}p^{iv}.\frac{1}{3}p^{v}$; $d' = p.\frac{1}{2}p^{v}.\frac{1}{3}p^{vi}.\frac{1}{4}p^{vii}$; $e' = p.\frac{1}{2}p^{vi}.\frac{1}{3}p^{vii}.\frac{1}{4}p^{viii}.\frac{1}{5}p^{ix}$; &c.

The co-sine of $na = \overline{A - Ba' + Cb' - Dc' + Ed, \&c.} \times M$.

XVIII. Let $A = -n'' + nn'$ | $A' = \frac{1}{n}.n'' - n'$

$B = +n^{iv} - nn''' + An'$ | $B' = \frac{1}{n}.\overline{n''A' - n^{iv}} + n'''$

$C = +n^{vi} + nn^{v} + Bn' - An'''$ | $C' = \frac{1}{n}.\overline{n''B' - n^{iv}A'} + \overline{n^{vi}} - n^{v}$

$D = +n^{viii} - nn^{vii} + Cn' - Bn''' + An^{v}$ | $D' = \frac{1}{n}.\overline{n''C' - n^{iv}B' - n^{vi}A' - n^{viii}} + n^{vii}$

&c. &c.

The tangent of $na = nt + At^3r^{-2} + Bt^5r^{-4} + Ct^7r^{-6} + Dt^9r^{-8}$ &c.

Or $= \overline{n + AN + BN^2 + CN^3 + DN^4 + \&c.} \times t$, where $N = \frac{tt}{rr}$.

Or $= na' + Ab' + Bc' + Cd' + De'$, &c. where $a' = t$; $b' = Na'$; $c' = Nb'$; $d' = Nc'$; &c.

Or $= \frac{n - n''N + n^{iv}N^2 - n^{vi}N^3 + n^{viii}N^4, \&c.}{1 - n'N + n'''N^2 - n^{v}N^3 + n^{vii}N^4, \&c.} \times t$.

Co-tang. of $na = \overline{r^2 + A't^2 + B't^2N + C't^2N^2 + D't^2N^3 + E't^2N^4, \&c.} \times \frac{rr}{nt}$; where $N = \frac{tt}{rr}$.

Or $= \overline{1 + A'N + B'N^2 + C'N^3 + D'N^4 + E'N^5, \&c.} \times \frac{1}{r} r^2 t$; where $N = \frac{rr}{t't'}$.

Or $= \frac{\overline{1 - n'N + n'''N^2 - n^{v}N^3 + n^{vii}N^4 - n^{ix}N^5, \&c.}}{\overline{n - n''N + n^{iv}N^2 - n^{vi}N^3 + n^{viii}N^4 - n^{x}N^5, \&c.}} \times \frac{rr}{tt}$; where $N = \frac{tt}{rr}$.

XIX. Let $A = n'$ $A' = \frac{1}{n}.n''$

$B = An' - n'''$ $B' = \frac{1}{n}.\overline{n''A' - n^{iv}}$

$C = Bn' - An''' + n^{v}$ $C' = \frac{1}{n}.\overline{n''B' - n^{iv}A + n^{vi}}$

$D = Cn' - Bn''' + An^{v} - n^{vii}$ $D' = \frac{1}{n}.\overline{n''C' - n^{iv}B' + n^{vi}A' - n^{viii}}$

&c.   &c.

Secant of $na = \overline{1 + AN + BN^2 + CN^3 + DN^4 + EN^5, \&c.} \times M$.

Or $= \frac{1}{\overline{1 - n'N + n'''N^2 - n^{v}N^3 + n^{vii}N^4, \&c.}} \times M$; where $N = \frac{tt}{rr}$, $M = \frac{rf^n}{r^n}$.

Co-secant of $na = \overline{1 + A'N + B'N^2 + C'N^3 + D'N^4 + E'N^5, \&c.} \times M$; where $N = \frac{tt}{rr}$, $M = \frac{rrf^n}{ntr^n}$.

Or $= \frac{1}{\overline{n - n''N + n^{iv}N^2 - n^{vi}N^3 + n^{viii}N, \&c.}} \times M$; where $N = \frac{rr}{tt}$, $M = \frac{r^{n-2}}{tf^{n-2}}$.

XX. Let c be the chord of an arc (a) of the circumference of a circle, whose diameter is d. Put $N = \frac{cc}{dd}$.

The chord of $na = nc + \frac{1+n}{2}.\frac{1-n}{3}AN + \frac{3+n}{4}.\frac{3-n}{5}BN + \frac{5+n}{6}.\frac{5-n}{7}CN + \frac{7+n}{8}.\frac{7-n}{9}DN$, &c. where A, B, C, &c. stand for the respective preceding terms.

As the preceding theorems are easily deduced from the first, so the following are most readily seen to be the immediate consequences of these; and all depending on no other principles than what are generally made use of in common computations.

XXI. Putting s, s', t, t', f, f', for the sine, co-sine, tangent, co-tangent, secant, co-secant, of an arc (a), and v its versed sine; let $q' = \frac{1}{1}$; $q'' = \frac{1}{2}q'$; $q''' = \frac{1}{3}q''$; $q^{iv} = \frac{1}{4}q'''$; $q^{v} = \frac{1}{5}q^{iv}$; &c. $N = \frac{aa}{rr}$.

Then $s = \overline{1 - q'''N + q^{v}N^2 - q^{vii}N^3 + q^{ix}N^4 - q^{xi}N^5, \&c.} \times a$.

$= a - q'''a^3r^{-2} + q^{v}a^5r^{-4} - q^{vii}a^7r^{-6} + q^{ix}a^9r^{-8}$, &c.

$= a - \frac{1}{2.3}AN + \frac{1}{4.5}BN - \frac{1}{6.7}CN + \frac{1}{8.9}DN$, &c. where A, B, C, &c. stand for the respective preceding terms.

And $s' = r - q''a^2r^{-1} + q^{iv}a^4r^{-3} - q^{vi}a^6r^{-5} + q^{viii}a^8r^{-7}$, &c.

$= \overline{1 - q''N + q^{iv}N^2 - q^{vi}N^3 + q^{viii}N^4 - q^{x}N^5, \&c.} \times r$

$= r - \frac{1}{1.2}a^2r^{-1} + \frac{1}{3.4}AN - \frac{1}{5.6}BN + \frac{1}{7.8}CN$, &c. A, B, C, &c. as before.

XXII. Also $v = q''a^2r^{-1} - q^{iv}a^4r^{-3} + q^{vi}a^6r^{-5} - q^{viii}a^8r^{-7}$, &c.

$= \frac{1}{1.2}a^2r^{-1} - \frac{1}{3.4}AN - \frac{1}{5.6}BN - \frac{1}{7.8}CN - \frac{1}{9.10}DN$, &c.

$= \overline{\frac{1}{1.2}N - \frac{1}{3.4}AN - \frac{1}{5.6}BN - \frac{1}{7.8}CN, \&c.} \times r$. A, B, C, &c. as before.

XXIII. Let $A = +q' - q''$ And $A' = -A$

$B = -q''' + q^{v} + Aq'$ $B' = -B - AA'$

$C = +q^{v} - q^{v'} + Bq' - Aq'''$ $C' = -C - BA' - AB'$

$D = -q^{vii} + q^{vii'} + Cq' - Bq''' + Aq^{v}$. $D' = -D - CA' - BB' - AC'$.

&c. &c.

Tangent $t = a + Aa^3r^{-2} + Ba^5r^{-4} + Ca^7r^{-6} + Da^9r^{-8}$, &c.

Or $= \overline{1 + AN + BN^2 + CN^3 + DN^4 + EN^5, \text{\&c.}} \times a$.

Co-tangent $t' = a^{-1}r^2 + Aa + B'a^3r^{-2} + C'a^5r^{-4} + D'a^7r^{-6}$, &c.

Or $= \overline{rr + Aa^2 + B'Na^2 + C'N^2a^2 + D'N^3a^2, \text{\&c.}} \times \frac{1}{a}$.

XXIV. Also let $\alpha = +q'$ And $\alpha' = +q''$

$\beta = -q''' + \alpha q'$ $\beta' = -q^{v} + \alpha' q''$

$\gamma = +q^{v} - \alpha q''' + \beta q'$ $\gamma' = +q^{v'} - \alpha' q^{v} + \beta' q''$

$\delta = -q^{vii} + \alpha q^{v} - \beta q''' + \gamma q'$ $\delta' = -q^{vi'} + \alpha' q^{v'} - \beta' q^{v} + \gamma' q''$.

&c. &c.

Secant $s = r + \alpha a^2r^{-1} + \beta a^4r^{-3} + \gamma a^6r^{-5} + \delta a^8r^{-7}$, &c.

Or $= \overline{1 + \alpha N + \beta N^2 + \gamma N^3 + \delta N^4, \text{\&c.}} \times r$.

Co-secant $s' = a^{-1}r^2 + \alpha' a + \beta' a^3r^{-2} + \gamma' a^5r^{-4} + \delta' a^7r^{-6}$, &c.

Or $= \overline{rr + \alpha' aa + \beta' Naa + \gamma' N^2aa + \delta' N^3aa, \text{\&c}} \times \frac{1}{a}$. where $N = \frac{aa}{rr}$.

XXV. Putting $p' = \frac{1}{2}$; $p'' = \frac{3}{4}p'$; $p''' = \frac{5}{6}p''$; $p'^{v} = \frac{7}{8}p'''$; $p^{v} = \frac{9}{10}p'^{v}$; &c. $N = \frac{ss}{rr}$.

Then arc $a = \overline{1 + \frac{1}{3}p'N + \frac{1}{5}p''N^2 + \frac{1}{7}p'''N^3 + \frac{1}{9}p'^{v}N^4, \text{\&c.}} \times s$.

Or $= s + \frac{1}{3}p'AN + \frac{1}{5}p''BN + \frac{1}{7}p'''CN + \frac{1}{9}p'^{v}DN$, &c.

Or $= s + \frac{1\cdot1}{2\cdot3}AN + \frac{3\cdot3}{4\cdot5}BN + \frac{5\cdot5}{6\cdot7}CN + \frac{7\cdot7}{8\cdot9}DN$, &c. where A, B, C, &c. stand for the respective preceding terms.

XXVI. If v be the versed sine of an arc a, diameter being d, $M = \frac{v}{d}$, $R = \sqrt{dv}$.

Then arc $a = \overline{1 + \frac{1\cdot1}{2\cdot3}M + \frac{3\cdot3}{4\cdot5}AM + \frac{5\cdot5}{6\cdot7}BM + \frac{7\cdot7}{8\cdot9}CM, \text{\&c.}} \times R$; A, B, C, &c. are as before.

XXVII. And putting $N = \frac{tt}{rr}$, $A = t$, $B = AN$, $C = BN$, $D = CN$, &c.

Then arc $a = t - \frac{1}{3}AN + \frac{1}{5}BN - \frac{1}{7}CN + \frac{1}{9}DN - \frac{1}{11}EN$, &c.

Or $= \overline{1 - \frac{1}{3}N + \frac{1}{5}N^2 - \frac{1}{7}N^3 + \frac{1}{9}N^4 - \frac{1}{11}N^5, \text{\&c.}} \times t$.

XXVIII. Also, if c be the chord of an arc (a), and $N = \frac{cc}{dd}$.

Then arc $a = c + \frac{1\cdot1}{2\cdot3}AN + \frac{3\cdot3}{4\cdot5}BN + \frac{5\cdot5}{6\cdot7}CN + \frac{7\cdot7}{8\cdot9}DN$, &c. where A, B, C, &c. stand for the respective preceding terms.

Extract of a Letter, dated at Rome, Aug. 5, 1747, from Mr. Hoare, a young Statuary, pursuing his Studies there, to his Brother Mr. Hoare, an eminent

Painter at Bath, giving a short Account of some Antique Pictures found in the Ruins of Herculaneum at Portici, near Naples. N° 484, p. 567.

This city was overthrown and swallowed up by an earthquake near 1700 years since. Some of the most remarkable curiosities we saw, were,

1. A picture of about 5 feet long, and 4 feet wide, representing the education of Achilles, by his master Chiron the Centaur. The figures are about half as large as the life. That of Achilles is standing in a noble action, and is seen in front, as the principal object of the picture. He seems to hearken with great attention to, and is looking steadily on the centaur, who is seen almost in a side view. The figures are both finely coloured, and well drawn; but that of the young man most exquisitely so.

2. Next to this is a picture of about 3½ feet high, and narrow, in which is a woman sacrificing. The figure is about 2 feet high. This picture seems to have been taken out of some compartment of ornaments.

3. Next to this is a broken piece, representing the judgment of Paris. The figures are about the same size as that last mentioned. They are not entire; the bottom part being broken off about the knees. This is also a very fine picture; but it is impossible to judge of all its beauties, as it is extremely changed and decayed; which is quite contrary to all the others, in particular to that of Chiron and Achilles; which is in a manner as fresh as if it had been but just painted.

4. The next is a fine picture of the story of Virginia. The figures are rather larger than those abovementioned. The characters and expressions of the heads are admirable. That of Appius gives a just idea of the furious transports in which the artist designed to describe him. Virginia is weeping. In short, all the figures are finely disposed, and the characters well adapted to the subject.

5. Two large pictures were in a nich in a basilica, about 5 or 6 feet high. The first represents Theseus victorious over the Minotaur. He is standing in a free and fine posture; one foot is on the head of the Minotaur. But what seemed odd, was the figure of that monster itself, which I had always seen differently represented; for in this picture the head only represents that of a bull, which is joined to the body of a man. Several little Genii, or Cupids, as we call them, all seem impatient to show their respect to their deliverer; one kisses his hand, another clasps round his leg, and several others are in different attitudes of gratitude. The figures are almost as large as small life.

6. The other picture represents Hercules and the goddess of nature. The figure of Hercules is standing, seen in a side view, reposing on his club; something like the statue in the Farnese palace at Rome. There is a Victory crowning the hero, and the goddess is sitting before him, and seems to applaud and thank him for his labours. There are numbers of symbolical figures besides in this picture. Behind the goddess is a Satyr, and at Hercules's feet a boy sucking a

doe. It is remarkable how delicately the doe seemed to dispose of her legs, not to hurt the child; while at the same time she is licking his knees, as a mark of her tenderness for him. This picture is equal to the first mentioned; being exquisitely drawn and coloured, and well composed.

7. There is a little picture, which seems extremely odd for its composition. It is about 1¼ foot long, and 8 or 9 inches high: it is a parrot drawing a chariot something like our modern chaises. In the chariot sits a sort of large horse-fly, whose two horns serve for the bridle and reins to guide the parrot.

8. Two pictures of about 4½ feet long, represent the stage of a theatre, with comedians playing their parts on it. The perspective in these pictures is very well observed.

9. A wedding, consisting of 3 figures only. They are much in the same taste of those of Aldobrandini's marriage at Rome. There are besides numbers of little frizes representing sacrifices, and other ceremonies, of the ancient Pagans; most of them on black or red grounds.

These pictures show, that the ancients understood perspective and landscape, I mean, the keeping particularly, which I have heard strongly disputed; but no one that has seen these pictures will, I believe, make any doubt of it.

It would be impossible to give you an exact description of all the pictures; as there are so many entire, besides the bits and fragments of others. Of some, the heads only remain; and of others, pieces of figures; numbers of small landscapes; views of architecture, flowers, and fruit, painted extremely light and elegantly. There are even some grotesque pictures, something in the taste of India painting. Most of the small ones have been taken out of compartments; the guardian showed me several places whence they had been taken. They still preserve a beauty superior to any thing we see now-a-days; the colouring, drawing, and liberty of pencil, may vie with the works even of Raphael himself.

There are two rooms full of them. And they are continually finding more pictures every day; and had I a month to spare, I would willingly go on foot to Naples, to have the pleasure of studying those I have already seen, and seeing those which have been discovered since.*

N. B. Cardinal Albani, at Rome, has an antique group of Theseus and the Minotaur; where the Minotaur has the head only of a bull, as in the picture abovementioned.

A Letter from Mr. G. Stovin to his Son, concerning the Body of a Woman, and an Antique Shoe, found in a Morass in the Isle of Axholm in Lincolnshire. By Mr. G. Stovin. N° 484, p. 571.

The beginning of June last (1747) a labouring man, of Amcotts in the isle

* See more of these curiosities in these Trans. N° 456 and 458.—Orig.

of Axholm, in the county of Lincoln, was digging turf or peat in the moors of Amcotts; and, about 6 feet below the surface, his spade cut the toe of a sandal, which dropped into the pit he was graveing peat in; also part of the foot dropped in, which terrified the man, and he left it. Hearing of this discovery, Mr. S. went with some servants to make further discovery; when they soon found the other sandal, whole and firm. It was very soft and pliable, and of a tawny colour, with all the bones of that foot in it, and all the gristly part of the heel. Proceeding further, they found the skin and thigh bones, which he measured, and found to be 18 inches long. They then found all the skin of the lower parts of the body, which was of the same colour as the sandals, and very soft, with fresh hair on it, &c. which distinguished it to be a woman. The skin drew or stretched like a piece of doe leather, and was as strong. They then found the skin of the arms, which was like the top of a muff or glove, when the bones were shaken out. They then found a hand, with the nails as fresh as any person's living; this hand is the lady's natural skin so tanned, with the nails.

These sandals must be very ancient, and have most certainly been made of a raw hide, as they and the skin of the lady were both of one colour, and both had one tanner; which probably is the moor water; which is exactly of the colour of coffee; and made so by such great quantities of oak and fir-wood, that are frequently dug out of these moors; several oak-trees affording 1000 pales for fencing, 5½ feet long, and 6 to 8 inches broad, which oak-wood is as black as jet. The fir-wood retains its turpentine smell, and in hot weather, exposed to the sun, the turpentine will drop from it. This wood is frequently split into laths for the roofs of houses or floors; and it is remarkable that no worm will touch them. They frequently find also hazle-nuts and fir-apples in abundance; which is a plain proof that the trees fell in autumn, when the fruits were at maturity. This lady was probably overwhelmed by some strong eddy of water; for she lay on one side bended, with her head and feet almost together.

It appears by the maps of the country, that this has been the rendezvous of all the waters from the south, west, and north parts of the kingdom; as for instance, the river Don, from Doncaster, Rotherham, and Sheffield, which took in many more streams: as the Idle, Trent, Torn, Dare, Rother, &c. &c. Then the river Trent, which runs south to Gainsbrough; then to Torksey, Newark, Nottingham, Derby, Burton on Trent, Strafford, Trentham in Staffordshire; and takes in a vast number of rivulets: then the Ouse, which comes from near Richmond, and takes in the Ure, Wharf, Bishop's Dike, Aire, Calder, and a great number of rivulets; which are all lost in that famous estuary the Humber.

It is also to be observed, that here is one morass 20 miles round, part in Hatfield-chace, another 10 miles round in the same chace, where the famous William of Lindham had his cell. In the middle of it, where his body was found, for 8 miles round, is all a morass.

These sandals are not like the Scotch or Irish brogues; though the Scots formerly inhabited but a little way off, viz. north of Humber. Perhaps the Danes may wear such, or the ancient Saxons; for both these people must be well acquainted with these parts, as the Danes under Edgar-Atheling encamped a whole winter in this neighbourhood, and had a station at Gigansburgh, now Gainsbrough, on this river Trent. The sandal is of one piece of leather, with a seam at the heel, and a thong of the same leather. See fig. 4, 5, 6, pl. 8.

At Roxby was a famous Roman pavement, 15 yards square, the Roman road, &c. also a square platform at Aldbrough, which seems to be Roman, though no discoveries have as yet been made there; but at Roxby large quantities of Roman coins have been found.

P. S. As to this water on these moors preserving human bodies,* it is most certain, viz. Part of a body taken up at Geel by Mr. Empson 50 or 60 years ago, and one in the great moor near Thorn, about 7 years ago, with the skin like tanned leather, the hair, teeth, and nails quite fresh.†

* See these Trans. N° 434.—Orig.

† Mr. Catesby, F. R. S. author of the History of Carolina, &c. being present, at reading the above account, when the sandal was exhibited, said, this shoe or sandal was exactly like what the Indians in Virginia wear at this day, and call Mokasin.

That ingenious artist and skilful antiquary Mr. Geo. Vertue communicated to Dr. C. M. D. his sentiments concerning this sandal in the following words:

" When the above letter was read at the Society of Antiquaries, there was produced a hand of the woman there mentioned, and a sandal or shoe taken from one of her feet; it being made of leather, tanned ox hide; but remarkable for being cut out of one flat piece, see fig. 6, so as to fold about the foot and heel; the form and make being so contrived without under heel-piece, as to be flat to tread on; the shape, that of a woman's foot, and the toe round pointed. This being of an ancient form, the society ordered an exact draught to be taken of both that and the hand; which drawings are preserved among others belonging to that society. It may be observed concerning the antiquity and use of leather shoes in England, that this shoe or sandal appears by its form to be ancient. I conceive it was before Edward the 4th's time, when by custom piked shoes had so increased in length, that all such as wore them in excessive length were to be mulcted, or have them cut shorter, in passing in or out of the city-gates of London. This very likely had passed among the better sort of people about the kingdom; for Chaucer in his time mentions the use of long piked shoes, so long as to be tied up by strings or small chains to their knees. Thus it might have been with men's shoes, but not in so long a degree for women's use; though observing ancient pictures of men and women in books of illuminations, piked shoes appear in several reigns from Edw. 3d to Rich. 3d in England."

" Also on our ancient monuments of stone or alabaster cumbent statues have mostly piked shoes. But some of earlier date than Edw. 3d have broad turn-up shoes at the toes, of the same like form and make as this woman's. The men's broad toes, and the women's narrow.

" Therefore I conclude this very sandal could not well be earlier than Edw. 1st or Hen. 3d; also that the cutting the form, and sowing to form the heel cleverly, by a stitching behind the heel with

Concerning the Grubs Destroying the Grass in Norfolk. By Mr. Henry Baker, F.R.S.* N° 484, p. 576.

Having seen some letters lately sent from Norfolk and Suffolk, giving an account, that prodigious numbers of what one letter calls grubs, and another large maggots, full as thick and almost as long as a man's little finger, are dispersed over the fields, and do great mischief in those counties, Mr. B. immediately imagined that they must be the aureliæ or chrysalides of some species of beetle: and desiring to get what further information he could concerning them, he wrote with that intent to Mr. Arderon at Norwich, F. R. S.

Of these grubs Mr. Arderon gives the following information. They are, says he, a species of insects but too common about Norwich, and have been more or less numerous in that county for these 20 years past. They are the erucæ of the scarabæus arboreus vulgaris major of Mr. Ray, that is the tree beetle, or blind beetle, vulgarly in Norfolk called the dor.

In different parts of England they are called the brown tree beetle, the blind beetle, the chafer, the cock-chafer, the jack horner, the jeffry-cock, the May-bug, and the dor. By the Dutch they are named, baum-kaefer, roub-kaefer, koren worm, or corn worm, because they destroy the roots of corn; and in Zealand, molenaers or millers, as Goedartius says, chap. 78, because they bite the leaves of several sorts of trees into particles as small as if they were ground. In England he has likewise heard them called millers; but supposed it to be from a white mealy powder with which their wings are covered. The French call them hanetons.

I have seen, says Mr. Arderon, whole closes of fine flourishing grass in summer time become withered, dry, and as brittle as hay in a few weeks, by this vermin's eating off the roots; in doing which they are so dextrous, that many yards of this withered grass might be rolled up in one piece, all the fibres that fastened it to the ground being gnawed away.

Closes of turnips often undergo the same fate from these devouring insects, which act as if designing to do as much mischief as possible; for when one of them fixes on a turnip, he eats only the middle small root, which soon causes it to wither and die, and then moves on to the next. In like manner they destroy the roots of wheat, rye, &c. and almost every other useful vegetable in their

a small leather thong, may have been in use before that of waxed thread used by shoe-makers, formerly called cordwainers."

Fig. 4, shows the shoe sideways, laced as when on the foot. Fig. 5, the same seen from above. Fig. 6, the same unlaced, and laid flat, to show the manner of its being cut out of the raw hide. —Orig.

* The insects whose ravages are here described are the larvæ of the common cockchafer or *scarabæus melolontha.* Linn.

way. And what makes this pest the more deplorable, is the long time of their continuance in their eruca, or most mischievous state, which, according to Goedartius, is 4 years at least, but Mouffet writes, that in Normandy they are observed to be most numerous every 3d year, which is therefore called l'an des hannetons. And it is not improbable, that in the open fields where they are well fed, they may come to their perfect state a year sooner than those did which Goedartius almost starved in glass jars.

Mr. Arderon says, he has frequently been told by people of credit and observation, that neither the severest frosts of our climate, nor the being immerged in water, will destroy these erucæ; some having been exposed for many days to the keenest frosts, and others covered with water for as long a time, which were found to revive, and become as vigorous as ever.

Crows and hogs devour these erucæ greedily; but their numbers are too great to be thus much diminished. The most effectual way, though very laborious, is to beat them off the trees in the day time with long poles, and then sweep them together and burn them. On a farm at Heathal, near 5 miles s. w. from Norwich, of 80l. per Annum, belonging to St. Helen's hospital in this city, in the occupation of Mr. James Ebdin, these insects were so numerous last year, that the farmer and his servants affirmed they gathered 80 bushels of them, the erucæ of which had so spoiled the produce of his farm, that the court of this city, in compassion of the poor man's misfortune, allowed him 25l.

In the day time few of the beetles fly about, but conceal themselves under the leaves of oaks, sycamores, limes, &c. where they seem asleep till near sunset, when they take wing and fly about the hedges, as thick as swarms of bees; at which time they frequently dash themselves against people's faces with great violence, and by so doing occasioned the common proverb, as blind as a beetle.

Mouffet tells us, it is recorded, that on Feb. 24, 1574, there fell such a multitude of these insects into the river Severn, that they stopped and clogged the wheels of the water-mills; as to which Mr. A. takes notice, that their coming so early in the year was no less extraordinary than their multitudes; for the larger species seldom appear till the month of May; and a smaller sort, which come out in July and August, are seldom seen after the evenings grow cold.

We are told in the Transactions of the Dublin Society, that the country people in one part of that kingdom suffered so greatly by the devastation made by these insects, that they set fire to a wood some miles in length, which parted two adjacent counties, to prevent their dispersing themselves any farther that way.

Mr. Arderon further says that some ingenious persons account for the great increase of these insects from the decrease of rooks in this country, which they say greedily devour them in their grub state; and the decrease of rooks is owing to most of the ancient rookeries having been destroyed by the large fall of timber trees made of late years, which has obliged the rooks to remove into other parts of the kingdom.

A Letter from J. Wall, M.D. to Edward Wilmot, M.D., F.R.S. concerning the Use of the Peruvian Bark in the Small Pox.* N° 484, p. 583.

In this communication Dr. W. gives an account of several cases of small-pox accompanied with petechiæ, hæmorrhages, and other malignant symptoms, for which he prescribed the extract of the Peruvian bark, either in conjunction with the vitriolic acid or with alum. He preferred the extract to the gross substance, as being of equal efficacy, and less apt to load the patient's stomach. See the detail of these cases in this author's works, collected and published after his death by his son Dr. Wall of Oxford.

* The following particulars concerning Dr. John Wall are, for the most part, extracted from the 2d vol. of Dr. Nash's History of Worcestershire. The additional information is due to the politeness of his son, the present Dr. Martin Wall, of Oxford, to whom the medical world is indebted for the republication of his father's separate tracts in a collective form, with some valuable notes and additions, and particularly for those additions which relate to the chemical examination of the Malvern waters.

Dr. John Wall was a native of Worcestershire, and was born in 1708. After passing some time at another school, he was sent to the college school in Worcester, from whence he was elected scholar of Worcester College, Oxford, in 1726. Nine years after he was elected fellow of Merton College; soon after which he took the degree of M. D. and removed to Worcester to engage in the exercise of his profession. In 1740 he married a daughter of Martin Sandys, Esq. uncle to the first Lord Sandys. He died at Bath, whither he had gone for medicinal relief, in 1776, and was buried there in the abbey church.

Dr. J. W. was endowed with an unusual share of intellect, which he improved by early and close application to science, and more particularly by the cultivation of those branches of natural philosophy which have an immediate connection with the arts and with medicine. If he was much to be admired for his acquirements as a scholar, for his extensive knowledge in the various kinds of scientific pursuits, and for his superior skill and judgment as a physician, he was in an equal degree entitled to esteem for his great liberality of sentiment, for a disposition truly philanthropic, for manners the most engaging. Intent on measures of public utility, he was one of the earliest and warmest promoters of the Worcester Infirmary; and to his exertions, joined to those of some other eminent chemists, the city of Worcester owes the establishment of its porcelain manufacture. "Dr. W.'s amusement was the study of the polite arts, and particularly landscape and historic painting, of which he has left many very valuable specimens,* now in the possession of the different branches of his family. His eminence in this art was so great, that it has been said of him, if he had not been one of the best physicians, he would have been the best painter of his age."

Besides the treatise on the Malvern waters and the papers communicated to the Royal Society, Dr. W. published various observations in the Med. Trans. of the London College of Physicians, in the Gentleman's Magazine, &c. These scattered papers were reprinted collectively by his son, Dr. Martin Wall, in one vol. 8vo. 1780. The contents of this vol. are of great practical utility, (digna utique, quæ seræ posteritati traderentur. vide Annales Liter. Helmstadt, 1784,) and exhibit incontestable proofs of the author's diligence and skill in his profession.

* Such as Brutus and his sons; Regulus in the senate; Edward and Eleanor: Seneca in the bath; Gray's bard; Macbeth's witches, &c.

Of a Person who had no Ear for Music Naturally Singing several Tunes when in a Delirium. By the Rev. Dr. Doddridge at Northampton. N° 484, p. 596.

A clergyman's lady, whose husband is of some eminence in the learned world, in a frenzy after a lying-in, which was quickly removed, found during the time of it such an alteration in the state and tone of her nerves, that though she never had before or since any ear for music, nor any voice, she was then capable of singing, to the admiration of all about her, several fine tunes, which her sister had learned in her presence some time before; but of which she had not then seemed to take any particular notice.

A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries for the Year 1745, *pursuant to the Direction of Sir Hans Sloane, Bart. P. R. S. &c. By Joseph Miller, Apothecary.* N° 484, p. 597.

This is the 24th presentation of this kind, completing to the number of 1200 different plants.

Continuation of an Account of an Essay towards a Natural History of Carolina and the Bahama Islands; by Mark Catesby, F. R. S. with some Extracts out of the Tenth set. By Cromwell Mortimer, Secr. R. S. N° 484, p. 599.

This 10th set begins with plate 100, of the 2d vol. In this part of the work, the author, besides plants, has given us several insects, particularly some remarkable butterflies. He begins this set with the mahogany tree, the wood of which is of late years become so well known here in England, for all sorts of cabinet work, surpassing the red cedar in beauty, without having the disagreeable scent of that wood.

On the Death of the Rev. Dr. Greene, late Rector of St. George the Martyr in Queen's Square, London, and one of the Prebendaries of Worcester, where he died of a Hurt received, as he was riding out in the Neighbourhood of that City. By Tho. Cameron, M. D. N° 484, p. 609.

Oct. 20, about noon, Dr. Greene's horse started under him, at the waving of a ploughman's whip, and with a quick and violent jerk, turned quite short, first to the left, and then instantly, and with the same impetuosity, to the right. After galloping a few paces, the Doctor fell gently off into a hedge, without receiving any hurt from the fall. A chariot was borrowed, into which he was lifted and brought home; for after this he could never stand.

At 4 that evening Dr. C. first saw him, just after he had been blooded. He was very faint, cold all over, and his pulse scarcely perceptible, though naturally very strong; the scrotum so much swelled, that the penis was quite absorbed and lost in it, and its colour a very deep red. He ordered him a glass of wine with a bit of bread, for he had ate nothing all that day. This revived him, and raised his pulse a little.

He then told Dr. C. in answer to the questions he put, that the testicles were not hurt; that the twisting of the horse gave him at that instant the intolerable sense of being split asunder. Dr. C. said that a violent and sudden stroke, from the pummel of the saddle, on the os pubis, might probably give him that sensation: he replied, that it did not feel like a stroke, and still persisted in his first expression of being split asunder.

A warm fomentation was ordered to be constantly applied, and 1½ oz. of Glauber's salt, quickened with 2 grs. of emetic tartar, to be given in a quart of gruel. At 11 that night an emollient glyster was given, the salts having as yet done nothing. Before morning he had 6 large loose stools; but it gave him exquisite pain to be lifted on the bed-pan.

Next morning, Wednesday, the swelling was increased, and the colour deeper. Dr. C. prescribed an electuary of bark and salt of amber, to prevent if possible the approaching mortification. The stale beer poultice was applied; and that evening, a fever coming on, 10 oz. of blood were taken from his arm.

Next morning, Thursday, the salts were repeated without the emetic tartar, and he had 4 stools. All this while he had made no water, except about a spoonful just after he was put into the chariot. The lower part of the belly, where a distended bladder would certainly point, was not swelled, though the parts on the os pubis were very much so. The scrotum increased in bulk and bad colour every hour; and the inside of the right thigh grew very tumid, with great pain, and a very perceptible fluctuation in it. These observations convinced Dr. C. that the urine had found a way into the parts last mentioned; though indeed he could not account for it, but by supposing that the urethra had been bruised, even to laceration, between the pummel of the saddle and the os pubis. The surgeon, Mr. Russel, soon came into this opinion about the urine, but imagined the bladder must be burst. This Dr. C. could not comprehend, nor could he explain; for the bladder lies out of the reach of all external injury from the causes hitherto assigned in this case. They agreed however about 3 in the afternoon, Thursday, to make a puncture into the scrotum; from whence urine, manifest to the smell, issued pretty freely all night.

Next morning, Friday, a larger opening was made in the right thigh with the same effect. The parts subsided considerably; but the pulse rising, decoctum nitrosum was given with the bark. This evening the hiccup came on, and the scrotum looked livid.

Next day, Saturday, the common emulsion, with a little nitre, and the extract of bark with musk were ordered. But the hiccup increased, watry blisters appeared on the scrotum, the voice faultered, the head failed, and the pulse sunk. He became worse and worse, till he quietly expired on Sunday morning at 11 o'clock.

On dissection, they found the scrotum and corpora cavernosa penis mortified; the ossa pubis wrenched asunder to the distance of 4 inches, and a rent in the bladder, half an inch in length, a little above the neck, and exactly in the middle where the ossa pubis join. This was a very astonishing sight, and gave quite a new idea of the case; which if any physician could have discovered without inspection, he must have had more penetration than Dr. C. pretends to. We may now, however, reason about it, with a little more certainty than before; and it seemed that the body of the horse in twisting, acted with the power of a lever, to which the suddenness of the jerk, added in some measure the force of percussion. But all this leaves us still in wonder at the effect; for Dr. Greene was a very strong large boned man, 64 years of age, and the uniting surface of the ossa pubis was considerably broader in him, than either the surgeon, or Dr. C. had ever seen in any subject.

On the Difference of the Degrees of Cold marked by a Thermometer kept Within Doors, or Without in the Open Air. By the Rev. Henry Miles, D. D. N° 484, p. 613.

By an extract from Dr. M.'s register of the weather, is shown the state of his barometer and thermometers, for some days of the same week, in which is observed a sudden change of the temperature of the air, particularly on Thursday morning the 3d; by which may be seen the little use a thermometer is of, when kept within doors, to determine the state of the air abroad, as to heat or cold.

Dr. M. had 2 thermometers filled with mercury, and of the same construction, made by the late Mr. Sisson. The one was placed without his chamber window, in a north-east position, under cover, contrived to admit a free passage of the air, but to keep off sun and rain; the other within the window, about 3 feet from the former, where the sun never falls on it; the room is constantly occupied as a bed-chamber, but had no fire in it that season.

It appears that on Tuesday the 1st, at 8 in the morning, the thermometer without stood at 17 degrees above 0, or freezing point; that within at 14. At 9 at night, that without was at 0, and that within at 12 above 0. So that in the space of 13 hours the former had fallen 17 degrees, the latter only 2.

As the barometer had been for a good while past subject to sudden considerable variations, he suspected the severe cold on Wednesday night and Thursday morning would not continue long; accordingly, on observing the thermometer without at 4 in the morning, he found it at $\frac{9}{0}$, 9 degrees below the freezing point, that within at $\frac{5}{0}$, 5 degrees above freezing point. But at 8 o'clock the same morning, he found the thermometer without at $\frac{3\frac{1}{2}}{0}$, $3\frac{1}{4}$ degrees above freezing, and that within at 4 degrees above; so that in 4 hours time, that without had risen $13\frac{1}{4}$ degrees, and that within had fallen 1 degree. This naturally led him to examine what signs there might be of a thaw begun, but could find none, in the snow, which was 5 inches deep, or in the post, on the windows, but within an hour it was visible enough, and before 10 the houses dropped.

May not this sudden change of the temper of the air be attributed to a subterranean heat? and may not the shifting of the wind be caused, in a great measure, by the same?

A Child being taken out of the Abdomen, after having lain there upwards of 16 *Years, during which Time the Woman had* 4 *Children, all born alive. By Starkey Myddleton, M. D.* N° 484, p. 617.

In April 1731, Mrs. Ball, perceived that she was pregnant; and in October following, (then in the 6th month of her pregnancy) a child died in her lap of convulsions, the surprise of which occasioned a great fluttering within her, attended with a sensible motion of the child, which motion continued, though gradually weaker and weaker, for 6 or 7 days, after which she did not perceive it to move any more; but from this time she had constant pains, which appeared like labour-pains. Her midwife, for several days, expected a miscarriage; but finding herself disappointed, advised her to apply to Dr. Bamber, who after a proper examination finding sufficient indications of a dead child, ordered her some forcing medicines; on taking which about 3 times she discharged something, which the women supposed to be part of an after-birth, accompanied with a small quantity of water; in consequence of this discharge her pains ceased, but without any visible diminution of her belly.

After some time she again applied to the doctor, who thought it most advisable to discontinue her medicines, and leave the affair intirely to nature.

In this state she continued for about 20 months, viz. to July 1733, which was 2 years and 3 months from her first reckoning; she then again applied to Dr. Bamber, acquainting him that she was not yet delivered of the child she so long since came to consult him about, and that her pains were lately returned, and daily increased without any intermission. On examining her, he thought it proper to send her home immediately, directing her to promote her pains, by frequently supping some warm caudle, &c. by the use of which her pains became more regular, and the next day the Doctor made her a visit, and was informed she had discharged two waters, but nothing more: he then carefully examined her again, and plainly felt a child through the integuments of the abdomen, but could not give her any assistance.

It was about this time that Dr. Bamber first acquainted Dr. M. with the case, and desired that he would attend her as often as occasion might require, and that he would acquaint him if any thing like labour, or other remarkable alteration should occur. Accordingly Dr. M. made her a visit, and after a proper examination, was convinced of the certainty of the Doctor's assertion.

Her pains now began to abate, and she became tolerably easy; but about the latter end of Jan. 1733-4, she conceived again with child, and was delivered on the 28th of Oct. following by Dr. Bamber, who sent for Dr. M. to attend him in her labour: the Doctor soon delivered her of a fine boy, and after having brought away the placenta, he searched for the other child, which he had before felt through the integuments of the abdomen, but found it was lodged in the cavity of the abdomen, and beyond the reach of human art to relieve her. This fact every one then present was made sensible of.

October 22, 1735, Dr. M. was sent for to her in her labour, but before his arrival she was delivered of a boy; but he brought away the placenta, which gave him an opportunity of examining for the other child, and found it in the same situation as formerly.

October 9, 1738, Dr. M. was again sent for to her when in labour, but she was delivered of a boy before he arrived. On examining the womb, and the state of the abdomen, the child appeared just as before, without any alteration.

June 17, 1741, Dr. M. was again sent for in her labour, but found her just delivered of a girl; and on examining the parts, every thing appeared as before.

October 14, 1747, being greatly emaciated by constant pains, &c. she was admitted a patient in Guy's Hospital, where she died the 7th of November following, after having laboured under the distresses and uneasiness of carrying a dead child within her, in a manner loose, in the cavity of the abdomen upwards of 16 years.

The day after her death Dr. M. opened her, in the presence of Dr. Nesbit, Dr. Nichols, and Dr. Lawrence, when the uterus, and the several other contents of the abdomen appeared nearly in their natural state; but on the right side within the os ilium a child presented itself, which was attached to the ilium and neighbouring membranes by a portion of the peritonæum, in which the fimbria and part of the right Fallopian tube seemed to lose itself.

The child seemed no-ways putrid; but the integuments were become so callous, and changed from their natural state, that the whole seemed to resemble a cartilaginous mass, without form or distinction: the legs indeed were distinguishable, though they were much wasted and distorted. On opening the callous integuments of the head and face of the child, the bones appeared perfectly formed, with a few spots of tophous concretions on them.

On the Case of Margaret Cutting, who Spoke distinctly, though she had lost the Apex and Body of her Tongue. By James Parsons, M.D., F.R.S. N° 484, p. 621.

Several of the members of the Royal Society having been divided in their opinions concerning what was reported of Margaret Cutting, when they were first informed of her by Mr. Baker;* Dr. P. conceived it to be necessary, in order to render her case better understood, to lay before the Society the following particulars, which were the result of an examination made a few days before by Dr. Milward and himself; and which in general differs not from the opinion which that learned gentleman and he mentioned to the Society, on the occasion, which the science of anatomy necessarily suggested to them at that time. But James Theobald, Esq. a member of the Royal Society, having encouraged her to come to London, and having brought her to the meeting of the Society, this gave all the members here an opportunity of coming at the truth of her case; wherefore Dr. P. here offers first, an account of her then condition; and next, some considerations on the natural state and uses of the tongue; which will show how far she makes the lips and teeth supply the want of her tongue in speaking; and also enable gentlemen to judge better of the case.

Of the state of the woman's tongue in December, 1747. The apex and body of the tongue, being the only parts that naturally fill the cavity of the mouth, are intirely wanting in this woman, as closely to the region of the os hyoides, which is the root of the tongue, as can well be conceived; and which was then situated too low in the throat to be perceived, even when she opens her mouth at the widest.

But if any one laid the tops of the finger and thumb to the sides of her throat, and let her at the same time pronounce the letter k, he would feel the remaining root of the tongue rise towards the roof of her mouth, in order to perform it; however, she could not keep it there any longer than the moment of thrusting it up, for want of the ligament (which was destroyed with the tongue) that is destined, together with the muscles, to keep the whole tongue forwards in its due situation.

Now there are certain inequalities appearing on, and closely adhering to the floor of the cavity of the mouth, one of which being the most considerable, and having a resemblance in its substance to that of the surface of the tongue, has been, if he was rightly informed, inadvertently mistaken for a tongue, by a

* See Phil. Trans. No. 465, p. 586, Vol. viii. of these Abridgements.

gentleman professing surgery in the country; and which he thought, for want of a careful examination, performed the offices proper to the apex; but a little care and circumspection would have informed him, that those appearances are only fragments of the genioglossi muscles, and that on the separation of the sound parts from those mortified, such fragments as had escaped were retracted, and cicatrised down into their present state; nor is it difficult to conceive how the root of the tongue must of necessity sink lower down into the throat, by the loss of these muscles and the proper ligament; which naturally kept it higher than it could remain ever since their destruction.

If the mortification had reached the os hyoides, it must have reached, and destroyed the muscles of the larynx, and then the voice would have been destroyed; and also those of the pharynx, and then deglutition could never have been performed; the dreadful consequences of which need not be enumerated here; but she swallows well, and her voice is perfect, and therefore it is not very extraordinary she should command her voice by the proper muscles which remain untouched.

The nasal opening was quite exposed, because the uvula which covered it was also destroyed; for one pair of its muscles (the glosso-staphilini) arise from the tongue; by which no doubt the distemper was communicated to this part also.

She had her taste perfectly; which is hereafter accounted for.

Some Considerations on the Natural State and Uses of the Tongue.—The tongue is a fleshy substance, chiefly made up of muscles; and consists of a basis or root, a body, and an apex; the basis is the thickest and most substantial part, contains the os hyoides, and is naturally situated very low in the throat: from which the body rises upwards and forwards, and is terminated by the anterior part or apex; proceeding under the uvula and roof, and lying on the floor (if he might so call it) of the mouth.

As to its uses, it is said to be the instrument of speaking and tasting. As to the latter, experience shows us the very apex of the tongue is less capable of discerning tastes than the next part to it, and this than the parts yet farther back, all along the body to the root; so that though the taste of any thing is first perceived by the apex, yet the gust increases, the more the morsel approaches to deglutition, till it is quite protruded into the gula; because, as the tongue grows more thick backwards it contains more of the nervous papillæ than the smaller part, and also because there is a capacity of tasting in the membranes of the back part of the roof to the root; as if nature intended to increase the gust, that deglutition may be the better and more eagerly performed for the service of the animal: hence though the apex and body of the tongue be gone, yet there is not a depravation of taste, which is the case of the person now under consideration.

As to speech, which is only sound or voice articulated into expression, the tongue is not the sole organ for such articulation; the lips, teeth, and roof of the mouth are instruments also for the same purpose; the 2 latter for the necessary resistance to the apex of the tongue, and the lips for the absolute articulation and pronunciation of many letters; however the following short examination of the letters of the alphabet, as expressed by these organs, will demonstrate it.

The tongue expresses some letters with its apex, and some with its root. Those absolutely proper to the apex are only 5, d, l, n, r, t. And those to which it only assists, are the following letters, c, g, s, x, z; all which can be performed by the teeth alone, and which this person did very well.

Now the lip letters, and those expressed by the root of the tongue, she also performed as well as any person; the former are b, f, m, p; and the latter are k, q, x; and as to the vowels, and the aspiration h, since they are chiefly sounded by the exhalation of the voice, commanded partly by the lips in widening or straitening the capacity of the mouth, these she could also express; so that there was no letters she could not pronounce, except the 5 apex letters; and these she managed so well by bringing the under lip to her upper teeth, in the course of her conversation, that any one could instantly apprehend every word she said; and she further plainly proved the lips are a better succedaneum to the apex, than that could be to the lips if they were wanting.

Indeed it is natural enough for those who make the tongue the absolute and sole instrument of speech, to imagine it as absurd to say a woman spoke without a tongue, as that she saw without an eye; but when we consider the provisional assisting organs ordained by the wise Author of Providence, serving to this necessary and expressive accomplishment, Dr. P. hopes it will not seem so extremely marvellous, that she spoke without the body and apex of her tongue, as to create any further doubt of the matter.

Appendix to vol. xliv—*Containing some Papers, which were not ready to be inserted in the Order of their Date.*

I. *On several Species of small Water Insects, [Animalcula] of the Polypus Kind. By Mr. Abr. Trembley F. R. S. From the French. Vol.* 44, p. 627. [*Appen.*]

Mr. T. here gives an account of the observations he had made, subsequent to those inserted in the Phil. Trans. N° 474, on polypi; describing at the same time the apparatus he employed for this purpose. If we only propose, says Mr. T. to examine for some moments the figures and the motions of water-insects, [animalcula] we may be content with barely exposing such in the common way to the microscope in a few drops of water. But he can safely assert from divers repeatad experiments, that it will often happen, with regard to several sorts of these insects, that the simple observation of them in a drop or 2 of water, will not be sufficient to discover all that is singular either in their shape or motion. It is therefore very proper that an observer should endeavour to examine such insects, when they are more at ease, and in a larger quantity of water. And this will be found still more necessary, if we be desirous regularly to pursue their history. For then the same insects ought to be regularly observed, for many days successively, and they ought themselves also to be as nearly as possible under the same circumstances they would have been, had they remained in the same waters in which they naturally live.

He accustomed himself to keep great numbers of the small insects [animalcula,] on which he made observations in large glasses: and it was by observing what passed in those glasses that he endeavoured to discover the more general facts, relating to the natural history of these animals. After which, he found by many repeated experiments, that it was necessary to remove into glasses of a less size, Tab. 1, such of the insects as were to be set apart for more particular and curious microspical observations. He put water into these glasses, from the same ditches, out of which the insects he was observing had themselves been taken; and shifted this water more or less often as the circumstances might require.

To observe a small insect in one of these glasses, with a magnifier of a short focus, the insects [animalcula] should be placed very near to one of the sides of the glass; and it ought also to be kept steadily in the same place. It ought therefore to be either fixed to the side of the glass itself, or to some other body that may be conveniently so fixed. He chose for this purpose, substances that were slender and supple, such as the small branches or twigs of divers species of the equisetum palustre, or water horsetail. The clustering polypi are often found on these twigs, and they may be made to settle on them from elsewhere.

Now this is the method he took, to fix one of these twigs of horsetail against the side of his glass. Having chosen a small slip, on which there was one or more of the clustering polypi he took a piece of a peacock's feather, from which he cut away all the beards on both sides, excepting one at the extremity; on this one he made a knot near its insertion, but did not at first draw it close. He then brought this open knot to the small slip of the horsetail that was floating in the water of the glass, and getting one of its extremities into the knot, he then drew it close; and thus the slip of the horsetail is joined to the piece of the feather. He next took hold of the feather, and bending it near the middle, he forced its 2 ends b, f, fig 7, pl. 8, into the glass; he then let go the feather which he before held, and its elasticity would force its 2 ends against the sides of the glass A, by which means the small twig d l of the horsetail, which was already fixed to the extremity of the feather f d, would become also fixed to the side of the glass;

the consequence of which was, that the polypus sticking to the horsetail would be obliged to remain in such a situation, as to be within the reach of a magnifier that was of a short focus.

Nothing more was then wanting, but to place the magnifier before the object: for it would be both difficult and very inconvenient to hold it like a reading-glass in the hand. In the instrument k, i, h, g, e, it was screwed into a ring fixed to a small branch n, g, which had a ball g at its other extremity; this ball fitted a socket, and so made a joint, by which the first branch was joined to a second h, i, and that again in like manner to a 3d i, k, or 4th, if there should be occasion. The foot of the whole was fitted near the edge, into a small board or tablet that held the whole apparatus. By means of these joints, the magnifier e, might be turned any way, and might be conveniently brought near its proper distance from the object; yet as the branch which held it, could not well be without some spring, it would be still difficult to adjust the object exactly to the focus of the magnifier when it is short, if only the magnifier was to be moved for that purpose; and it was therefore found to be easier, when the magnifier was once right against the object, to move gently the glass in which that was contained, till it should be precisely in the focus of the magnifier: and for this purpose the small board on which the glass was placed, was well smoothed.

The light that came in at a common window was found sufficient for observing in the water such objects as are to be seen with the bare eye, or with a hand magnifying glass; but such as must be examined with a lens of a shorter focus, must be viewed by the light of a taper, placed beyond the glass, and whose flame is to be on the level with the object.

A magnifier thus once adjusted may remain in the same place before the object, for several days together, without being disordered; so that, to observe the progress of the insect [animalculum] during all that interval, no more will be necessary, than to place from time to time a taper behind the glass, and to apply the eye to the already fixed magnifier.

Several of these apparatuses may be placed on one and the same board by each other; and thus at the same time observations may be made and carried on with different sorts of insects, or with several insects of the same species; in order to come sooner and with more certainty at the knowledge of the facts desired.

Mr. T. could never have discovered the manner in which the clustering polypi are multiplied, but by the help of the expedient here described: and before he had the use of that apparatus, he only knew in general the figures of those polypi, and of the clusters that contained them. He had taken notice that those clusters grew, and he had reason to suspect, that a whole cluster came from a single polypus; but he still wanted to see this increase, and to find the moment of their multiplication; for he had reason then to suspect, from what he had seen with a glass held in his hand, that these clusters did not grow insensibly like plants; but that on the contrary, the operation was performed in a short portion of time. He therefore resolved to observe regularly for some time polypi of this kind with his microscope, while they should remain in circumstances, nearly as easy and as natural to them, as those they were in in their proper habitation.

This gave him the first thought of the above-described apparatus. And when he had prepared and fixed every thing, he set himself continually to watch for the moment of the multiplication of the clustering polypi; and he then found this moment, which he had so much wished to discover, the very same morning that he began to make use of his apparatus.

It was, as has been seen in the paper above referred to, in that species of polypi, some of which are represented in the 1st, 2d, and 3d figures of the 2d plate of this Vol. 9, that he first discovered the manner in which these small animals are multiplied. It is also in the same species easy to see that very odd motion, which they exhibit at their anterior extremity. This same motion, which has also place in other species of clustering polypi, is not in them so easy to be remarked; both because they are smaller, and also as this motion itself is swifter, than in the sort above-mentioned.

There is also to be observed at the anterior extremity of several other small insects, a sort of motion which has drawn the attention of all such as have happened to see it, and who have almost all been curious to inquire and satisfy themselves, whether those little wheels, which appear to turn with so swift and so regular a motion, are really wheels turning on an axis or not. This determined him now to mention that motion, though it is not his design to treat fully of it in this place, or to determine very precisely what he thought about it: as he would be very cautious how he asserted any thing positively on so nice a matter, till he should have repeated several experiments he had then made.

In order to discover what this motion might really be, he applied himself not only to observe it in the same animal placed in different altitudes, but also in different species of water-insects [animalcula] in which it is seen, and he has compared the phenomena of all these several motions one with another. These comparisons he has found in other cases to be of singular use, and the best means of preserving himself from those illusions, which very small objects, viewed in a microscope, especially while they are in motion, are but too apt to present.

All he had then learned from these comparisons, and all the other observations he had made, seemed to prove that there is some deception of the sight in the present case, and that the motion in question is not really what it at the first appears to be, a rotatory motion round an axis. And he knew some species of polypi, in which this motion is, comparatively speaking, but slow: and in these it is distinctly seen that this motion, though in general resembling that observed in the others, is not a revolving or rotatory motion: such, for example, is the motion which is noticed in that species of polypi which Mr. Leuwenhoeck has described in the 295th No. of these Trans.* This is one of those insects [animalcula] whose motion is the most to be admired, and it is besides exceedingly curious on many other accounts.

The motion in question is very slow in the clustering polypi, just when they are opening again after their division, and he was greatly mistaken, if it might not then be seen very plainly that this motion is not a rotation. The same remark applied to the tunnel-like polypus, and that almost during all the time that it employs in its separation.

Mr. T. made use of an expedient while he was observing the clustering polypi, by which he was able to retard the quickness of their motion. He poured by little and little a small quantity of spirit of wine into the glass they were kept in. This spirit of wine immediately either abated the velocity of their motion, or took it quite away, according to the quantity of it poured in. That which follows both in the one and in the other of these two cases, was of use, and threw light on the present question. Sometimes the spirit of wine forced the polypus entirely to draw in its lips within its body, and at other times even to detach itself entirely from its pedicle.

Another way to take off the celerity of this motion, is to remove the insects into a water which furnishes them much more sparingly with food; fasting probably weakens them, and from their weakness arises an abatement in the quickness of their motions. This last expedient is of use and conveniency for observing this motion while it is slower, for several days consecutively. And afterwards on returning the polypi into water stocked with food for them, the motion will soon be restored to its former briskness. He also remarked in the winter, that cold deadened the motion of the clustering polypi: and these animals in all probability are less voracious, and eat less in winter than they do in summer. When the motion in the clustering polypi has been retarded, either by fasting or by the cold, they become whiter, or of a paler colour than before; they also then cease to multiply.

What he here proposed to describe, was the manner in which the clusters are formed of a certain species of polypi, which multiply in the main like those represented in the figures of the 474th number of the Phil. Trans. and which differ chiefly from them in the form of their clusters. The polypus, when it is ready to divide, first draws in its lips into the body. It then by degrees puts on a round form, and presently after the little spherical body so formed, divides itself into 2

* Vol. v, p. 175, of these Abridgments.

other like spherical bodies. These last in a few moments again insensibly open; they then lose their spherical form, and put on that of a bell, or of a polypus as perfect and as compleat, as that by the division of which it was formed. This is the manner in which several species, which he observed of clustering polypi, are multiplied: the whole operation is performed by that sort, of which he has spoken in his former paper, in three quarters of an hour or an hour by these others.

The polypi of this sort are smaller and whiter than those others, which are represented greatly magnified in the above mentioned figures. The cluster which they form rests on a stem easy to be remarked: this stem is fixed to some other body at its lower extremity, and from its other arise branches, making obtuse angles with the stem itself; other branches again set out from these in different places, and from these last other new ones, and so on. At the extremity of each branch may be seen a polypus: and as all these branches are not of an equal length, so neither is every polypus, as in the other species, at the top of the cluster, or at an equal distance from the base of the stem; but on the contrary, there are here polypi to be discovered at all heights in the cluster. The assemblage of all these branches forms, together with the polypi at the extremities, a very pretty cluster or groupe, much resembling a tuft or a garland of flowers.

The stem, which carries all the cluster, and every branch in it, is capable of a remarkable sort of motion. Each will contract suddenly when it is touched, or when the glass containing the cluster is moved, and even sometimes when no reason is to be perceived for their so contracting fig. 9, a. The stem and the branches contract and shorten, by disposing themselves into spirals, all whose rings nearly touch each other. Every branch is by itself capable of contraction, independently of the rest: though it but rarely happens that any one branch does contract itself quite alone, for commonly in the action of contracting it happens to touch some other branch, and then that other immediately contracts with it. When the main stem, which bears the whole cluster contracts itself, then all the branches of the cluster contract together also; and the whole becomes entirely closed. A moment after, the branches and the stem again extend themselves, and the whole cluster thus recovers its ordinary figure. But when the cluster is considerably advanced, the stem then ceases to contract itself any more.

This cluster forms itself in the following manner. A single polypus detaching from the cluster, swims about in the water till it meets with some proper body to fix itself on. It then has a pedicle, but which is not longer than the polypus itself. In the space of 24 hours this stem becomes 8 or 9 times as long as it was at the first: and it is this pedicle which is to become the main stem of the new cluster. About a day after the polypus has been thus fixed, it divides itself into 2. Ten or 12 hours after, these 2 polypi again divide each into 2 more: they soon after put out branches, and thus retire to a greater distance from each other. It is now necessary to take notice, that when 2 of these polypi are thus formed by the division of one, the one of them usually is much larger than the other: this larger one remains at the extremity of the branch where it was, but which branch lengthens itself more while the other puts out a new branch, which seems to proceed from the first. The larger of these polypi again divides itself generally before the other; and every thing above described is reiterated several times. Thus a principal branch is formed, provided with several lateral ones. These lateral branches become principal, with regard to those which in their turn seem to spring from them, when the polypi at their extremities come to divide. All the polypi of a cluster do not detach themselves from it at the same time: those which are nearest to the origin of the branches usually detach themselves first. And every polypus so detached, goes and fixes itself elsewhere, every one thus becoming at last, if not prevented, the principal of a new cluster.

He has often kept polypi of this kind, in glasses of the size of that represented in fig. 7. And the first cluster he had placed in it, to observe its growth and progress, continued still well provided with polypi, when there were already numbers of other clusters formed in the same glass, all which owed their being to those that had detached themselves from the first cluster. He had seen sometimes portions of the peacock's feather in the water entirely covered with these clusters: and he was well assured that all these clusters came from the first he had lodged in the glass. Nay he had even carried his experiments so far as to be well assured, that every polypus of a cluster, as soon as detached and fixed elsewhere, became the principal of a new cluster. He mentioned this fact particularly, because he should make some use of it hereafter, when he came to take notice of a difference between this species of polypi, and another species mentioned afterwards.

When a cluster is in good part stripped of its polypi, the branches are no longer able to contract with the same quickness and readiness as before. Where there remain but a very few polypi, none but those branches to which polypi are still fixed continue to exert this power; which these also lose as soon as they are stripped of their few remaining polypi, after which they show no further capacity of moving.

From all which particulars it seems to result, that this motion in the stem, and in the branches of a cluster, is entirely derived from the polypi, which are fixed on the branches. And yet an observer, attending to the appearance only of this motion, can hardly help persuading himself at the first, that it is the branches which draw and give motion to the polypi.

The resemblance and the analogy which the figure of a cluster of polypi bears to the figure of a plant, would induce any observer for some time to imagine that the polypi, which he sees fixed to the branches of the cluster, do really proceed and spring from those branches, in the same manner as the leaves, the flowers, and the fruits of a vegetable, spring from the branches of the same.

It is nevertheless the contrary of all this that is true. The branches, composing the clusters of the polypi, spring from the polypi which are at their extremities. These polypi, which at the first appear to be the fruits of the clusters, may more properly be considered as their roots: and of the truth of this any one may easily satisfy himself, who will be at the trouble of examining regularly, and for some continuance, the whole progress of a cluster of these polypi. What further proves that these branches do really spring from the polypi, and that they derive their nourishment from the same, is, that the branches constantly cease to grow, whenever the polypi at their extremities are detached from them, either naturally or by any accident.

There is another species of polypi which form also a groupe resembling a cluster, or more properly an open flower.. This flower or cluster is supported by a very distinct stem, which is by its lower extremity fixed to some of the aquatic plants or extraneous bodies found in the water. From the other extremity of this stem issue out 8 or 9 branches, quite differently disposed from those of that species of polypi he last described. These 8 or 9 branches are perfectly alike, but it may be noted, that what is here called by the name of a branch, is indeed the assemblage of several other smaller branches, whose collective form much resembles that of a leaf, fig. 10. Every one of these assemblages is composed of one principal branch or nerve, which makes with the main stem of the cluster an angle somewhat greater than a right one. From either side of this principal nerve others again set out, and these lateral ones are the less extended in length, the nearer their origin is to the extremity of their principal branch. There is a polypus at the extremity of this principal branch, and another at the extremity of every one of the lateral twigs. There are others also on both sides of those lateral twigs, at different distances from their extremities, and these are more in number or fewer, in some proportion to the length of the twig itself. These polypi are all exceedingly small, and of a bell-like figure, and they discover about their openings a quick motion, very difficult to be seen with any distinctness.

There may also be observed in several places, on the branches of these clusters of polypi, fig. 10, certain round bodies, which Mr. T. at the first took for insects preying on the polypi, because he was acquainted with some such, nearly of that shape and size; but it is afterwards mentioned what those round bodies really are.

Every cluster has 8 or 9 of these branches or leaves, such as he has described. They do not all of them set out from the same point; but the points from whence they set out are not far asunder; each of these leaves is a little bent inwards, and they all form together a sort of a shallow chalice or cup. If the eye be placed right over the basis of this chalice, the appearance of the whole 8 or 9 branches is like that of a star with so many rays proceeding from the same centre.

When the cluster is touched, and even frequently without it, all the branches fold together inwards, and then constitute a small round mass. The stem, which carries all the cluster, contracts also at the same time, folding itself up like a workman's measuring rule, that consists of 3 or 4 different joints.

He saw for the first time the polypi he has been describing, on the 30th day of May of the last year 1746. They were on a water plant, which he had taken from a ditch, and disposed in one of his large glasses. They immediately struck him by their beauty, and he could not help being curious to know in what manner such clusters were formed. The relation they bore to the species first above described, and to some other species which he had before observed, gave him reason to believe that the cluster must have sprung from a single polypus, by means of several successive divisions. He was not however contented with judging of them from analogy only; he was desirous to be actually an eye-witness of their operations; and the observations which he therefore made on them, disclosed a new fact, which he should never have suspected, and which he could never have come to the knowledge of, if he had contented himself with the judgment he made of them from analogy only.

He supposed, when he began to observe, that every cluster in question came from a single small polypus, like those with which the clusters were so plentifully provided. He therefore began by endeavouring to get one of these polypi single, and fixed on such a body as he could well dispose in his glass, so as to keep it within the reach of a magnifier of a short focus: and he pursued for this purpose his ordinary method.

He took some clusters of these polypi well advanced, he put them apart in a glass filled with proper water to afford them sustenance; he put also into the same glass a slip of water horsetail, after he had carefully examined it, and so assured himself that there was no polypus on it. He expected that some polypi would soon detach themselves from the clusters, and that some of those polypi would fix on the horsetail, by which he should be enabled to set them apart, and to observe in other glasses the progress of the clusters, which would, as he made no doubt, be soon produced from them.

It was on the 30th of May, that he set the clusters apart in the glass. On the 31st he could discover nothing new, and on the 1st of June he had no opportunity of observing; but on the 2d in the morning he found against the sides of the glass several small clusters of polypi, of the species now described. He was surprised to find them so far advanced, for they could not have begun at the soonest before 10 o'clock at night, on the 30th of May. He saw on the 2d of

June in the afternoon on the slip of the horsetail, which he had placed in the same glass with the clusters of the polypi, a small body, which, as he had every reason to believe was newly fixed on it. He then took out the slip of the horsetail, and he lodged it, with the small body that was on it, in another glass, after which he examined that small body with his microscope, by the help of the apparatus first above described.

He then found that this body was much larger than any of the polypi of the present sort, and of a figure very different from them, fig. 11. This made him suppose that this body was not of the species of the polypi now before us, and that it was not from any thing of this sort that he was to expect the production of a cluster of this species of polypi. He resolved however to continue his observations on this minute body; which was oblong, and had a pedicle 3 or 4 times longer than itself.

It was on the 2d of June, at 5 in the evening, that he put it apart in a glass, and at half an hour after 8 the same evening, he perceived that it began to split from the top towards the bottom. When the separation was accomplished, each of the 2 bodies formed by this division was nearly of the same shape as the first, fig. 12. He then thought, judging still by analogy, that it would be some time before either of these bodies would again be ready to divide; but a very little after, he saw that they both became round, and that they disposed themselves precisely as if they were again going to separate. This novelty drew all his attention, and it again came into his mind, that this body which he had but just concluded not to contain the principle from which he was to expect the production of one of the clusters he was looking after, might possibly still be the very thing he was seeking for.

He now imagined that perhaps these bodies would again divide and subdivide themselves till they should come both to the shape and to the size of the polypi, which he had seen on the clusters; he however considered this idea but as a mere conjecture. The 2 little bodies did in effect divide presently after; but the 4 which resulted from this division, fig. 13, had neither yet the form nor the minuteness of the polypi in question. He now wanted to know whether these 4 bodies would again proceed to divide without interruption; and he saw them a little after again prepare for another division; this division was completed at 20 minutes after 11, and at midnight the 8 bodies which were formed by this third division were again almost completely divided. The cluster was then composed of 16 polypi, and he from that moment no longer doubted, but these were clustering polypi of the species last described. Among these 16 polypi, there were some which had already the perfect form of those he had observed on the more advanced clusters; and these were such as were nearest to the origin of the branches.

6

Few of these 16 polypi were of an equal size; those most distant from the origin of the branches were the largest, and their form also was the least like to that of a bell. At 3 in the morning on the 3d of June, the number of the polypi in the cluster was considerably increased; they were 16 at midnight, and he could now count 26, though he could only see part of the cluster, the rest of it being beyond the focus of the microscope; and at half an hour after 7 in the morning, he counted at least 40 polypi in that same part which he could see of the cluster.

In order to judge with more certainty of the progress of the multiplication of these polypi; he counted also those of another cluster, which was so situated as to be entirely within the reach of one of his magnifiers. This cluster began to be formed about 8 in the evening of the 2d of June; i. e. it was then that the round body first began to split itself into 2. At 11 the same night, that cluster consisted of 8 polypi; at half after 7 the next morning of 64, and before night of 110 at the least. So that in about 24 hours there were formed by repeated divisions of one single round body, no fewer than 110 polypi.

The cluster first spoken of continued to increase from the 2d of June at half an hour after 8 at night, when it first began to form itself, till the 13th, when the polypi began to detach themselves from it; and there remained no more on the cluster on the 15th.

The polypi which are at the extremities of the principal branches are constantly the largest; they are those which divide themselves the most frequently; and one of the 2 polypi resulting from this division is generally larger than the other. The largest remains at the end of the principal branch, while the less serves to form a lateral branch, and is itself the principal of all the polypi which that lateral branch is to bear.

One can hardly now be without curiosity to know, what those round bodies really are; those sorts of bulbs which contain in themselves the principle, from whence these whole clusters are to be produced. What gives origin to these bulbous bodies? are they produced in the clusters by divisions and subdivisions, as the polypi themselves are, which in other species are themselves the principles of the clusters? in these other species, every polypus may become the principle of a cluster and of a groupe of polypi, as soon as it has detached itself from the cluster where it had its origin. When one of these has once fixed alone any where, and divided itself, it nowise differs, either in shape or in size, from any of the polypi that were in the cluster it is now parted from, nor from any of those others that will be formed in the cluster, it is by its own future division and subdivisions to produce. But how is it with the new species we are now considering? does every polypus among these, as soon as detached from the cluster, fix itself also elsewhere, and there give origin to a new cluster? or are they only the bulbous bodies abovementioned, that have this prerogative, of being capable to produce a new colony?

These questions and doubts greatly raised his curiosity, from the time he first began to see the progress of a cluster of polypi, formed by the division and the subdivisions of one of these round bulbous substances; and that which now follows, is what he had been able to collect from the various observations, and from the several experiments, which he made, while endeavouring to acquire some satisfaction with respect to the same doubts and questions.

To know whether the polypi, which detach themselves from these clusters, do each of them contain in themselves the principles of other new clusters, he took all the precautions he had taken in other cases, and such as he had found easily to succeed with the clustering polypi of other sorts. But all was to no effect, and he could never find that any thing was produced by these polypi so detached. He had therefore every reason to presume that these polypi do not contain the principles of new clusters, and it seemed the most probable that they all perish without ever producing any thing whatever.

When he first began to seek for the origin of the round bulbous bodies here mentioned, he immediately recollected those other round bodies he had before taken notice of, and which he at the first suspected to be insects preying on these polypi. He therefore again sought for them in the clusters already formed; he soon found several of them, and he perceived that they neither attacked the polypi nor changed their situation. He then concluded that these round bodies were really the very bulbous ones in question, and whose origin he was now seeking for; he applied himself therefore to observe several of them, and he then discovered the following facts.

Some days after the clusters had begun to form themselves, he saw come out, not from the extremities of the branches, but from the bodies of the branches themselves in different places, small round buds, which grew very fast, and which arrived at their greatest size in 2 or 3 days. These bodies much resembled the galls which grow on the leaves of oaks; they were placed on the branches of the clusters, just as those galls are usually placed on the fibres of the leaves; and these bulbous substances really contain the principles of the clusters.

Two or 3 days after these bulbs have begun to form, they detach themselves from the branches out of which they sprung, and go away swimming till they can settle on some body, which they meet with in the water, and to which they immediately fix themselves by a short pedicle. The bulbs are then nearly round, only a little flatted on the underside, the pedicles continually lengthen themselves by degrees for about 24 hours, and during the same time the bulbs also change their figure, and become nearly oval. There are in a cluster but few of these bulbs, in comparison of the great number of polypi that are on the same; neither do these bulbs all come out at the same time.

It is now easy to judge of the remarkable difference there is between the 2 sorts of clustering polypi that are described in this paper. The clusters of the first species of polypi, and those of several others which he had also observed, do all come from polypi detached from the clusters already formed. But the clusters of the polypi of the second species here described, do not arise from polypi detached from other clusters, but from round bodies or bulbs, larger than those polypi, and of a form very different from them. These bulbous bodies are not formed like the polypi, by the division of others like themselves, but they spring from the branches of the cluster, as the flowers and the fruits of a tree spring from the branches of the same.

In divers other species of polypi, there are considerable intervals of time between their divisions. In the bulbous kind, if they may be called so, the first divisions are consecutive, and follow hard on each other, nor is there any interval of time between them, till the bodies which are to divide have already acquired the shapes of polypi.

The clusters of the bulbous sort have an origin entirely different from those of the other sorts of clustering polypi. Yet do these clusters enlarge, and the polypi upon them multiply, in the same manner as those of the other species.

Explanation of the figures in pl. 8, referred to in this paper.—Fig. 7 represents the necessary apparatus, for observing commodiously and regularly a clustering polypus with the microscope. In the glass A, is the end of a peacock's feather, bcf, bent at c, its extremities, by the spring of the feather, being kept close against the sides of the glass. At one of the ends f of the feather one of its beards is left on, which is long enough to fasten to it at m a slip of water horsetail dl, on which is a polypus, which is by this means kept so close to the side of the glass, as to be within reach of a magnifier of a short focus, such as e. This magnifier is screwed on to a ring whose arm ng has at its extremity g a ball playing in a socket, so as to make a joint; there are again other like joints at h and i, by the help of which the magnifier may be moved every way, and be conveniently brought near the object. The foot ik is fixed in the board on which the glass is placed. The light of a window in the day-time is sufficient to observe an object so placed within the glass, either with the bare eye, or with a hand magnifier; but if a magnifier of a short focus be necessary, the shutters must be closed, and a wax light must be placed behind the glass, at such a height as to have its light fall directly on the object; and a magnifier so placed may remain, if there be occasion, for several days in the same posture without any inconvenience.—Fig. 8 exhibits a cluster of polypi, of the first of the 2 species described in this paper, and which is here considerably magnified.—Fig. 9 shows another cluster of polypi of the same sort; the number of the polypi here shown being but small, because the cluster was drawn as it appeared within 2 or 3 days after it had first begun to form itself. One of the branches of this cluster is partly contracted, being in the situation, when a branch after contracting itself, is again expanding to its ordinary state. This cluster is yet considerably more magnified than that exhibited in fig. 8.—Fig. 10 represents one branch of a cluster of polypi of the second species described in this paper. On this branch, besides the polypi, which are of a bell-like form, are some of those round bodies from which the clusters of this kind of polypi first spring; and which remarkably distinguish it from many other species.—Fig. 11 represents one of these round or globular bodies which, after it has parted from the cluster, has fixed itself to some other body, and after the globule itself and its pedicle have begun to lengthen. It was in this condition June 2 at 5 in the evening.—Fig. 12 exhibits the 2 bodies, formed by the parting of that represented in fig. 11. This parting began at half after 8, and was completed at 9 the same evening.—Fig. 13 represents the 4 bodies, formed from the 2 represented in fig. 12, and these 4 bodies were also formed before 10 o'clock.

A Collection of the Magnetical Experiments communicated to the Royal Society by Gowin Knight, M. B. and F. R. S. in the Years 1746 and 1747. Appendix to N° 484, p. 656.

I. *An Account of some Magnetical Experiments, exhibited before the Royal Society, Feb. 19, 1746. Reported by the President, Martin Folkes, Esq.*

Mr. Knight first produced 2 almost equal bars of hardened steel, to which he had communicated a strong magnetic virtue. These bars were nearly square, each being of the length of about 15½ inches, and of the breadth and thickness of a little more than half an inch; one of these bars weighed 2 lb. 6 dwt. Troy, the other 4 dwt. less than 2 lb.; and either of them readily lifted with one of its ends better than 3½ lb. These bars were then laid down on a table, so as to be nearly in one and the same straight line, the north pole of the one being next to the south pole of the other, and at the distance of about an inch from it; the north poles of both bars thus pointing the same way, but without any regard to the position of the natural meridian.

Mr. Knight then produced a piece of natural magnet, which was one of the same he had formerly made use of, in some experiments he had before showed to the R. S. This piece was in length an inch and $\frac{1}{10}$, in breadth $\frac{7}{10}$, and in thickness about $\frac{4}{10}$ of an inch at a medium, being considerably thicker at the one end than at the other. This piece of magnet was then applied, so as to lie between the first 2 mentioned bars, with its thin end close to the north pole of one of them, and its thick end close to the south pole of the other. After it had lain in this position a few moments, it was taken out, and on presenting it to the magnetic needle of a small compass-box, it was observed that its thinner end, the same which had been just contiguous to the north pole of one of the bars, attracted the north end of the needle, and that the thicker end, the same which had been contiguous to the south pole of the other bar, attracted the south end of the same needle.

This same piece of stone was then again put in between the bars, but in a contrary position; the thicker end now lying next to the north pole of one of the bars, and the thinner end next to the south pole of the other. After a few moments it was again taken out, and presented as before to the compass-box: when it was found that the thin end now attracted the south end of the magnetic needle, and that the thicker end attracted the north end of the same.

The piece of stone was then again placed between the bars as at the first, and being again taken out and presented to the compass-box; the thin end was again found as at the first to draw the north end, and the thick end to draw the south end of the needle.

This same piece of magnet was then again placed between the bars, but in a position at right angles to both the former, one of its sides being now contiguous to the north pole of one of the bars, and its other side to the south pole of the other. After which being again in a few moments taken out, and presented to the compass-box as before; it was found that the side which had been in contact with the north pole of one of the bars, attracted the north end of the needle, and that the other side which had been in contact with the south pole of the other bar, attracted the south end of the same needle: while the two ends of the stone in which the polarity was before observed, were now found to be indifferent to either end of the needle; so that the line of direction of the poles in the stone now lay at right angles to the position in which it was situated in the former experiments.

Mr. Knight then produced two steel needles, of the same sort as those which are usually fixed to the cards of sea-compasses. These needles were of the length of $5\frac{3}{10}$ inches, and weighed severally with their caps 7 dwt. 8 gr., and 7 dwt. 9 gr.; one of these was tempered and of a blue colour, and the other was quite hard. He also produced two iron weights, severally weighing 14 dwt. 22 gr., and 15 dwt. 7 gr., both nearly of a cylindrical form, but with one of the ends rounded off.

The 2 large bars were then placed in a line, as in the former experiments, but with their ends so near together, as only to admit of the cap of one of the needles between them.

The tempered needle was then placed flat on the bars, so that nearly one half of it rested on one bar, and the other half on the other, the cap lying between the two. The needle was pressed close to the bars in this position; after which the bars were drawn away, both at the same time contrarywise, till they were clear of the needle; and this operation was repeated 3 or 4 times: after which, that end of the needle which had rested on the northern part of one of the bars, was found strongly to attract the north end of the needle in the compass-box; and the other end, which had rested on the southern part of the other bar, was found to attract in like manner the south end of the same needle in the box. The power of attraction also acquired by this needle, appeared to be very considerable, lifting easily with either of its ends, the two iron weights above mentioned, when cemented to each other with wax, and weighing together 1 oz. 10 dwt. 5 grains. The hard needle was then applied to the bars like the other, and with the very same success, lifting also, as the other had done, both the weights together.

The 2 needles were then themselves applied to each other, first the northern half of the one, in a contrary direction, to the northern half of the other; and then the southern half of the first, in a like contrary direction to the southern half of the last; and from these several positions, they were severally drawn till they were clear of each other, and this several times successively: after which operation it was found that the tempered needle had lost so far its virtue, that its northern end had hardly any effect on the needle in the box; that its southern end even began to attract the contrary end of the needle from what it did before, and that it was no longer able to lift at either of its ends any sensible weight. But as to the hard needle, that still retained a considerable share of its former virtue; its ends still strongly drawing the same ends of the needle in the compass-box, as they drew before, and either of them lifting with ease the heavier of the two above-mentioned weights.

Mr. Knight then produced one of his common small magnetic bars; which being applied to the forementioned large bars, in the same manner as the needles had been applied to the same, but in a position contrary to that of its present polarity, it had its poles thereby counterchanged or inverted, and was found to lift at that which was now become its northern end, the weight of 6 oz. 8 dwt. 5 grains.

Lastly he produced one of his large artificial armed magnets, composed of several thin plates of steel cramped together, with which he acquainted us he had some time before lifted 36 pounds, and with which he did now actually lift before us 31 lb. 9¾ oz.

The tempered needle mentioned above, and which had nearly lost all its virtue, had the same again restored in great measure, on being touched in the common way, on the armed poles of this artificial magnet; after which it discovered a strong verticity, and was able to lift at one of its ends, the heavier of the 2 above-mentioned weights, viz. somewhat more than 3 quarters of an oz.

The hard needle, which still retained a considerable part of the virtue it had acquired by the touch of the large steel bars, was lastly touched also in a contrary sense, on the armed poles of this artificial magnet; by which it not only lost the polarity yet remaining, but acquired a new one the other way; it would not however after this last touch lift more than 9 dwt.

After reading this report, Mr. Knight produced before the Society the two large bars and all the other particulars above mentioned, with which he publicly repeated all the same experiments; which notwithstanding the disadvantageous circumstances of the place, succeeded perfectly in every particular, and to the entire satisfaction of all the company.

It was then further proposed, that the tempered needle, having its virtue again destroyed, should be touched on the fine armed terella belonging to the Society, which was the noble present of their late worthy member James earl of Abercorn, which is esteemed one of the best in England, and is said to have lifted in his lordship's hands upwards of 40 pounds: the same was immediately brought, and the needle, being touched with it, was found to have acquired a strong polarity, and to lift about the same weight, as when it was before touched on Mr. Knight's large armed artificial magnet; viz. about 15 dwt.

2. *Some New Experiments lately made with Artificial Magnets. By the same. June 4, 1747.* p. 662.—The apparatus for touching needles, sometime before shown before the R. S., was as perfect as he could have wished, as far as relates to the intended use of it: but the manner in which the two bars were disposed in their cases made their length rather incommodious, especially in those of the largest size. This made him desirous of trying if some method could not be found out of placing the bars parallel to each other without danger of weakening their force, by which means the cases would be reduced to half their length. He remembered that some years ago, before he had tried some experiments to this purpose, by placing some bars parallel and in contact, but so that their poles were turned different ways: in which position he found the virtue of some of them remained pretty entire, but that others were weakened by it. He imagined the reason of their losing their force was this; that the magnetic virtue was by degrees habituated to pass out of the side of one bar into that of the other in contact with it, and thus was hindered from arriving at the ends in its full vigour. The reason why some suffered more than others, was doubtless to be ascribed to their difference in temper. He repeated the experiment about 2 months before, with a little alteration. He placed the bars parallel with their poles in an alternate position, as before, but not in contact, having kept them at

the distance of about a quarter of an inch. Then he applied to their ends 2 pieces of soft iron. Each piece was laid across from the north end of one bar to the south of the other, in the same manner as the lifter is applied to the feet of an armed loadstone. The intent of this was to draw the magnetic virtue down to the ends of the bars, and to convey it through the pieces of iron from one to the other. In this condition he let them lie for about a month, and then tried if they would lift the same weight as before, which he found they did, and he thought with more vigour. After this he repeated the experiment with other bars of various sizes, and with the same success: he therefore now ventured to fit them up in cases in the manner just described.

The success of this experiment had led him to another improvement: he provided a case of brass that would just contain 2 bars. At one end of the case were fixed 2 feet, of soft iron, like those of an armed loadstone, the upper surface of which was within the case in contact with the ends of the 2 bars: which being parallel to each other, and their poles in an alternate position, the north end of one bar will be in contact with one of the feet, and the south end of the other bar will be in like manner applied to the surface of the other foot. On fitting a lifter to this new kind of armour, he was able to support a weight of about 6 pounds: the bars were kept asunder at the distance of about a quarter of an inch, by a slip of wood, which slides in between them.

An instrument thus constructed, seems capable of answering all the purposes for which load-stones are used; for when the bars are taken out of the case, they are fit for touching needles, or other magnetical uses, which may require single bars; when in the case, the whole together becomes an armed magnet, able to lift a considerable weight. And if we want to separate iron filings from those of other metals, the feet, and all the lower part of the case will take them up in great plenty, and by drawing the bars a little way out of the case the filings will fall off.

3. *Some further Experiments relating to the General Phenomena of Magnetism. By the same.* p. 665.—The cause of the surprising phenomena of the loadstone has hitherto escaped our knowledge, though diligently inquired after by men of abilities. Such a discovery is not to be made without long experience, and a great variety of facts: and the nature of the subject is such, that the more facts we are acquainted with, the more we find ourselves perplexed. The conclusions drawn from some experiments are seemingly contradicted by others: and yet these seeming contradictions are often very reconcileable on further experience. If what here follows will in any-wise contribute to remove these difficulties, it may not be unacceptable, though he should not so properly explain the nature of the cause, as the manner in which it acts.

PROP. 1.—The magnetic matter of a loadstone moves in a stream from one pole to the other internally, and is then carried back in curve lines externally, till it arrives again at the pole where it first entered, to be again admitted.

Exper. 1.—If we lay a magnetical body under a piece of paper or glass, that is strewed over with steel filings or magnetical sand, and by striking the table put the filings in motion, they will readily dispose themselves in such a manner as to represent, with great exactness, the course of the magnetic matter. Steel rendered magnetical is best for this purpose, because it is of a more uniform texture than loadstones, and will on that account exhibit a more regular appearance. By this experiment the curve lines in which the magnetical matter returns back to the pole where it first entered are accurately expressed by the arrangement of the filings. The largest curves are such as take their rise from one polar surface, and are extended to the other; being larger in proportion as they arise nearer the axis or centre of the polar surface. Those curves which arise from the sides of a magnetical body are always interior to those which arise from the polar surface; and are less and less in proportion to their distance from the ends. If any one should doubt whether the magnetical matter, which thus disposes the filings, is really moving back in a direction contrary to that with which it passes through the magnetical body, let him try it in different parts with a small compass needle, and the fact will appear beyond dispute.

Exper. 2.—The larger the distance is from pole to pole in different magnets, the larger will these curves be. This appears from examining magnets of different lengths. And this is the reason why, in the same magnet, the curves are less in proportion to their greater distance from the ends of the bars. For the poles from whence these curves arise are proportionably nearer each other.

Exper. 3.—If the south pole of one magnet be opposed to the north of another, most of the magnetic matter is carried directly out of one into the other: and does not return back in curve lines till after having passed through both magnets. It appears from the arrangement of the filings, that the magnetic matter proceeding from the polar surface, does not now diverge from the axis as before, but runs more in straight lines, till it arrives at the polar surface of the other magnet. The curves arising from the sides, which before were bent towards the opposite end of the same magnet, are many of them now bent the contrary way, towards the corresponding sides of the other magnet. Those which are not bent the contrary way, are such as are too remote from the opposed pole of the other magnet to be influenced by it; and therefore continue their natural course.

Exper. 4.—While the bars are in the position of the last experiment, if a small loadstone be placed in the stream running from one to the other, in any position whatever, the stream will pass through the stone: which, being again removed, will be found to have a polarity exactly in the direction of that stream.

Exper. 5.—If the north or south poles of two magnets be opposed to each other, the filings will exhibit the appearance of two streams meeting; and the curves of each will all be turned towards the opposite pole of the same magnet. The appearance is altogether the same, whether the two north or two south poles be opposed to each other. So that it is not to be determined, from any of these experiments, at which of the poles the magnetic stream enters. As we have some reason to think it enters at the north pole, we may suppose that to be the case, without danger of error; provided we build nothing on the supposition, but what would hold good, mutatis mutandis, if the contrary should be true. This being supposed, when the south poles are opposite, the two streams coming out at them are directly contrary, by which the magnetic matter is accumulated, and therefore diverges so much the faster to return back to the north poles. When the north poles are opposed to each other, the streams of magnetic matter returning from the south poles are directly contrary; and, by crowding at once towards each polar surface, are accumulated between them, and converge towards them so much the faster.

These 5 experiments seem sufficient to establish the truth of the proposition; and many more might be produced to the same purpose.

PROP. 2.—The immediate cause why two or more magnetical bodies attract each other, is the flux of one and the same stream of magnetical matter through them.

Exper. 6.—It appeared in the 3d experiment, that when the south pole of one magnet was opposed to the north of another, a stream of magnetic matter was carried from one to the other, and did not return back to the pole where it first entered, till after having passed through both bars: and it is needless to observe that two bars in this position are in a state of attraction. The 5th experiment showed, that when the two south or north poles were opposed, there was no stream common to both. Now it is well known, that magnetical bodies in this situation are so far from attracting, that they strongly repel each other. If the 3d experiment be repeated, with the magnets placed at different distances from each other, we shall find that more of the magnetical matter will pass from one polar surface to the other, in proportion as the distance between is less. The attraction is therefore greater as the distances diminish. And at distances where none of the magnetic stream passes from one magnet to the other, there is no sign of attraction. So that this cause is not only co-existent with the effect, but also proportionable to it.

Exper. 7.—If a piece of soft iron, which has no fixed magnetism, be any where placed in the magnetical stream, it will be in a state of attraction while it remains in that stream, and no longer.

Exper. 8.—A ball of soft iron, in contact with the pole of a magnet, will attract a 2d ball, and that a third, and so on, till the stream become too weak to produce an attraction sufficient to support a greater weight.

Exper. 9.—Having hung a number of balls to each other, by applying the first to the north pole of a magnet, on presenting the south of another magnet to one of the middle balls; all those below it will be deprived of the magnetic stream, and instantly losing their power of attraction fall asunder: the ball to which the magnet was applied will be attracted by it, aud all the others will still remain suspended. But if the north end of a magnet be presented, then the ball to which it is applied will also drop.

Exper. 10.—In a magnet unarmed, the magnetic stream is carried back on all sides in curve lines to the contrary pole, as was seen in Exper. 1; but when armour is applied to each pole, the magnetic matter is conducted to the feet of the armour; and a lifter being thus applied to the feet, the whole stream coming out at one pole is carried back through it to the other; by which means the lifter is made to adhere to the feet of the armour with very great force. When the lifter is thus in contact, the magnet seems externally to have lost the greatest part of its force; though in reality it never acted with more. If instead of the lifter, we suspend a number of iron balls in contact, they will adhere together, and hang like a bracelet between the two feet; the returning stream passing now through them, as before through the lifter. Present the pole of a magnet, and they instantly fall asunder.

Prop. 3.—The immediate cause of magnetic repulsion, is the conflux and accumulation of the magnetic matter.

It appeared in the 5th Exper. that the same poles of 2 different magnets being opposed to each other, there was a conflux and accumulation of the magnetic matter; and we find by experience that all magnetical bodies in a like situation are in a state of repulsion.

Exper. 11.—Two small bars, the one hard, the other of a spring temper, being both magnetical matter, were opposed to each other, south to south; the filings produced the same appearance of repulsion, as described in the 5th experiment; then the bars being brought so near as to touch each other at the same poles, the repulsion was instantly changed into attraction.

On the Usefulness of Thermometers in Chemical Experiments; and concerning the Principles on which the Thermometers now in Use have been Constructed; with the Description and Uses of a Metalline Thermometer. Newly Invented by Cromwell Mortimer, M.D., Sec. R.S. &c. p. 672. *Appendix to* N° 484.

Chemistry, being the most extensive branch of experimental philosophy, has furnished mankind with the greatest number of curious and useful discoveries: for not only the art of separating metals from their ores, of which metals are formed such variety of useful instruments, but likewise cookery, which is so much concerned about the food of mankind during health, and also pharmacy, which furnishes medicines for restoring health when lost, the art of dying, and many other useful manufactures, all owe their improvements to this science; many of which have occurred unexpectedly to the operator while he had something else in view: but in many cases the chemists complain, that, having once accidentally hit on a curious experiment, on endeavouring to repeat it, they have never been able to make their process succeed exactly, as it did the first time, though they made use of the same materials, in the same quantity, and conducted the process through exactly the same operations. Where then must the cause of the miscarriage lie? Surely in the degree of heat made use of in the two experiments; for in many common operations, how usual is it for a preparation to be spoiled either by too little, or most commonly by too much fire, too long or too short a time applied! In order therefore to prevent these many miscarriages, Dr. M. advises the chemist, in his operations, to observe his clock with as much exactness as the astronomer does in his observations; and in order to know to a certainty the very degrees of heat he ever made use of in any process, that so he may be able to repeat and continue the same again in any repetitions of the same experiment. Let him have his laboratory furnished with various kinds of thermometers, proportioned to the degree of heat he intends to make use of. He will find these instruments as useful to him in his processes, as they have proved to the curious gardener in his stoves, who by them is taught to keep his plants in the same degrees of heat as are natural to them in their respective climates; which has been set forth in tables, after a very ingenious manner, by Mr. Sheldrake of Norwich. And besides enabling him to perform his operations with more exactness, these instruments would save him a great deal of fuel; for as liquors while boiling are not capable of receiving a greater degree of heat, all fuel which is used more than to keep them in that state, is useless; and the like happens in many other cases.

These instruments would also be of great service to maltsters, brewers, distillers, and vinegar-makers; for, by thermometers placed in different parts of the heap of wetted malt, the proper heat for its sprouting might be determined, and then regulated: the same for the heat of the kiln when the malt is spread on it. By thermometers the brewer may ascertain the heat of the water when he pours it on the malt, the heat of the wort when he sets it to work, and the heat while working: and in like manner the distiller and vinegar-maker, in short, every artificer who employs heat in his business, may by these instruments be certain of every degree necessary in each part of his work.

Many experiments show that all known bodies, whether fluid or solid, increase their bulk, or rarefy, by an addition of heat; and on the contrary, contract or become more dense by the diminution of heat, which is the presence of cold: and these alterations are always more or less sensible as the bodies are more or less dense.

The air we live in, as it is the most rare and light fluid, so are its alterations the most sensible; and indeed he knows of no experiments which determine how far it is capable of being expanded by heat, or condensed by cold; only we find that it will make its way through any fluid in which it lay dormant, when its elastic property is rouzed by the approach of such a heat as will make the fluid boil. On the other hand, when compressed by a fluid so contracted by cold as to freeze, or become solid, its elasticity will only bear a certain degree of compression, till the force with which it endeavours to restore itself, exceeds the force by which the parts of the solid, that confines it, adhere to each other, and so bursts its prison; as we often see during hard frosts in ice, and likewise glass, and other hard bodies, whose parts cannot stretch.

Next to air is alcohol, or the highest rectified spirits of wine: this and water, and all other liquids, are capable of receiving no greater degree of heat than what makes them boil, as was first demonstrated by M. Amontons; but that ingenious inventor of the quicksilver thermometer, Mr. Fahrenheit, has discovered, that when the barometer marks a greater pressure of the atmosphere, the same liquor will receive 8 or 9 degrees more of heat, than when the barometer is at the lowest. Hence Boerhaave gives the hint, that from nice experiments being made of the different degrees of heat, marked by a thermometer in boiling water, compared with the different heights of the barometer, and tables formed on them, a thermometer applied to boiling water might, at sea, where the motion of the ship hinders observations with the barometer, serve to determine the difference in the gravity of the atmosphere.

These, and all other liquids, by a certain determinate degree of cold peculiar to each sort, lose their fluidity, and freeze, or become solid, but not in the same order as by heat they boil; for, by cold, oil or water is sooner frozen than spirit of wine, though this will boil sooner than oil or water. All solid bodies likewise, as minerals, metals, and even stones, will become fluid, or melt, at a certain degree of heat peculiar to each species; and, when thoroughly melted, it is probable they are capable of receiving no higher degree of heat; and, on the absence of that heat to a certain degree, they all return to their natural solid state. Hence we may reasonably conclude, that solidity is the natural state of all bodies; and that some are only accidentally fluid, because their constitution is such as to melt by those degrees of heat which our atmosphere is most commonly subject to. All solid bodies are observed to contract into smaller dimensions by cold, and gradually to expand at the approach of heat, till at last, being by heat forced to the greatest degree of expansion, the particles of which they are composed losing their cohesion, they become fluid; but no experiments have yet been made, which determine whether solids, exposed to cold beyond certain degrees, will cease to contract any more.

The learned Dr. Muschenbroek, prof. of astronomy at Utrecht, and F.R.S. has lately invented a very ingenious instrument, which he calls a pyrometer, and which Dr. Desaguliers has made some improvements on*; a full description of which he has given in his course of Experimental Philosophy, vol. 1, p. 421, &c. By this instrument the elongation of rods of several kinds of metals, by the approach of a certain number of flames of a spirit-lamp, and likewise their as sudden contraction, on the extinguishing one or more of those flames, is rendered sensible to the eye: which sufficiently evinces the matter of fact, and puts it beyond all doubt.

From the property of bodies contracting and expanding in cold and heat, have all thermometers been constructed, that have ever been made use of, to observe and compare the different degrees of Heat, either in our atmosphere, or in other bodies. The most simple and most sensible of any, is that aërial thermometer described by Mr. Boyle, in his new experiments and observations touching cold. It consists of a glass bubble, with a very slender stem, not thicker than a raven's quill. The bubble is left full of air, and a few drops of water being conveyed into the stem in an erect position, will there remain suspended to a certain height; but, by the least addition of heat, the air in the bubble expanding will push the water up higher; or, by the approach of cold, the air contracting, the water will fall lower in the stem. This instrument may be of use in small degrees of heat, and in cold, till the water begins to freeze, when it becomes useless.

The next in order of sensibility, is that first invented by Cornelius Drebbel of Alcmar, and improved by Boerhaave. It consists of a hollow glass lens, joined to a stem of a larger size than in the preceding, and a basin into which the end of the stem is inverted. The air in the lens must be so much rarefied, and the stem being inverted into a tinged liquor in the basin, the liquor will rise up some way in the stem; then, by the application of heat to the lens, the liquor in the stem will be pushed down, and by cold the liquor will rise up. This instrument will give notice of the smallest changes in the air; but it cannot be immersed into any liquid for chemical experiments, unless the stem be made much longer, and bent down in form of a syphon: and even then it would be very unhandy, and like the preceding would never serve for any degree below what would freeze the liquor made use of, nor for any above what would force out the confined air through the liquor in the basin. Besides, both these instruments, being subject to the pressure of the atmosphere, they are not proper, without comparing the

* This instrument hath since been greatly improved by that ingenious watch-maker Mr. John Ellicot, F.R.S. See Phil. Trans. N° 443.—Orig.

barometer at the same time, to determine the degrees of heat at a great distance of time between each experiment.

The most usual kind of thermometers, is that described in the account of the experiments by the academy of Cemento; which being the common ones, made of spirit of wine tinged, it is needless to describe. The bounds of the degrees of heat which these will measure, and which is commonly called the range of the instrument, are from the degree which freezes spirit of wine, up to that which makes it boil. The spirit-thermometers, commonly made here in London, are so graduated, that when the spirit is rarefied to the degree that the most sultry sunshine, commonly known in our climate of $51\frac{1}{2}^{\circ}$ N. lat. can raise it, there is placed the mark 0, or degree of no cold. Some few are marked 10 or 20 above this, when intended to be used in hotter climates; but all are graduated downwards from this: so that 45° is the point of temperate, and 65° the point of freezing, and 100° is placed just above the ball. But the most accurate spirit-thermometers, are those lately made by M. Reaumur, member of the Royal Academy of Sciences at Paris; he has taken a great deal of pains and used great exactness, in fixing the certain points of freezing of water, of temperate air, and of boiling water. He determines the freezing point, by leaving his thermometer a considerable time in water, into which is put a good deal of ice, at a time when the water would not freeze of itself; and this he marks 0, or the degree of no heat; and his scale is marked with numbers running downwards from 0, measuring the degrees of cold, and upwards measuring the degrees of heat: at $10\frac{1}{4}$ upwards he marks the point of temperate, which he determines by placing his instrument in a subterranean cavern, which is neither affected by frost nor sunshine, but is observed to keep an equable temperature all the year round; such as deep cellars and wine vaults commonly do. In boiling water he finds that his thermometer rises to his 80th division,* or 80 degrees, which are formed by dividing the spirit when condensed to the freezing point, into 1000 equal parts; so that, with the heat which makes water boil, the spirit is expanded only $\frac{80}{1000}$ more than with the cold which freezes water.

These spirit-thermometers are of use in experiments where somewhat greater cold than the freezing of water is required; but they can never be of use in any degrees of heat beyond the boiling of the spirit itself; because it then becomes volatile, or rises up in steam, and not only expands no more, but likewise the quantity is diminished by the particles which fly up from the surface of the liquor, and are suspended in the top of the tube.

* But, with submission to so great a man, I cannot apprehend that his thermometers, when the spirits are raised up to 80, do mark any greater degree of heat than their own specific boiling heat, which, if they be alcohol, or the most rectified spirits, answer to 174 of Fahrenheit's scale; if of the strength of common brandy, to 190.—Orig.

Many have filled their thermometers with various sorts of oils:* these indeed will measure many degrees above the boiling of water, till they boil themselves; and then they have the same defect as the spirit ones just mentioned, which is the liquor losing of its bulk by evaporation; and they congeal much sooner than water, and so are useless in measuring any degrees of cold.

The most useful instruments, as they comprehend the largest range, are the mercurial thermometers, brought into use by that ingenious artificer Fahrenheit, F.R.S. See Phil. Trans. N° 381: but to do justice to a most worthy member of the Royal Society, viz. Dr. Halley, he first gave the hint, and even proposed the making thermometers of quicksilver long before Fahrenheit's time, see Phil. Trans. N° 197. However, Fahrenheit deserves thanks from the world for having brought these instruments into use, because they will measure the greatest degrees of cold yet known; for no cold hitherto observed has been able to freeze or render mercury solid:† and in measuring heat, they go far beyond boiling water, even beyond the melting of tin or lead. Fahrenheit begins his scale from 0, the point to which the mercury has been observed to fall by the greatest cold in Ysland; and computes, that the mercury then ‡ occupies 11124 parts. This is his point of no heat. Then reckoning upward from this, he finds that when the mercury is rarefied only 32 parts or degrees more, common water just begins to freeze: in a temperate air it will rise to about 60. The most sultry sunshine seldom raises it to 90; the heat of an animal body to 96; the boiling of alcohol to 174; the boiling of water to 212; and before the mercury itself boils, it will rise to 600.

The following curious and surprising experiment of Fahrenheit's, concerning the artificial production of cold is related by Boerhaave, is his Chemistry, Tom. I. p. 164. Fahrenheit had a mercurial thermometer made with so long a stem, that he could carry down the scale 76 parts or degrees below 0. With this instrument he found, that cold might be produced by gradually pouring spirit of nitre on powdered ice, till the mercury would subside to 40° below 0, that is 72°§ lower than the cold which freezes common water. Boerhaave, ib. p. 161, mentions a very pretty way of determining the freezing point: he advises to hang the thermometer free in the open air, not against any wall or building; and near it hang a piece of very fine linen or muslin just dipped in clean water: when this begins to grow stiff, you will find the mercury stand at about the 33d degree; and

* See Dr. Martin's essays Med. and Philos. p. 225. Sir I. Newton's thermometer was made of linseed oil. See his scale of heat, Phil. Trans. N° 270, p. 824.—Orig.

† That this has since been done, will appear, in a future volume of the Transactions.

‡ See Boerh. Chem. Tom. I, p. 174.—Orig.

§ But what is this to the marvellous natural cold of Siberia, 120° below 0? See the preface to Gmelin's Flora Siberica.—Orig.

it will also stand at the same height when a hoar frost appears on the ground; which he considers as a certain sign of the beginning of freezing.

Having thus given an account of the several sorts of thermometers hitherto used, and what degrees of heat they are proper to measure, we find none of them capable of measuring the greater degrees of heat, which are the most commonly made use of by the chemists in many of their operations. Besides, all the above instruments, being made of glass, are easily broken by accidents, and as liable to crack of themselves, by being taken out of a great heat, and too suddenly exposed to cold. Dr. M. therefore considered whether the above mentioned property of solids, and especially of metals contracting with cold, and expanding with heat, might not be applied to the construction of an instrument capable of measuring all degrees even of the greatest cold, as well as the greatest heat, to the melting copper or iron, which require more heat than any other metals to melt them. Though the alterations in metals are but small, in respect of those in spirits, or even mercury, yet it being found that iron, e. g. becomes $\frac{1}{60}$ longer* when red-hot, than when of its natural temperature; and Dr. Derham, in his last paper read before the R. S. concerning the vibration of pendulums, says, that a rod $39\frac{126}{1000}$ inches long, becomes $\frac{1}{10}$ inch longer than its natural dimensions in temperate air, by being exposed to heat equal to that of the human body; $\frac{8}{100}$ inch longer in hot sunshine; that it was $\frac{2}{10}$ or $\frac{1}{5}$ inch longer than its natural state, by being heated in a flaming heat; that it became $\frac{7}{150}$ shorter than its natural length by being quenched in cold water; and still $\frac{8}{100}$ shorter by being put into a mixture of salt and snow. From which experiments we may conclude, that from Fahrenheit's cold of 40 below 0, to the greatest heat iron can bear without melting, a rod of three feet long will have about $\frac{1}{4}$ inch increase; which increase of length will be range enough to make all the intermediate degrees observable on an instrument.

Suppose, in fig. 1, pl. 9, AB a rod of iron at its natural length by the heat of the atmosphere, placed upright on one end; on the point of which rests a bar CD moveable on an axis at a; and that, by making a fire about the end B of the rod, till it is just ready to melt, the rod will increase in length A b, and consequently push the bar into the situation c d. Now it is obvious that though the elongation of the rod A b be even scarcely perceptible to the eye, yet if on the bar CD the distance a A from the axis to the place where the rod B A pushes against it be very small, and the other part of the bar aD very long, the arch Dd may be increased at pleasure, so as to bear to be divided into any number of divisions that shall be found necessary: for the arch Dd will always be to the arch cc in the same proportion as the distance Da is to ac; and likewise the chords of these arches Dd and Ab will be in the same proportion; $\gamma\ \delta$, is the situation of the lever on the level; and

* Vide Sturm. Coll.—Orig.

if it be found inconvenient to make the arm AD so long, as to make very minute alterations in the length of the rod AB easily observable, this inconveniency may be readily removed by having a second bar EF, turning on the axis g, whose arm gE bearing up against the extremity D of the first bar or lever, will rise with it, or be pressed down by it; and the other arm gF being lengthened at pleasure, the arch Ff will be as large as you find convenient; or even a 3rd and 4th lever may be added.

The instrument, as executed by Mr. Jackson, in the year 1736, on the advice of Mr. Graham and Mr. Ellicot, is represented in fig. 2 and 3.

In fig. 2, AB is a round rod of steel or brass a quarter of an inch thick, and 3 feet 1 inch long: when the rod is of brass 3 feet long, the point A must be of steel 1 inch long, to prevent its wearing away, or losing its point; which conical point is made to screw on and off. CD, cd, are two iron supporters, joined by a flat cross bar at bottom Dd 2 inches long, in the middle of which is a point $\frac{1}{8}$ inch high under B, which goes into a hole at the bottom of the rod B, and serves to keep the rod in its place at bottom, as the cross bar ** having a hole in it, through which the rod passes, does in the middle or about $\frac{2}{3}$ up the supporters, and the point A goes into a small hole in the under side of the lever; all which keep the upright rod firm and steady in its place. The iron supporters are flat, or parallel to the front of the machine from c to x and c to x, where they are twisted half round, so that the lower parts XD, xd stand at right angles with the upper parts. This contrivance gives the freer access to the rod for the sand or fluid into which the machine is set to measure its heat, the supporters standing 2 inches asunder at Dd; and that the degrees of heat may be compared uniformly in different experiments, the bottom of the rod must always be immersed to the same height in the matter to be examined; and therefore he made a mark, a small furrow ✝ quite round the rod, $1\frac{1}{2}$ inch from the bottom B. For the deeper the rod is immersed into any matter, it will be lengthened the more by the same degree of heat.

EF is the lever, which turns on an axis G. At F is fastened a string, which, passing twice round the small pulley H, has a weight I hanging to the other end of it, of about $\frac{1}{4}$ lb. being enough to keep the string always stretched. At the other end E of the lever is hung another weight L, which must be heavy enough not only to counterbalance the longer arm GF, but press down on the point A with a weight sufficient to keep it steady.

MNO is the back part of the plate, like the dial plate of a clock made of brass, the front of it being as in fig. 3. The pulley H turns on an axis C in fig. 3, which goes through the plate, and on the other side or front of the dial plate carries a hand or index AB.

N. B. G being the fulcrum of the lever, fig. 2, the distance GA being very small, and the distance GF being very great, the smallest motion at A will produce a very great one at F, and therefore the index will turn very sensibly on the plate.

The proportions of the rod and lever are discretionary; his rods, both of steel and brass, were 3 feet long in one solid piece, but they had each a point or cone of steel 1 inch high, that screws on the top at A. The lever has 4 inches from E to A, 1¼ inch from A to G, and 12 inches from G to F; the distance of G above C is 1½ inch, the brass pulley H is ¼ inch diameter; all the other parts of the machine are of oak. The main support or pillar PQ is 1 inch square, 2½ feet high, and at bottom is let through a groove at Q made in a large heavy block or pedestal of wood RS. In this groove the pillar may be raised higher or lower to adjust the height of the pillar to the situation, which the bottom of the rod AB may require in different experiments; and it is to be fixed in that place by a screw at T, which goes through the front of the block, and presses against the bottom of the pillar.

Fig. 3 represents the dial plate, or front of the plate marked MNO in fig. 2. It is a plate of brass, with strong paper glued on it, and may be of any size, Dr. M.'s is 11 inches over. AB is the hand or index, which slips on very stiff on the axis C, that carries the pulley H in fig. 2. The outer circle is to be left wide enough to contain the chemical characters or marks to be made on it; the arch DE contains the divisions of Fahrenheit's mercurial thermometer; and the arc FG those of Reaumur, or the spirit of wine thermometer.

To adjust this instrument for use, place the bottom of the rod B in fig. 2, immersed up to the mark ✝ in cold river or rain water, in a vessel proper to be set over the fire; and when it has boiled for a quarter of an hour, turn the index AB in fig. 3, till it stands in the horizontal position, as at B, being the point of boiling-hot water, and which answers to division 212 on Fahrenheit's arch. Then take it out of the water, and dry it, by holding it a little over the fire: and now great care must be taken, that nothing alters the situation of the index on the axis, a nut to screw on the axis at C may be a good way to keep it fixed. If the instrument be left to cool in the air, the index will fall below B, showing the degrees of cold, or less heat than boiling water; and if put into melting tin, lead, &c. it will show the degrees of heat above boiling water. A brass rod will serve for an instrument to measure the greatest degrees of cold, and all the degrees of heat, to the melting of silver or gold, but to make one to measure greater degrees of heat, the rod must be of steel, or the finest iron. A rod of brass, according to Dr. Muschenbroek's experiments, was found to lengthen 377, when one of iron lengthened only 230 parts. An iron rod, being regulated by boiling water as above directed, will measure not only the heat of melted tin and lead, but of silver, gold, and copper, and will even show the degree when iron itself begins to melt, which will be the greatest degree of elongation of the rod just before its bottom runs; and he imagines, that an instrument may be constructed with supporters, and a rod made of tobacco-pipe clay,* which, being regulated by boiling mercury, for it must never touch water, may be adapted to measure still greater degrees of heat, till the materials themselves melt into glass.

Dr. M. advises, that not only the scale of this kind of thermometer, but also of all others, be determined by experiments, without regarding any equality as to measure between the divisions, and that in every individual that shall be made; for a difference in the length and thickness of the rods will make a difference in the scale, as much or more than the inequality in the cavity of the stem, or glass tube of other thermometers, which can never be just, if applied to a scale whose divisions are made equal; unless the cavity of the stem be perfectly equal, which it is impossible for any workman to do, and which is very seldom, if ever, hit on by chance. Therefore, in these instruments, let the point B in fig. 3, or the horizontal position of the index, be the situation of the index when the rod has stood a quarter of an hour in boiling water; there mark ▽ boiling on the outer circle; on Fahrenheit's arch mark 212. Then set the machine up to the mark + into melting tin, which is the metal that melts easiest. When the rod is arrived to its greatest elongation in that metal, inscribe the character ♃ on the outer circle; do the like with lead, and set the character ♄ at it. At the boiling of mercury put the mark ☿, and on Fahrenheit's arch mark 600, the utmost extent his mercurial instruments can measure: then proceed to the melting of silver, and set the mark ☾; at the melting of gold place the mark ☉; at the melting of copper place the mark ♀; at the melting of iron place the mark ♂, the most difficult to melt of all metals.

As the divisions pointed out by the index will be different with rods of different metals or substances, we may make different circles on the plate for the range of the different rods, and mark them; the iron rod, the brass rod, the clay rod; and set the several marks above specified on each circle apart; or, to avoid confusion, we may have a different instrument for each kind of rod.

According to Fahrenheit's scale, the heat of the strongest sunshine is at about 80; spirit of wine boils at 176; water at 212; the lixivium of salt of tartar at 240; spirit of nitre at 242; oil of vitriol at 546; quicksilver at 600.†

As all chemical digestions, where an equable heat is to be continued for some time together, will come in between hot sunshine and the boiling of quicksilver,

* This idea, of using argillaceous bodies as thermometers, for measuring very high degrees of heat, was afterwards further acted on by Mr. Josiah Wedgwood, in vol. 72, &c. of the Philosophical Transactions.

† See Augustin. Grischow Thermometria comparata accuratius, et Harmonica. Berolini 1740, 4to. p. 10.—Orig.

a thermoscope of that range will be sufficient for common uses; and therefore one fitted with a brass rod will answer these purposes.

In large furnaces for running down ores, or melting great quantities of metal together, it is not possible to place such an instrument; but then in lead and tin there may be small outlets contrived, into which some of the melted metal may be permitted to flow, and remain in contact with the same body of metal within, where the instrument may be placed; and for placing a thermoscope in iron, copper, or glass furnaces, there may be a place contrived, which shall not open into the furnace, but have the thickness of a stone or brick left between; on which the instrument may be placed; and though in such a situation it will not measure the actual heat within the furnace, it will always give the relative or comparative heat in the like circumstances at different times, and so show how to regulate the heat within.

Though a chemist shall have one of these instruments to measure the heat he may have used in any experiment, and have noted down the several degrees made use of, and the time each lasted, he still labours under another difficulty, which is the not being able to command any required heat, and that it shall last a certain required time, unless it be below that of boiling water, which may be procured and continued by various contrivances of lamps, either of spirits, or of oil; but how to continue a fire for 12 or 24 hours together, without attendance, which shall continually keep quicksilver boiling, lead in fusion, or may be let down so low as not to exceed the heat of sunshine, and then be raised again, and that without letting out the fire, or moving the vessels, may seem almost impracticable; but by an improvement of the furnace the ancient chemists called their athanor, he thinks it may be effected.

Remarks on the foregoing paper. By the Rev. Stephen Hales, D.D. p. 693. —What I intended to do, says Dr. Hales, was only this, viz. to get a leaden wire, of such a size and strength as to bear its own weight, to have it as long as the longest gun-barrel I could procure, and to have it sustain a lever as above; then to pour boiling water into the barrel, for a long time, till the lever rises no more; the water to have vent at the bottom, yet so as to have the gun-barrel always full of water; the breech-pin to be out, and the leaden rod to rest on a piece of wood set upright, according to the course of its fibres, not sidewise. To give at the same time to a mercurial thermometer the heat of boiling water. Then to take the freezing point of the leaden and mercurial thermometers; and afterwards to graduate all the intermediate degrees, from the mercurial thermometer on the leaden thermometer, as they occur. Thus a standard thermometer may be made to graduate others by; but I need not now set about it, since that is done above.

"All solid bodies are observed to contract with cold." I have found that wood does not contract or dilate lengthwise with heat or cold. I am told that Mr. George Graham is about making this experiment, as I am also, in order to regulate pendulums.—I fear that Boerhaave's wet linen, which is so thin, may begin to freeze before all the mercury or spirit of wine in the ball of the thermometer has the same degree of cold; though hanging there long before and after freezing will bring it pretty near.—"A rod of iron 3 feet long will have about $\frac{1}{4}$ inch increase," or $\frac{1}{144}$ part.

Continuation of a Paper concerning Electricity. By William Watson, F. R. S. printed in these Trans. N° 477,* p. 695.

As water is a non-electric, and of consequence a conductor of electricity, there was reason to believe that ice was endowed with the same properties. On making the experiment, Mr. W. found his conjectures not without foundation; for on electrifying a piece of ice, wherever the ice was touched by a non-electric, it flashed and snapped. A piece of ice also held in the hand of an electrified man, as in the beforementioned processes, fired warm spirit, chemical vegetable oils, camphor, and gunpowder prepared as before.

Prop. 1.—In common with magnetism, electricity counteracts, and, in light substances, overcomes the force of gravity. Like that extraordinary power likewise, it exerts its force in vacuo as powerfully as in open air; and this force is extended to a considerable distance through various substances of different textures and densities.

Corol.—Gravity is the general endeavour and tendency of bodies towards the centre of the earth; this is overcome by the magnet with regard to iron, and by electricity with regard to light substances both in its attraction and repulsion; but Mr. W. has never been able to discern that vortical motion, by which this effect was said to be brought about by the late Dr. Desaguliers and others, having no other conception of the manner of its acting than as rays from a centre, which indeed is confirmed by several experiments: one of which, very easy to be tried is, that if a single downy seed of cotton-grass be dropped from a man's hand, and in its fall come within the attraction of the rubbed tube, the down of this seed, which before seemed to stick together, separates, and forms rays round the centre of the seed. Or if you fasten many of these seeds with mucilage of gum Arabic round a bit of stick, the down of them, when electrified, which otherwise hangs from the stick, is raised up, and forms a circular appearance round the stick. As these light bodies are directed in their motions only by the force impressed upon them, and as their appearance is constantly radiatim, such appearance by no means squares with our idea of a vortex.

* See p. 151 of this volume.

Prop. 2.—In common with light, electricity pervades glass, but suffers no refraction from it; having, from the most exact observations, found its direction to be in right lines, and that through glasses of different forms, included one within the other, and large spaces left between each glass.

Corol.—This rectilineal direction is observable only as far as the electricity can penetrate through unexcited originally-electrics, and those perfectly dry: nor is it at all material, whether these substances are transparent, as glass, semidiaphanous, as porcelain, or thin cakes of wax; or quite opaque, as thick woollen cloth, as well as woven silk of various colours; it is only necessary that they be originally-electrics. But the case is widely different with regard to non-electrics; in which the direction, given to the electricity by the excited originally-electric, is altered as soon as it touches the surface of a non-electric, and is propagated with a degree of swiftness scarcely to be measured, in all possible directions, to impregnate the whole non-electric mass in contact with it, or nearly so, however different in itself, and which must of necessity be terminated by an originally-electric, before the electricity exerts the least attraction; and then this power is observed first at that part of the non-electric the most remote from the originally-electric. Thus, for example, by an excited tube held over it, leaf-gold will be attracted through glass, cloth, &c. held horizontally in the hand of a man standing on the floor, and this attraction is exerted to a considerable distance. On the contrary, the rubbed tube will not attract leaf-gold, or other light bodies, however near, through silver, tin, the thinnest board, paper, or any other non-electric, held in the manner beforementioned. But if you rub the paper over with wax melted, and by that means introduce the originally-electric therein, you observe the electricity acts in right lines, and attracts powerfully.

Prop. 3.—Electricity, in common with light likewise, when its forces are collected, and a proper direction given to it on a proper object, produces fire and flame.

Corol.—The fire of electricity is extremely delicate, and sets on fire, as far as yet experienced, only inflammable vapours. Nor is this flame at all heightened by being superinduced on an iron rod, red-hot with coarser culinary fire, as in a preceding experiment; nor diminished by being directed on cold water. However, to know if this flame would be affected by a still greater degree of cold; he made an artificial cold, by which the mercury, in a very nice thermometer adjusted to Fahrenheit's scale, was depressed in about 4 minutes, from 15 degrees above the freezing point to 30 degrees below it: that is, the mercury fell 45 degrees. From this cold mixture, when electrified, the flashes were as powerful, and the stroke as smart, as from the red-hot iron. This experiment seems to indicate, that the fire of electricity is affected neither by the presence nor absence of other fire. For, as red-hot iron, by Sir Isaac Newton's scale of heat, is fixed to 192 degrees, and as the ratio between Sir Isaac's degrees and Fahrenheit's is as 34 to 180, it necessarily follows, that the difference of heat between the hot iron and the cold mixture is 1040 degrees; and yet this vast difference makes no alteration in the appearance of the electrical flame. We find likewise, that as the fire, arising from the refraction of the rays of light by a lens, and brought to a focus, is observed first at some small distance from their surfaces, to set on fire combustible substances; the same effect is produced in like manner by electrical flame.

A Sequel to the Experiments and Observations tending to Illustrate the Nature and Properties of Electricity. By the same. N° 484, p. 704.

Mr. W. caused a machine to be made for electrical purposes; the wheel of which was 4 feet in diameter. In the periphery of this wheel were cut 4 grooves, corresponding with 4 globes of 10 inches diameter, disposed vertically at about 3 inches distance from each other. One, two, or any number of these globes might be used at pleasure. They were mounted on spindles of 2 inches diameter, and their mean motion round their axis was about 1100 times in a minute. As it is next to impossible to have these globes blown and mounted perfectly true, he had the leather cushions, with which they were rubbed, stuffed with curled hair, an elastic substance, that the globes in their rotations might be as equally rubbed as possible.

He lined one of these globes to a considerable thickness, with a mixture of wax and resin, to observe whether the electricity would be the sooner or more strongly excited; but no difference was found in the power of this globe from the others, which were without this treatment.

The power of electricity is increased by the number and size of the globes to a certain degree; but by no means in proportion to their number and size; therefore, as the bodies to be electrified will contain only a certain quantity of electricity, when that quantity is acquired, which is soonest done by a number of globes, the surcharge is dissipated as fast as it is excited.

After the globes had been a few times used, he found he had a much greater quantity of electrical power, with much less labour than when he used only tubes. He could attract and repel light substances at a much greater distance than before; fire spirits of wine, camphor, and all other substances whose vapours were inflammable, with great ease, and at any distance, with non-electrics placed on originally-electrics, though not equally easy when the weather was moist.

He discovered with this machine, and communicated to several members of this Society, several of the experiments said to be first made by M. le Monnier, at Paris, before the letter communicating them was received by our most worthy president from thence.

He had another machine made, which carried a globe of 16 inches diameter. He united the power of this large globe with that of 3 of the others beforementioned; but found the strokes from the excited non-electrics not increased according to expectation. In 2 experiments indeed, where the dissipation of the whole power of these globes was visible as fast as it was excited, the effect of this additional globe was very considerable. The first was, when 2 pewter plates were held, one in the hand of an electrified man, and the other by one standing on the floor; when these plates were brought near each other, the flashes of perfectly pure and bright flame were so large, and succeeded each other so fast, that when the room was darkened, he could distinctly see the faces of 13 people who stood round the room. The other was from a piece of large blunt wire hanging to the gun-barrel; from the end of which, when electrified, and any black non-electric unexcited was brought near, though not near enough to cause a snap, a brush of blue lambent flame, totally different from the former, was very conspicuous when the room was dark, of more than an inch long and an inch thick. Here the phosphoreal smell might be perceived at a considerable distance. If the back of a hand was brought so near this wire as to occasion a snap, and these snaps were received for some time, you would feel them like so many punctures on the skin, occasioning red spots, which have lasted 24 hours.

If, when a person is electrified, he brings his hand on the clothes of one that is not, they both have a sensation exactly resembling that of many pins running into the skin, which continues as long as the globes are in motion. This is most perceptible when the clothes are of thin woollen cloth or silk; animal substances, less so when of linen or cotton, which are vegetable.

If some oil of turpentine be set on fire in any vessel held in the hand of an electrified man, the thick smoke thence arising, and received against any non-electric of a large surface, held in the hand of a 2nd man standing on an electrical cake; this smoke, at a foot distance from the flame, will carry with it a sufficient quantity of electricity for the 2nd man to fire any inflammable vapour. The electrical strokes have been likewise perceptible on touching the 2nd man, when the non-electric held in his hand has been in the smoke of the oil of turpentine between 7 and 8 feet above the flame. Here we find the smoke of an originally-electric a conductor of electricity.

Likewise if burning spirit of wine be substituted instead of oil of turpentine; and if the end of an iron rod in the hand of the 2nd man be held at the top of the flame, he will kindle other warm spirits held near his finger. Here we find that flame conducts the electricity, and does not perceptibly diminish its force.

Mr. W. now proceeds to take notice of that surprising effect, that extraordinary accumulation of the electrical power in a phial of water, first discovered by professor Muschenbroek. The experiment is, that a phial of water is suspended to a gun-barrel by a wire let down a few inches into the water through the cork; and this gun-barrel, suspended in silk lines, is applied so near an excited glass globe, that some metallic fringes inserted into the gun-barrel touch the globe in motion. Under these circumstances a man grasps the phial with one hand, and touches the gun-barrel with a finger of the other; on which he receives a violent shock through both his arms, especially at his elbows and wrists, and across his breast.

That a gun-barrel is absolutely necessary to make this experiment succeed, is imaginary; a solid piece of metal of any form is equally useful. Nor has he yet found, that the stroke is in proportion to the quantity of electrified matter; having observed the stroke from a sword as violent as that from a gun-barrel with several excited iron bars in contact with it.

He has tried the effect of increasing the quantity of water in the glasses of different sizes, as high as 4 gallons, without in the least increasing the stroke. If filings of iron be substituted instead of water, the effect is considerably lessened. If mercury, much the same as water; the stroke is by no means increased in proportion to their specific gravities, as might have been imagined.

If a dry twig of birch, or any other wood, be run through the cork, instead of the metallic wire, the stroke is not greater than is usually felt from the gun-barrel without the application of the water. The stroke is likewise lessened, if the phial be held in the hand with a glove on.

If you grasp the phial with your hand, and do not at the same time touch the wire, the acquired electricity of the water is not diminished. So that, unless by accident or otherwise the wire is touched, the electrified water will contain its force many hours, may be conveyed several miles, and afterwards exert its force on touching the wire.

If, when any number of persons join hands, or communicate by any metallic medium standing on the floor, one grasps the phial, and joins with the rest; on the gun-barrel's being touched by the last person of the line, the whole number are struck, and he who grasps the phial as forcibly as the rest. But if two phials be employed, and he grasp them both, with a piece of wire of sufficient length held between his fingers, which wire touches both phials, and its end be taken hold of by the 2nd person of the line; if then the last person touch the excited gun-barrel, all in the line are violently struck, except the person who grasps the phials; but he feels little or nothing of the stroke.

Mr. W. now proceeds to show by what steps, in his inquiries into the nature of electricity, he discovered that the glass tubes and globes had not the electrical power in themselves, but only served as the first movers and determiners of that power. He observed that by rubbing a glass tube, while standing on a cake of wax, in order, as he expected, to prevent any of the electrical power from discharging itself through him into the floor; contrary to his expectation, that power was so much lessened, that no snapping was to be observed on another person touching any part of his body. But if a person not electrified held his hand near the tube while it was rubbing, the snapping was very sensible. Afterwards he met with an experiment of the same kind, in a treatise published by professor Bose, intitled, Recherches sur la cause et sur la veritable theorie de l'Electricité, which that ingenious gentleman says, had given him great trouble by its oddness. The experiment is, that if the electrical machine be placed on originally-electrics, the man who rubs the globes with his hands, even under these apparently favourable circumstances, gives no sign of being electrified, when touched by an unexcited non-electric. But if another person, standing on the floor, does but touch the globe in motion with the end of one of his fingers, or any other non-electric, the person rubbing is instantly electrified, and that very strongly.

Muschenbroek's phial of water seems capable of a greater degree of accumulation of electricity, than any thing we are at present acquainted with; and we see when, by holding its wire to the globe in motion, the accumulation being complete, that the surcharge runs off from the point of the wire, as a brush of blue flame. A method has been discovered here by a gentleman, Mr. Canton, by which the quantity of accumulated electricity may be measured to great exactness. The manner of measuring is this: when the phial is sufficiently electrified by applying its wire to the glass globe, and which is known by the appearance of the brush of flame at the end of the wire, as beforementioned; hang a slender piece of wire to the suspended gun-barrel for this purpose detached from the globes. On applying the wire of the electrified phial to that hanging to the gun barrel, you perceive a small snap; this you discharge by touching the gun-barrel with your finger, which likewise snaps; and thus alternately electrifying and discharging, you proceed till the whole electricity of the water is dissipated; which sometimes is not done, under 100 discharges. If you do not discharge the electricity every time, the snaps from the wire of the electrified phial to the gun-barrel are scarcely perceptible. In proportion to the number of strokes, you estimate the quantity of the acquired electricity of the water. That you could, by stopping the electricity, excite non-electrics; and, by accumulating their power, make them exert more force than originally-electrics would at any point of time, was that capital discovery of the late Mr. Gray; and is to be regarded as the basis, on which all the present improvements of our knowledge in electricity are founded; and till which discovery, though some of the effects of electricity were observed above 2000 years ago,* little progress was made.

The electrical æther is much more subtil than common air, and passes to a certain depth through all known bodies. It passes most readily through metals, water, and all fluids, except resinous ones; then animal bodies dead or alive, in proportion as they are more or less wet; then stones, wood, and earths. It passes to a certain thickness only through resins, dry animal substances, wax, and glass. For this reason bodies are called electrics per se, or non-electrics; not only for their rubbing the electricity from other bodies, but likewise as they permit more or less of the electrical æther to pass through them. This æther has not only the property with air of moving light substances; but it seems to have another, and that is elasticity.

That this fluid is more subtil than common air, is more particularly demonstrated by its passing through several glasses at the same time; through any one of which, though ever so thin, air cannot pass. It likewises passes through all known bodies, except originally-electrics, and even through these to a certain degree. Its elasticity is proved by its extending itself round excited electrics, and excited non-electrics, to a considerable distance; as well as by its increasing the motion of fluids. This is demonstrated by the experiment with a small glass siphon where the elacticity of the electrical æther overcomes the attraction of cohesion. The stream through this slender tube is most complete when the non-electric is brought near, so as when the room is somewhat darkened, the stream of water appears as a stream of blue flame, much like that from the blunt wire. This stream is stopped, either by touching any part of the non-electrics in contact with the globes, by placing the machine and the man who turns the wheel on electrics per se, by which the current of the electrical æther from the floor to the machine is prevented; or by removing the non-electric from the leg of the siphon, by which the dissipation of the electrical æther from the excited non-electric becomes general. So that we find, that though we can repel light bodies from many parts of excited non-electrics at the same time; the whole force of the electrical current is necessary, to drive off so ponderous a fluid as water. May we likewise not infer the elasticity of electrical æther, from the ingress of the blue flame from the end of a blunt wire held near the axis of the wheel, or any part of the wood-work of the machine, after the revolutions of the globes are ceased? certainly we see an influx of electrical fire to all bodies, till their determined quantity is restored. Is not the elasticity of this æther deducible likewise from the violent shock we feel in our bodies in the experiments with water?

* Theophrastus, who lived above 300 years before the date of the Christian æra, takes notice of amber and the lyncurium attracting not only straws, and shavings of wood, but also thin pieces of copper and iron.—Orig.

There seems to be a quantity of this electrical æther in all bodies. And as it is an elastic fluid, wherever there is an accumulation of it, there is an endeavour by the nearest unexcited non-electric to restore the equilibrium. The restoring of this equilibrium he takes to be the cause of the attraction of excited glass tubes and globes, as well as that of excited non-electrics; for here the blast of electrical æther constantly sets in from the nearest unexcited non-electrics towards those excited, and carries with it whatever light bodies lie in its course. This setting in of the current of electrical æther, towards excited non-electrics, is likewise very perceptible to the feeling as a blast of cold wind; if when a person is electrified, he hold his hand over a plate with some bran in it, by which blast the bran is carried against his hand. These light substances are again repelled by the blast from the excited bodies, as soon as they come in contact, and sometimes before. The successions of these alternate attractions and repulsions are extremely quick, so that sometimes the eye can hardly keep pace with them. And if you put a glass globe of about an inch in diameter, very light and finely blown, into a plate of metal, and hang another plate over it; electrify the upper one, and bring the other under it, and you will find the strokes from the alternate attractions and repulsions almost too quick for the ear. There was a German, who travelled with a small electrifying machine, who, by a process of this sort, made two small bells ring. One of the bells was suspended to an electrified wire, which was conducted without touching along the sides of the room; at about an inch distance, detached from this wire, a little clapper was hung by a silk line; at an equal distance from this last was hung another little bell, which communicated with the sides of the room. As soon as the machine was in motion, the electrified bell attracted the clapper, which immediately by the repulsive blast was blown off to the unexcited bell. By the time the 2nd bell was struck, the former attracted again; and this jingling of the two bells continued not only during the motion of the machine, but several seconds after it was stopped. This was occasioned by the small volume of the clapper being able to convey away only a small quantity of the electrical æther at each stroke; by which it was some time before the equilibrium was restored.

To demonstrate likewise, that the restoring this equilibrium is not imaginary, Mr. W. mentions an experiment of a gentleman, Mr. Wilson, who has taken great pains in these inquiries. Take two plates of any metal, very clean and dry, whose surfaces are nearly equal; hang one of them to any excited non-electric, and bring under it on the other a whole leaf of silver. When you find on application the silver leaf is attracted, lower the bottom plate; if it is too low, you will observe the leaf silver jump up and down; if too high, it will only be attracted in part, and thus dissipate the electrical power. But if you get it at the proper distance, which will very easily be found by trials, the silver will be perfectly suspended at right angles with their planes, like the trapezium of the geometers, and touch neither of the plates; it will be extended likewise to its utmost dimensions. You frequently observe, both at the top and bottom of the silver, the electrical fire. The same effect is produced, if you reverse the experiment, by electrifying the bottom plate, and suspending the other over it. Now Mr. W. conceives, that the space occupied by this leaf of silver, is that where the equilibrium of the electrical æther is restored; for if you take away the under plate, through which from the floor the flux of this æther is furnished, or if that plate be placed on an electric per se, by which this flux is prevented likewise, the silver leaf is blown away.

It may be imagined, that it is possible for the silver to be suspended, without supposing a flux of the electrical æther from the nearest unexcited non-electric, as well as from the excited one; that is, by the simple electrical attraction. But to obviate this, it must be remembered, that the electrified gun-barrel both attracts and repels light substances at the same time. Can this attraction and repulsion be conceived without the operation of the electrical æther both to and from the gun-barrel at the same time? does not this point out an afflux as well as an efflux? are not the electrical repulsions as strong at least as the attractions; do not we see light bodies, either between excited originally-electrics, or excited non-electrics, and unexcited non-electrics, dart like a ball between two rackets of equal force?

Mr. W. proves the afflux experimentally, as well as the efflux, in the following manner. When the silver lies still, though the motion of the globes is continued between the two plates, one suspended to the gun-barrel, and the other placed on an electrical cake, a person standing on the floor needs only bring a small glass siphon in a vessel of water, and apply its longer leg near the plate placed on the wax; for on this the silver is immediately suspended; and the water, which before only dropped, now runs in a full stream, and appears luminous.* Does not, in this case, the current of the water point out the direction of the current of electrical æther?

Mr. W. now endeavours to show why our bodies are so shocked in the experiments with the electrified water; the difficulty of which seemed unsurmountable, till he had made the following discoveries. 1. That the electricity always described a circuit between the electrified water and the gun-barrel. 2. That the

* This experiment is more elegant, if the upper plate, attracting the silver, is suspended high enough for a person standing on an originally-electric, conveniently to bring the other plate under it with one hand, and to hold a pewter plate in the other. If the originally-electric is sufficiently thick, the silver will not be supended; but if the glass siphon in a small vessel of water is brought very near the pewter plate, the water runs into the plate, and the silver is immediately suspended. —Orig.

electrical fire came from the floor of the room. 3. That it would not pass from the floor quick enough for the person to be shook, if his shoes were dry. 4. That the force was increased in proportion to the points of contact of non-electrics with the glass containing the water.

Then the solution of this phenomenon became more easy, which he thus offers.

1. He has showed that a quantity of electricity is furnished from the nearest unexcited non-electrics, equal to that accumulated in excited originally-electrics and excited non-electrics. 2. This being so, when the phial of water held in one hand of a man is highly electrified, and he touches the gun-barrel with a finger of his other, on the explosion which hence arises, this man instantaneously parts with as much of the fire from his body, as was accumulated in the water and gun-barrel; and he feels the effects in both arms, from the fire of his body rushing through one arm to the gun-barrel, and from the other to the phial. 3. As much fire as this man then parted with, is instantaneously replaced from the floor of the room, and that with a violence equal to the manner in which he lost it. 4. But this flux of electrical æther, either from the floor to the man, or from the man to the water, is prevented for reasons sufficiently obvious, if the glass containing the water be thick; or if the points of non-electric contact are few; or if the man be placed on originally-electrics; or, which is the same thing, if the soles of his shoes be dry. 5. As we find that the electricity passes at least equally quick through dense mediums, which are non-electrics, as through those which are more lax and spongy; may we not therefore conclude, that the cause why we feel most pain at the joints of our arms, and in the tendons of our heels, arises from the texture in the tendons and tendinous ligaments of those parts?

END OF THE FORTY-FOURTH VOLUME OF THE ORIGINAL.

Concerning an Apparent Motion observed in some of the Fixed Stars. By James Bradley, D. D. Astronomer Royal, and F. R. S. N° 485, p. 1. *Vol. XLV. Anno* 1747-8.

The great exactness with which instruments are now constructed, has enabled the astronomers of the present age to discover several changes in the positions of the heavenly bodies; which, by reason of their smallness, had escaped the notice of their predecessors. And though the causes of such motions have always subsisted, yet philosophers had not so fully considered what the effects of those known causes would be, as to demonstrate a priori the phenomena they might produce, so that theory itself is here, as well as in many other cases, indebted to practice for the discovery of some of its most elegant deductions. This points out to us the great advantage of cultivating this, as well as every other branch of natural knowledge, by a regular series of observations and experiments.

The progress of astronomy indeed has always been found to have so great a dependance on accurate observations, that till such were made it advanced but slowly: for the first considerable improvements that it received, in point of theory, were owing to the renowned Tycho Brahè; who far exceeding those that had gone before him in the exactness of his observations, enabled the sagacious Kepler to find out some of the principal laws relating to the motion of the heavenly bodies. The invention of telescopes and pendulum clocks affording proper means of still further improving the praxis of astronomy; and these being also soon succeeded by the wonderful discoveries made by our great Newton, as to its theory; the science, in both respects, had acquired such extraordinary advancement, that future ages seemed to have little room left for making any great improvements. But in fact we find the case to be very different; for as we advance in the means of making more nice inquiries, new points generally offer themselves that demand our attention. The subject of the present letter is a proof of the truth of this remark; for as soon as I had discovered the cause, and settled the laws of the aberrations of the fixed stars, arising from the motion of light, &c. of which an account is given in N° 406 of the Philos. Trans.; my attention was again excited by another new phenomenon, viz. an annual change of declination in some of the fixed stars; which appeared to be sensibly greater about that time, than a precession of the equinoctial points, of 50″ in a year, would have occasioned. The quantity of the difference, though small in itself, was rendered perceptible, through the exactness of my instrument, even in the first year of my observations; but being then at a loss to guess, from what cause that greater change of declination proceeded, I endeavoured to allow for it in my computations, by making use of the observed annual difference, as mentioned in the same Transaction.

From that time to the present I have continued to make observations at Wansted, as opportunity offered, with a view of discovering the laws and cause of this phenomenon: for, by the favour of my very kind and worthy friend Matthew Wymondesold, esq. my instrument has remained where it was first erected; so that I have been able, without any interruption, which the removal of it to another place would have occasioned, to proceed on with my intended series of observations for the space of 20 years: a term somewhat exceeding the whole period of the changes that happen in this phenomenon.

When I shall mention the small quantity of the deviation which the stars are subject to, from the cause that I have been so long searching after; I am apprehensive that I may incur the censure of some persons, for having spent so much time in the pursuit of such a seeming trifle: but the candid lovers of science will, I hope, make due allowance for that natural ardour with which the mind is urged on towards the discovery of truths, in themselves perhaps of small moment, were it not that they tend to illustrate others of greater use.

The apparent motions of the heavenly bodies are so complicated, and affected by such a variety of causes, that in many cases it is extremely difficult to assign to each its due share of influence, or distinctly to point out what part of the motion is the effect of one cause, and what of another: and while the joint effects of all are only attended to, great irregularities and seeming inconsistencies frequently occur; whereas, when we are able to allot to each particular cause its proper effect, harmony and uniformity usually ensue.

Such seeming irregularities being also blended with the unavoidable errors which astronomical observations must be always liable to, as well from the imperfection of our senses, as of the instruments that we make use of, have often very much perplexed those who have attempted to solve the phenomena: and till means are discovered by which we can separate and distinguish the particular part of the whole motion, that is owing to each respective cause, it will be impossible to be well assured of the truth of any solution. For these reasons, we generally find, that the more exact the instruments are that we make use of, and the more regular the series of observations is, that we take; the sooner we are enabled to discover the cause of any new phenomenon. For when we can be well assured of the limits, wherein the errors of the observations are contained, and have reduced them within as narrow bounds as possible, by the perfection of the instruments which we employ; we need not hesitate to ascribe such apparent changes, as manifestly exceed those limits, to some other causes. On these accounts it is incumbent on the practical astronomer, to set out at first with the examination of the correctness of his instruments; and to be assured that they are sufficiently exact for the use he intends to make of them: or at least he should know within what limits their errors are confined.

The lovers of this science in general, cannot but acknowledge their obligations to lord Macclesfield on this account: but I find myself more particularly bound to do it; since, by means of his lordship's most accurate observations, I have been enabled to settle some principal elements, which I could not at present otherwise have done, for want of an instrument at the royal observatory, proper for that purpose: for the large mural quadrant, which is there fixed to observe objects lying southward of the zenith, however perfect an instrument it may be in itself, is not alone sufficient to determine, with proper exactness, either the latitude of the observatory, or the quantity of refraction corresponding to different altitudes: for it being too heavy to be conveniently removed, and the room being too small to admit of its being turned to the opposite side of the wall, where it now hangs; I cannot, by actual observations of the circumpolar stars, settle those necessary points; and therefore have endeavoured to do it, by comparing my own with his lordship's observations: and until this defect in the apparatus belonging to the royal observatory be removed, we must be indebted to his lordship for the knowledge of its true situation.

A mind intent on the pursuit of any kind of knowledge, will always be agreeably entertained, with what can supply the most proper means of attaining it. Such, to the practical astronomer, are exact and well-contrived instruments; and I reflect with pleasure on the opportunities I have enjoyed, of cultivating an acquaintance and friendship with the person, that of all others has most contributed to their improvement: for I am sensible that if my own endeavours have, in any respect, been effectual to the advancement of astronomy, it has principally been owing to the advice and assistance given me by our worthy member Mr. George Graham; whose great skill and judgment in mechanics, joined with a complete and practical knowledge of the uses of astronomical instruments, enable him to contrive and execute them in the most perfect manner.

The gentlemen of the Royal Academy of Sciences, to whom we are so highly obliged for their exact admeasurement of the quantity of a degree under the arctic circle, have already given the world very convincing proofs of Mr. G.'s care and abilities in those respects; and the particular delineation which they have lately published, of the several parts of the sector, which he made for them, has now rendered it needless to enter on any minute description of mine at Wansted; both being constructed on the same principles, and differing in their component parts chiefly on account of the different purposes for which they were intended.

As mine was originally designed to take only the differences of the zenith distances of stars, in the various seasons of the year, without any view of discovering their true places; I had no occasion to know exactly what point on the limb corresponded to the true zenith: and therefore no provision was made in my sector for the changing of its situation for that purpose. Neither was it necessary that the divisions or points on the arc should be set off, with the utmost accuracy, equidistant from each other; because, when I observe any particular star, the same spot or point being first bisected by the plumb-line, and then the screw of the micrometer turned till the star appears on the middle of the wire, fixed in the common focus of the glasses of the telescope? I can thereby collect how far the star is from that given point at the time of observation: and afterwards, by comparing together the several observations that are made of it, I am able to discover what apparent change has happened. The quantity of the visible alteration in the position of the stars, being expressed by revolutions and parts of a revolution of the screw of the micrometer; I endeavoured to determine, with great care, the true angle answering to them: and, after various trials, I thoroughly satisfied myself, both of the equality of the threads of the screw, and of the precise number of sounds corresponding to them.

But though these points could be settled with great certainty, I was nevertheless obliged to make one supposition; which perhaps to some persons may seem of too great moment in the present inquiry, to be admitted without an evident proof from facts and experiments. For I suppose, that the line of collimation of my telescope has invariably preserved the same direction, with respect to the divisions on the arc, during the whole course of my observations. And indeed it was on account of the objections, which might have been raised against such a postulate, that I thought it necessary to continue my series of observations for so many years, before I published the conclusions, which I shall at present endeavour to draw from them.

Whoever compares the result of the several trials that have been made by the gentlemen of the Academy of Sciences, for determining the zenith point of their sector, since their return from the north; will, I presume, allow that mine is not an unreasonable or precarious supposition: since it is evident, from their observations, that the line of collimation of that instrument underwent no sensible change in its direction, during the space of more than a whole year: though it was several times taken down and set up again, in different and remote places; whereas mine has always remained suspended in the same place.

But besides such a strong argument for the probability of the truth of my supposition, I have the satisfaction of finding it actually verified by the observations themselves; which plainly prove, that at the end of the full period of the deviations which I am going to mention, the stars are found to have the same positions by the instruments, as they ought to have, supposing the line of collimation to have continued unaltered from the time when I first began to observe.

I have already taken notice, in what manner this phenomenon discovered itself to me at the end of my first year's observations, viz. by a greater apparent change of declination in the stars near the equinoctial colure, than could arise from a precession of 50″ in a year: the mean quantity now usually allowed by astronomers. But there appearing at the same time an effect of a quite contrary nature, in some stars near the solstitial colure, which seemed to alter their declination less than a precession of 50″ required; I was thereby convinced that all the phenomena, in the different stars, could not be accounted for, merely by supposing that I had assumed a wrong quantity for the precession of the equinoctial points.

At first I had a suspicion that some of these small apparent alterations in the places of the stars might possibly be occasioned by a change in the materials, or in the position of the parts of my sector: but considering how firmly the arc, on which the divisions or points are made, is fastened to the plate in which the wire is fixed that lies in the focus of the object-glass; I saw no reason to apprehend that any change could have happened in the position of that wire and those points. The suspension therefore of the plummet being the most likely cause, from whence I conceived any uncertainty could arise; and the wire of which had been broken 3 or 4 times in the first year of my observations: I attempted to examine whether part of the before-mentioned apparent motions might not have been owing to the different plumb-lines that had been made use of. In order to determine this, I adjusted a particular point of the arc to the plumb-line, with all the exactness I could; and then, taking off the old wire, I immediately hung on another, with which the same spot was again compared. I repeated the experiment 3 or 4 times, and fully satisfied myself that no sensible error could arise from the use of different plumb-lines; since the various adjustments of the same point agreed with each other, within less than half a second.

Having then, from such trials, sufficient reason to conclude, that these second unexpected deviations of the stars, were not owing to any imperfection of my instrument; after I had settled the laws of the aberrations arising from the motion of light, &c. I judged it proper to continue my observations of the same stars; hoping that, by a regular and longer series of them, carried on through several succeeding years, I might at length be enabled to discover the real cause of such apparent inconsistencies.

As I resided chiefly at Wansted, after my sector was erected there in the year 1727, till the beginning of May 1732, when I removed from thence to Oxford. I had, during my abode at Wansted, frequent opportunities of repeating my observations; and thereby discovered so many particulars relating to these phenomena, that I began to guess what was the real cause of them.

It appeared from my observations, that during this interval of time, some of the stars near the solstitial colure had changed their declinations 9″ or 10″ less, than a precession of 50″ would have produced; and at the same time, that others near the equinoctial colure, had altered theirs about the same quantity more, than a like precession would have occasioned: the north pole of the equator seeming to have approached the stars, which come to the meridian with the sun, about the vernal equinox and the winter solstice; and to have receded from those which come to the meridian with the sun, about the autumnal equinox and the summer solstice.

When I considered these circumstances, and the situation of the ascending node of the moon's orbit, at the time when I first began my observations; I suspected that the moon's action on the equatorial parts of the earth might produce these effects: for if the precession of the equinox be, according to Sir Isaac

Newton's principles, caused by the actions of the sun and moon on those parts; the plane of the moon's orbit being at one time, above 10° more inclined to the plane of the equator, than at another; it was reasonable to conclude that the part of the whole annual precession, which arises from her action, would in different years be varied in its quantity; whereas the plane of the ecliptic, in which the sun appears, keeping always the same inclination to the equator; that part of the precession, which is owing to the sun's action, may be the same every year: and hence it would follow, that though the mean annual precession, proceeding from the joint actions of the sun and moon, were 50″; yet the apparent annual precession might sometimes exceed, and sometimes fall short, of that mean quantity, according to the various situations of the nodes of the moon's orbit.

In the year 1727, when my instrument was first set up, the moon's ascending node was near the beginning of Aries; and consequently her orbit was as much inclined to the equator as it can at any time be; and then the apparent annual precession was found, by my first year's observations, to be greater than the mean: which proved that the stars near the equinoctial colure, whose declinations are most of all affected by the precession, had changed theirs, above a tenth part more than a precession of 50″ would have caused. The succeeding year's observations proved the same thing, and in 3 or 4 years' time the difference became so considerable, as to leave no room to suspect that it was owing to any imperfection, either of the instrument or observations.

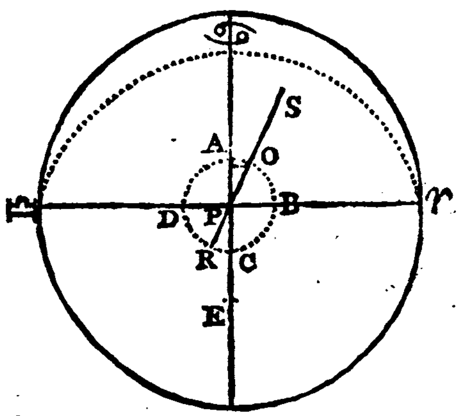
But some of the stars which I had observed, that were near the solstitial colure, having appeared to move, during the same time, in a manner contrary to what they ought to have done by an increase in the precession; and the deviations in them being as remarkable as in the others; I perceived that something more than a mere change in the quantity of the precession, would be requisite to solve this part of the phenomenon. On comparing my observations of stars near the solstitial colure, that were almost opposite to each other in right ascension, I found that they were equally affected by this cause; for while γ Draconis appeared to have moved northward, the small star which is the 35th Camelopardali Hevelii in the British Catalogue, seemed to have gone as much towards the south: which showed that this apparent motion, in both those stars, might proceed from a nutation in the earth's axis; whereas the comparison of my observations of the same stars formerly enabled me to draw a different conclusion, with respect to the cause of the annual aberrations arising from the motion of light. For the apparent alteration in γ Draconis, from that cause, being as great again as in the other small star, proved that that phenomenon did not proceed from a nutation of the earth's axis; as on the contrary, this may. On making the like comparison between the observations of other stars, that lie nearly opposite in right ascension, whatever their situations were with respect to the cardinal points of the equator, it appeared that their change of declination was nearly equal, but contrary; and such as a nutation or motion of the earth's axis would effect.

The moon's ascending node being got back towards the beginning of Capricorn in the year 1732, the stars near the equinoctial colure appeared about that time to change their declinations no more than a precession of 50″ required; while some of those near the solstitial colure altered theirs above 2″ in a year less than they ought. Soon after, I perceived the annual change of declination of the former to be diminished, so as to become less than 50″ of precession would cause; and it continued to diminish till the year 1736, when the moon's ascending node was about the beginning of Libra, and her orbit had the least inclination to the equator. But by this time, some of the stars near the solstitial colure had altered their declinations 18″ less, since the year 1727, than they ought to have done from a precession of 50″. For γ Draconis, which in those 9 years should have gone about 8″ more southerly, was observed in 1736 to appear 10″ more northerly, than it did in the year 1727.

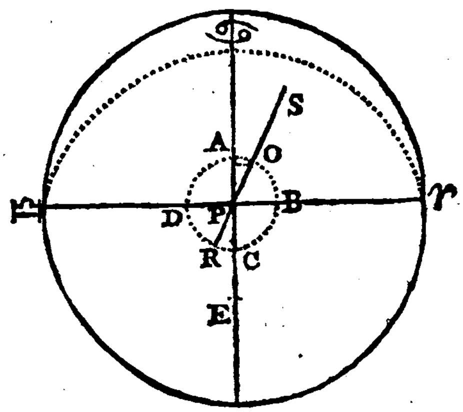
As this appearance in γ Draconis, indicated a diminution of the inclination of the earth's axis to the plane of the ecliptic; and as several astronomers have supposed that inclination to diminish regularly; if this phenomenon depended on such a cause, and amounted to 18″ in 9 years, the obliquity of the ecliptic would, at that rate, alter a whole minute in 30 years; which is much faster than any observations, before made, would allow. I had reason therefore to think that some part of this motion at the least, if not the whole, was owing to the moon's action on the equatorial parts of the earth; which I conceived might cause a libratory motion of the earth's axis. But as I was unable to judge, from only 9 years observations, whether the axis would entirely recover the same position, that it had in the year 1727, I found it necessary to continue my observations through a whole period of the moon's nodes; at the end of which I had the satisfaction to see that the stars returned into the same positions again; as if there had been no alteration at all in the inclination of the earth's axis: which fully convinced me that I had guessed rightly as to the cause of the phenomena. This circumstance proves likewise that, if there be a gradual diminution of the obliquity of the ecliptic, it does not arise only from an alteration in the position of the earth's axis, but rather from some change in the plane of the ecliptic itself: because the stars, at the end of the period of the moon's nodes, appeared in the same places, with respect to the equator, as they ought to have done, if the earth's axis had retained the same inclination to an invariable plane.

During the course of my observations, our ingenious secretary of the R. S., Mr. John Machin, being employed in considering the theory of gravity, and its consequences with regard to the celestial motions, I acquainted him with the phenomena that I had observed, and at the same time mention what I suspected to be the cause of them. He soon after sent me a table containing the quantity of the annual precession in the various positions of the moon's nodes, as also the corresponding nutations of the earth's axis; which was computed on the supposition that the mean annual precession is 50″, and that the whole is governed by the pole of the moon's orbit only; and therefore he imagined that the numbers in the table would be too large, as in fact they were found to be. But it appeared, that the changes which I had observed, both in the annual precession and nutation, kept the same law as to increasing and decreasing, with the numbers of his table. Those were calculated on the supposition, that the pole of the equator, during a period of the moon's nodes, moved round in the periphery of a little circle, whose centre was 23° 29′ distant from the pole of the ecliptic; having itself also an angular motion of 50″ in a year about the same pole. The north pole of the equator was conceived to be in that part of the small circle, which is farthest from the north pole of the ecliptic, at the time when the moon's ascending node is in the beginning of Aries: and in the opposite point of it, when the same node is in Libra.

Such an hypothesis will account for an acceleration and retardation of the annual precession, as also for a nutation of the earth's axis: and if the diameter of the little circle be supposed equal to 18″, (which is the whole quantity of the nutation, as collected from my observations of γ Draconis) then all the phenomena, in the several stars which I observed, will be very nearly solved by it.



Let P represent the mean place of the pole of the equator, about which point, as a center suppose the true pole to move in the circle ABCD, whose diameter is 18″. Let E be the pole of the ecliptic, and EP be equal to the mean distance between the poles of the equator and ecliptic, and suppose the true pole of the equator to be at A, when the moon's ascending node is in the beginning of Aries; and at B, when the node gets back to Capricorn; and at C, when the same node is in Libra: at which time the north pole of the equator being nearer the north pole of the ecliptic, by the whole diameter of the little circle AC equal to 18″, the obliquity of the ecliptic will then be so much less than it was, when the moon's ascending node was in Aries. The point P is supposed to move round E, with an equal retrograde motion, answerable to the mean precession arising from the joint actions of the sun and moon; while the true pole of the equator moves round P, in the circumference ABCD, with a retrograde motion likewise, in a period of the moon's nodes, or of 18 years, and 7 months. By this means, when the moon's ascending node is in Aries, and the true pole of the equator at A, is moving from A towards B; it will approach the stars that come to the meridian with the sun about the vernal equinox, and recede from those that come with the sun near the autumnal equinox, faster than the mean pole P does. So that, while the moon's node goes back from Aries to Capricorn, the apparent precession will seem so much greater than the mean, as to cause the stars, that lie in the equinoctial colure, to have altered their declination 9″ in about 4 years and 8 months, more than the mean precession would do: and in the same time the north pole of the equator will seem to have approached the stars, that come to the meridian with the sun at our winter solstice, about 9″; and to have receded as much from those that come with the sun at the summer solstice.



Thus the phenomena before recited are in general conformable to this hypothesis. But to be more particular; let S be the place of a star, PS the circle of declination passing through it, representing its distance from the mean pole, and ♈PS its mean right ascension. Then if O and R be the points where the circle of declination cuts the little circle ABCD, the true pole will be nearest that star at O, and farthest from it at R; the whole difference amounting to 18″, or to the diameter of the little circle. As the true pole of the equator is supposed to be at A, when the moon's ascending node is in Aries; and at B, when that node gets back to Capricorn; and the angular motion of the true pole about P, is likewise supposed equal to that of the moon's node about E, or the pole of the ecliptic; since in these cases the true pole of the equator is 90 degrees before the moon's ascending node, it must be so in all others.

When the true pole is at A, it will be at the same distance from the stars that lie in the equinoctial colure as the mean pole P is; for I neglect at present the case of such stars as are very near the pole of the equator; and as the true pole recedes back from A towards B, it will approach the stars that lie in that part of the colure represented by P♈, and recede from those that lie in P♎; not indeed with an equable motion; but in the ratio of the sine of the distance of the moon's node from the beginning of Aries. For if the node be supposed to have gone backwards from Aries 30°, or to the beginning of Pisces; the point which represents the place of the true pole, will in the mean time have moved in the little circle, through an arc, as AO, of 30° also; and would therefore in effect have approached the stars that lie in the equinoctial colure P♈, and have receded from those that lie in P♎, $4\frac{1}{2}''$; which is the sine of 30° to the radius AP. For if a perpendicular fall from O on PA, it may be conceived as part of a great circle passing through the true pole and any star lying in the equinoctial colure. Now the same proportion, that holds in these stars, will obtain also in all others; and hence we may collect a general rule, for finding how much nearer or farther any particular star is to, or from, the mean pole in any given position of the moon's node.

For if from the right ascension of the star, we subtract the distance of the moon's ascending node from Aries; then the radius will be to the sine of the remainder, as 9″, is to the number of seconds that the star is nearer to, or farther from the true, than the mean pole. When that remainder is less than 180°, the star is nearer to the true than to the mean pole; and the contrary, when it is greater than 180°.

This motion of the true pole, about the mean at P, will produce also a change in the right ascensions of the stars, and in the places of the equinoctial points, as well as in the obliquity of the ecliptic; and the quantity of the equations, in either of these cases, may be easily computed for any given position of the moon's nodes. But as it may be needless to dwell longer on the explication of the hypothesis, I shall now proceed to show its correspondency with the phenomena, relating to the alterations of the polar distances of some of the stars which I have observed: by stating the observations themselves, together with the computations that are necessary, in order to form a right judgment about the cause of these appearances.

I have endeavoured to find the exact quantity of the mean precession of the equinoctial points, by comparing my own observations made at Greenwich, with those of Tycho Brahè and others, which I jugded to be most proper for that purpose. But as many of the stars, which I compared, gave a different quantity, I shall assume the mean result; which gives a precession of one degree in 71 years and a half; this agreeing very well likewise with my observations that were taken at Wansted. The numbers in the following tables, which express the change of declination in each star, are computed on the supposition that the mean obliquity of the ecliptic was 23° 28′ 30″, and that it continued the same during the whole course of my observations. And as the moon's ascending node was in the beginning of Aries about the 27th day of March, 1727, I have reduced the place of each star to that time, by allowing the proper change of declination from that day, to the day of each respective observation.

It being also necessary to make an allowance for the aberrations of light, I have again examined my observations that were most proper to determine the transverse axis of the ellipsis, which each star seems to describe, and have found it to be nearest to 40″; which number I therefore make use of in the following computations.

The divisions or points on the limb of my sector are placed 5 minutes of a degree from each other; and are numbered so as to show the polar distances nearly; the true polar distance exceeding that which is shown by the instrument, about 1′ 35″. When I first began to observe, I generally made use of that point on the limb which was nearest to the star's polar distance, without regarding whether it was more northerly, or more southerly than the star: but as it sometimes happened that the original point, with which I at first compared the star, became in process of time pretty remote from it, I afterwards brought the plummet to another point, that was nearer to it; and carefully examined what number of revolutions of the screw of the micrometer, &c. corresponded to the distance between the different points that I had made use of: by which means I was able to reduce all the observations of the same star to the same point, without supposing the several divisions to be accurately 5′ asunder.

I have expressed the distance of each star from the point of the arc, with which it was compared, in seconds of a degree and 10th parts of a second, exactly as it was collected from the observations; though I am sensible that the observations themselves are liable to an error of more than a whole second; because I meet with some that have been made within 2 or 3 days of each other, that differ 2″, even when they are not marked as defective in any respect.

It would be too tedious, to set down the whole number of the observations that I have made; and therefore I shall give only enough of them to show their correspondency with the beforementioned hypothesis in the several years in which any were made of the stars here recited. When several observations have been taken of the same star, within a few days of each other, I have either set down the mean result, or that observation which best agreed with it. I have likewise commonly chosen those that were made near the same season of the year, in such stars as gave me the opportunity of making that choice; particularly in γ Draconis, which was generally observed about the end of August or the beginning of September; that being the usual time when I went to Wansted, on purpose to observe both that, and also some of the stars in the Great Bear. But the weather proving cloudy at that season in the year 1744, prevented my making a single observation, either of γ Draconis, or any other star, while I was there; which is the cause of one vacancy in a series of 20 succeeding years, in which that particular star had been observed. Such stars as were either not visible in the day-time, towards the beginning of September, or came at such hours of the night as would have incommoded the family of the house in which the instrument is fixed, were but seldom observed after I went to reside at Oxford: which is the reason why the series of observations of those is so imperfect, as sometimes to leave a chasm for several years together. But notwithstanding this, I doubt not but on the whole, they will be found sufficient to satisfy the astronomer of the general correspondency between the hypothesis and the phenomena, in the several stars; however different their situations are, with respect to the cardinal points of the equator.

As I made more observations of γ Draconis, than of any other star; and it being also very near the zenith of Wansted; I will begin with the recital of some of them. The point on the limb with which this star was compared, was 38° 25′ from the north pole of the equator, according to the numbers of the arc of my sector. The first column, in the following table, shows the year and the day of the month, when the observations were made; the next gives the number of seconds that the star was found to be south of 38° 25′; the 3d contains the alterations of the polar distance, which the mean precession, at the rate of one degree in 71½ years, would cause in this star, from the 27th day of March 1727, to the day on which the observation was taken; the 4th shows the aberrations of light; the 5th, the equations arising from the beforementioned hypothesis; and the 6th gives the mean distance of the star from the point with which it was compared, found by collecting the several numbers, according to their signs, in the 3d, 4th, and 5th columns, and applying them to the observed distances contained in the 2nd.

If the observations had been perfectly exact, and the several equations of their due quantity; then all the numbers in the last column would have been equal; but since they differ a little from one another; if the mean of all be taken, and the extremes be compared with it, we shall find no greater difference, than what may be supposed to arise from the uncertainty of the observations themselves; it no where amounting to more than 1½″. The hypothesis therefore seems, in this star, to agree extremely well with the observations here set down; but as I had made above 300 of it, I took the trouble of comparing each of them with the hypothesis: and though it might have been expected that, in so large a number, some great errors would have occurred; yet there are very few, viz. only eleven, that differ from the mean of these so much as 2″; and not one that differs so much as 3″. This surprising agreement therefore, in so long a series of observations, taken in all the various seasons of the year, as well as in the different positions of the moon's nodes, seems to be a sufficient proof of the truth both of this hypothesis, and also of that which I formerly advanced relating to the aberrations of light; since the polar distance in this star may differ, in certain circumstances, almost a minute, viz. 56½″, if the corrections resulting from both these hypotheses are neglected; whereas, when those equations are rightly applied, the mean place of the star comes out the same, as nearly as can reasonably be expected.

γ Draconis.			South of 38° 25′	Precession.	Aberration.	Nutation.	Mean Dist
1727	September	3	70″.5	— 0″.4	+ 19″2	— 8″9	80″.4
1728	March	18	108.7	— 0.8	— 19.0	— 8.6	80.3
	September	6	70.2	— 1.2	+ 19.3	— 8.1	80.2
1729	March	6	108.3	— 1.6	— 19.3	— 7.4	80.0
	September	8	69.4	— 2.1	+ 19.3	— 6.4	80.2
1730	....	8	68.0	— 2.9	+ 19.3	— 3.9	80.5
1731	....	8	66.0	— 3.8	+ 19.3	— 1.0	80.5
1732	....	6	64 3	— 4.6	+ 19.3	+ 2.0	81.0
1733	August ..	29	60.8	— 5.4	+ 19.0	+ 4.8	79.2
1734	....	11	62.3	— 6.2	+ 16.9	+ 6.9	79.9
1735	September	10	60.0	— 7.1	+ 19.3	+ 7.9	80.1
1736	....	9	59.3	— 8.0	+ 19.3	+ 9.0	79.6
1737	....	6	60.8	— 8.8	+ 19 3	+ 8.5	79.3
1738	....	13	62.0	— 9.6	+ 19.3	+ 7.0	78.7
1739	....	2	66.6	— 10.5	+ 19.2	+ 4.7	80.0
1740	....	5	70.8	— 11.3	+ 19.3	+ 1.9	80.7
1741	....	2	75.4	— 12.1	+ 19.2	— 1.1	81.4
1742	....	5	76.7	— 12.9	+ 19.3	— 4.0	79.1
1743	....	2	81.6	— 13.7	+ 19.1	— 6.4	80.6
1745	....	3	86.3	— 15.4	+ 19.2	— 8.9	81.2
1746	....	17	86.5	— 16.2	+ 19.2	— 8.7	80.3
1747	....	2	86.1	— 17.0	+ 19.2	— 7.6	80.7

I made about 250 observations of β Draconis; which I find correspond as well with the hypothesis, as those of γ; but since the positions of both these stars, in respect to the solstitial colure, differ but little from each other, it will be needless to set down the observations of β. I shall therefore proceed to lay down some observations of a small star that is almost opposite to γ Draconis in right ascension, being the 35th Camelopardali Hevelii in the British Catalogue. Mr. Flamsteed indeed has not given the right ascension of this star; but that being necessary to be known, in order to compute the change of its declination arising from the precession of the equinox, I compared the time of its transit over the meridian, with that of some other stars near the same parallel; by which I found that its right ascension was 85° 54¼′ at the beginning of the year 1737.

This small star was compared with the same point of the limb of my sector, as γ Draconis; and the 2d column, in the following table shows how many seconds it was found to be south of that point, at the time of each respective observation. The other columns contain, as in the foregoing table, the equations that are necessary to find what its mean distance from the same point would have been, on the 27th day of March, 1727, which is exhibited in the last column. The whole number of my observations of this star did not much exceed 40; the greatest part of which were made before the year 1730; in some of the following years none were taken; and only a single one in any other, except in 1739. However, their correspondency seems sufficient to evince the truth of the hypothesis: for if the mean of these, contained in the table, be taken, not one, among the rest of the observations, will differ from it more than 2″.

35th Camelopard. Hevelii.		South of 38° 25′	Precession.	Aberration.	Nutation.	Mean Dist. South.
1727 October....	20	73″6	+ 0″9	— 6″7	+ 8″9	76″7
1728 January....	12	60.8	1.2	+ 6.1	8.8	76.9
March.....	1	57.8	1.4	+ 9.4	8.7	77.3
September..	26	75.2	2.3	— 8.8	8.1	76.8
1729 February...	26	56.4	2.8	+ 9.4	7.6	76.2
1730 March.....	3	57.8	4.4	9.4	5.4	77.0
1731 February...	5	59.1	5.6	8.5	+ 3.0	76.2
1733 January ...	31	64.1	8.7	8.2	— 2.9	78.1
1738 December..	30	61.8	17.2	4.3	6.5	76.8
1739 February ...	4	56.9	17.3	8.5	6.3	76.4
1740 January....	20	56.0	18.6	7.0	+ 4.0.	77.6
1747 February ...	27	32.3	28.5	9.4	+ 8.4	78.6

The observations of the foregoing stars are the most proper, to prove the change of the inclination of the earth's axis to the plane of the ecliptic; those which follow will show in what manner the stars, that lie near the equinoctial colure are affected, as well as others that are differently situated, with respect to the cardinal points of the equator. Some of these stars are indeed more remote from the zenith than I would have chosen, if there had been others, of equal lustre, in more proper positions; because experience has long since taught me, that the observations of such stars as lie near the zenith, do generally agree best with one another, and are therefore the fittest to prove the truth of any hypothesis. I shall begin with those near the vernal equinox. *α* Cassiopeæ was compared with the point marked 34° 55′; and at first was found to be more southerly, but afterwards became more northerly than that point, as in the following table; the last column of which shows its mean distance south of that point on the 27th of March, 1727. The observation of the 23d day of December, in the year 1738, differs 3″ from the mean of the others; as does also another, that was taken 5 days after this; neither of which being marked as uncertain, I judged it proper to insert one of them, though it gives the mean place of the star near 2 seconds more northerly than any other, in a series of above 100; all of which correspond, with the mean of these here recited, within less than 2″; excepting two, that give the stars mean distance almost 3″ more southerly; but these last mentioned are marked as dubious; and indeed they appear to have been bad, by comparing them with several others that were made near the same time, from which they differ almost 2″.

1

α Cassiopeæ.			South of 34° 55′	Precession.	Aberration.	Nutation.	Mean Dist. South.
1727	September	9	55″0	+ 9″0	+ 2″2	+ 2″4	68″6
1728	....	17	30.8	29.4	+ 4.6	5.2	70 0
1729	June....	8	35.7	43 8	— 16.3	6.8	70.0
	December	3	N. 9.4	53.5	+ 16.5	7.7	68.3
1730	June....	11	S. 13.8	64.0	— 16.2	8.4	70.0
	December	9	N. 30.8	73.8	+ 16.3	8.8	68.1
1732	January..	8	N. 49.2	95.4	12.9	8.9	68.0
1733	....	21	64 8	116.0	+ 10.0	7.9	69.1
1734	June....	13	(2.8	143.8	— 16.1	5.0	69.9
	December	11	105.4	153.7	+ 16 2	+ 3.7	68.2
1738	....	23	176.3	234.0	+ 15.2	— 7.2	65.7
1740	June....	2	169.1	262.8	— 16.5	— 8.9	68.3
1747	February	27	332.3	397.0	+ 0.2	+ 4.7	69.6

Though I have taken no observation of τ Persei since the 22d day of January 1740; yet as this star is very near the zenith, and a sufficient number were made about the times when the equation, resulting from the hypothesis, was at its maximum; I judged it proper to insert some of them in the next table; the last column of which shows how much the star's mean distance was south of 38° 20′ on the 27th day of March, 1727. Among near 60 observations I meet with 2 only that differ from the mean of these, so much as 2″; and those differ almost as much from the mean of others, that were taken near the same time: so that the hypothesis seems to correspond, in general, with the observations of this star, as well as with either of the foregoing.

τ Persei.			South of 38° 20′	Precession.	Aberration.	Nutation.	Mean Dist. South.
1727	September	16	60″1	+ 7″4	— 3″2	+ 6″7	71″0
	December	29	39.7	11.9	+ 12.9	7.2	71.7
1728	....	21	22.5	27.2	12.8	8.7	71.2
1729	....	2	S. 9.2	42 0	11.5	9.0	71.7
1731	January	3	N. 8.2	59.0	12.8	8.3	71 9
1732	....	8	22.0	74.8	12.7	6.7	72.2
1733	....	21	34.6	91.0	11.7	+ 4.3	72.4
1738	December	23	117.0	183.4	12.8	— 9.0	70.2
1740	January..	22	132.5	200.2	11.7	8.6	70.8

After the last recited observations, it may perhaps seem needless to add those of α Persei, which is farther from the zenith; but however, as this star lies very nearly at an equal distance from the equinoctial and solstitial colures, and the series of observations of it is somewhat more complete than that of τ Persei; I shall insert one at least, for each year in which it has been observed; by which it may appear that the hypothesis solves the phenomena of stars in this situation, as exactly as in others: for if a mean be taken of the numbers in the last column of the following table, which expresses the mean distance of the star south of

41° 5′, on March 27, 1727, it will agree within 2 seconds, with every one of 80 observations that have been made of this star.

α Persei		South of 41° 5′	Preces-sion.	Aberra-ration.	Nutation.	Mean Dist. South.
1727 December	29	79″4	+ 10″5	+ 11″4	+ 7″9	109″2
1728 April....	7	87.5	14.3	— 0.8	8.2	109.2
July....	5	94.6	17.7	— 11.4	8.5	109.4
December	13	65.7	23.8	+ 10.6	8.8	108.9
1729	3	53.4	37.2	9.7	8.9	109.2
1731 January..	3	38.6	52.3	11.4	7.8	110.1
1732	8	26.8	66.2	+ 11.4	+ 5.9	110.3
1734 July....	11	S. 21.3	101.0	— 11.4	— 1.1	109.8
1738 December	24	N. 56.3	162.6	+ 11.2	9.0	108 5
1740 January..	21	71.8	177.4	10.9	— 8.2	108.3
1747 February	27	182.5	275.4	6.6	+ 8.5	108.0

Having already given examples of stars lying near both the solstices and the vernal equinox; I shall now add the observations of one that is not far from the autumnal equinox, viz. η Ursæ Majoris, the brightest star in that part of the heavens which approaches the zenith of Wansted within a degree; and which, by reason of its lustre and position, gave me the opportunity of making my series of observations of it, more complete than of many others. This star was compared with the point marked 39° 15′, and was south of it as in the following table; in which it is seen, that the observations of the years 1740 and 1741 give the polar distances 3″ greater, than the mean of the other years. Had there been only a single observation taken in either of those years, part of this apparent difference might have been supposed to arise from their uncertainty; but as there were 8 observations taken within a week, either before or after the 3d day of June 1740, which agree well with each other; and 3 were made within 20 days in September 1741, which likewise corresponded with each other; I am inclined to think that the beforementioned differences must be owing to something else, besides the error of the observations. This phenomenon therefore may deserve the consideration of those gentlemen, who have employed their time in making computations relating to the quantity of the effects which the power of gravity may, on various occasions, produce. For I suspect, that the position of the moon's apogee, as well as of her nodes, has some relation to the apparent motions of the stars that I am now speaking of.

My series of observations of several stars abound, of late years, with so many and long interruptions, that I cannot pretend to determine this point; but probably the differences before taken notice of in the observations α Cassiopeæ, and some others, that I have found also among the observations of other stars, that are not here recited, may be owing to such a cause; which though it should not have any large share of influence, may yet, in certain circumstances, discover a defect in an hypothesis that pays no regard at all to it. But whether these differences do arise from the cause already hinted at; or whether they proceed from any defect of the hypothesis itself in any other respect; it will not be very material in point of practice; since that hypothesis, as it was before laid down, appears to be sufficient to solve all the phenomena, to as great a degree of exactness as we can in general hope or expect to make observations. For if I take the mean of all the numbers in the last column of the following table for η Ursæ Majoris, and compare it with any one of 164 observations that were taken of it, the difference will not exceed 3 seconds.

η Ursæ Majoris.		South of 39° 15′	Precession.	Aberration.	Nutation.	Mean Dist. South.
1727	October 17	153″.3	— 10″.2	+ 1″.0	— 5″.2	138″.9
1728	January 24	176.4	15.2	— 17.6	5 8	137.8
	July 17	150.8	23.9	+ 17.8	6.9	137.8
	October 11	170 6	28.2	+ 2 6	7.3	137 7
1729	January 16	196.6	33 1	— 17.8	7.8	137.9
	July 21	170.4	42 4	+ 17.8	8.4	137.4
1730	July 19	189.6	60 6	+ 17.8	9.0	137.8
	December 28	232.4	68.7	— 16.7	8.9	138.1
1731	September 18	218.1	81.9	+ 9 4	8 4	137.2
1732	January 10	250.7	87 7	— 17.7	8.0	137.3
	April 13	238.7	92.3	— 0.8	7.7	137.9
1734	July......... 11	255 7	133.3	+ 17.6	2 3	137.7
1735	September 10	280.8	154.6	+ 11.4	+ 1.2	138.8
1736	 8	294.7	172.8	11.6	4.1	137.6
1737	July......... 3	303.0	187.8	17.2	6.1	138.5
1738	June......... 29	319.0	205.8	16 8	7.9	137.9
1739	April 25	348.0	220.8	2.5	8.8	138.5
1740	June 3	360.3	241.1	12 8	8 9	140.9
1741	September 23	390.9	265.0	7.9	+ 7.4	141.2
1745	 5	466.7	337.1	12.4	— 3.3	138.7
1746	 20	492 0	356.2	8.8	5.9	138.7
1747	 2	507.2	373 5	13.2	7.8	139.1

It may be perceived, by inspecting the tables which contain the observations of α Cassiopeæ and η Ursæ Majoris, that the greatest differences that occur in them may be diminished, by supposing the true pole of the equator to move round the point P, in an ellipsis, instead of a circle. For if the transverse axis, lying in the direction AC, be 18″, and the conjugate, as DB, be about 16″; the equations, resulting from such an hypothesis, will make the numbers in the last columns agree with each other, nearer than as they now stand. But since this would not entirely remove the inequalities in all the positions of the moon's nodes, I shall refer the more accurate determination of the locus of the true pole to theory; and at present only give the equations for the precession of the equinoctial points, and the obliquity of the ecliptic, as also the real quantity of the annual precession, to every 5th degree of the place of the moon's ascending node, in the following tables; just as they result from the hypothesis, as at first laid down; it appearing, from what has already been remarked, that these will be sufficiently exact for practice in all cases.

The Equation of the Equinoct. Points.

☽'s ☊ from ♈	Sig. O / Sig. VI	I / VII	II / VIII	Subst. / Add.
0°	0.″0	1.″3	19.″6	30°
5	2.0	13.0	20.5	25
10	3.9	14.5	21.2	20
15	5.8	16.0	21.8	15
20	7.7	17.3	22.2	10
25	9.6	18.5	22.5	5
30	11.3	19.6	22.6	0
Subst. / Add.	Sig. V / Sig. XI	IV / X	III / IX	☽'s ☊ from ♈

The Equation of the Obliquity of the Ecliptic.

☽'s ☊ from ♈	Sig. O / Sig. VI	I / VII	II / VIII	Add. / Subst.
0°	0.″0	7.″8	4.″5	30°
5	9.0	7.4	3.8	25
10	8.9	6.9	3.1	20
15	8.7	6.4	2.3	15
20	8.5	5.8	1.6	10
25	8.2	5.2	0.8	5
30	7.8	4.5	0.0	0
Add. / Subst.	Sig. V / Sig. XI	IV / X	III / IX	☽'s ☊ from ♈

The Annual Precession of the Equinoctial Points.

☽'s ☊ from ♈	Sig. O	I	II	III	IV	V	
0°	58″.0	57″.0	54″.2	50″.3	46″.5	43″.7	30°
5	57.9	56.6	53.6	49.7	46.0	43.4	25
10	57.9	56.2	53.0	49.0	45.5	43.2	20
15	57.7	55.7	52.3	48.4	45.0	43.0	15
20	57.5	55.2	51.7	47.7	44.5	42.8	10
25	57.3	54.7	51.0	47.1	44.1	42.8	5
30	57.0	54.2	50.3	46.5	43.7	42.7	0
	Sig. XI	X	IX	VIII	VII	VI	☽'s ☊ from ♈

Sir Isaac Newton, in determining the quantity of the annual precession from the theory of gravity, on supposition that the equatorial is to the polar diameter of the earth as 230 is to 229, finds the sun's action sufficient to produce a precession of 9″$\frac{1}{4}$ only; and, collecting from the tides the proportion between the sun's force and the moon's, to be as 1 to 4$\frac{1}{2}$, he settles the mean precession, resulting from their joint actions, at 50″. But since the difference between the polar and equatorial diameter is found, by the late observations of the gentlemen of the Academy of Sciences, to be greater than what Sir Isaac had computed it to be; the precession, arising from the sun's action, must likewise be greater than what he has stated it at, nearly in the same proportion. From whence it will follow, that the moon's force must bear a less proportion to the sun's than 4$\frac{1}{2}$ to 1; and perhaps the phenomena, which I have now been giving an account of, will supply the best data for settling this matter.

As I apprehend that the observations already set down, will be judged sufficient to prove in general the truth of the hypothesis before advanced; I shall omit the recital of more, that I made of stars lying at greater distances from the zenith; those not being so proper, for the reasons before mentioned, to establish the point that I had chiefly in view. But as it may perhaps be of some use to future astronomers, to know what were the mean differences of declination, at a given time, between some stars that lie nearly opposite to one another in right ascension, and not far from either of the colures; I shall set down the result of the comparison of a few, that differs so little in declination, that I could determine the quantity of that difference with great certainty.

By the mean of 64 observations, that were made of α Cassiopeæ before the end of the year 1728, I collect, after allowing for the precession, aberration, and nutation, as in the foregoing tables; that the mean distance of this star was 68″.7 south of 34° 55′, on the 27th day of March 1727. By a like comparison of 40 observations, taken of γ Ursæ Majoris during the same interval of time, I find this star was, at the same time, 39″.6 south of 34° 45′. I carefully measured, with the screw of the micrometer, the distance between the points with which these stars were compared; and found them to be 9′ 59″ from each other, or one second less than they ought to have been. Hence it follows, that the mean distance of declination between these 2 stars, was 10′ 28″.1, on the 27th day of March 1727.

By the mean of 65 observations, that were taken of β Cassiopeæ, before the end of the year 1728, this star was 25″.8 north of 32° 20′, on the 27th day of March 1727; and by the mean of 52 observations, ε Ursæ Majoris was 87″.6 south of 32° 30′ at the same time. The distance between these points was found to be 9′ 59″.3; whence it follows, that the mean difference of declination between these 2 stars was 11′ 52″.7, on March 27th 1727.

By the mean of 100 observations, taken before the end of the year 1728, the mean distance of γ Draconis was 79″.8 south of 38° 25′ on March 27th 1727; and by the mean of 35 observations, the 35th Cameloperd. Hevel. was south of the same spot 76″.4. So that the mean polar distance of γ Draconis was only 3″.4 greater than that of the 35th Camelopard. Hevel.; but as the equation for the nutation, in both these stars, was then near the maximum, and to be applied with contrary signs; the apparent polar distance of γ Draconis was 21″.4 greater, on the 27th day of March 1727.

The differences of the polar distances of the stars, as here set down, may be presumed, both on account of the radius of the instrument and the number of observations, to be very exactly determined, to the time when the moon's ascending node was at the beginning of Aries; and if a like comparison be hereafter made, of observations taken of the same stars, near the same position of the moon's nodes; future astronomers may be enabled to settle the quantity of the mean precession of the equinox, so far as it affects the declination of these stars, with great certainty: and they may likewise discover, by means of the stars near the solstitial colure, from what cause the apparent change in the obliquity of the ecliptic really proceeds, if the mean obliquity be found to diminish gradually.

The fore-mentioned points indeed can be settled only on the supposition, that the angular distances of these stars do continue always the same, or that they have no real motion in themselves; but are at rest in absolute space. A supposition which, though usually made by astronomers, yet seems to be founded on too uncertain principles, to be admitted in all cases. For if a judgment may be formed, with regard to this matter, from the result of the comparison of our best modern observations, with such as were formerly made with any tolerable degree of exactness; there appears to have been a real change in the position of some of the fixed stars, with respect to each other; and such as seems independent of any motion in our own system, and can only be referred to some motion in the stars themselves. Arcturus affords a strong proof of this: for if its present declination be compared with its place, as determined either by Tycho or Flamsteed; the difference will be found to be much greater than what can be suspected to arise from the uncertainty of their observations.

It is reasonable to expect that other instances of the like kind must also occur among the great number of the visible stars: because their relative positions may be altered by various means. For if our own solar system be conceived to change its place, with respect to absolute space; this might, in process of time, occasion an apparent change in the angular distances of the fixed stars; and in such a case, the places of the nearest stars being more affected, than of those that are very remote; their relative positions might seem to alter; though the stars themselves were really immoveable. And on the other hand, if our own system be at rest, and any of the stars really in motion, this might likewise vary their apparent positions; and the more so, the nearer they are to us, or the swifter their motions are, or the more proper the direction of the motion is, to be rendered perceptible by us. Since then the relative places of the stars may be changed from such a variety of causes, considering that amazing distance at which it is certain some of them are placed, it may require the observations of many ages to determine the laws of the apparent changes, even of a single star: much more difficult therefore must it be to settle the laws relating to all the most remarkable stars.

When the causes which affect the places of all the stars in general are known; such as the precession, aberration, and nutation; it may be of singular use to examine nicely the relative situations of particular stars; and especially of those of the greatest lustre, which it may be presumed lie nearest to us, and may therefore be subject to more sensible changes; either from their own motion, or from that of our system. And if at the same time that the brighter stars are compared with each other, we likewise determine the relative positions of some of the smallest that appear near them, whose places can be ascertained with sufficient exactness; we may perhaps be able to judge to what cause the change, if any be observable, is owing. The uncertainty that we are at present under, with respect to the degree of accuracy with which former astronomers could observe, makes us unable to determine several things relating to the subject that I am now speaking of: but the improvements which have of late years been made in the methods of taking the places of the heavenly bodies, are so great, that a few years may hereafter be sufficient to settle some points which cannot now be settled by comparing even the earliest observations with those of the present age.

It were to be wished therefore, that such persons as are provided with proper instruments, would attempt to determine, with great care, the present relative positions of several of the principal stars, in various parts of the heavens; especially of those that are least affected by refraction: that cause having many times so uncertain an influence on the places of objects that are very remote from the zenith, that wherever it is concerned, the conclusions, deduced from observations that are much affected by it, will always remain doubtful, and too precarious, in many cases, to be relied on.

The advantages arising from different persons attempting to settle the same points of astronomy near the same time, are so much the greater, as a concurrence in the result would remove all suspicion of incorrectness in the instruments made use of. For which reason, I esteem the curious apparatus at Shirburn castle, and the observations there taken, as a most valuable criterion, by which I may judge of the accuracy of those that are made at the Royal Observatory; and as a lover of science I cannot but wish, that our nation abounded with more frequent examples, of persons of like rank and ability with lord Macclesfield, equally desirous of promoting this, as well as every other branch of natural knowledge, that tends to the honour and benefit of our country.

Of Certain Shell-Fish, Lodged in a large Stone brought from Mahon Harbour. By James Parsons, M. D., F. R. S.* N° 485, p. 44.

Dr. Lister and Rumphius also have figures of this Mediterranean fish, which they call pholas: but since this term barely denotes the place of its residence, let us endeavour to give it a proper name, which may be done by considering its similarity to some genus already known.

* This shell-fish is the *Pholas Dactylus* of Linnæus, a common inhabitant of the European rocks.

The above-mentioned gentleman says, the stones are from half a hundred to 4 or 5 hundred weight each, lying at all depths to 20 feet under water; full of cells, each containing a single fish, called by the inhabitants the dottle fish; which name he judiciously supposes to be a corruption of the word dactylus from their form. He also says, the fish is of the same nature with the common muscle, but much more delicious, and that eating them is never attended with those poisonous symptoms that have been often thought to be caused by eating muscles.

The shells are indeed in all respects, except one, like the mytulus vulgaris, or common muscle; these being small at the hinge-end, and having a broad thin edge at the opposite; whereas the former are nearly equal at both ends, as well as straight and somewhat depressed; and as to the structure of the fish of both, they are alike, though with this small difference, that the lingula of the common muscle is detached towards the point, and that of the other is confined all along. He therefore submits it, whether either of the following names would not properly express it, viz. mytulus cylindroides, the cylindroid muscle, or mytulus dactyliformis, the date muscle. Its external form conducing much to encourage the latter, which Mr. More has hinted at, in calling the stone the dottle, dotting, or dating-stone; for as to the place of its residence, that belongs rather to its general natural history than its distinguishing name.

Dr. Lister, and after him Mons. D'Argensville, have drawn our pholas with 5 shells; but there is reason to suspect they are only bi-valves; for on examining those inclosed in the present specimen, in company with Mr. Hill, none of them appeared to have more than 2 shells. And in a specimen of one of these fish, given to Mr. Peter Collinson by Sir Charles Wager, which lodged itself in the bottom of a ship, there were only 2 shells found. Now these ought in like manner, after its generical character, to be ranged among the chamæ; and as they have a proboscis, which none of the mytuli have, he also offers the following name for this fish, viz. chama longa rugis asperis, alba. The long rough white chama.

Mr. Baker showed him another species of pholas, which he lately took out of a stone from the coast of Cornwall, and which has more of the pectuncle than any other kind, in its form, cardo, and shutting close; which the shells just mentioned cannot do: now these 3 distinct kinds of shell-fish can never be said to be rightly called by the single name of pholas.

The common objection to these fish boring their way into the stones in which they are found, viz. that the stones are first in a soft state, and so harden about them, may be obviated by the following considerations: first, that in Mr. More's great stone, when it was broken, there appeared through its substance several petrified fossil shells; which clearly show that its formation was of a more ancient date than the age of these muscles can admit of. Secondly, that the holes on the surface are narrower, in general, than the cavity in which the fish lies; and which demonstrates that they enter young, and are capable of increasing their room as they grow larger, by abrading the sides of their cells: and this is further apparent by the sandy matter found in the bottoms of those cells, which the fish cannot well get rid of, when it happens that the orifice is higher than the bottom; abundance of which was observed in some of the holes; and which is easily thrown quite out, when the orifice is depending; for in these they observed none: and this is further confirmed by what Dr. Woodward relates in the first vol. of his Catalogue, p. 25, that certain pillars of white Carrara marble taken out of the sea, on the coast of Leghorn, after lying there a number of years, were destroyed by the boring of these pholades.

As to the manner of their penetrating the stones, he cannot give the least account of it.

An Account of the Experiments made to discover whether the Electrical Power, when the Conductors of it were not supported by Electrics per se, would be Sensible at Great Distances: with an Inquiry concerning the respective Velocities of Electricity and Sound: to which is added an Appendix, containing some further Inquiries into the Nature and Properties of Electricity. By Mr. Wm. Watson, F.R.S. N° 485, p. 49.

In a former paper Mr. W. took notice, that among the many surprising properties of electricity, none was more remarkable, than that the electrical power, accumulated in any non-electric matter contained in a glass phial, described on its explosion a circuit through any line of substances non-electrical in a considerable degree; if one end of it was in contact with the external surface of this phial, and the other end on the explosion touched either the electrified gun-barrel, to which the phial in charging was usually connected, or the iron hook always fitted in it. This circuit, where the non-electric substances, which happen to be between the outside of the phial and its hook, conduct electricity equally well, is always described in the shortest route possible; but if they conduct differently, this circuit is always formed through the best conductor, how great soever its length is, rather than through one which conducts not so well, though of much less extent.

It has been found, that in proportion as bodies are susceptible of having electricity excited in them by friction, in that proportion they are less fit to conduct it to other bodies; in consequence, of all the substances we are acquainted with, metals conduct best the electrical powers; for which reason the circuit before spoken of is formed through them the most readily. Water likewise is an excellent conductor; for the electrical power makes no difference between solids and fluids as such, but only as they are non-electric matter.

Mons. le Monnier the younger at Paris, in an account transmitted to the Royal Society, takes notice of his feeling the stroke of the electrified phial along the water of two of the basins of the Thuilleries, the surface of one of which is about an acre, by means of an iron chain which lay on the ground, and was stretched round half their circumference. On these considerations it was conjectured, as no circuit had as yet been found large enough so to dissipate the electrical power as not to make it perceptible, that if the non-electrical conductors were properly disposed, an observer might be made sensible of the electrical commotion quite across the river Thames, by the communication of no other medium than the water of that river. In any other part of natural philosophy, as we should draw conclusions only from the facts themselves, it was determined to make the experiment.

The making this experiment drew on many others, and as the gentlemen concerned flatter themselves that they were made with some degree of attention and accuracy, they thought it not improper to lay a detail of all the operations before the Royal Society. To try this experiment, it was absolutely necessary that a line of non-electric matter, equal in length to the breadth of the river, should be laid over it, so as not to touch the water in any part of its length; and the bridge at Westminster was thought the most proper for that purpose, where the water from shore to shore was somewhat more than 400 yards.

Accordingly on July 14, 1747, several members of the Royal Society met to assist in making the experiment. A line of wire was laid along the bridge, not only through its whole length, but likewise turning at the abutments, reached down the stone steps on each side of the river low enough for an observer to dip into the water an iron rod held in his hand. One of the company then stood on the steps of the Westminster shore, holding this wire in his left hand, and an iron rod touching the water in his right; on the steps facing the former on the Surry shore, another of the company took hold of the wire with his right hand, and grasped with his left a large phial almost filled with filings of iron, coated with sheet-lead, and highly electrified by a glass globe properly disposed in a neighbouring house. A third observer standing near the second dipped an iron rod held in his left hand into the water, and touching the iron hook of the charged phial with a finger of his right hand, the electricity snapped, and its commotion was felt by all the three observers, but much more by those on the Surry shore. The third observer here was no otherwise necessary, than that the river being full, the iron was not long enough to be fixed in the mud on the shore, and therefore was in want of some support. The experiment was repeated several times, both then and afterward, and the electrical commotion felt across the river. The length of this circuit, through which the electricity was propagated, was at least 800 yards, more than 400 yards of which was formed by the stream of the river.

The observers on the Westminster shore not feeling the electrical commotion equally strong with those of Surry, was judged to proceed from other causes besides that of distance. For it must be considered, that the conducting wire was almost throughout its whole length laid on Portland stone standing in water. This stone, being in a great degree non-electric, is of itself a conductor of electricity; and this stone standing in water, no more of the electricity was transmitted to the observers on the Westminster shore than that proportion, in which iron is more non-electric, and consequently a better conductor of electricity than stone. Whether the conducting wire on the bridge was broken or no, and, consequently, whether the observers on the Westminster shore felt the electrical commotion or no, not only the observers on the Surry shore, who with their wire formed part of the line, felt the shock in their arms; but those persons who only stood on the stone steps there, and touched the wire with their fingers, felt the electrical commotion in the arm of that hand which touched the wire. Hence, and from a person feeling the electrical commotion standing on the wet stone steps of the Westminster shore, though not forming part of the line, but only touching the wire with his fingers, it was concluded, that besides the large circuit before spoken of, there were formed several other subordinate circuits, between the same steps of the Surry shore, and the bridge by means of the water; by which that part of the electrical power, felt by the observers on the Surry side of the river, and not by those on the Westminster side, was discharged.

Dr. Bevis having observed, and which was likewise tried here, that however well an electrified phial was charged, its iron hook would not fire the vapours of warm spirit of wine held in a spoon and applied to it, if the person who held the phial, and he who held the spoon, did not take each other by the hand, or have some other non-electrical communication between them; it was therefore thought proper to try the effects of electricity on some warm spirit of wine through the large circuit beforementioned. Accordingly the observers being placed as before, both on the Westminster and Surry shores, no other alteration was made in the beforementioned apparatus, than that the wire which connected the gun-barrel with the iron hook of the coated phial being laid aside, the coated phial itself was charged at the gun-barrel, and then brought in the hands of an observer near the warm spirits in the spoon, which was placed on the short iron rod beforementioned, which was connected with the wire which went to the observers on the Surry shore. On presenting properly the iron hook of the charged phial to the warm spirit, it was instantly fired, and the electrical commotion felt by the observers on both sides of the river.

It was then thought proper to try the effects of the charged phial on the warm spirit, when the wire was divided which was laid over the bridge; on presenting the iron hook to the spirit, a sufficient snap was given to the spoon to fire the spirit, but nothing so smart as in the former experiment where the large circuit was completed.

It was then tried what the effect would be on the spirit, if the charged phial was divested of its long wire which lay over the bridge, and was only held in the hand of an observer; while the spoon with warm spirit was placed in contact of the iron rod beforementioned, to which the wire was connected, which went to the observers on the Surry shore; and the spirit was fired with much the same degree of smartness as in the last experiment.

In these and all the subsequent operations, wires were made use of to conduct the electricity preferable to chains, as by great numbers of experiments it had been fully proved, that whatever difference there was in the bulk of the conductor, viz. whether it were a small wire, or a thick iron bar, the electrical strokes communicated were equally strong; and it had been further observed, besides the difficulty of procuring chains of a requisite length for the present purposes, that the stroke at the gun-barrel, when the electricity was conducted by a chain, was cæteris paribus not so strong, as when that power was conducted by a wire. This was occasioned by the junctures of the links of the chain not being sufficiently close, which caused the electricity in its passage to snap and flash at the junctures, where there was the least separation; and these smaller snappings in the whole length of the chain lessened the great one of the gun-barrel.

Encouraged by the success of these trials, the gentlemen were desirous of continuing their inquiries, and of knowing whether the electrical commotions were perceptible at a still greater distance. The New River near Stoke Newington was thought most convenient for that purpose; as at the bottom of that town, the windings of the river are such, that from a place which he calls A to another B, the distance by land is about 800 feet, but the course of the river is near 2000. From A to another place C, in a right line is 2800 feet, but the course of the water is near 8000 feet.

Accordingly, on Friday July 24, 1747, there met at Stoke Newington the president of the Royal Society and several other gentlemen: when every thing being disposed as before, and the wire extended from A to B and C, over the meadows, without touching the water. When every thing was thus disposed, and the signals given, the charged phial was exploded several times, and the electrical commotion every time smartly felt by the observers both at A and B. In the like trials with the places A and C, the commotions were perceptible from A to C; a distance not less than 2800 feet by land, and near 8000 by water.

To execute this, to the former wire, which was already conducted to B, another was added, which there crossed the river without touching the water; and reached almost to C, where the first of a line of gentlemen held as before the wire in one hand, and the last dipped the iron into the water. The wire from the machine to A was as before. Its effects were plainly though but faintly perceived each time by some of the observers, but never by them all. The electrical commotion was always felt by that observer, who held the extremity of the wire, but never by him who held the iron rod in the water. It was in one experiment felt by the observer who held the wire, not felt by the next who held the hand of the former, and yet plainly perceived by the third who joined the second. Those who did not themselves feel the electrical commotion here, did as at B, see the involuntary motions of those who did. The observers at A felt the shocks in the same degree, whether the other observers were stationed at B or C.

This experiment further demonstrates the distance to which the electrical power may be conveyed: but the same difficulty occurs here as in the last, viz. whether the circuit was completed by the water of the river, or by the ground which was wet?

To resolve this doubt then, the gentlemen met again July 28, 1747: when the electrical commotion was first tried from A to B beforementioned, the iron wire in its whole length being supported, without any where touching the ground, by dry sticks placed at proper intervals, of about 3 feet in height. The observers both at A and B stood on originally-electrics, and, on the signal, dipped their iron rods into the water. On discharging the phial, which was several times done, they were both very much shocked, much more so than when the conducting wire lay on the ground, and the observers stood on it, as in the former experiments. The same experiment was tried with the observer at A, instead of the iron rod, dipping a narrow slab of Portland stone into the water of about 3½ feet in length; when the shock was felt, but not so severe as through the iron rod. This demonstrated, as was before suggested, why the electrical commotion was not felt stronger by the observers on the western shore of Westminster bridge, viz. that Portland stone standing in water will conduct electricity very considerably.

The gentlemen then tried what would be the effect, if the observer at B stood on a cake of wax holding the wire as before, and touched the ground of the meadow with his iron rod at least 150 feet from the water; and if the observer usually placed near the river at A, had his wire carried 150 feet over the river as the former, stood on an originally-electric, and touched the ground with his iron rod. On the explosion of the charged phial, which was several times done, both the observers were smartly struck. This demonstrated, that in these instances the moist ground of the meadows made part of the circuit. The observers were distant from each other about 500 feet.

The observers then, stationed as in the last experiment, stood on the wax cakes as before, without touching the ground with the iron rods, or any part of their bodies, and the charged phial was exploded 4 times. These were not at all felt by the observer next to B, and without the greatest attention would not have been perceived by him next to A; and then only in some of the trials, the feeling of the electricity was like that of a small pulse between the finger and thumb of that hand which held the wire. The loaded phial was again discharged 4 times more, without any other alteration in the disposition of the apparatus than that the observer next to B stood on the ground; when the electrical commotion was perceived by that observer, though not so sharp as when the other observer at the same time stood on the ground. The observer next to A felt the tingling between his finger and thumb as before.

The gentlemen were desirous of trying the electrical commotion at a still greater distance than any of the former through the water, and where, at the same time by altering the disposition of the apparatus, it might be tried, whether that power would be perceptible through the dry ground only at a considerable distance. Highbury Barn beyond Islington was thought a convenient place for this purpose, as it was situated on a hill nearly in a line, and almost equidistant from two stations on the New River, somewhat more than a mile asunder by land, though following the course of that river their distance from each other was 2 miles. The hill between these stations was of a gravelly soil; which, from the late continuance of hot weather without rain, was dry, full of cracks, and consequently was as proper to determine whether the electricity would be conducted by dry ground to any great distance, as could be desired. Accordingly, on Wednesday, Aug. 5, 1747, they met at Highbury Barn. The electrifying machine being placed up one pair of the stairs in the house there, a wire from the coated phial was conducted on dry sticks as before to that station by the side of the New River, which was to the northward of the house. The length of this wire was 3 furlongs and 6 chains, or 2376 feet. Another wire fastened to the iron bar, with which, in making the explosion, the gun-barrel was touched, was conducted in like manner to the station on the New River to the southward of the house. The length of this wire was 4 furlongs 5 chains and 2 poles, or 3003 feet. The length of both wires, exclusive of their turnings round the sticks, was 1 mile 1 chain and 2 poles, or 5379 feet. For the more conveniently describing the experiments made here, we will call the station to the northward D, and the other E.

At this distance the gentlemen proposed to try, first, whether the electrical commotion was perceptible, if both the observers at D and E, supported by originally-electrics, touched the conducting wire with one hand, and the water of the New River with an iron rod held in the other? 2dly, Whether that commotion was perceptible, if the observer at E, being in all respects as before, the observer at D, standing on wax, took his rod out of the water? 3dly, Whether that commotion was perceptible to both observers, if the observer at D was placed on wax, and touched the ground with his iron rod in a dry gravelly field at least 300 yards from the water?

To try the first proposition, several explosions were made with the observers at D and E, touching the water, and standing on wax, with their iron rods in the water; when the observers at both stations felt the electrical shock.

To try the second proposition, 4 explosions were made with the observer at D standing on an originally-electric, and taking his iron rod out of the water, the observer at E as before. In each of these the observer at D felt a small pulsation between his finger and thumb of that hand, which held the wire. The observer at E felt each of these as strong as before. The 4 other explosions were made without any other alteration in the apparatus, than that the observer at D stood on the ground about 4 yards from the water without any communication with it. The observer at E felt the shocks in his arms as before; but the observer at D standing on the ground was shocked in the elbow and wrist of that arm which held the wire, and in both his ancles.

To try the third proposition, 8 explosions were made with the observer at D standing on an originally-electric with his rod in the water of the river as before; but the observer at E was placed in a dry gravelly field about 300 yards nearer the machine than his last station, and about 100 yards distant from the river. He there stood on the wax, holding the conducting wire in one hand, and touched the ground with an iron rod held in the other. The shock was each time felt by the observer at D, but sensibly weaker than in the former trials; but the observer at E felt them all equally strong with the former; the first 4 in his arms, when he stood on the wax, and touched the ground with his iron rod; the other 4 in his arm and ancles, when he stood on the ground without the iron rod.

By the experiments of this day, the gentlemen were satisfied that the dry gravelly ground conducted the electricity as strongly as water; which, though otherwise at first conjectured, they now found not to be necessary to convey that power to great distances; as well as that, from difference of distance only, the force of the electrical commotion was very little if at all impaired.

In one instance the circuit was formed from the phial by the observer at D and his wire, a line of ground which reached from the station at D to the broken wire that lay on the ground, and so much of this wire as reached to the short iron rod, which touched the gun barrel in making the explosions. This induced the gentlemen to conclude, as from many experiments it was manifest, that when the intervening substances conduct electricity equally well, the circuit was performed in the shortest way possible, that when the observers holding their iron rods in the river at D and E were both shocked, the electricity was not conveyed by the water of the river, being 2 miles in length, but by land, where the distance was only 1 mile; in which space that power must necessarily pass over the New River twice, through several gravel-pits, and a large stubble-field. So that admitting the electricity did not follow the track of the river, the circuit from D to E was at least 2 miles, viz. somewhat more than 1 mile of wire, which conducted the electricity from the house to the stations, and another mile of ground, the shortest distance between those stations. The same inference was now drawn with regard to the experiments at A, B, and C, in the New River before recited, viz. that as in all of them the distance between the observers was much greater by water than by land, the electricity passed by land from one observer to the other, and not by water.

From the shocks which the gentlemen received in their bodies, when the electrical power was conducted on dry sticks, they were of opinion, that from difference of distance simply considered, as far as they had yet experienced, the force was very little if at all impaired. When they stood on originally-electrics, and touched the water or ground with an iron rod, the electrical commotion was always felt in their arms and wrists: when they stood on the ground, and touched either the water or ground with their iron rods, they felt the shock in their elbows, wrists, and ancles; when they stood on the ground without the rod, the shock was always in the elbow and wrist of that hand which held the conducting wire, and in both ancles. The observers here being sensible of the electrical commotion in different parts of their bodies, was owing in the first instance to the whole of it passing, because the observer stood on wax, through their arms, and through the iron rod; in the second, when they stood on the ground, the electricity passed both through their legs, and through the iron; in the third, when they stood on the ground without either wax or rod, the electricity directed its way through one arm, and through both legs to complete the circuit.

The gentlemen were desirous of closing the present inquiry, by examining not only whether the electrical commotions were perceptible at double the distance of the last experiments in ground perfectly dry, and where no water was near; but also, if possible, to distinguish the respective velocities of electricity and sound. To execute this, required their whole sagacity and address; for they had met with very great difficulties in the last day's operations, where the wire was conducted but little more than a mile; all which could not but be greatly augmented by doubling that distance; because it was necessary, that the house, in which the electrifying machine was placed, should be visible at least at one of the stations; and that the space between that house and the stations, through which the wire was conducted, should be very little intersected by hedges, roads, or foot-paths; neither should the wire in this space be subject to be disturbed by the horses or cattle, which were grazing; nor ought it to touch in its passage the trees, or any other vegetables, which at this season of the year were every where luxuriant. To find a place within a convenient distance of London with these requisites was not very easy; but at last Shooter's Hill was pitched on, as the most convenient. As only one shower of rain had fallen during the preceding 5 weeks, the ground could not but be very dry; and as no water was near, if the electrical commotion was felt by the observers at the stations, it might be safely concluded, that water had no share in conducting it.

Accordingly, Aug. 14, 1747, they met at Shooter's Hill for this purpose. It was here determined to make 12 explosions of the coated phial, with an observer placed at the 7-mile stone, and another at the 9-mile stone, both standing on wax, and touching the ground with an iron rod. This number of explosions was thought the more necessary, as the observers at these stations were not only to examine whether the electricity would be propagated to so great a distance, but if it were, the observer at the 7-mile stone was by a second watch to take notice of the time lapsed between feeling the electrical commotion, and hearing the report of a gun fired near the machine, as close as might be to the instant of making the explosion; and therefore, to examine this matter with the requisite exactness, this number of explosions should be made.

To execute this, the electrifying machine was placed up one pair of stairs in a house on the west side of Shooter's Hill, and a wire from the short iron rod, with which the gun-barrel was touched in making the explosions, was conducted on dry sticks as before into a field near the 7-mile stone. The length of this wire, exclusive of its turnings round the sticks, was a mile, a quarter, and 8 poles, or 6732 feet. In great part of this space it was found very difficult to support the wire, on account of our scarcely being able to fix the sticks in the strong gravel there almost without any cover of soil; nor could the wire in some places be prevented from touching the brambles and bushes, nor in one field the ripe barley.

Another wire was likewise conducted on sticks from the coated phial to the 9-mile stone. In this space, the soil being a strong clay, the wire was very well secured, and in its whole length did not touch the bushes. The length of this wire was 3868 feet. As much as the place, where the observers were stationed in a corn-field, was nearer the machine than the 7-mile stone, so much were the other observers placed beyond the 9-mile stone, that their distance from each other might be 2 miles. The 40 feet of wire in these two measures exceeding

2 miles, was what connected the short iron rod beforementioned, and the coated phial, with their respective conducting wires.

The observers being placed at their respective stations, the observer at the machine proceeded in making the explosions of the coated phial; by which the observers at the 9-mile stone were very strongly shocked; and they were also felt at the 7-mile stone. This demonstrated that the circuit here formed by the electricity was 4 miles, viz. 2 miles of wire, and 2 miles of ground, the space between the extremities of that wire. A distance without trial too great to be credited! how much farther the electrical commotion will be perceptible, future observations can only determine.

The electrical commotion by the observers near the 7-mile stone was but slightly felt; nor could it be otherwise expected, the wire in many parts of its length touching, as was beforementioned, the moist vegetables; which, in as many places as they were touched, formed subordinate circuits. We find, in all other instances, that the whole quantity of electricity, accumulated in the coated phial, is felt equally through the whole circuit, when every part is in a great degree non-electric; so here the whole quantity, or nearly so, determined that way, was felt by the observers at the 9-mile stone; while those at the other station felt so much of their quantity only, as did not go through the vegetables; that is, that proportion only in which iron is a greater non-electric than the vegetables.

Though the electrical commotions, felt by the observers near the 7-mile stone, were not strong; they were equally conclusive in showing the difference between the respective velocities of electricity and sound. The space through which sound is propagated in a given time, has been very differently estimated by the authors who have written on this subject. Roberval gives it at the rate of 560 feet in a second; Gassendus at 1473; Mersenne at 1474; Du Hamel, in the History of the Academy of Sciences at Paris, at 1172; the Academy del Cimento at 1185; Boyle at 1200; Roberts at 1300? Walker at 1338; Sir Isaac Newton at 968; Dr. Derham, in whose measure Mr. Flamsteed and Dr. Halley acquiesced, at 1142. But by the accounts since published by M. Cassini de Thury in the Memoirs of the Royal Academy of Sciences at Paris for the year 1738, where cannon were fired at various as well as great distances, under great variety of weather, wind, and other circumstances, and where the measures of the different places had been settled with the utmost exactness, sound was propagated at a medium at the rate only of 1038 French feet in a second. The French foot exceeds the English by $7\frac{1}{9}$ lines, or is as 107 to 114; and consequently 1038 French feet are equal to 1106 English feet. The difference therefore of the measures of Dr. Derham and M. Cassini is 34 French or 36 English feet in a second.* According to this last measure, the velocity of sound, when the wind † is still, is settled at the rate of a mile, or 5280 English feet in $4\frac{77}{100}''$.

To return to our purpose: the length of the conducting wire from the machine to the observers near the 7-mile stone was 6732 feet; the length of that to the 9-mile stone 3868 feet. The first of these measures only was made use of in the present operations concerning the velocity of electricity. In 12 discharges of the coated phial, which were felt by the observers near the 7-mile stone, and who, by a second watch of Mr. Graham's, measured the time between feeling the electrical commotion and hearing the report of the gun, with the utmost attention and exactness, was at a medium $5\frac{1}{4}$ seconds. And as the gun was distant from these observers 6732 feet, it follows, from the experiments, which have been made on the velocity of sound, that the real instant of the discharge of the gun preceded that of the observers hearing its report, at this time, when the strength of the wind was not so great as to enter into the computation, $6\frac{870}{1000}''$; or preceded the instant when the electrical commotion was felt only $0\frac{837}{1000}''$. But this instant was, from the nature of the experiment, necessarily prior to that of the electrical explosion, which was not made till the fire of the gun was actually seen; and therefore the time between the making of that explosion and its being actually felt by the observer, which must have been less than $0\frac{837}{1000}''$, was really so small, as not to fall under any certain observation, when it is to be distinguished from that which must of necessity be lost, between the firing of the gun and the electrical explosion itself.

In all the experiments where the circuit was formed to any considerable length, though the coated phial was very well charged, the snap at the gun-barrel, on the explosion, was not near so loud as when the circuit is formed in a room; so that a by-stander, though versed in these operations, from seeing the flash and hearing the report, would imagine the stroke at the ends of the conducting wire to be very slight; the contrary of which, when the wire has been properly conducted, has always happened.

From a review of these experiments, the following observations may be deduced.

1. That in all the preceding operations, when the wires have been properly

* M. Cassini de Thury afterwards measured the velocity of sound at Aiguemortes in Languedoc, and found the observations there from those made about Paris vary only half a toise in a second. See Mem. de l'Acad. Royale des Sciences, pour l'année 1739, p. 126.—Orig.

† Dr. Derham found, that when sound was carried against the wind, not only its distance but its velocity was lessened; and in M. Cassini's memoir, there is an experiment, where sound being carried against the wind, which then blew very strong, was retarded near a twelfth part of the usual time in its progress.—Orig.

conducted, the electrical commotions from the charged phial have been very considerable only, when the observers at the extremities of the wire have touched some substance readily conducting electricity with some part of their bodies.

2. That the electrical commotion is always felt most sensibly in those parts of the bodies of the observers, which are between the conducting wires, and the nearest and the most non-electric substance; or in other words, so much of their bodies as comes within the electrical circuit.

3. That on these considerations we infer, that the electrical power is conducted between these observers by any non-electric substances, which happen to be situated between them, and contribute to form the electrical circuit.

4. That the electrical commotion has been perceptible to two or more observers at considerable distances from each other, even as far as 2 miles.

5. That when the observers have been shocked at the end of 2 miles of wire, we infer that the electrical circuit is 4 miles, viz. 2 miles of wire, and the space of 2 miles of the non-electric matter between the observers, whether it be water, earth, or both.

6. That the electrical commotion is equally strong, whether it is conducted by water or dry ground.

7. That if the wires, between the electrifying machine and the observers, are conducted on dry sticks, or other substances non-electric in a slight degree only, the effects of the electrical power are much greater than when the wires in their progress touch the ground, or moist vegetables, or other substances in a great degree non-electric.

8. That by comparing the respective velocities of electricity and sound; that of electricity, in any of the distances yet experienced, is nearly instantaneous.

The gentlemen concerned were still desirous, if possible, of ascertaining the absolute velocity of electricity at a certain distance; because, though last year in measuring the respective velocities of electricity and sound, the time of its progress was found to be very little, yet they were desirous of knowing, small as that time was, whether it was measureable; and Mr. W. had thought of another method for this purpose.

Accordingly, Aug. 5, 1748, there met at Shooter's Hill for this purpose the president of the Royal Society, and several other gentlemen: when it was agreed to make the electrical circuit of 2 miles, in the middle of which an observer was to take in each hand one of the extremities of a wire, which was a mile in length. These wires were to be so disposed that, this observer being placed on the floor of the room near the electrifying machine, the other observers might be able in the same view to see the explosion of the charged phial and the observer holding the wires, and might take notice of the time lapsed between the discharging the phial and the convulsive motions of the arms of the observer in consequence of it; as this time would show the velocity of electricity, through a space equal to the length of the wire between the coated phial and this observer.

When all parts of the apparatus were properly disposed, several explosions of the charged phial were made; and it was invariably seen, that the observer, holding in each hand one of the extremities of these wires, was convulsed in both his arms in the instant of making the explosions. Instead of one, 4 men were then placed holding each other by the hand near the machine, the first of which held in his right hand one extremity of the wire, and the last man the other in his left. They were all seen convulsed in the instant of the explosion. Every one who felt it, complained of the severity of the shock. It was then tried whether, as the ground was wet, if the explosion was made with the observer holding the extremity of each wire standing on the ground near the window of the house, any difference would arise in the success of the experiment: no difference was found, the observer being shocked in the instant of the explosion as before in both his arms, and across his breast. On these considerations they were fully satisfied, that through the whole length of this wire, being 12276 feet, the velocity of electricity was instantaneous.

Mr. W. took notice, in a sequel to the experiments relating to electricity,* of an observation of professor Bose of Wittemberg, viz. "that if the electrifying machine is placed on originally-electrics, the man who rubs the globe with his hands, even under these apparently favourable circumstances, gives no sign of being electrised when touched by an unexcited non-electric. But if another person, standing on the floor, does but touch the globe in motion with the end of one of his fingers, or any other non-electric, the person rubbing is instantly electrised, and that very strongly." This experiment, almost a year since, Dr. Bevis carried further, by placing whatever non-electric touched the globe as a conductor, whether it were a man or a gun-barrel, on originally-electrics. If then, either the man who rubbed the globe, or he who only held his finger near its equator, were touched by any person standing on the floor, a snapping from either of them was perceptible on that touch.

Mr. W. offers a solution of this phenomenon, and then gives another, to the same purport, from Mr. Franklin of America.

At this time says Mr. W. I am the more particular concerning the solution of this singular appearance, as Mr. Collinson, a worthy member of this society, has received a paper concerning electricity from an ingenious gentleman, Mr. Franklin, a friend of his in Pennsylvania. This paper, dated June 1, 1747, I very lately perused, by favour of our most worthy president. Among other curious remarks, there is a like solution of this fact; for though this gentleman's experiments was made with a tube instead of a globe, the difference is no-ways material.

* Printed for C. Davis, London, 1746. 8vo. p. 32.—Orig.

As this experiment was made, and the solution given on the other side of the Atlantic Ocean, before this gentleman could possibly be acquainted with our having observed the same fact here, and as he seems very conversant in this part of Natural Philosophy, I take the liberty of laying before you his own words.

"1. A person standing on wax, and rubbing a tube, and another person on wax drawing the fire; they will both of them, provided they do not stand as to touch one another, appear to be electrised to a person standing on the floor; that is, he will perceive a spark on approaching each of them with his knuckle. 2. But if the persons on wax touch one another during the exciting of the tube, neither of them will appear to be electrised. 3. If they touch one another after exciting the tube and drawing the fire as aforesaid, there will be a stronger spark between them, than was between either of them and the person on the floor. 4. After such a strong spark neither of them discover any electricity.

"These appearances we attempt to account for thus: we suppose that electrical fire is a common element, of which every one of these 3 persons has his equal share before any operation is begun with the tube. A, who stands on wax, and rubs the tube, collects the electrical fire from himself into the glass; and his communication with the common stock being cut off by the wax, his body is not again immediately supplied. B, who stands on wax likewise, passing his knuckle along near the tube, receives the fire which was collected by the glass from A; and his communication with the common stock being cut off, he retains the additional quantity received. To C, standing on the floor, both appear to be electrised: for he, having only the middle quantity of electrical fire, receives a spark on approaching B, who has an over quantity, but gives one to A, who has an under quantity. If A and B approach to touch each other, the spark is stronger; because the difference between them is greater. After such touch, there is no spark between either of them and C, because the electrical fire in all is reduced to the original equality. If they touch while electrising, the equality is never destroyed, the fire only circulating. Hence have arisen some new terms among us. We say, B (and bodies alike circumstanced) is electrised positively; A, negatively; or rather, B is electrised plus, A, minus. And we daily in our experiments electrise plus or minus, as we think proper. To electrise plus or minus, no more needs be known than this; that the parts of the tube or sphere that are rubbed, do in the instant of the friction attract the electrical fire, and therefore take it from the thing rubbing. The same parts immediately, as the friction on them ceases, are disposed to give the fire, they have received, to any body that has less. Thus you may circulate it, as Mr. Watson has shown;* you may also accumulate or subtract it, on or from any body, as you connect

* See my Sequel, p. 64.—Orig.

that body with the rubber, or with the receiver, the communication with the common stock being cut off."

The solution of this gentleman, in relation to this phenomenon, so exactly corresponds with that which I offered very early last spring, that I could not help communicating it.

In bodies having the power of readily conducting electricity, this seems to depend very little on their specific gravity, simply considered: metals for instance, and water, are in a great degree non-electrics, and consequently conduct electricity the best of any substances, that have yet fallen under our notice; whereas the calces of metals, though very dense bodies, and very greatly more so than water, prevent in a great degree the quick propagation of the electrical power. So that a phial coated within and without with ceruse, i. e. the calx of lead, and electrised, did not, on the application as usual of one hand to the external surface, and touching the prime conductor with the other, occasion any shock, or make any explosion, more than the simple stroke from the prime conductor. The same observation holds good with regard to red lead, litharge, and lunar caustic or the calx of silver, none of which snap when electrised. For the same reason, filings of iron, which are rusty, i. e. have their surfaces converted into a calx, are much less proper to be put in glasses to make the Leyden experiment, than those that are not; inasmuch as these last cause a much louder explosion than the first.

Mr. W. procured a glass jar as large as possible, so that the glass might be very thin; the height of which was 22 inches, the periphery 41. This was covered within and without, leaving a margin of an inch at top, with leaf-brass. As much of the internal surface as was covered amounted to 1129 square inches. But the difficulty he met with in procuring this glass, was sufficiently recompensed by the great increase of the explosion from it, when fully electrised, and discharged in the same manner as before. The report was vastly louder; all the attendant phenomena greatly exceeded any thing of this kind he was before acquainted with. As the quantity of metal within this jar did not exceed 2 drams, this experiment gives further weight to his opinion in regard to the manner of increasing the effects of the Leyden experiment; and from what the phenomena of that surprising experiment principally proceed; viz. not from the volume of the prime conductor, nor from the quantity of non-electrical matter contained in the glass, but from the number of points of non-electrical contact both within and withoutside of the glass, and from the density of the matter constituting those points. It must be observed that, cæteris paribus, the electrical explosion is greater from hot water included in glasses, than from cold; and from these glass jars warmed, than when they are cold.

The explosions from the large glasses just mentioned fully electrised, as well as from small ones under the same circumstances, will not be considerable, unless the circuit be completed; that is, unless some matter, non-electric in a considerable degree, and in contact with the coatings of the phials, is brought into contact, or nearly so, with such non-electrics as communicate with the matter contained in the phials themselves. When indeed the circuit can be completed, the explosion from the large glasses is prodigious; the whole quantity of electricity accumulated, or nearly so, being discharged in an instant. But the fact is otherwise if the circuit is not completed, and the iron rod in the mouth of one of these phials is touched by a non-electric (the hand of a man for instance) not in contact with the tail wire: for then there will be no explosion, no shock; but the person approaching his finger near the iron rod, will see a succession of small sparks, more intensely red than that large one seen when the phials explode at once; and the person making the experiment will feel a very pungent pain, but confined to that finger which touches the iron rod. This succession of sparks continues till the electricity accumulated in the phials is nearly exhausted. So that the explosion from any given quantity of electricity, accumulated as beforementioned, is greater or less in proportion to the time expended in making that explosion: in like manner as a given quantity of grained gunpowder rammed hard in a pistol, is almost instantaneously fired, and that with a great report; when the same quantity of gunpowder rubbed fine, and rammed hard, takes a considerable time in burning as a squib, and makes no explosion.

From what he has advanced, it may possibly be conjectured, that the electrical effluvia occupy only the surfaces of bodies electrised; as we found that a very small quantity of matter, distributed under a very large surface, would occasion a greater accumulation of electricity, than a much more considerable quantity of matter under a less. But that the electricity occupies the whole masses of bodies electrised, and passes through their constituent parts, Mr. W. thinks is clearly demonstrated by the following experiments.

He enveloped an iron rod, about 3 feet in length, with a mixture of wax and resin, leaving free from this mixture only one inch at each end. This iron was warmed, when thus fitted, that the whole of its surface where it was intended might be covered. This rod, when electrised at one of its ends, snapped as strongly at the other, as though it was without the wax and resin. This could not have happened from the electricities passing along the surface of the iron rod, because there it was prevented by the originally electrics, and consequently must of necessity pass through it.—Again, a phial of water in the experiment of Leyden can be electrised, and may be caused to explode, though the wire, touching the water in the phial in making that experiment, be run through a wax stopple, exactly fitted to the mouth of the phial.

He caused a glass tube, open at each end, and about 2 feet and a-half long, to be capped with brass cemented to the ends of the tube. In the centre of each of these caps was fastened a slender brass rod; and these were disposed so in the tube, as to come within half an inch of each other. When the tube was properly suspended in silk lines, with one of its extremities near a glass globe in motion, the brass work at both ends snapped equally strong. As the electricity could not pass along the surface of this tube warmed and wiped clean, this effect could not have happened, unless the electricity pervaded the substance of the brass caps. On touching the brass at the end of the tube most remote from the electrifying machine, the snaps from one of the brass rods within the tube to the other were seen to correspond with the snaps without. More experiments of this kind might be added, but these he presumes, are sufficient to show, that the electricity occupies the whole masses of non-electric bodies electrised.

Mr. W. mentions a series of experiments he had made in vacuo; from the comparison of which with the experiments in open air, it appears that our atmosphere, when dry, is the agent by which, with the assistance of other electrics per se, we are enabled to accumulate electricity in and upon non-electrics; that is, to communicate to them a greater quantity of electricity than they naturally have: hence also we see, that on the removal of the air, the electricity pervades the vacuum to a considerable distance, and manifests its effects on any non-electrics, which terminate that vacuum: and by these means that originally-electric bodies, even in their most perfect state, put on the appearance of non-electrics, by becoming the conductors of electricity.

On the Bones of a Fœtus being Discharged through an Ulcer near the Navel. By Mr. Francis Drake, Surgeon, F.R.S. N° 485, p. 121.

The wife of James Burman, labourer, at Scawby near Brigg in Lincolnshire, was about 29 years of age when she married. About two years after, having had a child at full time, she conceived again, and went regularly on for 4 months. She then got a fall, and about 3 weeks after felt a load in her belly, which continued, on the right side of the same, between 2 and 3 years. The woman then grew very big with another child, which pressed so much on the lump as to give her great uneasiness. However, she went on to her time with her double burden; and 3½ years after the accidental fall, she was delivered of a live child at full growth: from which time she became worse, with violent pain about the navel, and an inflamed tumour appeared near that part. On application to a neighbouring surgeon, fomentations were used, which produced a suppuration at a small breach near the navel. The surgeon did not know what to make of this swelling, and therefore did not venture to enlarge the orifice; but it continued discharging a fetid purulent matter for three or four months longer.

About a year, or more, after her last delivery, the woman was suddenly seized in the night-time, and a hardish mass of flesh, seemingly about 8 inches long, was discharged through the old opening in her belly. The lump was rather thicker than an ordinary man's wrist, and which, being opened, contained all the bones of a fœtus, of about 4 months' growth.

At this time the woman was much emaciated, occasioned by the large discharge of pus from the wound; and what was much more extraordinary, whatever she ate or drank came half-digested through the opening. White bread, or better diet, came through in that manner; but coarse rye-bread, or such-like, was not digested at all. For which reason, the poor woman must have inevitably perished, had she not been supported by a charitable gentleman's family in the village, with diet fit for her miserable circumstances.

She continued to discharge her excrement in this manner for 6 months, and then that symptom left her; after which the ulcer was kept open other 6 months, when it dried up of itself naturally, with a very firm but small cicatrix.

Of the Giants' Causeway in Ireland. By the Rev. Richard Pococke,† LL.D., Archdeacon of Dublin, and F.R.S.* N° 485, p. 124.

The sea-cliffs are very high thereabouts, but what is called the Causeway is a low head, extending from the foot of the cliffs into the sea like a mole. This head does not appear at first so grand as it is represented in the views engraved of it; but when one comes to walk upon it, and consider it more attentively, it appears to be a stupendous production of nature. The head ends in 2 points: Dr. P. measured the more western to the distance of 360 feet from the cliff, and it appeared to extend about 60 feet farther, which he could not measure, as the

* This causeway is before taken notice of in N° 199, 212, 235, and 241 of these Transactions.—Orig.

† Dr. Richard Pococke, a relation of the learned orientalist, was the son of Mr. R. P., sequestrator of the church of All-Saints in Southampton, and head master of the free-school there, where our author was born, 1704; from which school he was removed to Corpus Christi College, Oxford. Here he took his degree of LL.D. in 1733. In 1737 he began his travels to the East, where he spent 5 years, returning in 1742. The year following he published the first part of his travels; and in 1745 came out the 2d vol. of the same, which he dedicated to the Earl of Chesterfield; who being then made lord lieutenant of Ireland, Dr. P. attended him thither as domestic chaplain, by whom he was soon after appointed arch-deacon of Dublin. In the duke of Devonshire's administration he was promoted to the bishoprick of Ossory, in 1756; and to the see of Meath in 1765 by the duke of Northumberland; but the same year he died of an apoplectic fit, while on a visitation, at 61 years of age.

His Travels are justly held in great esteem. He was a great traveller, and visited other parts, as well as the East, and the course of the Nile a considerable length. He had also some other papers besides the above, in the Philos. Trans., and elsewhere. And among the manuscript treasures in the British Museum are several volumes (4811.... 4827), the gift of bishop Pococke, of curious literary and antiquarian researches.

sea was then high: and at low tides it might be seen about 60 feet yet farther, on a descent losing itself in the sea. He also measured the more eastern point 540 feet from the cliff, and saw about as much more of it as of the other, when it winds about to the eastward, and is also lost in the water. One may walk on this head on the tops of the pillars to the edge of the water. These pillars are of all angular shapes, from 3 sides to 8. The eastern point, towards that end where it joins the rocks, terminates for some way in a perpendicular cliff, formed by the upright sides of the pillars, some of which he measured, and found to be 33 feet 4 inches in height. They say there are in all 74 different sorts of figures among them. Each pillar consists of several joints or stones lying one upon another, from 6 inches to about a foot in thickness: some of these joints are in the middle so convex, as for those prominences to be nearly quarters of spheres, round each of which is a ledge, on which the stones above them have rested, every stone being concave on the under side, and fitting in the exactest manner on that which lies next below it. The pillars are from one to 2 feet in diameter, and consist most commonly of about 40 joints, most of which separate very easily, though some others, which are more strongly indented into each other, cohere strongly enough to bear being taken away in pairs.

But the causeway is not, he thinks, the most singular part of this extraordinary curiosity, the appearance of the cliffs themselves being yet more surprising; these and their several strata he examined from the rocks on the other side of a little bay, about half a mile to the east of the causeway. He thence observed, that there runs all the way a stratum from the bottom, of black stone, to the height of about 60 feet, divided perpendicularly at unequal distances by stripes of a reddish stone, looking like cement, and about 4 or 5 inches in thickness. On this there is another stratum of the same black stone, divided from it by a stratum 5 inches thick of the red. Over this another stratum of stone 10 feet thick, divided in the same manner; then a stratum of the red stone 20 feet deep; and above that a stratum of upright pillars. Above these pillars lies another stratum of black stone 20 feet high; and above this is again another stratum of upright pillars rising in some places to the top of the cliffs, in others not so high, and in others again above it, where they are called the chimneys.

This face of the cliffs reaches for 2 computed miles east from the causeway, that is about 3 measured English miles, to the house of Mr. John Stewart, 2 miles west of Balintoy. The upper pillars seem to end over the causeway, and he thinks become shorter and shorter as one goes from it, lying between 2 binds of stone like seams of coal, and like those little pillars found in Derbyshire. These binds probably meet together all round, and inclose this extraordinary work of nature; and if so, the pillars must be very short towards the extremities.

He was led to this conjecture by the following observations: the lower stratum of pillars is that which goes by a descent into the sea, and which makes what is called the Giants' Causeway: and where this descent approaches the sea, it seems probable that the pillars become shorter and shorter, so as to end not much farther off. Now the upper bind of this stratum may have been of so soft a nature, as by degrees, in process of time, to have been washed away by the sea. And in the cliff over the causeway are several pillars lying along in a rude manner almost horizontally, which seemed to be some of the pillars of the upper stratum fallen down by the giving way of the bind which was under them, and over the lower ones that compose the causeway. And here most probably the upper pillars ended, as they are seen no farther in the cliff. He saw the tops of pillars even with the shore, both on the east and west sides of the causeway, and some much lower than the causeway itself; and it is probable that these are much shorter than those of the causeway, which he measured above 30 feet higher than the tops of them.

When on the causeway, he saw in the cliff, to the south-east, what they call the Organs, about a quarter of a mile off, and a third part of the way up the cliff. They appeared small, and somewhat like a black stalactites: they were not commonly known to be such pillars as the others; but they are so, and belong to the lower stratum. When with great difficulty he climbed up the steep hill to them, he found they were hexagonal, and larger pillars than most of the others, being about 2 feet in diameter; and he measured 5 sides of one of them, which were of 13, 15, 12, 21, and 16 inches respectively. The joints he could come at were about 9 inches thick, and each pillar consisted of between 40 or 50 of them: these joints are almost flat and plain, the convexities on their upper faces being so small as to be scarcely discernible. He inquired whether any of these pillars were found in the quarries within land, and the people there told him they were not; but he has since been assured by others, that there are some found 2 or 3 miles from the shore.

Of a Metalline Thermometer. By Maurice Johnson, Esq. President of the Gentlemen's Society at Spalding. N° 485, p. 128.

This machine, placed in the Gentlemen's Society of Spalding, was the invention of the late Mr. Samuel Frotheringham, a grazier at Holbeach in Elloe Holland, Lincolnshire. He and Mr. John Ingram of the same place, watch-maker and whitesmith, whose father, originally a blacksmith at Cowbitt, and inventor of the machine for cutting watch-wheels, was also a most accurate artificer, having made and fixed up in the Spalding Museum this metal thermometer, which on experience and observation, was found to answer and go truly.

This thermometer was composed of an upright bar a, fig. 4, pl. 9, of the best iron, 4 feet long, and an inch and a quarter broad, having a polished brass bar of the same length and width screwed to it before, with 4 steel screws, and being also capped (b) with steel, and thereon a lever (c) moving on a stud of steel, which communicates with another less lever (d) also on a stud, having a chain (e) at the end of it, which laps round an axis (f), to which the index was fixed, which shows the degrees marked on a semicircular arch (g): under the steel screw-heads there are small slits in the brass bar, except the lowermost, which is fixed, which admit of its expanding, by which it protrudes and operates on the first-mentioned lever, which being raised moves the less lever, and so draws the chain which turns the axis fixed to the index, which shows the degree of warmth of the weather marked on the semicircular arch. At h is a screw through two studs, to draw the great lever backward and forward, as occasion may be; i is a counter-balance to the small lever to draw the hand back when the brass bar shrinks.

To the above the editor adds the following note.

In the beginning of the year 1735, I invented, and caused to be constructed, a thermometer on the same principles as this: I found that a rod of brass 3 feet long was sensibly affected by the changes of heat of the weather, having one exposed in my garden during the hard frost of the winter 1739 and 40. And my instrument was very sensible with either a brass rod or an iron rod, when the bottom of it was placed in a sand-heat for chemical uses; but I shall refer the reader to the appendix to the preceding number, where I have given a full description of my invention, and the reasons why I did not publish it before; though I have shown the instrument to scores of people ever since May 1735, and sent a description and draught of it to M. Buffon, superintendant of the Royal Physic Garden at Paris in the year 1744, in order for his laying it before the Royal Academy of Sciences at Paris, from which I had some time before received a diploma on having the honour of being appointed one of their corresponding members. C. M.—Orig.

An Abstract of the Remarkable Case and Cure of a Woman, from whom a Fetus was extracted, that had been lodged 13 Years in one of the Fallopian Tubes. By Dr. J. Mounsey, Physician to the Army of the Czarina.†* N° 486, p. 131.

A soldier's wife, of Abo in Finland, who had been the mother of 2 children, being pregnant a third time in the year 1730, was afflicted with violent pains

* After his return from abroad Dr. Mounsey was elected physician to Chelsea Hospital. This situation he enjoyed for a number of years, to the great disappointment of several other physicians, to whom the reversion of that place had been promised. Dr. M. was a very eccentric character; and although the few written documents, which he has left on medical or other subjects, possess no remarkable excellence; yet we are told that in his conversation he discovered great strength of understanding, joined to an exquisite degree of humour. He died in Decr. 1788, at the advanced age of 96. In the morning on which he died, he said to his attendant, "I shall certainly lose the game." Being asked what game? he replied, "The game of 100, which I have played for very earnestly many years; but I shall now lose it, for I expect to die in a few hours:" and his prediction proved true.

and twistings in the bowels, swooning fits, vomitings, and great disorders in her back and lower belly. These symptoms, and complaints of several other kinds, continued to make her uneasy, till she found her burden increase, which fell from side to side as she changed the position of her body, and could be pressed by her hand from one place to another, but was rather apt to remain on the right side.

After quickening, her health became better, she grew larger than ordinary, and was supposed to carry twins. About the time her delivery was expected, she was taken ill with violent pains by fits across her lower belly; but had none in her back, nor any forcings downward. Next day these pains went off, which made her suppose she had misreckoned. But after this her breasts swelled, and gave milk in plenty; and her menses came on, attended with more violent pains than she had had before; and such large discharges of blood from time to time, that she could neither speak nor move; and even after the floodings were stopped her blood and strength seemed quite exhausted: she often fainted away, and was sometimes thought to be dead.

She continued very sickly for 10 years afterwards; during which time her burden was moveable, and fell from side to side. But in the month of September 1741, she felt a pain beneath the navel, with a swelling and redness, which in about 3 weeks appeared like a small boil. This she pierced with an awl, and a yellow-coloured water ran from it without any smell, and continued so to do for near 3 weeks more, when it discharged a purulent stinking matter.

In the month of June 2 small bones came out, which were given to the surgeon that visited her; who only applied a piece of plaster, persuading her that a cure was impossible. Other small bones worked themselves out afterwards, till October 1742, when the head-quarters of the Russian army being at Abo, this unhappy woman applied to Dr. Mounsey, who, after a careful examination, undertook to deliver her. And accordingly, desiring the assistance of Mr. Geitle, surgeon to the regiment of Abo, a grooved probe was thrust into the fistula, and an incision made with a bistory, upwards and obliquely, from the linea alba into the cavity of the abdomen; but she being unruly, and the operation not going on to the doctor's liking, he proceeded no farther till the next day; only some loose bones were extracted, and the wound dressed with tents and compresses, to keep in the omentum, and keep the wound open.

At the next operation the incision was carried downwards, and then another incision continued from the first was conducted upwards, and slanting at a small distance from the first; taking care to keep as near as possible the direction

† This history is also printed in the Swedish language, in the Acts of the Royal Academy at Stockholm.—Orig.

where the adhesion of this body to the peritonæum appeared to be, and avoiding to make the external wound larger than absolutely needful, lest the omentum and guts should fall out; and particularly lest the suppuration should exceed the strength of nature, which was here already nearly exhausted.

A large opening was now made, but the fetus being closely enveloped by its containing sac, the doctor durst not venture to draw it out by force, for fear some of the naked bones might lacerate the internal parts: therefore dilating this sac with the point of a pair of probe-scissars, directed by the fingers of the left hand, he pierced and cut in pieces the skull, which afterwards he extracted piece-meal.

The matter that first issued out had a very nauseous smell, and consisted of membranes, fat, and corrupted flesh. On opening the cranium, the cerebrum appeared of its natural colour. The operation having been long, and the woman fainting away, the wound was dressed, without attempting to extract more at that time. In the evening she was taken with vomitings; but by proper internal medicines, and flannel stoups wrung out of hot wine, applied over the whole belly, and often renewed, she found ease, and got better. The loss of blood, during the whole operation, was inconsiderable.

Next day the bones of the trunk, and most of the other large ones, with their ligaments and rotten flesh, were taken out. The matter discharged for several days was of a dark-brown colour, occasioned by blood issuing from the dilated pores of the internal surface of the sac, which rendered the matter at first of a deep-red colour; but that changed daily and gradually, till at last it became white. The doctor imagines this discharge to resemble the lochia after child-birth; for after the fetus was extracted, the woman's breasts swelled, and gave milk in plenty for 2 months, in quantity, colour, and consistence, as if after delivery at the proper time.

The doctor examined this sac very diligently with his fingers, while thin, and not contracted, but formed into wrinkles, through which he felt the rectum, the vesica urinaria, and, as he thought, the fundus uteri. Many small bones lay in the folds; but, as she complained not of pain, they were left till the suppuration began, excepting those the sac in contracting itself threw out.

Besides fomentations, balsamics, proper bandages, &c. vulnerary detergent injections were found very useful, thrown in in large quantities, both to wash out the putrid flesh, and bring away the scales of bones which were still concealed in the folds of this sac.

The sac contracted itself daily, grew smooth and white within, and narrower as it approached the uterus, which gave reason to believe it one of the Fallopian tubes.

The wound was cured in about six weeks, and the woman, delivered from a long state of misery, grew fat and lusty, and now enjoys good health.

Comparing these circumstances together, it seems reasonable to believe this fruit never was in the cavity of the womb, but that the impregnated ovum was stopped in its passage through one of the Fallopian tubes, where it grew, and was detained so many years; and that the inflammation, which happened below the navel, was not owing to the rottenness of the fetus, or to its bare bones seeking a discharge, but rather to some accidental friction of the containing sac against the peritonæum, thus producing adhesion, obstruction, inflammation, &c. The fetus, before this accident, must have remained all these years entire, and without perfect corruption: for it took no less time, after its communication with the common air, before it showed marks of putrefaction, than a fresh subject, kept in the same degree of heat, would have done.

The doctor's observations on the bones were: that they had a full proportion to those of a child at nine months, and that the fibres were more compact, and their articulations stronger. The sockets for the teeth were fixed on each side of the jaws; the dentes incisores of the upper jaw were high and large; the molares had almost all begun to ossify in their alveoli; at least the crown, which was the cortical part, was formed, and they were filled internally with a cretaceous substance. In new-born children those parts are seldom found so far advanced, which gives reason to believe this child did not die within the ordinary time of pregnancy, and that the different accidents, before mentioned to have happened, were chiefly owing to the preternatural situation of the fetus.

Some places in the skull appear to have been carious, and corroded by some sharp humours; and nature, supplying its ossifying juices, had repaired these places, and rendered them more solid and whiter than the rest, but very uneven and scabrous, from the different times and directions of the bony sproutings. There were likewise exostoses on the ends of the thigh-bone, and some other bones.

It is very difficult to determine about what time the growth of these productions began or ended. Supposing it from the time of the disorders that happened in the first months of pregnancy; would not such a disease have caused death to the fetus, before it had come thus to a full growth? If it was the consequence of the violent accidents which happened about the time of the natural birth, the child then must have continued alive some considerable time afterwards, during which these bony excrescences were formed; there being a perfect ossification, as performed by the laws of circulation, and not by any vegetative or petrifying power, as in inanimate bodies.

Two or 3 of the lateral processes of the spine were what first passed through the little ulcer; the rest of the bones, (except a few that were lost in cleaning) were presented by the doctor to the museum of the Royal Society. They retained a very strong and singular smell, though they were immediately cleansed from the rotten flesh, and well washed.

The Motion of Projectiles near the Earth's Surface considered, Independent of the Properties of the Conic Sections. By Mr. Thomas Simpson, F. R. S.* N° 486, p. 137.

After so much has been already said on the motion of projectiles in vacuo, it may seem needless to attempt any thing further on that head; yet as a thorough knowledge in the art of gunnery is become more than ever necessary, and as gentlemen employed in the practice of that art are too often deterred from applying themselves to the theory, by the difficulties they imagine they shall meet with in the conic sections, Mr. S. gives those thoughts on the subject, in which little or nothing new is to be expected besides the method.

When he first drew up this paper (which was about 2 years ago) he intended, had health permitted him, to make the proper experiments, to have also attempted something with respect to the resistance of the atmosphere, the effects of which are too considerable to be entirely disregarded: but if the amplitude of

* Mr. Thomas Simpson, F. R. S., and professor of mathematics in the Royal Military Academy at Woolwich, was born at Market-Bosworth in Leicestershire, in 1710. He was bred to his father's business, that of a stuff-weaver, which he followed a considerable part of his life; but by the most sedulous application to study, in cultivating his excellent natural talents, he acquired so deep a knowledge of mathematics, as entitled him to rank with the most scientific men of his age. He commenced his communications to the Ladies' Diary in 1735, by questions published there in 1736, which show him to have been then no inconsiderable mathematician at 25 years of age.

About the year 1736 Mr. S. came up to London, to better his condition, and for some time he worked at his trade in Spitalfields, and gave lessons in mathematics at leisure hours, to which profession he afterwards wholly devoted himself.

His first separate publication, a Treatise on Fluxions, in 4to, came out in 1743; which was followed by several other excellent works, at short intervals, during the rest of his life. In 1743 he was appointed professor of mathematics at the Royal Military Academy, Woolwich; and in 1745 he was elected a F. R. S. Besides Mr. S.'s own separate publications, and several papers in the Philos. Trans., he had many pieces inserted in various periodical works; as magazines, and the Ladies' Diary, of which last and very useful little work, he was the editor, from the year 1754 to 1760, both inclusive; during which time he raised that work to a high degree of respectability. But Mr. S.'s great and continual application to study at length subdued his constitution, though naturally strong, and terminated his useful labours; he being quite worn down, in 1761, when only in the 51st year of his age.

Mr. S. was a rare instance of the effects of diligent application united to natural good talents. He raised himself to the first rank in the mathematical sciences. He had a fine genius; and produced many excellent discoveries; and he was one of the most elegant writers on the subject of his labours. In Dr. Hutton's Dictionary may be seen a pretty full account of Mr. S.'s life, with a particular analysis of his various publications.

the projection, answering to one given elevation, be first determined by experiment, which the method supposes, the amplitudes in all other cases, where the elevations and velocities do not very much differ from the first, may be determined, by the proportions here laid down, to a sufficient degree of exactness: because, in all such cases, the effects of the resistance will be nearly as the amplitudes themselves; and were they accurately so, the proportions of the amplitudes, at different elevations, would be exactly the same as in vacuo.

Mr. S. then proceeds to explain his method in several problems and corollaries. But it will be read to more advantage in the author's volume of Select Exercises, first published in 1752, where it was inserted, very much enlarged and improved.

The Case of Henry Axford, who after having been dumb for Years, recovered the Use of his Tongue by means of a Frightful Dream. By the Rev. Mr. Archdeacon Squire, F.R.S. N° 486, p. 148.

Henry Axford, son of Henry Axford, of the Devizes in Wiltshire, an attorney, when a child was subject to convulsion-fits, which followed him pretty frequently till he was about 25 years of age. After this, his health became extremely good. At about 28 years old, he perceived a hoarseness coming on him, which was soon after attended with all the symptoms of a common cold, till, in about 6 days after his first seizure, he became quite speechless, not only losing the articulate use of his tongue, but being scarcely able to make the least noise with it. His cold soon went off in the usual manner, and he got quite as well in health as ever he had been in his life; but still continued absolutely speechless. He had advice from all the neighbouring physicians, but to no purpose; for nothing they did could restore him to the former use of his tongue.

He continued in this dumb way about 4 years; till one day in the year 1741, he got very much in liquor, so much, that on his return home at night to the Devizes, he fell from his horse 3 or 4 times, and was at last taken up by a neighbour, and put to bed in a house on the road. He soon fell asleep; when dreaming that he was fallen into a furnace of boiling wort, it put him into so great an agony of fright, that struggling with all his might to call out for help, he actually did call out aloud, and recovered the use of his tongue from that moment as effectually as ever he had it in his life, without the least hoarseness remaining, or alteration in the old sound of his voice, as near as can be discerned. And so it continued ever after.

Concerning the Hearing of Fish. By Mr. Wm. Arderon, F.R.S. N° 486, p. 149.

Though fishes are not provided with organs for hearing, similar to those serving to that purpose in other animals, Mr. A. observes it would be too presumptuous to declare, without experiment, that they are unable to hear, by organs differently placed, whose situation and structure, for want of due examination, we are not acquainted with.* To be able therefore to judge from real facts, without being in the least prejudiced by what has been written for or against their capacity of hearing, he had, for almost three years past, been often trying experiments on several kinds of fishes; viz. perches, ruffs, bansticles, millers-thumbs, minnows, &c. which he had kept in glass jars for that purpose; and at the hours of feeding them, as well as at other times, by different noises, such as whistling, hallooing, the sounds of several musical instruments, and every other means he could contrive, endeavoured to discover their sense of hearing, if they were indeed endowed with that sense; but could never perceive they were affected by any of these noises.

But whether fishes do or do not hear, it is certain their senses of feeling and seeing are exquisitely quick; and he believed by the extreme sensibility of these two, one may explain most of the accounts that have been brought by writers as proofs of their hearing; such as their coming, when called by their names, as Plutarch relates of Marcus Crassus's lamprey; their flocking in throngs when called to be fed, as Mr. Bradley tells us he saw the carps do in the pond of one Mr. Eden at Rotterdam; and their flying away from the hallooing and noises made by sailors, as Wolfgang reports the dolphins do, when the sailors have a mind to fright them. But may we not as reasonably imagine these dolphins fly from the sailors, their ships and boats, on account of the violent action with which such hallooings usually are performed, as merely on account of the noise they make? and in the other cases, is it not as probable, that the fish in ponds, either by their sight or feeling, discovered the approach of their benefactors, whose coming they were accustomed to expect, as that they were sensible of their voices calling them?

He had often struck with his thumb-nail against the edge of a glass jar, in which he kept 2 ruffs, a stroke not harder than the beat of a pulse, which would cause them in a moment to dart from the bottom of the jar to the top; though he was sure they did not see him. But if he made the same motion without hitting the glass, or if he made a hundred times louder noises than the striking of his nail against the glass, at a very small distance from it, he could not perceive they were in the least affected; which, if duly considered, might be thought to amount to a proof of the deafness or want of hearing in this kind of fish at least; and that their delicate sense of feeling supplies them with the knowledge of the motions of bodies, when their other senses fail. Indeed he had often been convinced by experiment, that their feeling is exceedingly acute, perhaps more so than in other animals; whence he had been led to imagine, that their fins

* Since Mr. A.'s observations were written, the structure of the organ of hearing in fishes has been accurately examined and described by various anatomists.

may possibly be the organs more immediately sensible of the slightest motions in the medium wherein they dwell. The curious, who have observed the fins of fishes with the microscope, find them to be composed of infinitely fine vessels, arteries, veins, muscles, and membranous fibrillæ, whose structure seems more delicate than is necessary for parts that serve only as oars to waft the fish along.

At other times, if by striking on the top of the jar with a small key, the stroke or tremor has been a little more violent, the fish would shut down their back fins in a moment, and remain motionless at the bottom of the glass. The sudden appearance of his hand at the top of the jar would likewise produce the same effect; but noises made near them seemed to give them no disturbance.

If the eyes of fishes be carefully examined, when swimming in a glass vessel, the cornea or black uvea of their eyes may be seen, sometimes advancing forwards, and at other times retiring back, just as their sight is directed to near or distant objects, through a grosser or finer medium; the form of their eyes altering, as occasion requires, to make them distinguish objects; and their eyes have so great a liberty in the orbits, that they are able to turn them any way, upwards, downwards, to one side or the other, nearly a quarter of a circle, which makes them full amends for the want of motion in their necks, and enables them to change or direct their optical axis to any designed place in a moment.

Those who have been accustomed to fly-fishing can bear witness, that the sight of fishes is quick and distinct almost beyond belief: for it is not uncommon to behold a fish dart itself 20 or 30 yards in an instant at a fly thrown out at the end of a long line, and catch it even before it can well touch the water. Few other creatures are perhaps capable to distinguish objects so small at so great a distance, at least not so perfectly as these do; for, let the artificial fly differ in colour, shape, or size, but very little from the natural one it should represent, and not a fish will meddle with it.

These instances of the exquisite feeling and seeing of fishes, together with their want of organs that can be certainly known to serve them for hearing, as well as of sufficient facts to prove that they do hear, might, he thought, amount to the highest probability, that they are really destitute of that sense,* and have no need of it, notwithstanding the contrary opinions of some authors: and their living in an element, where land animals are capable of remaining but a very short time, may render an absolute certainty in this case unattainable.

But in order to discover what land animals can do, or what fish, had they organs of hearing similar to those of land animals, would be capable of doing,

* It is not hereby denied, that fishes of the cetaceous kind may probably hear, as well as some other kinds produced in the sea, that have parts in common with land animals. These observations are confined to the common fish of our rivers.—Orig.

he endeavoured last summer to find out by experiment. 1st, Whether sound made in the open air can be heard by a land animal immerged under water. 2d, Whether, and in what manner, sound made under water can be heard by a land animal in the open air. And, 3d, Whether, and in what manner, sound made under water can be heard by a land animal that is likewise under water.

To satisfy his first inquiry, whether sound made in the open air can be heard by a land animal under water; he caused 3 people, stripped quite naked, to dive down at the same time, and to remain about 2 feet below the surface of the water, in which situation he spoke to them as loud as he was able. At their coming above water, they repeated his very words, but said he spoke very low. He caused the same persons afterwards to dive down about 12 feet under water, and a gun was discharged over them, which they all said they heard, but that the noise was scarcely perceivable.

As to the second inquiry, whether, and in what manner, sound made under water can be heard in the open air; he caused a young man to dive some feet down, and then to endeavour to halloo, which he did; and he could hear him, though very faintly. But imagining the sound might come up with the water he discharged at his mouth while he hallooed, he contrived a kind of hand-granado, which he threw into a place in the river about 9 feet deep. The fuzee burnt under water near 10 seconds, and then the granado went off, giving a prodigious hollow sound, and shaking the adjacent ground to such a degree, that the whole of a large building, some yards distant from the explosion, was put into a tremor, far beyond what could be expected from so small a quantity of powder.

He satisfied his third inquiry, whether, and in what manner, sound made under water can be heard by a land animal that is likewise under water, by procuring a young man to dive down with a bell in his hand; and he assured him, that he heard its tinkling very distinctly, at all depths under water, with little or no difference from what he did when rung in the open air; he likewise affirmed, that he plainly heard the noise and rushing of the water, which came violently through a flood-gate, about 20 feet distant from the place he was then in.

The Substance of some Experiments of Planting Seeds in Moss, lately made by Mr. Charles Bonnet, of Geneva, F. R. S.* N° 486, p. 156.

Mr. Bonnet was inclined to try whether plants were capable of vegetation, when they were only set in moss, instead of being planted in the earth. With

* Charles Bonnet was a celebrated naturalist of Geneva, where he was born in 1720. Very early he applied to the study of natural history, and particularly to the study of insects, his first knowledge of which he received from the works of Reaumur. He was indefatigable in his inquiries into the habits and generation of that tribe of insects termed in French pucerons, in English tree lice. In

this design, he filled with moss several garden pots, and he compressed the moss more or less, as he judged the several plants he intended to place in them might respectively require a closer or a looser soil.

He then sowed in moss, wheat, barley, oats, and pease. And he found, first, that all the grains sowed in that manner came to maturity later than those of the same sorts which were sowed at the same time in mould. 2dly. That the stems from the several grains sowed in the moss were generally taller than those which sprung from the ground. 3dly. There came from the grains sowed in the moss a greater number of blades than from the grains sowed in the earth. 4thly. The grains sowed in moss produced more plentifully than the others. 5thly. Those grains that were gathered, from the produce of those which vegetated in the moss, having been again sowed some in moss, and some in earth, succeeded well in both.

Mr. Bonnet has also planted in moss, pinks, gilly-flowers, daisies, tuberoses, tulips, hyacinths, jonquils, and narcissuses; and all these plants succeeded as well as others of the same sorts, which he at the same time planted in mould. He also placed in moss, cuttings and layers of vines, and these cuttings and layers became vines; and these vines in a short time grew larger than others, that came from cuttings and layers planted at the same time in the ground.

A Continuation of an Account of an Essay towards a Natural History of Carolina, and the Bahama Islands. By Mark Catesby, F. R. S. with some Extracts out of the Appendix. By C. Mortimer, Secr. R. S. N° 486, p. 157.

This is the 11th extract from Mr. Catesby's work, and contains an account of various animals and vegetables, brought down to the conclusion of this author's Natural History. The descriptions, says Dr. Mortimer, of this magnificent

making his observations on this minute part of the animal creation, he was obliged to have frequent recourse to the microscope, in consequence of which his eyes became disordered, and his general health much impaired. But as he possessed a mind which could not endure to be unemployed, he turned his attention to the physiology of vegetables, and published some interesting observations on the use of the leaves of plants. He further engaged in metaphysical inquiries, the result of which he communicated to the public in his Essay on Psychology. His principal works on natural history are his Insectologie, and his Contemplation de la Nature; which last has been translated into most of the European languages, and into the Italian, with valuable notes and additions, by the Abbe Spallanzani. Mr. Bonnet's collected works, printed at Neufchatel 1779, amount to 3 vols. 4to.

Mr. B. was a member of various learned societies, and corresponded with the most distinguished naturalists and physiologists of his time; such as Reaumur, de Geer, du Hamel, Haller, Spallanzani, &c. &c.

His ardour for scientific pursuits did not prevent him from serving his country in a civil and political capacity. He was a member of the Grand Council of the Republic of Geneva, and for many years he took a leading part in the deliberations of that assembly. He died in the 73d year of his age, in 1793.

work are given both in English and French; and the figures being drawn by the ingenious author after life, were afterwards etched by himself, and all the illuminated sets were coloured under his directions, and all touched up and finished by his own hand.

The Inscription on a Roman Altar found near Stanhope in the Bishopric of Durham. Communicated to the Royal Society by the Rev. Thomas Birch, F. R. S. N° 486, p. 173.

SILVANOINVICTOSACRVM
CTETIVSVETVRIVSMICIA
NVSPREFAIAESEBOSIAA:
NAEOBAPRAMEXIMIAE
FORMAECAPTVMQVEM
MVLTIANTECESSO
RESEIVSPRAEDARI
NONPOTVERVNTVSLP

Silvano invicto sacrum
C. Tetius Veturius Micia-
nus, Præf. Alæ Sebosia-
næ, ob Aprum eximiæ
formæ captum quem
multi Antecesso-
res ejus prædari
non potuerunt Votum solven lubens posuit.

Of an Extraordinary Fish, called in Russia Quab; and on the Stones called Crabs' Eyes. Communicated by Mr. Henry Baker, F. R. S. N° 486, p. 174.

As to the quab, which some report to be first a tadpole, then a frog, and at last a fish, it is very well known, says Dr. Mounsey, physician in Russia, to him; but with regard to such changes, he believes them to be entirely fabulous. He has indeed seen, in the chamber of rarities at Petersburg, this fish, preserved in spirits, under all these appearances; but was not permitted to take out any one of them, in order to remove the scruples he made; however, desiring as far as possible to come at the truth, he turned the bottle hastily on one side, to make the fish fall to the glass, which he thought they did with more seeming hardness than could be supposed in fishes; which induced him to conjecture, that they are pieces of art, the idea of which has been taken from the resemblance of the head of this fish to that of a frog. Whence he supposes they may be made of wax, and kept in this manner to amuse the world. If there be, he says, such a thing in nature, which he does not think probable, it must be peculiar to some one place, of which he has no knowledge. He has made inquiries about these imagined changes, of people of many nations, but could never learn any thing to the purpose. He has seen the fish itself in several countries, and found they spawned like other fishes, and grew in size, without the least similitude to what has been asserted. He adds further, that these fishes delight in very clear water, in rivers with stony or sandy bottoms, and are never found in standing lakes, or rivers passing through marshy or mossy grounds, where frogs chuse most to be.

As to inquiries concerning the crabs' eyes, he expresses a surprise to find naturalists differ so much from each other, and yet not one of them he has ever seen giving any true account of the situation, formation, and casting of these concreted bodies. He therefore is so obliging to send the following description from his own observation and knowledge. Those concretions called crabs'-eyes, are found, he says, in the bodies of craw-fish. Each fish annually produces two, one on either side of the anterior and inferior part of the stomach, and each is generated about a point lying between the coats of it. The flat or concave side lies next the internal coat, which is very thin and clear, though strong and horny; the convex side is consequently outwards, and is immediately covered by the fleshy and softer coats of the stomach, whose fibres make impressions on its surface. Between these two membranes it grows by degrees lamellatim, and is supplied with petrifying juices discharged through the mouths of vessels or sudamina opening on the internal surface of the outer coat. The inner membrane, being horny, gives resistance only; hence the stones are concave on that side, and the first remarkable scale, on which all the others are formed, may be perceived in the centre, the brims or circumferences of many of the rest being very apparent. At the time when these stones are not to be found in the animal, there are little circular spots, somewhat opaque, and whiter than the rest of the stomach, to be perceived in their place; nearly opposite to which are tenacious mucilaginous substances, formed like little placentulæ, and called by some the glands of the brain; these are larger, and more perceptible, when the stones are wanting; but are not turned into stones by different degrees of induration, as some have imagined them to be.

It is believed, he says, that they cast these stones with their shells, which they shed every spring, but he finds this is not the way of getting rid of them; for, a little before, or after the time of their casting their shell, the stones break through the internal or horny coat of the stomach, and being ground or broken by the three serrated teeth in it, become dissolved in the space of a few days, which makes it difficult to find them just at this time, and so gives ground to imagine they are cast with the shells. He says however, he has found several of them in the stomach partly consumed; and a further proof that they are so consumed is, he thinks, their being never discovered in rivers, though the fish themselves be in great plenty there; and in the shops it is observable, that many of these stones are of a brown hue; which is the case of such as have been already lodged in the cavity of the stomach, when the fish was taken. They likewise eat the old shells immediately after shedding them.* What the use of these stones to the creature is, he cannot positively determine, but supposes they may be designed to furnish new petrescent juices to its fluids; which may be also assisted by the old shells which they devour, the particles of which, as well as of the stones, are probably disposed of, according to their degree of purity, and properly deposited at the extremities of vessels, for the reproduction of their annually new crusty dress; which, he observes, does not greatly recommend the opinion that these stones have a dissolving quality, of service against the stone in the human kidneys or bladder.

The Doctor has sent along with this particular account, specimens of the craw-fish both boiled and raw, which differ little or nothing from those catched in our rivers here; in which it is assured the like concretions may be also found at a certain time of the year: he has likewise sent some of their stomachs dried, where the stones appear, situated in the manner above described between the 2 coats; and in one of them they are got through the internal coat into the stomach itself. He also sent several specimens of the beginning scales, or concretions, of different sizes, which he collected himself, in dissecting these creatures; several of the formed stones of his own taking out, some of a larger size, which were given him by a gentleman, who took them out of the craw-fish in the river Donne, and others still larger, which he chose from the apothecary of the army. These last were from Astracan: and he observes that the fish and stones are much the largest in the great rivers there, where there are fishers for craw-fish on account of the stones only; which they separate from the fish at different fisheries after different manners. At some they are beaten to pieces with wooden pestles; then washing away the flesh and shells, the stones are found remaining at the bottom of the vessel; at others they are laid in heaps till they rot; and then being washed, the stones are easily separated and gathered. All the apothecaries' shops throughout the whole Russian empire are furnished with them, and great quantities besides are exported.

This seems to be a very particular and exact account of these productions, which are frequently prescribed in medicine. Their price, we find, is extremely low in the countries where they are gathered; and yet fictitious bodies, made of

* I have, says Mr. Baker, observed the same thing in the small fresh water shrimp; which I have kept in a glass with water throughout several of the periods of its casting its shell, which it does once in about 4 or 5 weeks. The water newt also eats its skin as soon as pulled off, if it be not taken away.—Orig.

chalk, tobacco-pipe clay, or other such like materials, cast in moulds, so as to represent real crabs'-eyes, are often substituted instead of them.

Brevis Historia Naturalis, sive de Vita, Genere, Moribusque Muris Alpini: Autore Jacobo Theodoro Klein, Reipubl. Gedanens. N° 486, p. 180.

An account of the marmot, the natural history of which is now so well known, as to render even an abstract of this paper unnecessary.

Concerning Electricity. By the Abbé Nollet, F. R. S. and of the Royal Academy of Sciences at Paris. Translated from the French, by T. Stack, M. D., F. R. S. N° 486, p. 187.

When a vessel full of liquor, which runs out through a pipe, is electrified, the electrified jet or stream is thrown farther than usual, and is dispersed into several divergent rays, much in the same manner as the water poured out from a watering pot. Every body at first sight judges that the stream is accelerated, and that the electrified vessel will soon be empty. Being unwilling to rely on first appearances, M. N. resolved to ascertain the fact, by measuring the time, and the quantity of the liquor running out. And in order to know if the acceleration, supposing there was any, was uniform, during the whole time of the running out, he made use of vessels of different capacities, terminating in pipes of different bores, from 3 lines diameter to the smallest capillaries; and the following contain in gross the result of upwards of a hundred experiments, as it is not so easy a task to draw a safe conclusion, as may at first be imagined.

1. The electrified stream, though it divides, and carries the liquid farther, is neither accelerated nor retarded sensibly, when the pipe, through which it issues, is not less than a line in diameter. 2. Under this diameter, if the tube be wide enough to let the liquid run in a continued stream, the electricity accelerates it a little; but less than a person would believe, if he judged by the number of jets that are formed, and by the distance to which it shoots. 3. If the tube be a capillary one, from which the water ought naturally to flow only drop by drop; the electrified jet not only becomes continued and divided into several, but is also considerably accelerated; and the smaller the capillary tube is, the greater in proportion is this acceleration. 4. And so great is the effect of the electrical virtue, that it drives the liquid out of a very small capillary tube, through which it had not before the force to pass, and enables it to run out in cases where there would not otherwise have been any discharge.

These last facts have served as a basis to his inquiries. He considered all organized bodies as assemblages of capillary tubes, filled with a fluid that tends to run through them, and often to issue out of them. In consequence of this idea, he imagined, that the electrical virtue might possibly communicate some motion to the sap of vegetables, and also augment the insensible perspiration of animals. He began by some experiments, the result of which confirmed his notions. He electrified, for 4 or 5 hours together, fruits, green plants, and sponges dipped in water, which he had carefully weighed; and he found, that after this experiment, all these bodies were remarkably lighter than others of the same kind, weighed with them, both before and after the experiment, and kept in the same place and temper. He also electrified liquors of all sorts in open vessels; and he remarked, that the electrification augmented their evaporation, in some more, in others less, according to their differ natures. Therefore he took two garden-pots, filled with the same seeds; he kept them constantly in the same place, and took the same care of them, except that one of the two was electrified for 15 days running, for 2 or 3, and sometimes 4 hours a day. This pot always showed its seeds raised 2 or 3 days sooner than the other, also a greater number of shoots, and those longer, in a given time: which shows that the electrical virtue helps to open and display the germs, and facilitates the growth of plants.

He chose several pairs of animals of different kinds, cats, pigeons, chaffinches, sparrows, &c. and put them all into separate wooden cages, and then weighed them. He electrified one of each pair for five or six hours together: then weighed them again. The cat was commonly 65 or 70 grains lighter than the other; the pigeon from 35 to 38 grains, the chaffinch and sparrow 6 or 7 grains: and in order to have nothing to charge on the difference that might arise from the temperament of the individual, he again repeated the same experiments, by electrifying that animal of each pair, which had not been electrified before; and notwithstanding some small varieties which happened, the electrified animal was constantly lighter in proportion than the other.

Electricity therefore increases the insensible perspiration of animals: but in what proportion? Is it in the ratio of their bulks, or in that of their surfaces? neither the one nor the other, strictly speaking, but in a ratio much more approaching to the latter than to the former. So that there is no room to apprehend that a human person electrified would lose near a 50th part of his weight, as it happened to one sort of bird; nor the 140th part, as to the pigeon, &c. All that he had been hitherto able to learn on this head, was, that a young man or woman, from 20 to 30, being electrified during 5 hours, lost several ounces of their weight, more than they were wont to lose, when they were not electrified. These last experiments are difficult to pursue with exactness; because the clothing, which cannot strictly be compared to the hair or feathers of animals, retains a good share of the perspired matter, and hinders us from forming a good judgment of the whole effect of the electrical virtue.

This forced electric perspiration is very naturally accounted for, if we consider, that the electrical matter pervades the interior parts of bodies, and that it visibly darts from within outward: for it is very plain, that these electrical emanations must carry with them whatever they find in the small vessels, through which they issue.

This explanation will occur to every one who has seen the principal phenomena of electricity. But how shall we account for all the following effects? all those animals whose perspiration is increased on their being electrified; all those seeds which shoot and grow quicker; all those liquors which evaporate; all that acceleration of liquids, flowing through tubes; all those particulars happen in the same manner when, instead of electrifying those bodies themselves, they are only held near electrical bodies of a pretty large bulk. The notion he had for 3 years past formed of electricity, not only affords an explication of this, as simple as the former, but it was this same notion that led him to the experiments, and made him even foresee their success.

Several Essays towards Discovering the Laws of Electricity. By Mr. John Ellicot, F. R. S. N° 486, p. 195.

In the preceding letter from the Abbè Nolet, containing his observations on the increase of the transpiration of animals, and the growth of vegetables, by means of the electrical effluvia, he takes notice, that he was led to those inquiries, from the acceleration which was given to the motion of fluids through capillary tubes, on heir being electrified. As Mr. E. formerly made several experiments on this subject, he submits the following observations on those experiments: in which he has chiefly endeavoured to prove, that the acceleration of the motion of fluids through capillary tubes or syphons, is not barely owing to their being electrified, but that, in all cases whatever, there are some other circumstances necessary, in order to produce this effect. And he doubts not but to make this fully appear, by showing that water, being electrified, may either be made to run in a constant stream through a capillary tube or syphon, or only to drop, as if it had not been electrified at all: and also, that the water may be made to run from the same syphon in a constant stream, without being made electrical, but cease to run, and only drop, the moment it becomes electrical. He first lays down the following general principles: 1. That the several phenomena are produced by means of effluvia. 2. That the particles composing these effluvia strongly repel each other. 3. That the said particles are strongly attracted by most if not all other bodies whatever.

That the electrical phenomena are produced by means of effluvia, is in general acknowledged by all the authors on electricity, however they may differ in opinion with regard to the bodies in which they are contained. The properties abovementioned of these effluvia, may be easily deduced from most of the treatises lately published on this subject. But to leave no room for any objection, he observes, that the existence of these effluvia is proved by all those experiments in which a stream of light is seen to issue from the electrified body; particularly those streams which are seen to issue in diverging rays from the end of the original conductor, when made of metal, and reduced to a point; from their being felt to strike against the hand like a blast of wind, when it is brought near the stream; and from that offensive smell which generally accompanies these experiments, and which is always more perceptible, the more strongly the sphere is excited.

That the particles composing these effluvia repel each other, appears from those experiments in which two bodies, how different soever they may be in kind, repel each other when they are sufficiently impregnated with these effluvia. As a feather, by the exited tube; the several fibres of the same feather, or two cork balls, which will be found strongly to repel each other, as long as they retain any considerable quantity of these effluvia. Which property will always decrease, as the quantity they contain diminishes.

That these effluvia are strongly attracted by most if not all other bodies, is so evident from almost all the electrical experiments, as to make any particular examples of it needless here; especially as there will be occasion to take notice of the strong attraction between the electrical effluvia and water, in accounting for these experments.

Exper. 1. If a vessel of water be hung to the prime conductor, having a syphon in it of so small a bore, that the water will be discharged from it only in drops on the water's becoming electrical by means of the machine; it will immediately run in a stream, and continue to do so, till the water is all discharged, provided the sphere is continued in motion.

That water does not run in a constant stream, but only in drops, from a syphon of a small bore, is doubtless owing to the same cause by which it is sustained above the level in capillary tubes. If therefore water is made to run in a stream barely by its being impregnated with the electrical effluvia, it should follow, that if one or more capillary tubes be placed in a vessel of water; that which is sustained in them would either sink down to a level with the rest of the water, on its being made electrical, or at least that it would not continue at the same height as before; but when the experiment is made, the water is found to continue exactly at the same height, whether it is electrified or not.

Again, if the bare electrifying the water was the cause of its running in a stream, it would continue to run in the same manner, as long as the water continued electrical; which it will not do: for, on stopping the motion of the machine, the stream immediately ceases, and the water only drops from the syphon, notwithstanding its being strongly impregnated with the electrical effluvia. To account then for the water's being made to run in a stream in this experiment, it is observed, that so long as the machine is in motion, there is a constant succession of the electric effluvia excited, and which visibly runs off from the end of the prime conductor in a stream, and as they are in like manner carried off from all bodies hung to it, those effluvia which run off from the end of the syphon, being strongly attracted by the water, carry so much of it along with them, as to make it run in a constant stream.

That the attraction between the water and electric effluvia is sufficient to produce this effect, might be proved by a variety of experiments; but he only observes, that to this attraction it is owing that silk lines and glass tubes (which, from their imbibing so very small a quantity of these effluvia, are generally made use of as supports in many of the electrical experiments) on only being wetted, become strong conductors: and that if an excited tube be held over a vessel of water, the water is found to imbibe a very considerable quantity of this electric matter; and, on the approach of a finger, or any other non-electric body, the water is perceived to rise towards it; and when the finger is brought so near the surface as to draw off the effluvia, they carry several particles of the water along with them towards the finger, in a direction directly contrary to that of gravity; and therefore may well be supposed, when acting in the same direction, to have an influence sufficient to produce a stream, as in the experiment.

And that this current of the electric effluvia is the true cause why the water runs in a stream from the end of the syphon, is further evident, in that whatever tends to increase or diminish the current of the effluvia, produces the same effect on the water. He has already observed, that when the effluvia are strongly excited, they are seen to pass off from the end of the prime conductor in luminous rays; and the same may be observed with respect to those which pass with the water from the end of the syphon; but when any non-electric body is brought under the syphon, as, by its attraction, the current of the effluvia is increased, so these luminous rays are also extended to a greater length. Again, when the motion of the machine is stopped, the current of the electric effluvia is stopped, and the water immediately ceases to run in a stream, notwithstanding its being strongly impregnated with the electrical effluvia.

And that the water is strongly impregnated, will not only appear from the drops being sooner divided into small particles, than they would be if they had not been electrified, but from those particles being separated to a greater distance from each other, by the repulsive property of the electric effluvia; and when any of the water is received into a dry glass vessel, on the approach of a finger towards its surface, a spark issues from it in the same manner as from water electrified by an excited tube; or when any non-electrical body is brought under the syphon, by whose attraction the effluvia may be drawn off, the water immediately accompanies it in a stream.

Exper. 2. If the vessel of water with the syphon in it be suspended by any non-electric body, over another strongly electrified, the water will immediately run from the syphon in a stream; but if supported by a piece of silk, or any other electrial body, the water will immediately cease running, and only be discharged in drops. These phenomena may, from what has been already said under the former experiment, be easily accounted for.

That the water is made to run in a stream, is plainly owing to the mutual attraction between the electrised body and the water; which attraction will continue as long as the vessel which contains the water, by being supported by a non-electric, is prevented from retaining any of the electric effluvia; these effluvia being drawn off by the non-electric body to which the vessel is suspended: but on the contrary, when the vessel is suspended by an original electric, the effluvia, not being attracted by it, will be prevented from running off, and the water will soon be found to have imbibed a quantity of them, sufficient, by their repelling property, to greatly weaken, or wholly to destroy, the former attraction, when the water will cease to run in a stream, and only drop, as if it had not been held near any electrised body.

In order to discover the laws of electricity, Mr. E. states the following experiments.

Exper. 1. If a glass tube be rubbed by a very dry hand, and a finger be brought near any part of it, a spark of fire will seem to issue from it, and strike against the finger; and if the finger be carried at a like distance from the end of the tube towards the hand in which it is held, a number of sparks at a small distance from each other will be seen coming from it, and a snapping noise will be heard. The tube is then said to be excited, or to be electrical; and at some times, when it is strongly excited, sparks will issue from the tube in streams, not only while it is rubbing, but will continue to dart out from it for a considerable time after the rubbing has ceased, and a very strong offensive smell will be perceived.

Exper. 2. If the tube, when thus excited, be held over some pieces of leaf-gold, or any light bodies whatever, they will be attracted towards it; and the more strongly the tube is excited, the greater distance they will be attracted from; and when they come near the tube (though without touching it) they will be repelled from it, and continue to be so, unless touched by some other body, when they will be attracted by the tube as before: but if the tube be but weakly excited, they will be attracted quite to the tube, to which they will sometimes adhere, without being repelled from it.

Exper. 3. If a ball (of cork suppose for lightness) be hung by a silk line, and the excited tube be applied to it; it will not only be attracted, but will have an attractive quality communicated to it from the tube; and if any light bodies be brought near the ball, they will be attracted by it.

Exper. 4. As the tube, when strongly excited, will not only attract, but afterwards repel any light bodies brought near it; in like manner the cork-ball will be endued with the same property; so that a smaller ball will first be attracted towards it, and then repelled from it, the same as the leaf-gold in exp. 2, and on touching any other body it will be again attracted; and this may be repeated several times, when the smaller ball is much less than the larger one, though the effect will constantly be weaker and weaker, as every time the less ball is attracted, it carries off with it some of the electric virtue, and is also endued with the same properties as the larger ball.

Mr. Gray, Mr. Dufay, and others have observed, that this electrical quality is not only to be excited in glass, but in most solid bodies capable of friction (metals excepted); though in some it will be scarcely sensible, and that it is found to be strongest in wax, resins, gums, and glass: and as glass is the easiest procured of a proper form, it has generally been used in making these experiments. It has been further observed, that those bodies in which the electrical quality is capable of being excited the strongest by friction, will receive the least quantity of it from any other excited body, and therefore are properly made use of to support any body designed to receive the electrical virtue. The truth of this will sufficiently appear from the following experiment.

Exper. 5. Hang up two lines, one of silk, and the other of thread; that of thread will be attracted by the tube at a much greater distance than the silk. Again; fasten to each string a feather, or other light body; if the tube be brought to the feather fastened to the silk, it will be first attracted, and afterwards repelled; and from the virtue communicated to it from the tube, the several fibres of the feather will strongly repel each other. But when the tube is brought to the feather fastened to the thread, the feather will be strongly attracted, and continue to be so without ever being repelled, the virtue passing off by the thread it is hung to. If a glass ball be hung to the silk line, it will be weakly attracted by the tube; but one of cork or metal much stronger.

Exper. 6. Let a rod of iron be sustained by silk lines, and by means of a glass sphere (which can be more regularly and constantly excited than a tube) be made electrical, it will be found to have all the properties of the excited tube mentioned in exp. 1. A stream of light will come from the end of it, if it be pointed; it will attract, repel, and communicate this virtue to any other non-electric body; on the approach of a non-electric, a spark of fire, with a snap attending it, will come from it; which spark will be greater or less, as the bodies approaching it have more or less of the electrical quality residing in them; and there will likewise be the same offensive smell as was observed of the tube.

From these experiments, containing the principal phenomena of electricity, may justly be drawn the following conclusions: 1st. That these remarkable phenomena are produced by means of effluvia; which, in exciting the electrical body, are put into motion, and separated from it. 2dly. That the particles composing these effluvia strongly repel each other. 3dly. That there is a mutual attraction between these particles, and all other bodies whatever.

The particles of these effluvia are so exceedingly small, as easily to pervade the pores of glass, as is evident, in that a feather, or any light bodies inclosed in a glass ball hermetically sealed, will be put in motion on the excited tube being brought near the outside of it; and it has been generally thought that they pass through the pores of the densest bodies; and there are several experiments which render this supposition not improbable ; though he acknowledges he has not yet met with any one that he thinks quite conclusive.

He now proceeds to show how, from these principles, the phenomena of some of the more remarkable experiments of electricity may be accounted for.

Exper. 7. Let a rod of iron, pointed at one end, be suspended on silk lines, as in exp. the 6th, and by the sphere be made electrical. When the rod is strongly electrified, a stream of light in diverging rays will be seen to issue from its point; and if any non-electric body be held a few inches from the point, the light will become visible to a greater distance; and if the non-electric body be likewise pointed, a light will seem to issue from that in diverging rays in the same manner as from the electrified rod. But if the non-electrical body be flat, and held at the same distance from the rod as the pointed one was, no light will be seen to come from it.

The principal phenomena to be accounted for in this experiment are; why a light is only seen at the point of the rod, and not through the whole length of it? why the light is visible to a greater length, when the point is approached by a non-electric? and, why a light is seen to issue from the non-electric when it is pointed, and not when it is flat.

On which he observes, that whenever the sphere is excited, the electrical effluvia are put into motion, and made to form an atmosphere round about it; whence, by their repulsive property, they endeavour to expand themselves on all sides equally; but being strongly attracted by the iron, a great part of them are drawn off along the rod, about whose surface they likewise form an atmosphere, which will be denser or rarer, in proportion as the attraction of the rod is greater or less; and as the repulsive power of these effluvia will always increase in proportion with their density, it will follow, that whenever the sphere is so strongly excited, that the effluvia surrounding it are denser than those surrounding the rod, they will, by their repulsive property, drive the effluvia off from the end of it in a stream, and that with a very great velocity; as is evident, from their striking against the hand like a blast of wind when brought near the end of the rod: and as this velocity is partly owing to the attraction of the rod, so this attraction continuing quite to the end of it, the velocity of the particles will there be greatest; and as they approach towards the point, they will be brought nearer together, and therefore become denser there than in any other part of the rod; and therefore, if the light be owing to the density and velocity of the effluvia, it will be visible at the point, and no-where else.

And that the light is thus produced, will appear by considering that whatever increases or diminishes either the velocity or density of the particles, will increase or diminish the light. For, if the motion of the wheel which turns the sphere be stopped, the current of the effluvia will also be stopped, and the rays of light will no longer be seen to issue from the point, and yet the whole rod will continue to be electrical; but on putting the sphere again into motion, the effluvia will become visible as before, and will increase as the sphere is more strongly excited. Again, the light will be visible to a greater or less distance, as the point is more or less acute; and this light is always brightest next the point, and grows fainter as the rays diverge, this is plainly owing to the different density of the rays at equal distances; for when the point is more acute, the rays will diverge less, and therefore will be denser to a greater distance than when it is less acute.

When a non-electric with flat end, is brought within a few inches of the point of the electrified rod, the electric stream will be attracted by it, and the rays made to diverge less than before; and the effect will be the same as if the point was more acute; viz. a continuation of the light to a greater distance, and which will be further increased by the additional velocity the particles will acquire from the attraction of the non-electric.

Mr. E. now endeavours, from the same principles, to account for those phenomena, which are produced on a nearer approach of the non-electric to the electrified rod.

Exper. 8. If the non-electric body, whether flat or pointed, be brought nearer to the end of the rod, than in the last experiment, there will be a small stream of light produced, reaching quite from the electric to the non-electric body; and if brought still nearer, there will issue a spark attended with a small snapping noise, which will be succeeded by others at equal intervals; and if the non-electric be held at some distance from the side of the rod, its point will often appear luminous, but no part of the electrified rod will be so. If it be brought nearer, there will likewise be sparks produced at nearly equal intervals from each other, which will sometimes appear as issuing from the side of the electrified rod, at others as coming from the non-electric. If a finger be used as the non-electric, it will receive a smart stroke; and if spirit of wine, heated so as to emit an inflammable vapour, be made use of, it will be kindled by the spark.

These phenomena may, on the afore-mentioned principles, be thus accounted for. If the non-electric rod be pointed, and brought so near as, by its attraction, to prevent the rays issuing from the point of the electrified rod from diverging, they will be drawn off parallel to each other, and consequently be equally luminous throughout the whole distance between the two rods.

If the non-electric be brought still nearer, the attractive force will be so much increased, as not only to affect the effluvia, when they are driven off from the point of the electrified rod, but to be capable of drawing them off from a considerable part of the rod beyond the point; and that with a velocity, and in a quantity, sufficient to occasion both the spark and blow, as well as the noise that is heard.

The same is the case, when the non-electric rod, or a finger, is held against the side of that which is made electrical: at a greater distance a light will appear as issuing from the non-electric, the particles attracted from a large surface of the rod (and therefore not visible as coming from it) being made to converge to a point, are rendered luminous, and if brought nearer there will issue sparks in the same manner as when held to the end.

The sparks are always produced in the space between the non-electric and the rod, and often appear as issuing from the non-electric. This appearance is probably owing to those particles which, by their elasticity, are reflected back again from the non-electric towards the rod, and which, by striking against those coming from it, produce both the sparks and noise that is heard; and as the particles often appear in luminous rays at the point of the non-electric, it thence happens, that the spark is often kindled so near to the non-electric, as to appear as issuing from it.

Several ingenious gentlemen, from this appearance of a light at the point of the non-electric, have imagined there was a current of electrical effluvia continually issuing out of it, and which, setting in towards the electrified rod, was the cause of the attraction of the electricity: and this conjecture of theirs will seem to be greatly favoured by the following experiment.

If some of the fibres of a down-feather be fastened to the end of a small skewer or wire, and made electrical, they will strongly repel each other, and will expand themselves on all sides to the greatest possible distance from each other; but if a non-electric person bring the point of a pair of compasses, or any other small-pointed body near them, they will be repelled from it, and driven up together as with a blast of wind; and in the dark a light will be seen as issuing from the point; from whence it might be concluded, that the fibres are repelled by effluvia issuing out of the point of the non-electric.

As the Abbé Nollet endeavours to account for the attraction of electricity on this principle, Mr. E. offers some considerations which, notwithstanding these appearances, have induced him to be of a different opinion; and they are founded on the following observations. 1. That however replete any bodies may be with the electric matter, none of these phenomena are ever produced, unless the effluvia are first excited in some particular body, and put in motion, either by rubbing, or some such like operation. 2. That the effluvia are not to be equally excited in all bodies, but much stronger in some than in others; and that, in particular, they are not capable of being at all excited in metals by friction. 3. The attractive and repulsive property will be stronger or weaker in any body, in proportion to the quantity of excited effluvia with which it is impregnated. 4. That those bodies which are most easily excited by friction, will receive the least quantity of the electrical effluvia from any other excited body; and, on the contrary, metals, or those bodies in which they cannot be excited by friction, will receive the most.

From these observations he thinks it may be shown, that this appearance of light is so far from proving that the effluvia come out of the non-electric, at whose point they are visible; that from thence it cannot be concluded the body has any of the electrical matter residing in it, but is rather a proof to the contrary. For the same appearance would be produced from the setting in of the effluvia into the non-electric; and this might be confirmed, if necessary, by a variety of experiments. And as those bodies, at whose point this light appears the strongest, afford us no signs of their having any of the electrical effluvia residing in them, either by their attracting or repelling other bodies, or by their being capable of being excited in them by friction, as in glass, &c. nor in short any sort of evidence whatever, but what arises from this appearance; may we not expect some better proof of their being possessed of these effluvia, before we admit of their issuing out of them?

But the true cause of this remarkable phenomenon he apprehends to be, the different density of the effluvia at the extremities of the 2 bodies: for the effluvia will be much denser at the extremity of a pointed body than at an obtuse one: and as the force by which the particles endeavour to expand themselves, increases in proportion to their density, it follows, that the particles will be reflected back with greater violence from the pointed body than the other; and this force exceeding the attractive power of that particular part of the feather, to which it is directed, the fibres will be repelled by it; whereas the force, with which the particles endeavour to expand themselves from the obtuse body, being less than the attractive power, it follows, that the fibres of the feather will continue to be attracted by it.

Exper. 9.—Take two plates of metal, very clean and dry, whose surfaces are nearly equal; hang one of them horizontally to the electrified rod, and bring under it on the other any thin light body, as leaf-silver, &c.; when the upper plate is made electrical, the silver will be attracted by it; and if the under plate be held at a proper distance, will be perfectly suspended at right angles to the plates, without touching either of them; but if they be either brought nearer together, or carried farther asunder, the leaf-silver will cease to be suspended, and will jump up and down between them. The same effect will be produced, if you reverse the experiment, by electrifying the bottom plate, and suspending the other over it.

If the upper plate be electrified when the leaf-silver is brought near, it will be attracted upwards by it, and thus become electrical, and as long as it continues to be electrical, it will be attracted downwards by the non-electric plate. Whenever, therefore this last attraction added to the gravity of the silver, which acts in the same direction, is equal to the contrary attraction upwards, the leaf-silver will, by means of these 2 opposite forces, be kept suspended between the plates, and will continue to be so as long as the equality of these forces is preserved.

That the leaf-silver is always nearer to the non-electrical than to the electrified plate, is owing to its receiving its supply of effluvia from the atmosphere surrounding the electrified plate: for as the plate is more strongly electrified than the silver, its atmosphere of effluvia will be denser to a greater distance than that surrounding the leaf-silver, and therefore can supply an equal quantity at a greater distance than what the lower plate can receive from the silver, whose atmosphere is rarer; and therefore, as the silver will always be suspended in that part where the 2 currents are equal, without which the proportion would be destroyed, it will consequently be always nearer to the non-electrical than to the electrified plate. If the experiment be reversed, by electrifying the under plate, and making the upper one the non-electric, the only difference will be, that the gravity of the silver must then be added to the attraction of the electrified plate, and will therefore cause the silver either to be nearer the non-electrical one, or the plates to be moved a little farther asunder, or perhaps both.

A Brief Account of a Roman Tessera. By Mr. John Ward, F.R.S., and Prof. Rhetor. Gresh. N° 486, p. 224.

The brass plate, which accompanies this paper, was dug up, some time since at Market-street in Bedfordshire; which lies in the Roman road called Watling-street, about 5 miles on this side Dunstable. The inscription engraven on the 2 sides is,

TES. DEI. MAR
SEDIARVM

which words may, as he apprehends, be read at length in the following manner:

Tessera Dei Martis Sediarum.

Of a very learned Divine, who was born with Two Tongues. By Cromwell Mortimer, M.D., and Sec. R.S. N° 486, p. 232.

[The supposed 2d tongue here mentioned was probably nothing more than an enlargement of the sublingual gland, which in process of time contracted to its natural dimensions.]

On the Sounds and Hearing of Fishes. By Jac. Theod. Klein, R.P. Gedan. F.R.S. Or some Account of a Treatise, entitled, "An Inquiry into the Rea-

sons why the Author of an Epistle concerning the Hearing of Fishes endeavours to prove they are all mute and deaf." *By Richard Brocklesby, M.D., F.R.S.* N° 486, p. 233.

Mr. Klein in the first place classes fishes into 2 orders, the first with lungs, the other furnished with orrgans analogous to lungs, which we call fish-ears, or gills: all the whale-kind, the dolphin, porpoise, and such like, have lungs. There are 2 families of the 2d class, to one of them belongs all that tribe, which have 1, 2, 5, or 9, air-holes at the back, or sides of the head, or in their thorax, in which concealed gills are found: the other family comprehends all kinds of fishes, whose gills are usually placed on each side of the back of the head. Our author's antagonist alleges, that all fishes of both orders are equally deaf; but that all naturalists except Reaumur are of a contrary opinion, that fishes hear distinctly.

He begins with an air of ridicule, and shows how far the letter-writer is ignorant of the various opinions, modern as well as ancient. Mr. Ray thinks to reconcile these, by allowing that some hear, while others are deaf; but the greatest part allow that fishes actually hear; and most except Scheuchzer, seem agreed about the auditory passages. But the letter-writer denies they have any organs of voice, merely on the proverbial authority, "mute as a fish;" hence he concludes they are likewise deaf. But in answer, it is replied, the spouting whale has all its internal organs, precisely similar to the organs of voice in other creatures, and therefore they may answer the same purposes, nay actually serve this end: for when the whales in the Greenland fishery are struck, they roar frequently so loud, as to be heard at 2 French miles' distance.

But some of the 1st family of our 2d class, as the skate, lamprey, conger, and others, our author has heard utter some kind of noise; and gives his opinion, that most sorts of cartilaginous fishes can do the same. From analogy he argues, that as no beast, from the lion to the meanest animal, nor from the eagle to the humming-bird, but can utter a voice, so he thinks the same general law is observed in the economy of fishes: but at the same time our author here seems to lay too much weight on what he supposes final causes, and metaphysical arguments, which have in all ages ruined natural philosophy.

But the letter-writer queries, whether fishes may not be mute in our air, and yet capable of some voice in their own element. Mr. K. takes the noise which carp and such fish make in hot weather, on the surface of the water, to be a voice: and this is most remarkable when the male impregns the row which the female has before deposited; yet this is often heard, when the fish is 6 or 7 inches under water. Mr. K. further enumerates many foreign fishes, and particularly our smelt, which put alive into vinegar hisses very audibly.

The letter-writer had objected against fishes, that they have no occasion for hearing because they never copulate, as other animals do: but Mr. K. describes the manner of whales, which is performed as that of other animals; and observes, that they bring forth their young alive: these follow the female, and suck milk from the teats, which are placed in them near the organs of generation; and in violent storms the dam takes her offspring into her mouth, and protects them from danger. This last is common to several of the skate-kind.

The letter-writer alleges, that fish never sleep; but our author assures us, that all such as have lungs do in the night-time, thrusting up their nostrils into the open air. For others he cannot be positive, as their history is little known.

The letter-writer premises 2 questions; first, whether fishes have any ears? or if the gills serve the same purpose? and answers positively in the negative to both: and therefore concludes they cannot hear. But Mr. K. asserts, that snakes, frogs, cameleons, and others of the lizard-kind, actually hear, without any of the usual external apparatus of hearing. For though they want the auricles and ears, yet have they auditory passages, by which sound is conveyed, and even internal organs, to which the meatus auditorius reaches. But Mr. K. further asserts, that all the whale kind, and in general such fishes as have lungs, have likewise a meatus auditorius, and the internal organs of hearing; and appeals to a public dissection of a porpoise, and another fish of the whale kind, made by himself; in which the os petrosum, with the other parts of these organs, had been separately shown; and calls in the concurrent testimony of Dr. Tyson, in his anatomy of a porpoise. [See also the writings of Camper, Monro, Cuvier, &c.]

Thus having satisfied us about such fish as have lungs, he goes on to consider the cartilaginous species, such as the skate, ray, and lamprey kind, which have organs of generation, and copulate like brutes; yet exclude the fetus while yet in the egg-state: and this from analogy, that these, and in general all other fish, as they have organs which serve them for lungs, so they may have what answers in others to the apparatus of hearing. [The organ of hearing in these fishes has been described by J. Hunter, and other anatomists.]

In proof of this he asserts, that all kinds of fish except these which have lungs, are always found to have stones in their heads naturally formed, and invariably placed in the same situation, being joined to the contiguous parts with ligaments and nerves, which take their rise from the substance of the brain; and having examined the head of a pike minutely with a microscope, he discovered the auditory pores in the stones, and persuades himself, that 3 pair of stones are to be referred to this use; therefore concludes, as there is some analogy in the organs, that all fishes in some measure hear.

The letter-writer further objects, that water is not the medium of sounds; and though air is actually contained in all water, yet it cannot be put into undulations, any more than the circumambient water; but that would require a much greater vibration than the external air can give. Thus, says he, if a person immerge his head a foot under water, he will hear nothing but a boiling din; and how ever great a noise is made in the open air, the event will be still the same; and if the air itself be put into the most violent agitation, the person will discover no difference in that sensation of his ears from what he perceived in the stillest water. Hence he concludes water incapable of transmitting sounds. Mr. K. replies, that as fishes are unanimously agreed to be capable of smelling, so, by analogy, it is probable they have hearing; for odours are conveyed by the air as well as sound. But he thinks the unnatural position of a man's head immerged a foot under water may be some cause for that confused noise, and opposes the experimental testimony of Abbé Nollet himself, who went different depths under water to satisfy himself how far sounds could be conveyed in that medium. At 4 inches under water he heard the sound of a gun discharged, of a clock striking, and of a hunter's horn: these, repeated at different depths, were heard first at 4, then at 8, afterwards at 18 inches, and lastly at 2 feet. A man's voice was also heard in the same manner. At different depths of water, none of them exceeding two feet, he could perfectly distinguish mixed sounds, when 2 bells were struck, or 2 pipes sounded together. He could distinguish under water, very distinctly, words uttered aloud: and proved this assertion, by declaring, when he came above water, what was said while he was under it. All sounds were heard more faintly, and attenuated; yet the difference of the sound, at 4 and 18 inches depth, was not answerable to the difference of the depth of water. He observed at first, that momentary sounds were not so well conveyed as continued; yet he afterwards determined, at the same depth, one tap of a drum-head, as plainly as a continued round. This he thinks was the same in a man's voice, and the sound of a pipe; but ingenuously owns, he was not fully satisfied in this experiment; and therefore does not lay so great a stress on its certainty as on the former.

Lastly, he held his head under the surface of the water, so as barely to cover him; but he could not hear the clock strike, which was audible in the open air at 45 feet distance, especially on a plain.

The Abbé therefore concludes, if fishes do not actually hear, it is for want of proper organs, and not because the medium cannot convey sounds.

Our author mentions the common notion of carp, and other fish, coming out of their holes at the sound of a bell to be fed; and adds a story, which Mr. Boyle somewhere relates, that near Geneva a man had a fish-pond, whose banks were so high from the plain on which it was, that one could not look over them into the pond; and therefore it was impossible the fish could see the person; yet they were at any time convened at certain sounds by the gardener, in order to be fed, as a creditable person asserts.

The letter-writer, having made a high partition in a pond, watched while an accomplice behind it made a very great noise, and discharged a gun, in order to frighten the fish, if possible, that were playing on the surface of the water; but they did not give any attention; yet as soon as ever they came in sight, the fish immediately made off.

Mr. K. thinks this objection of little weight, because the question is not, whether fishes, when they see nothing, can be frightened by sounds only.

On the whole, Mr. K. shows himself an experienced and diligent naturalist, and will (Dr. B. doubts not) be allowed to have fully proved the falsity of any assertion, that all fishes are entirely mute and deaf.

Of the Poisonous Root lately found mixed among the Gentian. By Dr. Brocklesby, F.R.S. N° 486, p. 240.

The following account is the best Dr. B. had received of the poisonous effects of a noxious root, lately found in a parcel of gentian, and exhibited for use to several persons instead of it. And as it is attended with such dangerous consequences, he thought even an imperfect relation of facts had better be given immediately, than to expect more circumstances, and wait so long for them, till greater mischiefs might happen, by the inattention of such as are constantly administering medicines. The account was sent by a gentleman of Hambleden parish, Buckinghamshire; and is found to agree in general with some other fatal instances that have since happened in London.

Mary Burgess, aged 60 years, about 5 o'clock in the morning, drank of an infusion of only 1 pennyworth (without other ingredients) of supposed gentian root, in $\frac{1}{4}$ pint of white wine: it is uncertain what precise quantity she took; but in 2 hours afterward she faltered in her speech, had such twitchings and convulsions of her hands, that the ignorant by-standers alleged the poor woman was drunk; and so left her in bed till 12 o'clock, to sleep it out. On their return however she appeared much worse, was speechless, and remained so for 3 whole days, and knew no person all that time. In her illness a purging came on, and at last carried her off.

Katharine Woodward, aged 44 years, took about a tea-spoonful of the same wine, and soon after fell down speechless, and her limbs were paralytic near 36 hours. She afterwards recovered her speech, but continued ill above a fortnight, and part of that time her under jaw was convulsed, and she bled both at mouth and nose in the beginning.

Mary Diggins, aged 33 years, tasted a much less quantity of the same wine than the former had done; and though terrified at her neighbour's bad symptoms, she drank warm water with oil, in order to vomit; yet she soon staggered, and grew delirious, could not swallow any solids, and lost the perfect use of her eyesight for a fortnight.

The vague reports of these, and Mr. Pots's cases induced him to obtain the favour of 2 or 3 druggists to look over some gentian root, one parcel of which had no less than a 20th part of a root, which at first sight was discovered to be no gentian.

This root, for which we have yet no name, is of a greyish brown colour externally, but it is browner and more resinous internally: most of that which he had seen was about the thickness of a finger; though some was much larger and whiter; which was a reason with several for thinking there were 2 sorts of it; and indeed some pieces emit a stronger and more nauseous smell: but this he apprehends might be occasioned only by a larger quantity of resin in them. All of them were of an acrid pungent taste, and left a dryness on the tongue.

He therefore judged it necessary to try what effects this root might have on dogs, that he might the better conjecture concerning them on the human species; and though no man has any right wantonly to torture or destroy in a cruel manner the least animal; yet when good purposes are answered in the whole by inferior natures yielding to superior ones, a man may, without just imputation to his moral character, sacrifice the interest of a baser order to the happiness of one superior.

With this intention he decocted $\frac{1}{2}$ oz. of this unknown root, powdered grossly, in 10 oz. of fair water, till 2 were evaporated; he then let the decoction stand 6 hours. After this he gave half of it, stirring up the powder, to a young dog. This made him instantly foam at the mouth; he grew sick, and vomited part of the dose; yet in less than $\frac{1}{2}$ an hour he reeled like one drunk, had twitchings of his limbs, and after some time the motion of his heart was irregular, and intermittent, though strong: he was sleepy about an hour, but came gradually to himself in $\frac{1}{2}$ an hour more, and ate victuals, which before he refused.

Two days after, the same dog took 4 oz. of decoction of gentian made as strong as the former; but he discovered not any bad symptom from it. He used this quantity, as gentian root is sometimes given to that quantity in the practice of physic. At above 10 days after he took the first decoction, and continued well.

Another dog took above a drachm weight of the unknown root, finely powdered, and mixed with butter: it instantly made him foam from the mouth, and caused sudden vomiting, and in $\frac{1}{2}$ an hour weakness of his limbs, and staggering, which lasted $\frac{1}{2}$ an hour, after which he recovered.

He tried to give a large quantity to another dog; but being too much like other irritating medicines, it caused so great a vomiting, as destroyed the effects which a smaller quantity had before produced. One of the dogs had some loose stools after taking it; another urined plentifully. Like experiments were made by Mr. Pierce at St. Thomas's Hospital, which had nearly the same event.

Though none of the dogs were killed by this drug, but remained to appearance well, yet all apothecaries have sufficient reason to examine very strictly their gentian, and to reject what they find not genuine, since one of the women before mentioned, and a man that he had heard of, were both dead; and since gentian is of general use in medical compositions, as well as the primary ingredient in the cordial bitters ladies make for their own use.

Of Large Subterraneous Caverns in the Chalk Hills near Norwich. By Mr. Wm. Arderon, F.R.S. N° 486, p. 244.

About a quarter of a mile from the city of Norwich, on the east side, and near the entrance of Moushold-heath, is a large subterraneous cavern, which has been formed in a long series of time, by digging out chalk for making lime. There is only one entrance into it, about 2 yards wide, and nearly the same height; however the height gradually rises, till at last it measures in some places from 12 to 14 yards. But though the entrance is so small, the whole area within is of such a large extent, that 20,000 men might with great ease be placed in it, and from the entrance to the farthest part of these cells, measures full 400 yards; and these passages are often 10 or 12 yards wide, with branchings out on the sides, into various lanes and labyrinth-kind of windings, which every now and then open into each other; which renders it no easy task to find the way out, when a person has been a little bewildered in these subterraneous mazes.

Most of these vaults are arched at top, by which the immense weight above is well supported; a weight no less than that of hills, whose perpendicular altitude above the tops of these arches is 20 or 30 yards, if not much more.

How deep or thick these rocks of chalk are, no one can tell; for in sinking the lowest wells, they have never been dug through; and consequently must be exceedingly deep. The chalk at the farther end of this cavern is so very soft, that it may be moulded with the hand like paste; which is probably its original consistence, and what it always retains, till it becomes exposed to the air. In the very lowest parts of these vaults Mr. A. has picked up several kinds of fossils, figured by marine bodies; such as echini, pectunculi, common or fluted cockle, belemnitæ, &c. Sounds made beneath these arched roofs are strongly reflected from side to side; so that the least whisper may be heard at a considerable distance. The beat of a pocket-watch was heard distinctly full 20 yards from where it was placed.

He visited this place on the 1st day of November last, to try the temperature as to heat and cold; and carried with him a thermometer regulated by one of Mr. Hauksbee's, which he set down at the farther end of these caverns; and letting it remain there for some time, he found the mercury rested at 52°; which comparing with the register he had kept, was within half a degree of a medium between the greatest heat and the sharpest cold known in that city for 10 years past; and it is very probable, if the 2 extremes had been taken more exactly, the temperature in these caverns would be found to come yet nearer to the medium of heat and cold in this climate; which is also within 1 degree of the temperature recorded of the cave at the Paris observatory.

At the foot of a high hill, adjacent to these vaults, issues out a curious spring, the water of which he found exactly of the same temperature with that underground; though, when the thermometer was exposed to the open air, it stood at 57°.

N. B. A terrible thunder storm, June 12, 1748, shook the earth to such a degree, as to throw down those chalk vaults.

Some Observations and Experiments made in Siberia, extracted from the Preface to the Flora Siberica, sive Historia Plantarum Siberiæ cum tabulis æri incisis. Auct. D. Gmelin. Chem. et Hist. Nat. Prof. Petropoli 1747, *4to. Vol.* 1. *By John Fothergill, M. D. Lic. Colleg. Med. Londin.* N° 486, p. 248.

By direction of the late Empress of Russia, several members of the Royal Academy of Sciences at Petersburg undertook a journey into Siberia, in order to inquire into the natural history of that country, and to make such experiments and observations, as might tend to give a just idea of that almost unknown region, and to the improvement of physics in general.

Dr. John George Gmelin, professor of chemistry and natural history at Petersburg, was placed at the head of this deputation, who, besides several of his colleagues, and some students, had a painter or two, a miner, a huntsman, and proper attendants in his retinue. They set out on this expedition in August 1733, and returned to Petersburg in Feb. 1742, after having spent near 9 years in visiting almost every part of Siberia.

The fruits of this undertaking are designed to be communicated to the public,* and one volume of the history of plants has already appeared, entitled as above. This is intended to be followed by several others,† containing not only a description of the plants, their locus natalis, &c. but their uses among the inhabitants, so far as the Professor could get information concerning them.

In a large preface to this first volume, the ingenious and indefatigable author has given a concise account of Siberia in general, its rivers, lakes, mountains, mines, the nature of the soil, fertility, &c. with several judicious experiments and remarks on the altitude of the earth above the level of the sea; but especially on the qualities of the air in that climate.

Pauda is allowed to be the highest of all that ridge of mountains called Wer-

* Professor Gmelin's travels in Siberia were published in the German language, in 4 vols. 4to. Many and very important additions, relative to the geography and natural history of Siberia, were afterwards made by the late professor Pallas.

† Three more vols. of this splendid work were afterwards published; a further account of which will be found in the 48th vol. of the Philos. Trans.

koturian. Our author endeavoured to take the height of it by means of the barometer. Dec. 11, 1742, at his lodgings at the foot of Pauda, the mercury in the barometer, in a cold place, but within doors, stood at $26\frac{81}{100}$ Paris measure. He then carried it up the mountain as high as he could go, which was about one-third of the whole height, where he hung up the barometer on a tree, from 9 to 11 in the forenoon, making a good fire pretty near it, lest the intense cold, which sunk the quicksilver in De Lisle's thermometer to 201, should affect the barometer, and lead him to ascribe that to gravity, which was only owing to the contraction of cold. Under these circumstances the quicksilver sunk to $25\frac{32}{100}$. Hence, according to Cassini's calculation, the first station will be 941 feet higher than the level of the sea: the second on Pauda 1505 feet, and the whole height of this mountain 4515, or 752 Paris toises; which, added to 941 feet, the height of his lodgings at the foot of Pauda, makes 5456 feet, or 909 toises, the height of Pauda's top above the sea; supposing the level of the sea to be 28 inches, as the Paris academicians have fixed it; though this differs from observations made on the barometer at the sea-coast of Kamschatka at Bolcheretz, where, from experiments made for above 2 years, the mean height of the mercury was 27 inches, $6\frac{1}{4}$ lines; and at Ochotz, during a year's observation, the mean height was found to be 27 inches and about $8\frac{1}{4}$ lines. Hence it would appear, that the sea of Kamtschatka is higher, with respect to the earth's centre, than the ocean and Mediterranean; and at Bolcheretz higher than at Ochotski.

The author finds that the plains in some parts beyond the lake Baical, are almost as high as the tops of high mountains in some other countries; mount Massane, according to the French geometricians, being but about 408 toises high, which differs but little from the plain country at Kiachta; which yet has considerable mountains rising in its neighbourhood. Whence our author concludes, that the elevation of the earth, in this tract, above the level of the sea, is very great, compared with the west part of Siberia and Europe.*

The coldness of the air of Siberia is the most remarkable quality. In some places it snows frequently in September, and not seldom in May; in Jacutsk, if

* M. De la Condamine, in his voyage through the inland part of South America, makes Quito to be between 14 and 1500 toises above the level of the sea. Suppose 1450
He tells us, that Pichincha is 750 higher 750
This makes in the whole .... 2200 toises above the level of the sea.

P. Martel, engineer, in his account of the Glacieres in Savoy, printed at London 1742, tells us, that the barometer at Geneva, by the side of the Rhone, stood at $27\frac{2}{12}$ I. which is 656 feet above the level of the sea, according to Scheuzer; and that the highest point of Mont Blanc, measured partly by the barometer, and where inaccessible from the snow that covers it, by trigonometrical operations, is 12459 feet, or somewhat more than 2076 toises above the level of the Rhone; which, added to the height of this above the sea, makes 13115 French feet, or about 2 English miles and two-thirds.

the corn be not ready to cut in August, which often is the case, the snow sometimes prevents it, and buries the harvest all together. At Jacutsk the Professor ordered a hole to be dug in the earth, in a high open place, on the 18th of June, the mould was 11 inches deep, below that was sand about $2\frac{1}{4}$ feet; it then began to feel hard, and in half a foot more it was frozen as hard as possible. In a lower place, at no great distance from this, he ordered another hole to be dug: the soil was 10 inches; soft sand 2 feet 4 inches; below this, all was congealed. So that the earth is scarcely thawed, even in summer, above 4 feet deep.

Our author inclines to the received opinion, that the eastern climates under the same latitude are colder than the western; and thinks this is confirmed by experiments made in different parts of Siberia. The mercury in De Lisle's thermometer often sunk in winter in very southern parts of this country, as near Selinga, to near 226, which is equal to $55\frac{1}{4}$ below 0 in Fahrenheit's thermometer. But the cold is often much more intense than this, as appears by the following experiments, made at Kirenginski.

Feb. 10, 1738, at 8 in the morning, the mercury stood at 240 degrees in De Lisle; which is 72 below 0 in Fahrenheit's. On the 20th it sunk one degree.

At the same place in 1736, Dec. 11, at 3 in the afternoon, 254 in De Lisle. Almost 90 below 0 in Fahrenheit.

Dec. 20, 4 o'clock p. m. 263 in De Lisle, or $99\frac{44}{100}$ below 0 in Fahrenheit.
Nov. 27, ... 12 at noon.......... 270D = $107\frac{73}{100}$F below 0.
Jan. 9.............................. 275 = $113\frac{65}{100}$.
Jan. 5 (1735).... 5 in the morning .... 260.
.... 6 .... 280 = 120.
.... 8 ....250 and rose by degrees till 11 at night, when it stood at 252.

Such an excess of cold could scarcely have been supposed to exist, had not experiments, made with the greatest exactness, demonstrated its reality. During this extreme frost at Jenisea, the magpies and sparrows dropped down as they flew, and to all appearance dead; though they mostly recovered when brought into a warm room. This was quite new to the inhabitants of that country; though it frequently happens in Germany in much less intense cold, when the weather sets in at once very severe. The air, says the author, was at that time extremely unpleasant; it seemed as if itself was frozen, being dark and hazy; and it was scarcely possible even to bear the cold in the door way for 3 or 4 minutes.

But the utmost limits of cold are yet unknown; or to what degree an animal can subsist in it when gradually inured to it. The history of heat is alike imperfect. Boerhaave thought, that a man could not bear, without the utmost danger, a greater heat than that which would raise the mercury to 90 in Fahrenheit's; but an ingenious and accurate correspondent of the author's at Astrachan informs him, that it not only rises there to this degree frequently, but even to 100, and he has seen it 103¼. Even in the bagnios in Russia, the heat is often equal to 100; it sometimes makes the quicksilver ascend to 108, 110, and to 116; and yet people not only bear them with impunity a few minutes, but often stay half an hour or an hour. [On this subject, see Dr. G. Fordyce's experiments, Phil. Trans. vol. 77.]

A New Discovery of the Usefulness of Electricity in Medicine. By John Henry Winkler, Prof. at Leipsic, and F. R. S. N° 486, p. 262. *From the Latin.*

Electricity has the property of dividing bodies very subtilly. It carries off with it the parts of those bodies it dissolves, to those places where the electric sparks appear. If odorous substances be ever so closely confined in glass vessels, it so divides them, that their exhalations penetrate the glass as easily as magnetism, and flow like a river through the atmosphere of cylinders and chains. The electric matter that issues from the other extremity of the cylinder, gives an aromatic odour to the hand that touches it. Yet the odour communicated does not stop in that part of the body on which the electrical stream has flowed, but with a continued aspiration pervades the whole human frame. Not only are the skin and garments scented, but even the very air breathed by the lungs, the spittle, and the sweat of the person, smell of the aromatics, which are agitated by electricity in the closed vessel.

All this has been proved by several experiments that have been carefully made. In 1747 Mr. W. filled a glass vessel with water, and dissolved nitre in it. After standing some weeks, the water became very clear, by the heavier parts subsiding. At the latter end of the year he put a wire into this clear water, and joined it to a metal tube suspended on silken threads. He put under this tube sometimes metals, sometimes metallic vessels full of water, in which were glass spheres filled with metalline particles. Then he excited the electricity, the electrical fire touching the bodies underneath, and he repeated the electricity several days. He then found a great quantity of nitrous parts in the metals and vessels, which had been touched by the electric fire under the metal tube. Other vessels, that were placed in the room where the experiments were made, but not touched by the electric matter from the tube, showed no traces of the nitre. Hence it appears that the parts of the nitre are taken out of water by electricity, and conveyed to places touched by the electric fire.

This conjecture was greatly confirmed the same year by a publication in Italy, by Sig. Jo. Franc. Pivati, on medical electricity; in which a manifest instance of the virtue of electricity was shown on the balsam of Peru; which was so concealed in a glass cylinder that, before the application of electricity, not the least smell of it could be discovered. A man, having a pain in his side, was electrified by this cylinder: he went home, fell asleep, sweated, and dispersed the power of the balsam. His clothes, bed, chamber, all scented of it. When refreshed by sleep, he combed his head, and found the balsam lodged in the hair, so that the very comb was perfumed.

The next day Sig. Pivati electrified a man in health, after the same manner, who was ignorant of what had been done before. On this man going into company half an hour afterwards, he found a warmth gradually diffusing itself through his whole body. He became more lively and cheerful than usual. The company were surprised at an odour, which they could not account for; but the man himself perceived that the perfume arose from his own body, and with equal surprise, not being aware that it was owing to the operation that had been performed on him.

Being struck with so extraordinary an account, M. Winkler tried the effect in some other instances. He put some pounded sulphur into a glass sphere, so well covered and stopped, that on turning it over the fire, not the least smell of sulphur was perceived. When the sphere was cooled he electrified it, when immediately sulphureous vapours issued from it, which, on continuing the electricity, filled the air so that the smell was perceived at the distance of 10 feet, and the persons were even driven away by the stench of the sulphur; Mr. W.'s body, clothes, and breath retained the odour even the next day.

After this, he tried the effects of a more agreeable smell, filling the sphere with cinnamon. Having treated it as before, the smell of cinnamon was soon perceived by the company, the whole room being perfumed by it. Mr. W. tried the balsam of Peru with the like success. A friend of his having been present at the experiment, and going abroad to supper, he was often asked by the company what perfume he had about him. The next day, when Mr. W. drank tea, he found an unusually sweet taste, owing to the fumes of the balsam still remaining in his mouth.

Considering these things, Mr. W. thinks that electricity must be of use in curing some diseases. There are two grand benefits to be expected from medicine; for, either noxious particles, that are mixed with the blood or other juices, are to be separated and expelled, or beneficial ones are to be introduced. In both these cases electricity may be of service. For as soon as it touches a human body, it immediately pervades it in such a manner, that no place is left free from it; nor is there any thing in the body, that can be rendered volatile, that is not dissolved, dissipated, and carried off by it. And many instances are on record that electricity has caused blood to flow from the nose and other parts of the body.

But electricity has not only a power of separating and expelling, but is also very efficacious in filling the blood with the virtues contained in plants and minerals, as is manifest from what has been said concerning the sulphur, cinnamon, and balsam of Peru. The electrical power of nourishing the blood, differs from the usual mode of healing, in this, that it supplies the blood with aliment without the aid of the stomach, and that it enriches the vital juice with those exhalations which pass through the glass, and excel in subtilty and purity. Medicines received by the mouth must pass into the stomach, before they can be mixed with the blood, and must wander through many and long paths, in which they must be changed. But the spirits raised by benign electricity, flow into the blood without these windings.

By the conjunction therefore of medicine and the electrical art, it is probable that new and happy cures of diseases may be performed; remarkable instances of which have been published by Pivati. He restored the obstructed course of the blood in a woman, by applying the usual medicines in such a manner, by means of the electrified glass cylinder, that their essence effectually reached the body. His assistance was implored by a young gentleman, who was so miserably affected by a corrupted humour in his foot, that it eluded all the attempts of the physicians. Sig. Pivati filled a glass cylinder with proper materials, and, having electrified it, applied it to the part affected, causing it to emit electrical sparks for a few minutes. When the patient went to bed, he had a good night. He sweated every night for 8 days together, after which he remained quite well. Also, S. Donadoni, bishop of Sebenico, came to S. Pivati, attended by his physician and some friends. He was 75 years of age, and had been afflicted with gouty pains in his hands and feet for several years. It had so affected his fingers that he could not move them, and his legs that he could not bend his knees; his servants were obliged at nights to carry him in a chair to the bedside, and lift him gently into it. S. Pivati, in this case, filled a glass cylinder with discutient medicines, which by the electrical virtue he transfused into the patient. He soon felt some unusual commotions in his fingers. The action of the electricity being continued a few minutes, the patient gradually found the benefit of it; he opened and shut both his hands, and gave a hearty squeeze with his hand to one of his attendants; he rose up, walked, smote his hands together, he helped himself to a chair and sat down, wondering at his own strength, and hardly knowing whether it was not a dream; he walked out of the chamber, and down stairs, without any assistance, and with the agility of a young man. Soon afterwards, S. Pivati relieved a lady of 60 years in like manner from the gout, with which she had been 6 months tormented. Her fingers were much swollen, and always trembling, and one of her arms was convulsed. After receiving the electricity for 2 minutes, the trembling of her fingers ceased; and the next day the swelling was so far abated, that she could draw on her gloves, and make use of her fingers. Hence

Mr. W. thinks there can be no room to doubt of the assistance that medicine may receive from the proper use of electricity.

On Medical Experiments of Electricity. By Mr. Henry Baker, F. R. S. N° 486, p. 270.

Though perhaps as many curious and well-contrived experiments have been made in England as in all the other parts of Europe, to discover the general laws and properties of electricity, we have not hitherto attended to the effects that may be produced by it in the bodies of living animals, any further than to assure ourselves they may be killed by it; a supposition that diseases may be cured by means of this power, having met with so little countenance among us, that very few trials have been made, to ascertain what, in distempered cases, it can or cannot perform. Foreigners, on the contrary, seem fond of believing, that the subtil electric fluid (be it fire, æther, or whatever else) which can pervade all bodies, and, being accumulated, even kill an animal, in certain circumstances, and by certain methods of application, may possibly in other circumstances, and applied in different degrees, and by different methods, so operate on the fluids or solids, and perhaps on both, that very beneficial and salutary effects * may result from it.

With this view the Abbè Nollet made several experiments on living birds, kittens, and human bodies; and if we may give credit to the accounts communicated to us, he found, in every trial, that perspiration was so considerably promoted by it, as to cause a very sensible difference between the weight of such animals as had been electrified, and others of the same kind that were treated exactly alike in every respect besides; whence he naturally concludes, that in cases where it is necessary to quicken the circulation of the fluids, and throw off a greater quantity of the perspirable matter, electricity must be greatly useful.

The philosophers in Italy and Germany have applied their industry to discover by experiment, how far electricity may, simply and in itself, be of service in several diseases, and likewise how far it may conduce towards conveying the more subtile and active effluvia of useful medicines, either into the whole body, or into some distempered part. Some remarkable cases are related in the preceding paper of the effluvia of certain substances conveyed by electricity into the body, and so removing several complaints. And Dr. Joseph Bruni, one of the principal physicians at Turin, and F. R. S. has likewise sent an account of experiments made at Rome and at Bologna, which are now laid before the society to show what attempts to the same purpose have been made in different countries, and by

* As is suggested by Dr. Mortimer in these Trans. N° 476. C. M.

different people. The Doctor says, that at Bologna, the electrical power has been applied to the cure of diseases, as follows.

A man, who had been for 12 months deaf of one ear, with a continual noise in it like the running of water, attended with most violent pain whenever he lay with that ear uppermost, coming to Dr. Verati for advice, the Doctor electrified him, bringing out abundance of fiery sparks around the distempered ear, which, in about 5 minutes that the electrification was continued, became as red as if a blistering plaster had been applied to it. But the redness disappeared in a few minutes after, the patient passed the night with less pain and noise, and was perfectly cured of his disorder.

A footman belonging to the said Doctor, being taken suddenly ill of a violent pain in the head, which continued many hours, he was electrified, the Doctor causing the sparks of fire to issue from the temple where the pain was felt. The part appeared red, the pain abated; in 3 hours it was entirely gone, and has never returned since.

A woman that nursed one of the Doctor's children, having had a most grievous disorder in her eyes for some months, with a continual running of water from one of them, and a constant pain over the eye-lid, came to the Doctor for advice, who immediately electrified her, bringing out the fiery sparks about the eye and eye-lid, by which the eye appeared very much blood-shot; but that went off in 7 or 8 minutes. The woman felt less pain the following night, and opened her eye in the morning more easily, and without being obliged to wipe it, as she did before: the watery humour and pain were much diminished; and the Doctor hoped that by repeating the operation twice more, he should be able quite to cure her.

Dr. Bruni gives next his information from Rome; that a gentleman there covered the internal surface of a cylinder of glass (which some use instead of a globe) with a purgative medicine; and that a man, electrified with it, found on the spot the same effects as if he had swallowed the medicine.

A Proposal for Checking in some Degree the Progress of Fire. By the Rev. Stephen Hales, D.D., F.R.S. N° 487, p. 277.

Dr. H. placed on 2 garden-pots a dry fir board, of half an inch thick, and 9 inches broad; and covered 9 inches length and breadth of it with an inch depth of damp garden earth; fencing this earth on each side with 2 courses of bricks, to make a fire-place to contain the wood fuel and live coals; which were frequently blown with bellows, to keep the fire to a vigorous heat: this was done for 2 hours' continuance, before the fir-board was burnt through; when there was only a weak lambent flame at the under part of the board; for it could not flame out for want of proper fuel; because the substance of the board was reduced to a brittle charcoal, by the heat of the inch-depth of earth which lay on it, which hindered the burning board from flaming. And it was observable that the edges of the board burnt only with a live coal like a match; being hindered from flaming by the earth which lay on the board.

May it not hence be reasonably inferred, that when a house is on fire, it may be a probable means considerably to retard the progress of the fire, to cover with earth the floors of the adjoining and more distant houses, which stand in the course of the progress of the flames? The thicker the earth is laid so much the better: but if time will not permit to lay it more than an inch thick, then supposing 27 men to carry each a cubic foot of earth, which will be a cubic yard of earth; then that cubic yard of earth will cover 36 square yards of flooring; which repeated several times, would soon cover all the floors of a house. And as the fire probably mounts with great fierceness up the staircase, it will be well to lay much earth on the stairs; which will help to give some check, especially as the earth on the floor and stairs may be wetted by the fire-engine; which moisture will be much the longer retained by means of the earth; whereas water, when not thus retained, soon glides away.

And as fires often catch from house to house at their upper parts, an upper floor covered with earth, with the rafters burning on it, will be longer in burning to such a degree as to fall on the next floor, so when fallen there, it will also be the longer in burning, and will flame the less, on account of the earth on that next floor; and consequently will not be so apt to fire the next house, as in the common case of floors without earth, which must needs therefore burn the more fiercely.

Note by Dr. Mortimer.—Two days after the fire-works had been played off in the Green Park on account of the late peace, Dr. M. went all over the building erected for that purpose, and was greatly pleased to see the Doctor's scheme confirmed by the practice of the engineers on that occasion; for the rooms in which the trains were fired, and which was immediately under the gratings on which the 6000 rockets rested and were fired from, had the floor covered over with fine sifted gravel about an inch deep, and the walls were whited over with a dirty sort of white wash, which he took for lime finely powdered, and mixed up with size and water, and done 2 or 3 times over. Both floors and walls were of deal.

Observations made during the last Three Years, of the Quantity of the Variation of the Magnetic Horizontal Needle to the Westward. By Mr. George Graham, F.R.S., at his House in Fleet-street, London. N° 487, p. 279.

1745	March 26	17°	0′	1745	March 21	17°	10′
	...... 29	17	0		April 22	17	15
	March 18	17	10		May 4	17	18

1745 May 14	17°	20′	1746. Feb. 24	17°	30′
.... 16. ...	17	15	1747. Dec. 19	17	40
Dec. 18	17	25 +	Jan. 4	17	40 —

The inclination of the dipping-needle was during the same time about 73¼ degrees.

As the variation of the needle at London has not been regularly published from time to time in the Phil. Trans., it may not be improper to take notice here, that according to the best observations extant, and which were made by persons of great skill and exactness, the needle at London declined to the eastward 11° 15′ in the year 1580. In 1657 there was no variation, the needle then pointing due north. In 1672 the variation was observed by the late Dr. Halley 2° 30′ towards the west, and in 1692 6° 0′. Towards the beginning of the year 1723, it was found by Mr. Graham, from the medium of a vast number of observations, to be then 14° 17′ the same way. So that, during the course of 167 years, elapsed since the year 1580, to the end of the last year 1747, the magnetic needle at London has moved to the westward 28° 55′. See before N° 148, and N° 383 of the Phil. Trans.

*Account of the Cornel-Caterpillar.** *By the Rev. Philip Skelton.* N° 487, p. 281.

In the beginning of May 1737, the warmest season that any one remembers, the cornel-trees, of which we have a good number about Monaghan, appeared almost covered with small caterpillars, of a duskish green, resembling in colour the bark of the tree, though a few, considerably larger than the rest, were yellow. These worms were employed partly in feeding on the leaves of the cornel, which was their only nourishment, and partly in crawling over the bark of the tree. As they crawled, they left each a fine thread, scarcely visible to the naked eye, sticking to the bark. These threads, being almost infinitely multiplied by the inconceivable number of worms employed in the work, formed the web, in which the threads are not interwoven, but cohere by some roughness or glutinous quality.

By the end of May there was not a leaf to be seen on any of the cornels, excepting a few, reserved for a very curious purpose, mentioned presently. But the worms, in stead of the green cloathing they robbed those trees of, gave them one of white, so entire, that it covered the whole bark, from the ground to the points of the slenderest twigs, and of so pure and glossy a colour, that the whole tree showed in the sun as if cased in burnished silver. The web was so strong, that if one disengaged it from the tree, near the root, one might have stripped it from the trunk, the branches, and the twigs, at one pull. As soon as the worms

* The insects here described are the larvæ or caterpillars of the *Phalæna Evonymella,* Linn.

had covered all the cornel-trees, they removed from thence, and covered all the ash, beech, lime, crab-trees, and even weeds, that grew near them, with the same, but a thinner kind of workmanship.

In travelling from one tree to another, many of them crawled along the ground, and over every thing in the way, still leaving a thread behind, and dispatching a part of their business as they went to a more convenient surface to finish the rest on. But he imagined some of them took an easier and more ingenious way. He found many of them hanging by their own threads from the most extended branches of the tree. While they were in this situation, a gentle puff of wind might, by exciting a pendulous motion, waft them to the next tree. This seems to be the method by which those very minute spiders, whose threads are made visible by the moisture adhering to them in a foggy morning, transport themselves from one bush to another, though destitute of wings, sometimes across narrow paths, and even rivulets.

About the beginning of June the worms retired to rest. Their manner of preparing for, and executing this, was very ingenious and curious. Some of them chose the under sides of the branches, just where they spring from the trunk, that they might be the better defended from the water, which in a shower, flowing down the bark of a tree, is parted by the branches, and sent off on each side.

Here they drew their threads across the angle, made by the trunk and branch, and crossing those again with other threads in a great variety of directions, they afterwards formed a strong tegument on the outside. Within this they placed themselves lengthwise among the threads, and rolling their bodies round, spun themselves into little hammocs of their own web, while in the mean time they shrunk into half their former length. Those hammocs, being suspended by the transverse threads, did not press each other in the least. That they might take up the less room, they lay parallel to each other, and in the most convenient order imaginable.

Others, still more ingenious than these, fastened their threads to the edges of certain leaves, which doubtless they had saved from their stomachs for this very purpose; and with that slender cordage pulling in the extremities of the leaves, drew them into a kind of purse, in the inside of which they formed the same kind of work, and laid themselves up in the same manner as above. By this method they saved themselves a labour, which the rest were at the expence of; for the leaf served them very well for an outward defence against the weather, and a place to fix their transverse threads to. It is probable they laid themselves up in great numbers together, not only because many were necessary to the work of providing a common covering, but also to keep each other warm, while nature was preparing for the great change, and also to confine some subtil vapour, issuing from their bodies, which might have been conducive to their reviviscence, and which had been easily dissipated, had they not lain close, and caught it from each other.

Between the worm, thus laid up, and the hammoc, in which it was enclosed, a tough and pliant shell, of a dark brown colour, was found. As the worms themselves were of a pretty dark colour, this superficial tincture seems to have been in a great measure purged off into the shell.

For after the worms had continued in this state during the whole month of June, whether they gnawed their way through the ends of their shells and hammocs, or that exit was prepared for them by some corrosive matter oozing from their mouth, he knows not, but they came out almost all in the space of one morning, the most beautiful fly or moth that his eyes ever beheld. Its shape was extremely elegant; its head, upper wings, body, legs, and antennæ, were of the purest white, and glittered as if they were frosted with some shining kind of substance. He rubbed some of this off, and on viewing it through an ordinary microscope, it appeared like the points of very minute feathers, or like small cones of polished silver. The upper wings were regularly studded with small, round, black spots, and extended themselves from its head somewhat beyond its tail. The under wings, which were a little shorter, were of a duskish colour, and prettily fringed at the extremities.

This beautiful and surprising work of nature seemed, after its resurrection, to have no dependence on material food. The cornel had recovered a new set of leaves by the time the fly appeared; but it never touched them; and those that came out in his room, lived as long there as the rest which enjoyed the open air, and the tree on which they were bred. If they did feed, it must have been on some other adventurer of the air, too minute to be visible to our eyes. Those that were confined to the room discharged a small drop of brown liquor, in which he supposed their eggs were contained; but as they were not deposited in a proper receptacle, they did not produce worms the next year.

As to the extraordinary increase of this insect in May 1737, the succession of seven or eight mild winters, which preceded that season, might, by preserving their eggs, give occasion to it. As they are one of the earliest kinds, the excessively warm May that year so effectually hatched their eggs, that they all came to perfection: whereas the more ordinary worms and flies, that make a later appearance, meeting with the sharp easterly winds that happened that summer to blow during the months of July and August, were in a good measure destroyed; otherwise it is possible they too might have had an extraordinary increase.

On the Everlasting Fire in Persia. By Dr. James Mounsey, Physician of the Czarina's Army. N° 487, p. 296.

As the natural history of Persia is but little known, and the authors of the Universal History have given no true account of the everlasting sacred fire which the Gauers worship, Dr. M. gives the following description of it, which he says may be depended on, as there was a Russian army for some years in the kingdom of Dagestan, where the fire is; and he took down what he relates from the mouths and journals of many officers that were there, and more particularly from what was communicated to him by archiater Fischer, who received an account of it from Dr. Lerch, physician of that army.

This perpetual fire rises out of the ground in the peninsula of Abscheron, about 20 miles from Baku, and 3 miles from the Caspian shore. The ground is very rocky, but has a shallow covering of earth over it. If a little of the surface be scraped off, and fire be applied to the hollow, it catches immediately, and burns without intermission, and almost without consumption; nor is ever extinguished, unless some cold earth be thrown over it, by which it is easily put out.

There is a spot of ground, about 2 English miles broad, which has this very wonderful property; and here is a caravansary, round which are many places where the earth continually burns; but the most remarkable is a hole about 4 feet deep, and 14 feet in diameter. In this caravansary live 12 Indian priests, and other devotees, who worship the fire, which, according to their traditions, has burnt many thousand years. It is a very old vaulted building, and in its walls are a great many chinks, to which, if a candle be applied, the fire catches instantaneously, and runs instantly wherever the chinks communicate; but it may be easily extinguished: they have hollow places in the house fitted to their pots, which they boil without any other fuel, and instead of candles they stick reeds into the ground; from the tops of which, on applying fire to them, a white flame immediately comes forth, and continues to burn without consuming the reeds, till they think proper to extinguish it, by putting little covers over them for that purpose.

They burn lime of the stones dug hereabouts, first making a hollow in the ground, and then heaping the stones on each other. This done, on applying fire to the hollow, a flame bursts out, and is dispersed at once with a very great crack through the whole heap of stones; and after it has continued burning for 3 days, the lime is ready: but stones placed in this fire for setting their pots on never turn to lime; which cannot be made but by heaping them on each other. The earth and stone are no farther warm than where the fire reaches: and what seems very well worth observation, this flame of fire gives neither smoke nor smell, however great it be.

About an English mile and half from this place there are wells of white naphtha; which is exceedingly inflammable; and though the flame of naphtha affords both smoke and smell, it is highly probable the perpetual fire just described is owing to naphtha, but so purified, in filtering through the stone, that it becomes divested of all such particles as produce smoke or smell. The stone and earth are grey in colour, and saltish to the taste; and indeed much salt is found on this peninsula of Abscheron. There is also a salt lake, near the side of which the white naphtha flows by 5 different springs. This naphtha is made use of only in the medicinal way. It is yellowish from the spring, but when distilled resembles spirits of wine. They give it internally for gonorrhœas, disorders of the breast, and for the stone, and they apply it externally in gouty cases, contractions of the sinews, and cramps.

Black naphtha is produced 8 or 9 miles from the perpetual fire; it is thick, and being distilled grows not clear, but yellow. About Baku there is some of it so thick, that they employ it for greasing wheels: but the best, and greatest plenty, is at Balachame, where there are above 50 springs, the greatest producing every day 500 batman, each batman containing 10 Russ pounds, which are somewhat less than English weight. You hear it make a considerable noise in rising out of the ground, though the spring be 20 fathoms deep.

In Baku they have little or no other fuel to burn besides naphtha, but it must be mixed with earth or ashes to make it fit for use. The fire it makes is only good to boil with; and this inconveniency attends it, that all their food so boiled smells and tastes of naphtha. For baking and roasting they make use of abrotanum, abysynthium, and such like; but in general naphtha is their fire.

An Abstract of Mr. Bonnet's Memoir concerning Caterpillars. Drawn up in French by Mr. Abraham Trembley, F.R.S.; here translated into English. N° 487, p. 300.

The paper lately communicated by the president from Mons. Bonnet of Geneva, contains various experiments he has made relative to the respiration of caterpillars. Malpighi first discovered, that those 18 openings or orifices, which are placed 9 on each side of the caterpillar, and which are called by the name of stigmata, serve to give respiration to this class of animals.

Mr. de Reaumur has repeated the experiments of Malpighi, and made several new ones on this subject. And he was of opinion, that these apertures served only for the inspiration of the air, which the caterpillar afterwards respired, through the whole superficies of its body.

Mr. Bonnet had reason to think these caterpillars do both inspire and expire the air by their stigmata; and that they did not expire any of it through the pores of their body. This paper gives an account of 36 several experiments, made chiefly with design to discover this fact, whether indeed these insects did both inspire and expire the air by their stigmata, or only inspire it. These experiments, like Mr. de Reaumur's, consist mostly in the plunging of caterpillars either into water, or some other liquor; some also they daubed or anointed over with fat and greasy substances, some quite over, and others only in some places. Mr. Bonnet was inclined to think, that the small bubbles of air observed all over their bodies, when they are immerged in water, did not come from the air included within them, and which they expired by the pores; but that they were formed by the air only lodged near the surface of the skin of the caterpillar, as it is about the superficies of all other bodies: he had endeavoured to contrive it so, as that no air might remain thus sticking to the skin of those insects on which he had made these experiments. And for this purpose, before he plunged them in the water, he first washed them all over with a hair-pencil or brush; and these being afterwards immerged in the water, but very few bubbles of air have been discovered on the outside of their bodies; and fewer as it appeared than Mr. de Reaumur had found on those, on which he made his experiments; neither was he himself of opinion that all those bubbles which he took notice of were formed by the air rushing out through the pores, but that some of them were also formed by the air sticking about the exterior part of the skin.

When a caterpillar is plunged in water, one bubble of air is almost constantly observed upon each of the stigmata. Mr. de Reaumur concluded, that the air was not expired by these stigmata, because he could never observe that any bubbles of air were ever driven out of these stigmata, as one would think there must have been, if the air was really expired by these apertures. Mr. Bonnet on the contrary, had seen some bubbles of air come out from these stigmata, and that had contributed to make him rather think that the air inspired was also discharged at the same orifices. But as these experiments are not decisive, he was unwilling absolutely to determine, but proposed making more new experiments.

A caterpillar can remain several hours under water without perishing; it only falls into a state of numbness; but if again taken out of the water, it is not long before it again shows signs of life, and recovers. Mr. Bonnet had sought by some experiments, to know if some only of these 18 stigmata of a caterpillar might not be sufficient for the purposes of respiration: he has plunged some of them only partially in water, sometimes by the tail, and others by the head foremost; but always so that either two or more stigmata might be out of the water;

and in these cases the caterpillar has not fallen into the torpid state above mentioned, as it constantly did when entirely immersed. He had lifted out of the water some of the stigmata of caterpillars that had been quite immersed, and that were so become torpid and motionless; and these had also soon after shown signs of life and motion. One of the caterpillars, on which Mr. Bonnet made experiments, lived 8 days suspended in the water, and only exposing to the air its posterior stigmata; that is, only the last two stigmata were out of the water.

During this time he carefully observed the caterpillar; and he remarked, from time to time, when the insect moved itself, that little streams of bubbles came out of the anterior stigma on the left side. It appeared to him, by this and some other experiments, that among all the 18 stigmata, the two anterior and the two posterior ones are of more use for the respiration of the caterpillars, than any of the others. He also found, that on choaking up these stigmata with butter, the animal seemed to suffer much more sensibly, than when he so choaked up all the intermediate ones.

Divers Means for Preserving from Corruption Dead Birds, intended to be sent to Remote Countries, so that they may arrive there in Good Condition. Some of which Means may also be employed for Preserving Quadrupeds, Reptiles, Fishes, and Insects. By M. de Reaumur, F.R.S., and Memb. Royal Acad. Sc. Paris. Translated from the French by Phil. Hen. Zollman, Esq., F.R.S.* N° 487, p. 304.

The method hitherto practised, is to send them stuffed, viz. to take off their skin with all the feathers on it, from the body and the thighs, leaving the legs, the wings, and for the better conveniency the whole neck with the bill sticking to it. Filling afterwards the skin thus taken off with some soft stuff, either straw, hay, wool, or flax, &c. or even stretching it over a solid mould of the shape of the bird, you give to this skin, as near as possible, the form of the body of the bird, which it had when it covered its flesh and bones.

The foregoing way of preserving the shape of birds requires a hand used to it,

* Mons. Reaumur was born at Rochelle, 1683. He quitted his family profession of the civil law, to pursue natural philosophy and natural history, in the latter of which he chiefly excelled. For which purpose going to Paris in 1703, he was admitted into the Academy of Sciences in 1708. His writings on most parts of natural history are very curious and voluminous; on animals, minerals, insects, shells, &c. He wrote on the torpedo, colouring of false stones, construction of thermometers, making of tin, china, and of steel from iron; on which last he published an account in 1722, which procured him a pension of 1000 livres. He wrote the interesting history of the auriferous rivers in France, with the easy way in which the grains of gold are extracted from their sands; also curious observations on the nature of flints, on banks of fossil shells for fertilizing the ground, on digestion, on insects, on preserving eggs, and hatching and rearing chickens, &c. His History of Insects, in 6 vols. 4to. is distinguished among his other productions as a valuable work. M. Reaumur died in 1757, at 74 years of age.

and even falls short of sufficiently imitating nature, unless with care and time. So that it is certainly most convenient only to send the bird as it has been received. There is no great skill required for putting one or several into a vessel full of spirit of wine, or very strong brandy. But their feathers do not show those various and bright colours, which are natural to them while they are immersed in some liquor, and which appear no longer on the bird's feathers when taken out of it. Besides, the vanes of the feathers are then disordered, and glued too much together. On these first appearances, it was judged too hastily, that spirituous liquors changed the colours of the feathers, and hindered the reducing of them to the order and pliableness they had on the animal, when dry and living. However, repeated experiments have made M. Reaumur sensible, that the colour of the feathers is proof against the strongest brandy, and even spirit of wine, and that after having dried the bird that had been soaked, we may easily put its feathers into their natural order, and make it appear as it was when alive.

To preserve birds which are to be sent from afar, you are only to keep them in brandy, or spirit of wine.

If any of the bird's feathers are bloody, you must wash them from time to time with a wet linen, till they no longer leave a mark on the linen, or in the water in which they are soaked. Lay the feathers straight by smoothing them with a finger from the head towards the tail in squeezing them together. This helps the feathers to take the position which is most natural to them, and in this position they are kept by wrapping the bird up in a rag, tying about the neck and the body several times a strong packthread: the feathers on the neck are chiefly those which must be kept from a wrong turn.

The precaution of taking out of the body the intestines and other parts it contains, is not absolutely necessary; it is better however to do it: if afterwards their place is supplied by filling the cavity of the belly with all the quantity it can contain of wool, hemp, cotton, or other soft matter; if you fill the neck, though without distending it, with the same soft matter, you will more surely preserve the shape and dimensions of the bird.

It is still more easy to hinder the birds from being tossed, and they will even be the better preserved, if before they are sent, you take them out of the liquor in which they have lain a sufficient time; it has made them fit to dry without any danger of corruption. Small birds, such as of the size of sparrows, and even of blackbirds, after having been covered 8 or 10 days with strong brandy, may be taken out without any fear of their being corrupted. Large birds, and especially such as are very fleshy, are to be kept longer in the liquor; but there are none or few, for which it may not be enough to have lain in it a month or 6 weeks. According as you take them out, you must range them one next to the other, and over each other in a box, filling up the intervals with a matter easiest to be had, as chaff of oats or barley.

Any box, of what form soever, may be fit for birds which are to be on the journey only for some weeks or a few months: such as are to travel years require more precaution. But barrels are preferable to boxes, for such birds as are to remain shut up for a year or longer; the smallest insects will not find a passage for creeping into a barrel, which will not permit the smallest drop of liquor to get out. Birds being put wet into the barrel, keep from drying up too much, and keep each other the closer.

There is still another way for it, which may appear more convenient, especially for birds of a large size. Which is, to preserve birds by a sort of embalming, and even by actual embalming, in countries where the spices are cheap. First, you begin with emptying the body of the bird, and then fill it with the powders mentioned below; you also fill its neck with the same powder, thrusting it in through the bill. If the bird is extremely fleshy, you may make an incision in the flesh of the thick part of each leg, and one in the flesh of each wing; that is, 2 on the breast, and one nearer the first and large bone of each wing, into which you put the powder; having afterwards brought the flesh together again, and put the feathers in order, those incisions will be hid so as not at all to disfigure the bird. But there are very few on which it is necessary to make such incisions; one may make some even inwardly, which will serve as well; having thrust your fingers into the belly, you may tear the integuments over against the thick part of the leg, and in other places, and make cavities to be afterwards filled up with the powder.

There are many powders proper to produce the principal effect intended here, which is to promote the bird's drying before it be so far corrupted as to occasion the falling off of the feathers. All sorts of spices may be used for it with success. You may even make use of a powder composed of as many sorts of spices as you will, the result of which will be at least, that the bird, after being dried, will smell the sweeter, and become, as it were, a piece of perfume. But instead of using resinous gums, as aloe, myrrh, frankincense, and other productions of plants, as cinnamon, cloves, pepper, ginger, &c. which are dear materials, you may content yourself with a salt which is cheap in most countries, it is sufficient to fill the cavity of the body and of the neck with alum reduced to powder. A material still easier to be had in all places, and very cheap, and which works with great effect, is lime. If it can be had quite unslacked, you will take it preferably; however, without scrupling to take such as is old, and which has been slackened by the humidity of the air.

After the body and the neck of the bird have been filled up, either with pulverized lime, alum, or any other powder, put it into the box or the barrel, in which it is to be transported. Take care, in placing it, to give a natural position to the neck, the legs, &c. At the bottom of the box or the barrel there is to be a layer, of the thickness of about an inch, of the same powder with which the cavity of the body is filled, or of any of those which are proper for it. You bury the bird in this powder, and put enough of it about it and upon it, so as to cover it with a layer of the thickness of an inch or more. The outward powder will make it dry the sooner, and keep off voracious insects, which will not care to attempt to pierce through it in order to come to the flesh they are fond of. During the first days, and even during the first weeks, the birds may cast a bad smell, which you need not be uneasy at, for it will lessen in proportion to the bird's drying; and it will dry so that none of the feathers will come off; and when it is once dried, they stick fast to it for ever. This way of preserving birds, which is very simple, has procured to M. Reaumur some from very remote countries, which arrived as wished for.

The fourth way is one by which birds are more speedily dried, than by that which is explained before; it is to dry them by the heat of an oven. You make use of that heat which remains in it after the bread is taken out of it; sometimes it is then too great, but there is a plain way to be sure that the degree of heat is not so great, which is, to put feathers into the oven, and to take them out 5 or 6 minutes after; if you find that they are not singed, nor turned red, you need not be under any apprehension for the feathers of the bird to be put into the oven. Small ones need remain in it only 1 or 2 hours, to be sufficiently dried; those of a middling size require a longer time; and those which are large and very fleshy, ought to be put in at several times. When they are cold, you may know whether they are dried enough, by pressing with the finger the flesh of the legs and of the breast; if it does not yield, or yields but little under the finger, the bird does not any more want to be put into the oven. The inconveniency attending its being kept there longer than necessary, is, that some parts of it, as for instance, the neck and the rump, are rendered too brittle. You will prevent the bird's bulk sensibly diminishing in the oven, if, before you put it in, you fill the cavities of its body and the neck with some soft stuff, like any of those which we mentioned to be used for filling the cavities of such birds as are intended to be preserved by means of spirit of wine, viz. hemp, flax, cotton, &c. What is the most difficult in the way of drying birds in the oven, is not hitting the proper degree of heat, and to know the time how long they are to be kept in it: here will be the difficulty, how, as this way of drying requires the bird may be kept in a natural attitude, before it is put into the oven: if dried, it will be fixed for ever in that which it once received.

Dried birds ought to be sent in boxes or barrels sufficiently closed up, that insects may not slip in during the journey; and care should be taken to fill up all the empty spaces left in the barrel with some of those soft stuffs, which we have already pointed out for such uses.

If, from the falling of the feathers, it appears that the insects have defeated the precautions taken against them, there is still a remedy left; you may stop the progress of the evil by putting the bird again into the oven, not so hot as to singe the feathers, but hot enough to kill the insects in less than half an hour.

A collection of nests is a proper repository to be joined to that of birds; it shows such works as hardly could be imitated by men, admirable for their form, their workmanship, and the materials employed in them: M. Reaumur made such a repository. If one can have nests not too bulky for easy transportation, you may be sure to see them with pleasure joined to the birds that have built them. The colours and figures of the eggs make also part of the history of birds; collections made of them will give satisfaction to curious minds; those which are to be sent would be in danger of being broken on the way, by the very substance they contain, if it comes to ferment. Before sending them therefore, you must empty them: to this end you make a small hole in each end, and shake them; and if this shaking will not be enough, you blow into one of the holes to force out through the other what liquid matter remains in the egg.

Quadrupeds that are not of too large a size, and particular to certain countries, may be put into a state fit to be sent to the most remote parts, by one of the four ways used to preserve birds. Fishes and reptiles, which, as well as quadrupeds, are engaging objects for naturalists, are easier to be sent; it is sufficient to put them into barrels full of strong brandy. They may also be dried, either by materials with which you may fill the cavities of their bodies, or by a gentle and well-managed heat. Insects, which offer to us so many admirable varieties, deserve the care of gathering them into collections, which cannot but be precious to those who have made those little animals their study. All those which are soft, as for instance worms and caterpillars, may be preserved in brandy. Their tender colours will run less hazard of being altered, if you put into the brandy such a quantity of sugar as it is able to dissolve. Beetles may also be put into the same liquor; but butterflies and flies would be spoiled in it: after having killed them, you must range them in lays in boxes, and separate those lays with beds of cotton.

A Beautiful Nautilites, shown to the Royal Society, By the Rev. Charles Lyttelton, LL.D., F.R.S., and Dean of Exeter.* N° 487, p. 320.

This curious fossil seems to be composed of a stony matter like marble, which

* Dr. Charles Lyttelton was the 3d son of Sir Thomas, and brother to George, lord Lyttelton. From Eton he went to University college, Oxford; whence he entered of the Inner Temple, where

has penetrated the cells of the nautilus while in its natural state. The diaphragm or partitions remain still distinct and visible. The different colour of the stony matter in some cells of a dark brown or hair colour, in others of a light brown or ash-colour, with the natural polish of the outside, gives it a beautiful appearance; as it is represented in fig. 5, pl. 9, where it is drawn of its natural size in 3 different views; viz. A shows the side view of it; B the fore part; C the back part. C.M.

It was found in Pool's Hole in Derbyshire. Its sutures or diaphragms resemble those of some of the larger cornua ammonis; but its shape bespeaks it to be a species of nautilus; and it is thought to be a non-descript, both in its natural and fossil state.

On the Roach kept in Glass Jars. By Mr. Wm. Arderon, F. R. S. N° 487, p. 321.

Of all the several kinds of fish which for some years Mr. A. kept in glass jars, none seems more impatient of imprisonment than the roach; nor if they are well looked after, and supplied often enough with fresh water, did he observe any, except the roach, to become distempered. But most commonly, after this fish has been a little while confined, the finny part of its tail begins to drop off piece by piece; and when the finny part is all gone, a sort of mortification seizes on the tail itself, and gradually creeps along till it reaches the intestines, at which time the fish immediately dies.

The last roach he had under this disorder was about the beginning of January; when in the space of a month, it had lost the greatest part of the fin, which induced him to clip off the rest, hoping thus to stop the progress of the mortification. But this was of no manner of service: the distemper still gained ground; and as it increased a fine fibrillous substance grew out from it. These fibrils, when examined by the microscope, show themselves to be a number of minute tubes, filled with a brownish liquor; which on pressing them, becomes immediately discharged.

When first he perceived this fibrous substance inveloping the fish's tail, he supposed it to be nothing but a mouldiness, of that kind which frequently is seen on decayed flesh and fish; but on trial he found it to be of a much stronger texture and consistence than such mouldiness is ever known to have; for though he several times let a full stream of water run upon it from a cock, he could never wash it off.

he was called to the bar; which however he afterwards quitted, taking holy orders, and in 1747 was appointed one of the king's chaplains. The year following he was made dean of Exeter, and in 1762 bishop of Carlisle; and he died in 1768.

Dr. Lyttelton was for many years president of the Society of Antiquaries, and contributed many valuable articles to the Archæologia.

This fish lived with him till the latter end of March, and then died; having for many days before its death lain at the bottom of the jar, without being able to rise. As the mortification advanced, and came nearer its intestines, the quickness of its taking water in at the mouth increased, till at last it took it in 3 times faster than a lively strong fish did. On cutting off part of the fish's tail, in hopes of stopping the mortification, the equilibrium of the body was so far lost, that it hung in the water most commonly with the head downwards, and could never afterwards continue in any other posture, without great strugglings, or sinking down to the bottom of the vessel. Which may serve to show how nicely and wonderfully the bodies of fishes are balanced, for keeping them in a horizontal position; since in this case the losing a few grains of the tail could so sensibly destroy the equilibrium, as to render the rest of its fins almost useless.

Of a Fustian Frock being set on Fire by Electricity. By Mr. Robert Roche. N° 487, p. 323.

Mr. R. has a son about 16 years old, who had been for 6 or 7 years troubled with sudden fits that entirely took away his senses. He got him all the helps he could, but to no purpose; at last he sent him to St. Bartholomew's Hospital, as an out-patient; whence he was turned out as incurable. Finding his case desperate, he considered the power of electricity, and made a large machine for electrifying; and afterwards shocking him commonly twice a day, he received some benefit. One day being on the pedestal, and very highly electrified, and having on a coarse fustian working frock, the condensing phial being on the conductor, and touching him to procure snaps as usual, he touched his right shoulder blade; when, to his great surprise, the furzy flax of the frock caught fire, with a great blaze, and burnt the whole breadth and length of the shoulder, the flame rising 6 inches above the collar, and would probably have set the frock on fire, had he not put it out with his hands. There was no fire in the room that day: this was about noon; neither was there any thing that could have any inflammable vapour there.

At 9 the same evening he made him put on the same frock, and touched the left arm, where the flax had not been burnt before; and it had the same effect as above.

Of a Child born with an Extraordinary Tumour near the Anus, containing some Rudiments of an Embryo in it. By John Huxham, M.D., F.R.S. N° 487, p. 325.

John Perrine's wife, of Charleton parish, in Devonshire, a brisk active young woman, he very infirm and consumptive, was delivered of a daughter at full time, July 11, 1746. The child was perfect as to all its limbs, head, body, &c.

but from the region over the os sacrum, glutæi muscles, and between the thighs, quite home to the pudendum, was growing a very large substance, which the midwife and others called a wen, in shape very like the ventricle of a sheep, and seemed, as to its colour and outward appearance, a continuation of the same skin with the rest of the body, but very full of blood-vessels. It hung down behind below the heels, and was larger than the whole body of the child itself. It felt very soft, and seemed to have matter fluctuating in it; but in the middle of the whole was evidently felt a hard substance. The pudendum as well as anus were in all respects natural, and both urine and stool were regularly discharged; but the anus was placed much more forward, and immediately under the pudendum; so that the fæces were discharged in the same direction as the urine.

The surgeon, Mr. Wills, made a puncture in the depending part of the tumour, and drew off near 2 quarts of a palish red water, without any smell. The orifice being left open, there was a continual issue of the same kind of water for several days; but by degrees it became more and more glutinous, and at length whitish like pus, and very fetid. As the discharge was great, the child grew weaker and weaker, and at the end of 15 days died.

The next day he opened the tumour, and found, near the os coccygis, an abscess within a cystis, in which were 4 ounces at least of white pus, exceedingly stinking; and, on further examination, he found several cartilaginous joints, as it were, somewhat resembling the tail of a sheep, continued from the point of the os coccygis. These were about 2 inches long, and enveloped with a kind of fleshy substance covered with a sort of fat; these, when cut through, appeared exactly like the inner part of lamb-stones. From those depended a substance like the head and neck of an embryo, the size of a large egg, which on being opened contained somewhat resembling brain, and a kind of cerebellum in the back part: it had a mouth and tongue on one side of the face, if it might be so called, but no appearance of eyes or nose; however there was an ear pretty evident.

In the large tumour there hung a kind of loose membrane, which perhaps might be part of a secundine.

Of the Fluents of Multinomials, and Series affected by Radical Signs, which do not begin to Converge till after the Second Term. By T. Simpson, F. R. S. N° 487, p. 328.

Though the application of infinite series, and the quadrature of the conic sections, to the inverse method of fluxions, has exercised the pens of the most able mathematicians, and produced many curious and useful discoveries; yet nothing has been hitherto given, he believes, by which the fluents of radical multinomials and series, which do not begin to converge till after the second term;

can be determined, so as to be of use in the solution of problems; the common method, by expanding the given expression, being altogether impracticable in this case.

This, he says, is not merely an abstracted useless speculation, but may be applied to good purpose in many difficult and important inquiries into nature; of which he gives an instance or two, and further observes, that most of the lunar equations, given by Sir Isaac Newton, are only such approximations as may be exhibited by the first term of a series derived by the method here delivered.

The rest of this paper however may be consulted to advantage in the Treatise on Fluxions, by the same author, published in 1750, viz. at art. 356, where it is improved, as given in connection with other methods and examples.

On the Weather in South Carolina, with Abstracts of the Tables of Meteorological Observations in Charlestown. By Dr. John Lining. N° 487, p. 336.

The vicissitudes of the weather, with respect to heat and cold, are perhaps no where greater than in Carolina, and the summer's heat is probably not inferior to that under most places of the equator; nor is the winter's cold much less, at some times, than that in Britain.

From near 8 years observation, the greatest increase of the heat of the air, which he discovered in 24 or 30 hours in spring, summer, autumn, and winter, was 19, 24, 13, and 16 degrees of Fahrenheit's thermometer; and the greatest decreases of heat, in the same spaces of time, in those seasons, were 35, 32, 27, and 27 degrees respectively. It frequently happens, that one day is 10 or more degrees warmer than the preceding day; but the decreases of heat are always greater and more sudden than its increases. On the 10th of Jan. 1745, at 2 p. m. the mercury in the thermometer was at 70; next morning it had sunk to the 26th degree; and on the 12th day in the morning it was at 15, which was the greatest and most sudden change he had seen.

In summer, the heat of the shaded air, about 2 or 3 in the afternoon, is frequently between 90 and 95 degrees; and on the 14th, 15th, and 16th of June, 1738, at 3 p. m. it was 98; a heat equal to the greatest heat of the human body in health. In winter he never but once saw the thermometer so low as 15; therefore the difference between the most intense heat and cold of the shaded air, in this province, is 83 degrees; which is a much greater range than could well have been expected in this latitude; and taking the mean between those extremes, 56 should be the temperate degree of heat in this province; but the sum of the thermometrical altitudes, divided by the number of observations which he made for some years together, gives 66, which may therefore more justly be reckoned the temperate heat in Carolina, which exceeds 48, the temperate heat in England, more than that exceeds the freezing point.

The mean heat of the shaded air, in spring, summer, autumn, and winter, taken from the mean nocturnal heat, and from the mean heat at 2 or 3 p. m. is 61, 78, 71, and 52 degrees.

The mean heat of the shaded air at 2 or 3 p. m. in spring, summer, autumn, and winter, is 65, 82, 75, 55 degrees; and the mean nocturnal heat in these seasons is 57, 74, 68, and 49 degrees. Therefore our winter's nocturnal heat, at a medium, coincides nearly with the temperate heat in England.

The thermometer, when suspended 5 feet from the ground, and exposed to the direct rays of the sun, and to those reflected from our sandy streets, has frequently rose in a few minutes, from 15 to 26 degrees, above what was at that time the heat of the shaded air. When we are therefore exposed in the streets to the sun in summer, we inspire air from 4 to 28 degrees warmer than the heat of the human body.

The thermometer, when buried in the sands of the streets, when the heat of the shaded air was 88, rose in 5 minutes to 108, though there was at the same time a moderate wind.

In June 1738, when the heat of the shaded air was 98, the thermometer sunk 1 degree in his arm-pits; but continued at 98 in his hand and mouth; from which we see what little concern the air has in cooling the blood in the lungs. Two men who were then in the streets, when the heat was probably 124 or 126 degrees, (as the shaded air's heat was then 98) dropped suddenly dead; and several slaves in the country, at work in the rice fields, shared the same fate. Dr. L. saw one of the men immediately after he died; his face, neck, breast, and hands, were livid.

From the barometrical table it appears, that the barometer's mean altitude, taken from its greatest and least height, is 30.09 inches; and that its range is only 1.22 inch. Therefore their atmosphere varies only $\frac{1}{25}$ part in its weight. In the warm months, the mean barometrical station, taken from its greatest and least altitudes in these months, is 30.09 inches; and he never saw its range in these months exceed $\frac{38}{100}$ parts of an inch; therefore the changes of their atmosphere's weight, in the warm months, will have but little effect on human constitutions, as the difference between its greatest and least pressure is but $\frac{1}{5}$ part of that in cold climates, where the range of the barometer is 3 inches. May not the great height of the barometer in the warm months in this climate, proceed from the vast quantity of water, which is at that time supported in the atmosphere, as the exhalation is then very great? or may it not proceed from the rarefaction of the mercury? for the weight of the mercurial column, at equal altitudes, will be different under different degrees of heat; and the mercury may therefore be supported at equal heights by columns of air of unequal weights.

It appears, from the barometrical table, that our easterly or northerly winds elevate the mercury, and that our southerly or westerly winds depress it.

Abstract of the Bills of Mortality in Bridgetown, Barbadoes, for the Years 1737—1744. By the Rev. Mr. John Clark. N° 487, p. 345.

Anno	Born	Males	Females	Bapt.	Buried.	Anno	Born	Males	Females	Bapt.	Buried.
1737	52	26	26	77	208	1742	87	42	45	130	296
1738	81	41	40	106	250	1743	92	43	49	126	252
1739	91	54	37	119	244	1744	89	46	43	120	166
1740	91	49	42	123	242						
1741	68	33	35	95	261	Totals	651	334	317	896	1919

The Elements of a Short Hand. By Samuel Jeake, Esq. N° 487, p. 345.

A succession of new short-hands published without the reason of their construction, having put Mr. J. on forming a method founded on nature, the only guide to perfection, he settled an alphabet in the following manner.

Having taken, in a book that lay by him, a paragraph as clear of the principal idea of the book as any he could find, consisting of near a thousand letters, he enumerated the repetitions of each of them, and wrote them down; and thus made the following table of the number of times each letter was repeated in 1000. It is true, it cannot be said the repetitions will be exactly the same in every thousand letters that may be taken either in the same book or another; but whoever will enumerate them, will not find difference enough to be of consequence.

a	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*i*	*k*	*l*	*m*	*n*	*o*	*p*	*q*	*r*	*s*	*t*	*u*	*w*	*x*	*y*	*z.*
81	20	23	45	99	18	18	54	78	3	36	15	66	83	12	0	50	61	95	50	25	0	23	1.

After having made this table, he considered that there were in nature no more than 8 simple characters, 4 right, and the other 4 crooked lines. The 4 right lines are, first the perpendicular line |, and secondly the line of level —; which makes the two sides of a square. Secondly the oblique line / ascending from the left to right, and the oblique line \ descending from left to right, making the two sides of the rhomb; which is the figure of the diamonds on the cards. The 4 crooked lines are only the semicircle when the diameter is either above or below it, or on the right or left hand of it as, ◡ ◠ ⊂ ⊃.

All characters whatever must be made up of these, and from their composition, which introduces ambiguity of signification, arises the difficulty of reading a short-hand, which uses the simple characters for some letters, and compound characters for other letters; or, which is as bad, for words.

This difficulty, being unavoidable in a short-hand of more than 8 letters, making it appear that 8 was the number of letters a short-hand ought not to exceed, he considered it in the following light.

1st. If *a*, *e*, *i*, *o*, and the aspirate *h*, be suppressed, there will be 19 letters only remaining to be represented by 8 marks. 2d. If *c*, *s*, *x*, *z*, which have a sound much alike, be represented by one character, there will remain 15 letters to be represented by the other 7 marks.—3d. If *c*, *g*, *k*, *q*, which have a sound not very different, be represented by one character, there will remain 12 letters to be represented by 6 marks.—4th. If *b*, *p*, *f*, be represented by one mark, there will remain 9 letters to be represented by 5 marks.—5th. If *d*, *t* be represented by one mark, only 7 letters remain to be represented by 4 marks.—6th. If *l*, *r* be represented by one mark, only 5 letters remain to be represented by 3 marks.—7th. If *m*, *n* be represented by one mark, only 3 letters remain to be represented by 2 marks.—8th. If *u*, *w* be represented by one mark, there will remain one mark to represent *y*, the only letter unmentioned.

Writing with suppression of the vowels has been always admitted into short-hands of all sorts, because the consonants are considered as radical letters, which indeed they ought to be. He suppresses *h*, as being not radical.

All short-hands are subject to ambiguity; for there being but 8 marks to represent 24 letters; and those 8 being used for 8 of them in the short-hand alphabets, the other letters must be described by characters compounded of these 8. The ranging of the letters into classes, as is done here, will hardly introduce a greater ambiguity than all short-hands are subject to. So that this method cannot be reckoned more puzzling to a reader than any of the rest.

1st. The repetitions of *d* being 45, and of *t* 95, amount to 140, for the repetitions of this class.—2d. The repetitions of *l* being 36, and *r* 50, amount to 86, for the repetitions of this class.—3d. The repetitions of *m* being 15, and *n* 66, amount to 86, for the repetitions of the third class.—4th. The repetitions of *u* being 50, and of *w* 25, give 75, for the repetitions of the fourth class.—5th. The repetitions of *c*, when of the nature of *s*, being about half its number in the table, may be reckoned 10, those of *s* 61, those of *x* 0, and those of z 1, give 72, for the repetitions of the fifth.—6th. The repetitions of *b* being 20, of *f* 18, and of *p* 12, give 50, for the repetitions of the sixth class.—7th. The repetitions of *c* before *a*, *o*, *u*, being about 13, of *g* 18, of *k* 3, and of *q* 0, give 34, for the repetitions of the seventh class.—8th. The repetitions of *y* being 23, gives 23 for the repetitions of the eighth class.

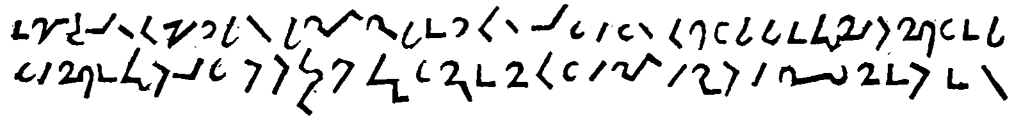
By a little reflection it will appear, that the marks applicable to these classes are in some measure determined: For a right line taking up less time than a crooked line in its description, it is plain the first 4 classes must be referred to the 4 right lines; and the 4 circular parts to the remaining last 4 classes. But the right lines are indifferent to all the first 4 classes, and the circular parts to the last 4 classes, for the reason just mentioned. So that so much as relates to the fixing the particular right line to represent the particular class, is at the liberty of the inventor of a short-hand, to adjust agreeable to his own fancy; and the same is true of the circular parts. Thus any one may perceive how far the fancy of a short-hand maker is properly bounded or at liberty.

He takes notice of one shortening rule; which is that of increasing the dimensions of a line, when the letter must be repeated successively: as in man, rare, and the like cases. This is a good rule of Mr. Weston.

An alphabet according to the classes.

dt.	lr.	mn.	uw.	csxz.	bfp.	cgkq.	y.

A Practice on the Lord's Prayer.



Which being expressed in letters of the common alphabet, will certainly convince the reader how easily a language may be read, though the vowels are omitted. *e. g.*

ur ftr wc rt n vn, llwd b ty nm, ty kndm cm, ty wll b dn n rt s t s n vn, gv s ts dy ur dly brd, nd frgv s ur dts, s w frgv ur dtrs, nd ld s nt nt tmpttn, bt dlvr s frm vl, fr tn s t kndm, t pwr, nd t glry, fr vr nd vr, mn.

The advantages of this short-hand in the state exhibited, when perfectly learned, so as to be written readily, will appear to be, 1. That, by suppression of *a e i o h*, or $\frac{400}{1000}$, only $\frac{6}{10}$ of the time of writing ordinary long-hand is necessary to write this. 2. That the simple strokes representing the consonants, not taking up above half the time of writing the consonants themselves, only half of $\frac{6}{10}$, or $\frac{3}{10}$ of the time of any thing written in long-hand, is necessary for writing this. 3. Right lines not taking up more than $\frac{2}{3}$ of the time of description of crooked lines, as the diameter is $\frac{2}{3}$ of the semiperiphery, it appears, if only right lines were used, these $\frac{3}{10}$ would be reduced to $\frac{2}{10}$, by the subtraction of $\frac{1}{3}$ of $\frac{3}{10}$. But, because the number of right lines, all things considered, should not be reckoned but about double the number of crooked ones, only $\frac{2}{3}$ of $\frac{1}{10}$ can be taken from the $\frac{3}{10}$; that is to say, the time taken up in writing this hand will be $\frac{9}{30} - \frac{2}{30} = \frac{7}{30}$ of the time taken up in writing of the common long-hand, or less than the $\frac{1}{4}$ of the time.

An Account of a Treatise by Wm. Brownrigg, M.D., F. R. S. intitled, "The Art of making common Salt, as now practised in most Parts of the World; with several Improvements proposed in that Art, for the Use of the British Dominions." Abstracted by W. Watson, F. R. S. N° 487, p. 351.

This work, in which the author has eminently distinguished himself both as a chemist and a philosopher, consists of 295 pages, exclusive of the preface, and of 6 copper-plates, exhibiting different views of salt-houses, instruments, &c. necessary to the preparation of salt. It is also enriched with notes of great importance to the work, not only of the author's, but also from the Phil. Trans. Medical Essays, Memoirs of the Royal Academy of Sciences at Paris, Pliny, Agricola, Alonso Barba, Ramusio, Boyle, Hoffman, Lister, Herrera, Dampier, Baccius, Pomet, Marsilli, Plot, Scheuchzer, Hales, Rastel, Leigh, Boerhaave, Shaw, and others.

That this art was capable of great improvements, especially as practised in Great Britain, was the sentiment of this Society soon after its institution; at which time its members were very intent on bringing it to a greater perfection; as may be gathered from the inquiries and suggestions of Dr. Beal, and the histories of several methods of making salt, which then were published by the Society. And though the English have, since that time, considerably improved their method of boiling salt; yet this art is still practised with greater skill and success by the Dutch, as the superior goodness of the fish, cured with their salt, sufficiently proves.

The commons of Great Britain, having taken into consideration the great importance of this art, judged some proposed improvements worthy their regard and encouragment; well knowing, that could this be brought to the same perfection in Britain as in some neighbouring countries, large sums of money might be saved in the nation, which are now paid to the French and others; its fisheries improved, and its navies and commerce, and many of its richest colonies, would no longer depend on its enemies for one of those necessaries, without which they cannot be supported.

These considerations have induced our author to give a brief account of the various methods of making salt, which are now used in Great Britain, and in other countries, where this art is practised with more success; and also to attempt several further improvements for the use of the British Dominions. The principal conclusions, deduced from a variety of observations and experiments, by our author, are as follows: 1. That, by the methods here proposed, an excellent bay-salt may be made in Britain in very large quantities, so as to be afforded cheaper than at the prices paid for foreign salt; and that the British colonies in America may very commodiously be supplied with bay-salt of their own manufacture, without having recourse for it to the French, Spanish, and Portuguese. 2. That, by the methods here proposed, an excellent kind of refined white salt may be made in Britain, as well from sea-water and rock-salt, as from natural brine, in any quantity wanted, so as to be afforded cheaper than foreign bay-salt; and which will also be better for curing fish, flesh, and other provisions.

Our author, treating of salt in general, takes notice of its excellence and usefulness; and that it has pleased the Author of nature to provide mankind with it in such abundance, that there are few countries which do not afford vast quantities of rock or fossil salt. Mines of it have been long discovered and wrought in England, Spain, Italy, Germany, Hungary, Poland, and other countries in Europe. The sea also affords such vast plenty of it, that all mankind might thence be supplied with quantities sufficient for their occasions. There are also innumerable springs, ponds, lakes, and rivers impregnated with common salt, from which the inhabitants of many countries are plentifully supplied with it.

In some countries, which are remote from the sea, and have little commerce, and which are not blessed with mines of salt, or salt waters, the necessities of the inhabitants have forced them to invent a method of extracting their common salt from the ashes of vegetables.

In short, this salt is dispersed all over nature; it is treasured up in the bowels of the earth; it impregnates the ocean; it descends in rains; it fertilizes the soil; it arises in vegetables; and from them is conveyed into animals; so that it may well be esteemed the universal condiment of nature.

Naturalists, observing the great variety of forms under which this salt appears, have thought fit to rank the several kinds of it under certain general classes, distinguishing it most usually into rock or fossil salt, sea-salt, and brine or fountain salt; to which may be added others of those muriatic salts, which are found in vegetable or animal substances. These several kinds of common salt often differ from each other in their outward form and appearance, or in such accidental properties as they derive from the heterogeneous substances with which they are mixed; but, when perfectly pure, they have all the same qualities; so that chemists, by the exactest inquiries, have not been able to discover any essential difference between them. It may for the present purpose be proper to distinguish common salt into the 3 following kinds, viz. into rock or native salt, bay-salt, and white salt.

By rock salt,* or native salt, is understood all salt dug out of the earth, which has not undergone any artificial preparation. Under the title of bay-salt, may be ranked all kinds of common salt extracted from the water, in which it is dissolved, by means of the sun's heat, and the operation of the air; whether the water, from which it is extracted, be sea-water, or natural brine drawn from wells and springs, or salt water stagnating in ponds and lakes. Under the title of white salt, or boiled salt, may be included all kinds of common salt extracted by coction from the water in which it was dissolved; whether this water be sea-

* By rock salt, or sal rupium, the ancient chemists mean salt adhering to the rocks above the high water-mark, being there lodged by the spray of the sea, evaporated by the heat of the sun; which is the purest salt of all for chemical uses, and is to be had off the rocks of Sicily, and several islands in the West Indies. C. Mortimer.—Orig.

water, or the salt water of wells, fountains, lakes, or rivers; or water of any sort impregnated with rock salt, or other kinds of common salt.

The first of these kinds of salt is in several countries found so pure, that it serves for most domestic uses, without any previous preparation, triture excepted. But the English fossil salt is unfit for the uses of the kitchen, till by solution and coction it is freed from several impurities, and reduced to white salt. The British white salt also is not so proper as several kinds of bay-salt for curing fish, and such flesh-meats as are intended for sea provisions, or for exportation into hot countries. So that, for these purposes, we are obliged, either wholly or in part, to use bay-salt, which we purchase in France, Spain, and other foreign countries.

Bay-salt in general may be divided into two kinds. First, bay-salt, drawn from sea-water, as is practised in France, Spain, Portugal, and many other countries. Secondly, bay-salt extracted from salt springs, ponds, and lakes; as at Cape de Verd islands, Tortuga, and other places. Of these the first is imported in large quantities into Great Britain and Ireland; our American colonies, in times of peace, are chiefly supplied with the latter; but in time of war they have large quantities of bay-salt from Lisbon, and other parts of Portugal,

Bay-salt is prepared in a manner the most simple and easy, when the water of ponds and lakes impregnated with salt is totally exhaled by the force of the sun and air, and the salt is left concreted into a hard crust at the bottom of the lake or pond. Of salt thus prepared, we have instances in many parts of the world; as in the Podolian desert near the river Borysthenes on the Russian frontiers towards Crim Tartary, in the kingdom of Algiers, and in other parts of the world. Bay-salt is also drawn from the brine of ponds and lakes, and our author gives an account of the preparing it in this manner in the Cape de Verd islands. He also takes notice of the bay-salt made at Tortuga, and other places in America. He describes likewise the manner of making marine bay-salt in France, and other parts of Europe. Every kind of bay-salt is prepared without artificial heat, and by only exposing the brine under a large surface to the action of the sun and air, by which, in proportion to the strength of the brine, and to the different temperature of climate and season, the salt crystallizes into what we call bay-salt, and comes under different appearances to us from different places, which arise principally from the cleanliness and care of the artist.

Our author, when treating of white salt in general, acquaints us, that though salt is made, in warm climates, with the greatest ease, and at the least expence, by the heat of the sun, after the methods already described; yet, in several countries, where bay-salt might be conveniently made, they prepare all their salt by culinary fires. Thus in Austria, Bavaria, and many other parts of Germany, and also in Hungary, and even in some parts of Italy, they constantly boil the water of their salt springs into white salt. But in other parts of Europe, as in Britain, and in the northern parts of France and Germany, an erroneous opinion long prevailed, that the heat of the sun was not there sufficiently intense, even in the summer season, to reduce sea-water, or brine, into bay-salt. And all arguments would probably have been insufficient to remove this prejudice from the English, had not the contrary been fully proved by experiments, which were first accidentally made in Hampshire. However, the method of making salt by coction will probably still continue to be practised in Britain; as the salt so prepared is for several uses preferable to bay-salt; and when prepared after a particular manner, is preferable to common bay-salt, even for curing provisions, as the practice of the Hollanders sufficiently testifies.

White salt, as it is prepared from various saline liquors, may therefore be distinguished into the following kinds: 1. Marine boiled salt, which is extracted from sea water by coction. 2. Brine or fountain salt, prepared by coction from natural brine, whether of ponds or fountains. 3. That prepared from sea water, or any other kind of salt water, first heightened into a strong brine by the heat of the sun, and the operation of the air. 4. That prepared from a strong brine or lixivium drawn from earths, sands, or stones impregnated with common salt. 5. Refined rock salt, which is boiled from a solution of fossil salt in sea water, or any other kind of salt water, or pure water. 6. Lastly, salt upon salt, which is bay-salt dissolved in sea water, or any other salt water, and with it boiled into white salt; and under these heads may be ranked the several kinds of boiled salt now in use. Our author has given us an exact history of the manner of preparing these different kinds of salt, as practised in different places, with miscellaneous observations and cautions relating to their respective processes.

From the process by which white salt is made from sea water by coction, it appears, that sea water, besides common salt, contains several other ingredients; some of which are separated before the common salt falls, and others remain in the bittern, after all the salt is extracted. Our author has given a full and circumstantial account of these in an express chapter, under the appellation of memoirs for an analysis of sea water.

The salt-boilers, and particularly those who prepare brine salt, have long been accustomed to make use of various substances, which they call additions or seasonings, and mix them with the brine while it is boiling, either when they first observe the salt begin to form, or else afterwards during the time of granulation. These additions they use for various purposes. First, to make the salt grain better, or more quickly form into crystals. Secondly, to make it of a small fine grain. Thirdly, to make it of a large firm and hard grain, and less apt to imbibe the moisture of the air. Fourthly, to render it more pure. And lastly, to make it stronger, and fitter for preserving provisions.

These additions are wheat-flour, resin, butter, tallow, new ale, stale beer, bottoms or lees of ale and beer, wine lees, and alum. Wheat-flour and resin are used for the property they possess of making the salt a small grain. Butter, tallow, and other unctuous bodies are commonly applied, as they are said to make the brine crystallize more readily; for which end some salt boilers more particularly prefer the fat of dogs: but others have little to plead for their using these substances, but immemorial custom: how far they have the effects ascribed to them can only be determined by experiments, as several boilers, who formerly used them, now find they can make as good salt without them. Wine lees, new ale, stale ale, the lees of ale and beer, are now generally rejected by the marine salt boilers; except in the west of England, where the briners, who use them, affirm that they raise a large grain, and make their salt more hard and firm, and some say that they make it crystallize more readily. Hoffman prefers the strongest ale; and Plot assures us, that it makes the salt of a larger or smaller grain, according to the degree of its staleness. The only good effects that fermented liquors can have as an addition, are probably owing to their acid spirit, which may correct the alcaline salts of the brine, and so render the common salt more dry and hard, and less apt to dissolve in moist air. If therefore it should be thought necessary to use any of these additions, in order to correct the alcaline quality of the brine, stale ale, or Rhenish wine,* ought to be chosen, as new ale contains but little acid.

Alum is an addition long known and used in Cheshire, together with butter, to make the salt precipitate from some sorts of brine, as we are assured by Dr. Leigh in his Nat. Hist. of Lancashire, Cheshire, &c. who first taught the Cheshire salt-boilers the art of refining rock salt. As the bad properties of their salt proceeded from hard boiling, they found every method ineffectual, till they had recourse to a more mild and gentle heat. And as alum has been long disused among them, it is not likely that they found any extraordinary benefit from it; otherwise they would scarcely have neglected it, and continued the use of butter. However Mr. Lowndes has lately endeavoured to revive its use; asserting, that brine-salt has always 2 main defects, flakyness and softness; and to remedy these imperfections, he tried alum, which fully answered every thing he proposed; for it restored the salt to its natural cubical shoot, and gave it a proper hardness; nor had it any bad effect whatever. But our author is of opinion, that whoever considers the nature of alum, will scarcely expect such extraordinary effects from it. Neither does it here seem wanted; for the grains of common salt will always be sufficiently hard, and of their natural figure, large size, and no ways disposed to run by the moisture of the air, if formed by a gentle heat, and perfectly free from heterogeneous mixtures: so that the goodness of

* Why not malt-vinegar? C. M.—Orig.

Mr. Lowndes's salt does not seem owing to the alum, with which it is mixed, but chiefly to the gentle heat used in its preparation.

The Dutch, who have long shown the greatest skill and dexterity in the art of boiling salt, make use of another addition, which they esteem the greatest secret of their art. This is whey, kept several years till it is extremely acid; now first revealed by our author to the British salt boilers, but long held in great esteem by the Dutch, for the good effects it has on their salt; which it renders stronger, more durable, and fitter to preserve herrings, and other provisions.

Bay-salt, as well as white salt, is of different kinds, and possessed of different qualities: with the different kinds of these, provisions must be cured, according to the uses for which they are designed. The Dutch indeed use no salt for curing provisions, besides their own refined salt. With it they can preserve flesh and fish of all kinds as well as with the strongest bay salt; and chuse to be at the expence of refining bay salt, rather than to defile their provisions with the dirt and other impurities, with which it commonly abounds.

Salt, esteemed the best for curing provisions, and for preserving them the longest time, is that which is the strongest and the purest. This may be known by the following characteristics; viz. it is usually concreted into large grains or crystals, which are firm and hard, and in respect to those of other kinds of common salt, the most solid and ponderous; it is not disposed to grow moist in a moderately dry air, to which it has been exposed a considerable time; its colour is white, and somewhat diaphanous; it has no smell; its taste is truly muriatic, and more sharp and pungent than that of other kinds of common salt. It has also several other distinguishing properties mentioned by our author. The salts which approach nearest to this degree of perfection, are the best kinds of bay-salt, and the strong Dutch refined salt; but most of the salt now made for sale is very far from answering to these characteristics.

Having related the various methods of preparing salt that now are in use, as far as they are come to our author's knowledge, it appears that this art is not brought to such perfection in the British dominions, as in several other countries, the salt there prepared being unfit for preserving many kinds of provisions. It remains now to show, that this want of a strong salt of British manufacture proceeds not from any defect in nature, but of art; and that if proper skill and industry be used in the British dominions, and due encouragement given there by the legislature, such improvements may be made in this art, that not only Great Britain, but Ireland also, and the British colonies in America, may be supplied with salt of their own manufacture, proper for curing all kinds of provisions, in quantity sufficient for all their occasions, in quality equal, if not superior, to any foreign salt now made, and at a moderate price. These are truths which the author hopes will appear evident from the facts and reasonings contained under the following positions:

Lemma 1.—The quantity of water which annually falls in rain, snow, and hail, is very different in different parts of Great Britain; there commonly falling almost double the quantity on the western coasts, that falls on the eastern coasts of that island.

Lemma 2.—The quantity of rain which falls in Lancashire, during the 4 hottest months of the year, viz. May, June, July, and August, does not at a medium amount to more than a third part of the quantity of water which falls in rain, snows, and hail, during the whole year.

Lemma 3.—The water which ascends in vapours from the sea very greatly exceeds that which descends thereon in rain and other aqueous meteors: but the quantity of water, which usually exhales from a given part of the ocean in a given time, cannot with any exactness be determined.

Lemma 4.—The quantity of water which commonly exhales in Great Britain from shallow ponds during the 4 hottest months of the year, greatly exceeds the quantity of rain which commonly falls on the surface of those ponds during the said months.

From these lemmata, which the author has supported by the observations, not only of himself, but of other learned men, are deduced the following propositions:

Prop. 1.—In several parts of England large quantities of bay salt may be extracted from sea water during the hottest months of the year, by receiving the salt water into ponds, and suffering its aqueous parts thence to exhale by the heat of the sun, and the operation of the air and winds.

Prop. 2.—In several parts of England large quantities of bay salt may very commodiously be extracted from sea water, after the same manner that is practised in France, and in other parts of Europe.

Prop. 3.—Bay-salt may be extracted in England from sea water in larger quantities, and with more certainty, than by the foregoing method, if care be taken to preserve the brine contained in the salt pits from being diluted with rains, and to promote the evaporation of the water by several artificial means, which may easily be put in practice.

Prop. 4.—In several parts of England large quantities of excellent bay salt may with great ease be made from the natural brine of salt springs, and also from rock salt dissolved in weak brine or sea water.

Prop. 5.—Bay salt may be prepared in England by the foregoing methods at a very moderate expence, equal in goodness to the best foreign bay-salt, and in quantity sufficient for the consumption of all the British dominions.

Prop. 6.—In several of the British colonies in America, bay-salt might, with little expence and trouble, be prepared from sea water, in quantities sufficient to supply the American fisheries, and all other occasions of those colonies, so as to become a considerable branch of their trade.

The author has supported all these propositions with great ingenuity; but we cannot pass over in silence the artificial means to promote the evaporatiou of sea water, mentioned in Prop. 3, as well as to preserve the brine contained in the salt pits from being diluted with rains. It will be proper, says our author, to make all the salt pits of the marsh in one long row extended from east to west, and for each pit to make covers of thin boards, or rather of coarse canvas, or sail-cloth, stretched on frames of wood, and painted white. These covers must all be fixed with hinges to strong posts and beams on the north side of the pits; so that they may be let down and drawn up with cords and pulleys, or by some other contrivance, somewhat like drawbridges. These covers thus fixed may be let down over the pits like a shed or penthouse in rainy weather; and in dry weather may be erected almost to a perpendicular, but inclining a little towards the south; so as to form a wall with a south aspect. Thus these may serve a double purpose, as coverings for the pits in wet weather, and as reflectors of the sun's heat upon them in dry weather, and thus greatly promote the evaporation of the aqueous parts of the brine. The hinges on which the reflectors turn may be fixed about 8 or 10 inches from the ground; by which means, when the reflectors stand upright, there will be an opening left beneath them, through which the air will continually flow in a brisk current, and greatly increase the evaporation of the water.

After having gone through that part of Dr. Brownrigg's work, which relates to bay salt, we proceed to the methods that gentleman proposes for preparing and improving white salt, which, if brought into use, may probably be of advantage not only to private undertakers, but also to the public. For it appears, that 2 very different kinds of white salt are required; the one for the use of the table, and the other as a condiment for provisions. Its whiteness, dryness, and the smallness of its grain, are the properties which chiefly recommend the first kind; and its great strength and purity the latter. It is this strong and pure kind of white salt, which is wanted in the British dominions; and it is therefore our author's principal design here to consider how this defect may be supplied; though at the same time instructions are given how to prepare table salt, not only better in quality, but also at a less expence than it is now prepared by the common methods.

Lemma 1. In the common processes for making white salt, the salt is deprived of a considerable part of its acid spirit, by the violent boiling used in its preparation.—*Lemma* 2. Most kinds of white salt are rendered impure by the mixture of various heterogeneous substances.—*Lemma* 3. White salt, by the violent coction commonly used in its preparation, is rendered less fit for preserving fish, flesh, and other provisions, than it would be if prepared with a more gentle heat. —*Lemma* 4. The heterogeneous substances which are commonly mixed with white salt, render it less proper for preserving provisions, than it would be if separated from them.

After having fully considered the foregoing, our author gives a method of preparing a kind of white salt proper for curing fish, flesh, and other provisions: also a method of refining salt. Most of the facts referred to in these disquisitions are such, as the constant practice of those who make salt sufficiently warrants us to rely on for true and certain; or else, they are the observations of judicious salt officers, daily conversant in these matters, or of curious and inquisitive navigators, merchants, travellers, and naturalists; or, lastly, the experiments of many learned physicians, chemists, and philosophers: the truth of which several facts, though many of them have long been published, has never been called in question. So that these observations and experiments may probably be more relied on by the public, than if they had only been made by our author: since they have the testimony of many skilful and unprejudiced persons, who could have no notion of the uses to which they have been here applied. If therefore the arguments founded on those facts should be esteemed any ways reasonable and satisfactory, the author presumes to remark, that it might not be unworthy the wisdom of the British legislature to direct a more full inquiry to be made into a matter of this importance, and to order proper works to be erected for making bay-salt, and for making and refining white salt, and to put those works under the management of able and judicious persons, to make exact and accurate trials, in order to discover the best and cheapest methods of doing them. And the methods which should be most approved of, might for the general good be made public, and established by law as a common standard, to which all those who make salt in the British dominions should be obliged to conform.

A Catalogue of the Immersions and Emersions of the Satellites of Jupiter, that will happen in the Year 1750; *of which there are* 173 *of the First,* 85 *of the Second,* 94 *of the Third, and none of the Fourth, by Reason of its great Latitude*; *in all* 322. *Computed to the Meridian of London from the Flamsteedian Tables. Corrected by James Hodgson, F. R. S., &c.* N° 487, p. 373.

These eclipses of Jupiter's satellites, with several former collections by the same gentleman, like an old almanac, are now of no further use, after their æra is passed.

Of the Storm of Thunder, which happened June 12, 1748. By the Rev. Henry Miles, D.D., F.R.S. N° 488, p. 383.

The preceding day had been remarkably hot, and in the afternoon very cloudy, with the usual indications of an approaching storm in the evening. At 1 next morning, a person apprehensive of the thunder, looking out at his window, was surprised to find an unusual clear sky, every where equal to what is observed in frosty weather, or after a high wind, except that in a few places were some thunder-clouds just above the horizon.

At 2 we heard thunder at a distance: at half past 3 Dr. M. perceived the storm approaching apace from the south, where the wind then was, but the darker clouds seemed to bear off chiefly to the east and west. At 4 fell a smart shower of rain, and about 5, 2 loud claps of thunder over our heads, but pretty high; the lightning was very pale, and the flashes large, descending in a spiral form, almost perpendicular to the horizon to the eastward, which is the situation of Stretham, and at about 2 miles distant from him. His barometers continued successively rising and falling during the storm, but very inconsiderably.

Hearing that 2 houses were damaged, situate at the foot of the hill on which the mineral wells are, fronting the east, by the wood side, he went next day to view them. The house to the south, which is a public house kept by Mr. Howard, seemed to have received the greatest shock. Some of the family being up, the front door stood partly open, when the storm began: the upper half was of glass, framed like a sash window, having 2 sliding shutters, one on each side, which had not been taken down. The glass between them was shattered to pieces, but the shutters no ways touched, except that a nail in one of them was forced in a little way. To the door-post, on the left hand, hung by an iron pin an iron bar, which served to fasten the door at night: this pin was driven out of the post, and the bar considerably bent, and in divers places melted in small spots, as were the hinges of the door, chiefly on the edges in both, and the door-post split. A sheet of lead on the pediment, or shelter over the door, was raised, and partly rolled up at one corner; the cornice underneath being torn off without being split, a good part of the tiling near the eaves and over the pediment was loosened, and some tiles beaten off, and the lathing, and some of the mouldings of the windows had taken fire.

In a bed-chamber fronting the road, on the 2d floor, where Mr. Howard lay, 3 boards of the lining of the room, on the east side, were driven inwards 5 or 6 inches at one end; but at the other the nails were a little loosened only. In a garret over this chamber, the upper part of a bed-post was shivered; and nearly over where this bed stood, a large hole was broken in the roof, on the west side, just by where one of the chimneys goes up; the chimneys having all additional funnels of brick-work on the top, of a roundish form, and plastered: these were struck, and inclined to the north, especially that which was on the south end of the house, the plaster being beaten off, and some of the bricks broken down. There were about 13 persons in this house, none of whom received any hurt; though a lad, who was in the kitchen, into which the door opened, before mentioned, and the window of which (near where he was standing) had several panes of glass broken, must certainly be much exposed. He said that the fire flew about him in sparks, like those which fly out of burning charcoal, but larger, and snapping as they do. Some pieces of glass were showed, which had been melted.

The adjoining house, inhabited by Mr. Figgins, had the plastering beaten off in the front in patches, and one of the chimneys cracked for a great length. In the kitchen window frame, one of the cross pieces, near the middle of the window, had a chip struck off from it about 5 inches in length, and at one end about a quarter of an inch thick, but thin at the other, and near the width of the frame, but none of the glass broke, nor the lead bent, though in a manner contiguous with the splinter beaten off. The same thing happened to a parlour window, on the other end of the house; both the shivers were found directly opposite to the windows, at 10 or 12 yards distant in the road.

In a small garret, which is next to Mr. Howard's house, where 2 maid-servants lay, the plaster was broken, to appearance inwards, on opposite sides of the room, and near the feet of the bed, which stood on each side about three quarters of a yard from the wall. The breach on the east side, near a window (some panes of the glass of which were broken) was opposite to the vailings of the bed, which were singed, and a hole burnt through them large enough to receive the end of one's fore finger. On the opposite side, just by the chimney, another breach was made, of the same height, in the wall, which was continued downwards for about a yard, but the curtains not at all singed. Directly against this breach, one of the maids, who had got up, and sat on the bed's side, was instantly struck down, but received no hurt: on inquiring of her, whether she seemed to receive a blow on any particular part of her body? she replied, she was struck all over alike.

But the most remarkable, though the least terrible effect, appeared on the frame of a pannel of wainscot, about 5 feet long, and about 1¼ wide, in the parlour fronting the east: on this pannel a landscape is painted, and the moulding belonging to it had been gilt, but on the last painting the room, the gilding was covered with the same paint: that which covered the gilt moulding was stripped off in irregular ragged streaks throughout, so that the gilding appeared as fresh as it may be thought to have looked when it was painted at first: and as the gilding does not seem to have been affected, so neither does the paint appear to have been cracked any where, but where the gilding lay under.

Remarks on Mr. Jeake's Plan for Short-hand. By John Byron, M.A., F.R.S. N° 488, p. 388.

In Mr. Jeake's paper it is inferred, from the continual succession of new short-hands, that none of them were constructed on right principles, which, in the opinion of the proposer of this plan, are briefly these: 1. There are in nature only 8 simple characters, viz. 4 rectilinear ones, (| — ⁄ ∖) and 4 crooked or semicircular (∩ ∪ C Ɔ). 2. To avoid the ambiguity and confusion that must arise from the use of compound characters, a perfect short-hand should consist of these 8 simple ones only. 3. But whereas there are 3 times as many letters (or more) in the common alphabet, the consequence is, that one character must serve for 1, 2, 3, or 4 letters; as their frequency of occurrence, or affinity to each other, shall suggest. 4. From these suppositions, among a variety of alphabets that would equally answer his intention, results the following, which (omitting as needless the letters *a*, *e*, *i*, *o*, *h*) he proposes for the plan of a perfect short-hand; and computes, with great exactness, that it may be written in less than one quarter of the time that common long hand will require.

The Alphabet.

⁄	—	∖	|	C	Ɔ	∩	∪
d t.	*l r.*	*m n.*	*u w.*	*c s x z.*	*b f p.*	*c g k q.*	*y.*

This, with a specimen of the Lord's prayer, as written in it, is the whole of his plan; which, as far as it goes, might have a plausible appearance to a person, at the first turn of his thought towards short-hand; but a little practical attention must have shown him how liable it was to the very objection that he intended to remedy, viz. ambiguity.

The first mark, for instance, (L), in this short specimen, stands for these 4 several words which occur in it, viz. *our*, *will*, *evil*, *ever*; and 40 more that might be enumerated, must, whenever they occur, be represented by it; not to mention how often it must occasion ambiguity in the beginning, middle, or end of a longer word, or marks, of which it is a constituent part only.

Now, though in the Lord's prayer it is easy, or in casual writing one of his learning and sagacity might be able, by a long familiarity with the characters, to determine the sense of what was written in them, yet it is evident that, to common learners, a difficulty so perpetually occurring must appear insuperable.

The postulatum, likewise, which this plan for short-hand is grounded on, is taken up too expeditiously; for there being in nature 4 rectilinear strokes, the horizontal, the perpendicular, and the acute, and grave (if I may so call them); it is manifest, by inspection, that from these 4 directions there will arise, at least, 8 curvilinear characters, as each of the straight ones admits distinctly of 2 opposite curves; and there is no absolute necessity that any of them should be always semicircular; a shape that, for the most commodious combination of simple characters, is in fact much oftner inconvenient than otherwise.

The alphabet then of simple characters may be fairly enlarged by one third; and room be also left for the fancy of an inventor to extend it farther, if he should find it convenient on the whole. He says on the whole; for the worst short-hand may happen to express a few particular words better than the best; and arbitrary marks for words or sentences may be often shorter than regular ones: but this is no inducement to write, in one case, by a bad method, and in the other by none at all.

Another oversight in this plan is the neglect of beauty and linearity; though the simplicity of its character does not perhaps admit of such enormous scrawling as others may. For, to instance again in the specimen; suppose the mark for the word temptation, which expresses a vast variety of different combinations of consonants, to be limited, by a previous knowledge of the language, to that word only, yet after all it is a very aukward one; and ought, by a common short-hand rule of leaving out such consonants as are not sounded (as the p is not in temptation) to have been formed in another manner, wherein the beauty and linearity, and of course the brevity of the mark, would have been preserved.

In short, this gentleman set out on right principles, which many hap-hazard undertakers have but little considered; but he had not leisure enough perhaps to examine them to the bottom; as was the case with Dr. Green of Cambridge (he that wrote the Greenian Philosophy, as he calls it), who formed a short-hand for his own private use, on much the same plan and principles. He gave Mr. B. one of his sermons in it; and on suggestion of the advantages that he might have taken, he said, that for want of time to consider of his scheme more thoroughly, when he first adopted it, he had overlooked them.

A perfect short-hand, Mr. B. supposes, would be a solution of some such problem as this: "A language being given, to assign the most compendious method of expressing it readily, and legibly, by an alphabet, and rules, the best adapted to that purpose."

This gentleman proceeds no farther than to make an alphabet for his plan;* but he must be sensible that were it ever so complete a one, many compendious

* Mr. Jeake only offers his plan as the mere elements of a short-hand, leaving it to practitioners to build upon his foundation, as they shall judge necessary from practice: he retains the *v* because it often stands for *v* or *ve* or *w*.—Orig.

applications of it might be obtained by a proper inquiry into the nature of our language (the most happily susceptible of this art of any) and the abbreviations which it admits of, very intelligibly, in writing. And in his alphabet he entirely omits the letter *h* (which is often wanted), and the vowels *a, e, i, o,* and yet retains the vowel *u,* which is certainly as needless as any of the rest: but as a single point, in 5 distinct situations, would have provided for them all alike, he might as well have added that to his plan, in order to express any particular vowel, on occasion; because it would not have hurt his alphabet; and because the reading of his short-hand without any vowels at all, is so extremely difficult. For, as one of his straight strokes (\\) must stand for the words *am, an, in, on, no, me, him, home, &c.* and one of his crooked ones (C) for *as, is, us, so, has, his, ease, ice, use, ax, ox,* &c. and so of the rest; he would himself, in all probability, be often at a loss to distinguish what he had written on his own plan.*

The consonants *j* and *v* he has taken no notice of; as if the common way of repeating 24 letters did really give a just idea of an alphabet; which it does not; nor can a perfect short-hand for our language (or any other respectively) well be planned, without considering the real alphabet, or table of every particular sound, or modification of sound; that is to say, vowel or consonant which occurs in it: and then adjusting the proper characters to them, and taking all the advantages that either nature or custom may afford.

Mr. B. hopes it will not be thought impertinent in him to offer these remarks on the above plan, of an art which he has taken so much pains to cultivate, and bring to that perfection which his first and last intention of introducing one common standard for the general practice of it required.

On the Sparkling of Flannel, and the Hair of Animals, in the Dark. By Mr. B. Cooke, F.R.S. N° 488, p. 394.

Mr. C. imagines this sparkling of the flannel,† and such like bodies, will be found to be quite electrical: and possibly the acid steams of the sulphur, burnt under the extended flannel in the time of bleaching, may unite with the oil (with which hair, as well as horns, are found by analysis to be replete), and form an animal sulphur, which, on friction, vibration, or any nimble agitation of these hairs, may become luminous.

It should have been mentioned, that the flannel had been worn but few days; and that it was immediately on shaking the under coat from that which was worn above it, that the sparks were emitted; and that their appearance was in a broad

* Vowels may be known to be antecedent or consequent, by the mark being written above or below the line of level: e. g. C *as.* C *sa*; \\ *am,* \\ *ma.* The ambiguities in many of these words are not important, viz. *as, has, is, his, use, us.* S. J.—Orig.

† See Phil. Trans, N° 483.—Orig.

streak almost contiguous, attended with a crackling or snapping, like what may be observed on moving the finger nimbly along over the prime conductor, when excited in the electrifying machine; of which the lady was able to form a comparison, having afterwards seen some experiments of that sort. This appearance returned at the same time, and on the same occasion, 2 or 3 nights after, but more languid, till it was quite lost.

A lady, who was informed of this, lessened the surprise, which had been thought almost ominous, by assuring, that she had seen the same phenomenon often in new flannel, but never in any that had been long worn or washed: and that the flannel being rendered damp with sea water, and afterwards dried, would heighten the flashing, which she imputed to the sulphur used in bleaching. Mr. C. further observes, that these sparklings had the crackling criterion of electrical fire; and that hair and wool, as well as silk, are electrics per se, and unctuous and sulphureous bodies more electric than others of the same density.

Of an Earthquake at Taunton. By the Rev. John Forster. N° 488, p. 398.

Between 10 and 11 o'clock at night, July 1, 1747, Mr. F. being in some company at Taunton, they were suddenly surprised with a rumbling noise like distant thunder, which was followed immediately by so considerable a motion of the earth, that the chairs rocked under them. The noise and shaking seemed to come from a distance, and approached gradually, in such a manner as if a loaded waggon had passed along, and continued nearly the same time as such a waggon would require to go about 100 yards. The motion went from south-east to north-west; which being the direction of the street, on one side of which the house stood, some of them imagined at first that a waggon had really gone along; but on running out and inquiring, they found there had been no waggon.

Though this happened between 10 and 11 o'clock at night, when most of the town were in bed, the shock was so sensible, that many people got up very much terrified; and these waking others, the consternation soon became general; insomuch that, though it was a rainy night, numbers of people ran out into their gardens, and spent the night there, being apprehensive of other shocks. The account then newly brought us of a dreadful earthquake at Lima, being fresh in every body's mind, contributed to increase the surprize.

A worthy clergyman, who lives 5 miles from Taunton, informed Mr. F. that the china and glasses on the cupboards in his house rattled and shook as if they would fall down, and the bells in his house rang. A person who was at that time coming on foot to Taunton also said, that the noise seemed to him like the discharge of cannon at a distance, and came rumbling onwards, till the earth moved under him in such a manner, that he could hardly keep on his legs: several others also that were abroad assured Mr. F. that they had much ado to save themselves from falling.

The extent of this earthquake, as far as can be learned, was from sea to sea; that is, from the South Channel to the Severn. It moved from south-east to north-west, and was felt in every parish through this whole course, which is in length about 40 miles; nor was its breadth much less; for it was felt at the same time both at Exeter and Crookhorn, distant also 40 miles, in a line directly across its before-mentioned course.

Remarks on Mr. Lodwick's Alphabet for Short-hand. By John Byrom, M.A., F.R.S. N° 488, p. 401.

Mr. Lodwick's Essay towards a Universal Alphabet, was given in N° 182 of the Philos. Trans., and is here criticised and condemned, by Mr. Byrom, who was the author of another more practicable short-hand himself, which he had good opportunity of perfecting, by the great experience he had in teaching it to numerous pupils.

A Roman Inscription found at Bath. Communicated to the R. S. by the Rev. William Stukely, M.D., Fellow of the Coll. of Phys., F.R.S., and Rector of St. George the Martyr, London. N° 488, p. 409.

L. V. VITELLIVS ▼ MAXIMI
NIAN ▽ F ▽ T ▼ ANCINVS ▼
CIVES ▼ HISP ▼ CAVRIESIS
EQ ▼ ALAE VETTONVM ▼ CR
ANN ▼ XXXXVI ▼ STIP ▼ XXVI ▽
H ▼ S ▼ L ▼

Thus to be read.—Lucius Vitellius Maximiniani filius Titus Ancinus, civis * Hispanus Cauriensis † equitum alæ Vettonum curator, annos XLVI. stipendii XXVI. hic sepultus est.

Remarks concerning some Electrical Experiments. By the Rev. Dr. Stephen Hales, F.R.S. N° 488, p. 409.

A warm thick piece of iron being suspended by 2 silk lines, had a warm very thick piece of brass laid on it, on which was placed a common hen's egg: when electrified, the flashes from the iron were of a bright silver light colour; from the brass, especially near it, the flashes were green; and from the egg, of a yellowish flame colour; which seems to argue, that some particles of those different bodies were carried off in the flashes, whence these different colours were exhibited.

* Like nubes, lapes, sepes. † Of the city of Coria in Spain.

It is suspected that great degrees of electrifying have occasioned some women to miscarry; and no wonder that such sudden shocks should do it.

Mr. King, the experimenter, observes, that a piece of linen that has never been washed, will soon give a good degree of electricity to a large warm glass tube; viz. on account of the mealy paste, which weavers dress the linen with; and therefore any piece of linen thus dressed will do.

The Dr. gave an account, in the General Evening Post of Sept. 1747, of the great benefit of ventilators in Newgate, and in the Success frigate for Georgia, which lay five months wind-bound in our channel with the transports for Cape Breton, the rest of which were all very sickly; but in the Georgia frigate, in which were about 300 men, all were in good health, and they got all in health to Georgia.

On the Cure of a Wound in the Cornea, and a Laceration of the Uvea in the Eye of a Woman. By Tho. Aery, M.D. Nº 488, p. 411.

A poor widow, aged 26, of a pale complexion, was for several years at times subject to the colic. Dec. 26, 1744, she received a wound in the cornea of her right eye, by the spear of a common fork, which also divided the uvea. Part of the aqueous humour was discharged, the eye lost its transparency, had a violent pain in it, and she could only distinguish objects when she looked down. Dr. A. ordered her a collyrium prepared of the bals. tolut. camphor. solut. in sp. vin. aq. plantag. cum paxillo tinct. mart. Mynsicht. A few drops of this blood-warm was to be used frequently; to bleed her largely in the arm, and her diet was to consist of water-gruel, aq. hord. and fresh broth.

Next day she had no pain in the eye, but complained she saw motes floating before it: he ordered her a purge, and an astringent fomentation to her temples and eyelids. The day following the eye was inflamed, and the lids tumefied, and she had a pain in her head. The collyrium was changed for rose-water and vinegar, an ℥ ss. roche alum *gr.* v. 3 drops twice a day. The 29th the inflammation increasing, the purge and bleeding were repeated, and the parts were fomented only with spirit of wine. The 31st the inflammation continued to decrease, till after a fright. Jan. 5th the inflammation increasing, the sides of the wound became a little protuberant. The purge was repeated, and a blister laid behind the right ear, and an emollient collyrium was used: next day the swelling of the eyelids was gone: the 11th she had a show of the menses, and the wound appeared healed: from the 15th to the 24th the inflammation continued to abate; only one day it increased by fretting and weeping much; but by bleeding she grew better, and so she continued to the 30th; except one day, on catching cold, her eye became exceedingly inflamed, which was relieved by bleeding. Feb. 4, she had a little pain in her eye, and the tunica adnata looked a little red. Soon after dropping in 2 drops of cold water, the eyelids swelled, and a violent inflammation of the eye ensued, with a speck appearing; but these symptoms went off by repeated applications of leeches and a mercurial purge. The 19th a sternutatory of hellebore and euphorbium was ordered. In a few days after the inflammation left her eye; when she complained she saw double; which complaint also soon left her.

The eye was myopical, and she saw the right side of objects a little darkened; yet she could read pretty small characters. The uvea was not united where it was divided, but still retained its natural power of contraction; the transparency of the humours and convexity of the cornea were the same as before; there was no scar on the cornea; the shape of the pupil was much altered.

On catching cold she was subject to a slight pain in her eye. At the above date there remained no other alteration than what he had just mentioned, and what necessarily followed from the contraction of the pupil, the not admitting a sufficient quantity of rays to pass to the retina, on which account she was short-sighted. Her seeing objects darkened on one side, might proceed from the artificial part of the pupil being situate nearer to the great canthus of the eye than usual in nature; by which the rays which fall on the side of the cornea, next to the little canthus of the eye, being partly intercepted, must occasion a defect in the picture; from which defect a darkness will be seen on one side of the object. To the weakness of the vessels of the eye we may attribute the pain of the eye on catching cold: it often happens to those who have had a severe ophthalmia, that during life the small vessels are too weak; and hence, from slight causes being distended, they will be painful and frequently red.

When her eye had little or no appearance of inflammation, he tried cold water, but with rather bad success. All cold applications to inflamed eyes, astringents or repellents, require the utmost caution in applying them; for if they produce not a good, they will produce a bad effect. In slight cases they often have very happy effects, but where the obstruent matter is so fixed that it will not suffer itself to be easily repelled back, the vessels being straitened, the fluids coagulated, the disease will be increased; which happened in this case from the application of cold water.

The good effects of evacuations are very evident in abating the inflammation: Wounds in the cornea, attended with a wound of the uvea, and a troublesome ophthalmy, heal without any scar.

Tables of Specific Gravities, extracted from various Authors, with some Observations on the same. By Richard Davies, M. D. N° 488, p. 416.

The ancients have left but few particulars concerning the different specific gravities of bodies, though it is plain they were in the general sufficiently acquainted with them. It was by the knowledge of the various weights of gold and silver, that Archimedes is recorded to have detected the noted fraud committed in Hiero's crown, as Vitruvius has at large related in his Architecture, l. ix, c. 13; and it is from the same great philosopher that we have derived the demonstration of those hydrostatical rules, by which the proportions are best to be known, of the several weights or densities of different bodies, having the same bulk or magnitude: as may be seen in his tract De Insidentibus Humido, lost in the Greek original, but retrieved in great measure, as it is said, from an Arabic translation. It was published in Latin, with a commentary by Frederic Commandine, at Bononia 1565, 4to, and the substance of it by Dr. Barrow in his Archimedes, printed likewise in 4to, at London 1675.

Pliny, in the 18th book of his Natural History, has set down the proportional weights of some sorts of grain, among which he says that barley is the lightest. And the same author, in his 33d book, speaking of quicksilver, observes that it is the heaviest of all substances, gold only excepted. Which Vitruvius had also taken notice of, and had mentioned besides the weight of a known measure of it, that of 4 Roman sextarii.

Again, Q. Rhemnius Fannius Palæmon, in his fragment De Ponderibus et Mensuris, has given an observation, of the proportional gravities of water, oil, and honey; stating that the sextarius of either water or wine weighed 20 oz. the same measure of oil 18, and of honey 30. Their specific weights were therefore in proportion as 1.0, 0.9, and 1.5, exactly agreeable to what Villalpandus determined about the beginning of the last century; yet was this author himself sensible that these were not to be considered as very nice experiments. After which he proceeds to describe a pretty good instrument for readily finding the different specific gravities of fluids; and shows how those of solids also may be hydrostatically discovered.

Francis Bacon, Lord Verulam, &c. in his Historia Densi et Rari, has given a table, which he calls, Tabula Coitionis et Expansionis Materiæ per Spatia in Tangibilibus (quæ scilicet dotantur pondere) cum Supputatione Rationum in Corporibus Diversis. This tract does not appear to have been published till after his death, which happened in the year 1626, but was probably written several years before, and the experiments were, as he tells us, even made long before that. Hanc tabulam multis abhinc annis confeci, atque ut memini, bona usus diligentia. It is probably therefore the oldest table of specific gravities now extant. The experiments there mentioned were not made hydrostatically, but with a cube of an ounce weight of pure gold, as he says, to which he caused cubes of other materials to be made equal in size: as he did also two hollow ones of silver, and of equal weights, the one to be weighed empty, and the other filled with such liquid as he wanted to examine. He was himself sensible that his experiments of this sort were, notwithstanding his care, very defective. From among these, notwithstanding their imperfection, as they appear to have been some of the first experiments of the sort regularly digested, and as they were besides made by so great a man, Dr. D. has extracted the specific gravities of the fixed metals, which he has inserted as examples in the following tables, after reducing them to the common form, on the supposition that pure gold was, according to Ghetaldus, just 19 times as heavy as water. And this he rather chose to do, than to make use of his Lordship's own weight of water given in the table, which in the manner he took it could not be very exact, and which besides would not have brought out the specific gravity of pure gold more than 18 times as much; and that of the other metals in proportion. This table contains in all 78 articles.

There are also in the 3d volume of the folio edition of his works, p. 223, Certain experiments made by the Lord Bacon about weight in air and water. These are truly hydrostatical, but very imperfect, Dr. D. has not therefore inserted any of them in the following collection.

Marinus Ghetaldus, a nobleman of Ragusa, published in 4to, at Rome, in 1603, his treatise entitled, Promotus Archimedes, seu de Variis Corporum Generibus Gravitate et Magnitudine Comparatis, where he has given a comparison between the specific gravities of water and eleven other different substances, from his own hydrostatical experiments, made with care and exactness. These are here inserted; expressing the numbers as they stand in his own book, but Dr. D. has afterwards also for uniformity reduced them to the decimal form. He has besides, at the end, transcribed at large the 2 tables of this author, in which every one of the 12 sorts of bodies, he treats about, is successively compared with all the others, both in weight and magnitude.

Father Johannes Baptista Villalpandus, a Jesuit of Cordova in Spain, in his Apparatus Urbis et Templi Hierosolymitani, printed in folio at Rome, in 1604, exhibited a table of the proportional weights of the 7 metals and some other substances, from his own experiments, made with great care, as he tells us, by the means of 6 equal solid cubes of the fixed metals, and a hollow cubical vessel 8 times as large, for the comparing mercury, honey, water, and oil with the same. His numbers, which are inserted under his name in the following tables, were also again published by Joh. Henr. Alsted, in his Encyclopædia Universa, printed in 2 vol. in folio, at Herborn, 1630, and by Henry Van Etten, in his Mathematical Recreations, whence they have been often transcribed into other books. Villalpandus's book, which is only the 3d volume of a work begun to be published several years before, was itself printed so soon after Ghetaldus's, that it is probable he either never saw that author, or not at least till after his own experiments were made.

Mr. Edmund Gunter, in his Description and Use of the Sector, printed after his death by Mr. Samuel Foster in 1626, having occasion to mention the specific weights of the several fixed metals, quoted Ghetaldus, and made use of his proportions, and so did also Mr. William Oughtred, in his Circles of Proportion, first published in 4to, 1633, with this only difference, as to the form, that he changed Ghetaldus's unit into 210, by which he expressed all his relations in whole numbers. It is likewise probable that D. Henrion took from the same place the numbers he applied in his Usage du Compas de Proportion, printed at Paris in 1631, 8vo; though he has not given them all with exactness, for the sake as it seems of using simpler vulgar fractions.

Father Mersenne, a French minim, in his Cogitata Physico Mathematica, printed at Paris in 1644, 4to, has given from the observations of his accurate friend Petre Petit, a table of the specific gravities of the metals and some other bodies, making gold 100, water 5¼, and the rest in proportion. These are here reduced to the common form, and inserted under his name in the following tables. The same were afterwards made use of by Father Francis Milliet de Chales, Jesuit, in his Cursus Mathematicus, Mons. Ozanam, Professor Wolfius, and several others. Dr. D. has not seen Petit's own book, but it was entitled L'Usage ou le Moyen de Pratiquer par une Regle toutes les Operations du Compas de Proportion—augmentées des Tables de la Pesanteur et Grandeur des Metaux, &c. had a privilege dated in 1625, though it is said not to have been printed till some years after. The same Father Mersenne has also taken notice, in his general preface, of a table of 20 specific gravities, some time before published by Mons. Aleaume, which he there sets down; but which he also observes are very incorrect. Dr. D. has not therefore inserted any of them in this collection.

Mr. Smethwick, one of the earliest members of the Royal Society, communicated to the same in July 1670, the weights of a cubic inch of several different substances, said to have been formerly taken by Mr. Reynolds in the Tower of London. This gentleman was the same who composed several tables relating to the price of gold and silver, which were published in a book entitled the Secrets of the Goldsmith's Art, at London, 1676, in 8vo. These weights are expressed in decimals of an avoirdupois pound, are carried to 8 places of figures, and seem to have been carefully and accurately collected. Dr. D. has therefore in the following tables reduced them to the common form, in order to give them their proper authority with the rest. He knows not whether these weights were ever before printed or not, neither can he give any account, after what particular manner the experiments were made, from which they were taken. They were communicated to him from the register books of the Royal Society; and he only observes, that the absolute weight here assigned of a cubic inch of common water, does not differ more than a small fraction of a grain, from the weight of the same afterwards determined by Mr. Ward of Chester.

The Philosophical Society, meeting at Oxford, directed several experiments to be made hydrostatically by their members, concerning the specific gravities of various bodies; which being digested into a table, were by Dr. Musgrave communicated to the Royal Society the 21st day of March 1684; soon after which they were printed in the 169th number of the Philosophical Transactions. These experiments were, according to Dr. Musgrave, made by Mr. Caswell and Mr. Walker; they are all originals, and are esteemed some of the most accurate that are extant.

The Hon. Robert Boyle, at the end of his Medicina Hydrostatica, first published at London in 1690, 8vo, subjoined a table of the specific gravities of several bodies, accurately taken from his own hydrostatical experiments. Besides which, there are also in the same tract, and in other parts of his works, several experiments of this excellent author's, which he has given occasionally, with the uses resulting from them. To such of these in the following collection, as were taken from the table just mentioned, his name is annexed; but to such of the others as occurred, Dr. D. has also added the volume, page, and column, of the late folio edition of his works in 1744, where the same are be be found. It may be noted, that in the first edition of the Medicina Hydrostatica, there were several errors of the press. Such of them as he could discover by calculation, he has corrected in the following pages.

There is a table published under the name of J. C. in the 199th number of the Philosophical Transactions, anno 1693: and this is evidently a supplement to that abovementioned of the Philosophical Society meeting at Oxford. The experiments were, according to the initials J. C. made by the same curious person Mr. John Caswell, and are therefore of the same estimation as the others.

M. Homberg, of the Royal Academy of Sciences at Paris, read a memoir in 1699, where he noticed the expansion of all substances by heat, and the contraction of the same by cold; whence it must follow, that the specific gravities of the same bodies would constantly be found less in the summer and greater in the winter. And this he showed from the experiments he had made on several fluids, both in the summer and the winter seasons, by means of an instrument he had contrived and called an aræometer, being a large phial, to which he had adjusted a long and slender stem, by which he could well determine when it was filled with equal bulks or quantities of the several fluids he proposed to examine. The result of his trials with this instrument he digested into a short table, which was printed in the memoirs of the Academy for the same year 1699. This table John Caspar Eisenschmid afterwards republished, with several additions, in his tract De Ponderibus et Mensuris, printed at Strasburg in 1708, 8vo, changing it to a more convenient form for his purpose, by reducing the different fluids to the known bulk of a cubical Paris inch. So much of this table as Dr. D.

thought might be of service, he has here subjoined to the others in the following collection, having also made an alteration in the form, the better to fit it for general use, by omitting the absolute weights of the several bodies in summer and winter, and placing instead of them, after the name of each body, a decimal number, expressing the proportion of its weight in winter to its weight in summer, supposed to be every where represented by unity.

Sir Isaac Newton, in his Optics, printed in 4to at London 1704, gave a table of the specific gravities of several diaphanous bodies. The experiments were made by him with a view chiefly to optical inquiries, and to enable him to compare their densities with their several refractive powers, we may therefore be well assured that they were made by the great author with the most scrupulous care and exactness. The table consists of 22 articles.

John Harris, D. D. in his Lexicon Technicum, first printed at London in 1704, fol. republished at large the several tables of specific gravities of the Oxford Society and I. C. from the Philosophical Transactions, and that of Mr. Boyle from his Medicina Hydrostatica, to which last he also added some experiments of his own, made as it seems with good accuracy. These are here extracted, and placed under his name in the following tables.

Mr. John Ward of Chester, in his Young Mathematician's Guide, first printed it seems in 1706, acquaints us, that he had himself for his own satisfaction, made several experiments on the different specific gravities of various bodies; and that he was of opinion, that he had obtained the proportion of the weight that one body bears to another of the same bulk and magnitude, as nicely as the nature of such matter, as might be contracted or brought into a less body, viz. either by drying, hammering, or otherwise, would admit of. And he has accordingly given, in the said book, the weight of a cubic inch of 24 different substances, both in Troy and Avoirdupois ounces and decimal parts of an ounce; which he further assures us required more charge, care, and trouble, to find out nicely, than he was at first aware of. This table appears to have been well-esteemed, and to have had the sanction of Mr. Cotes's approbation, by his taking it, when reduced to the common form, into that collection, which he drew up for his own hydrostatical lectures.

Roger Cotes, M. A. and Plumian professor of astronomy and experimental philosophy at Cambridge, first giving about the year 1707 a course of hydrostatical and pneumatical experiments, in conjunction with Mr. Whiston in that university, drew up, for the use of that course, a very accurate table of specific gravities, collecting from several places such experiments as he took to be most exact, and the best to be depended on. And as the judgment of so great a man cannot but give a general reputation to such experiments as he had so selected, Dr. D. in the following tables, distinguishes all such by the addition of the letter

C, after the names of such persons from whom they first appear to have been taken, adding also the name of Cotes at length, to such others as Dr. D. has not met with elsewhere, and which he therefore takes to have been transcribed from the memoranda of his own experiments. This table of Mr. Cotes's used first to be given in MS. to those who attended his lectures; but it was afterwards printed in a single sheet, relating to a course of experiments at Cambridge in 1720, and since in Mr. Cotes's Hydrostatical and Pneumatical Lectures, when they were published at large in 8vo by his successor Dr. Smith, afterwards master of Trinity College. In these printed lectures were inserted the gravities of human blood, its serum, &c. from Dr. Jurin, instead of those that had before been made use of from Mr. Boyle.

Mr. Francis Hauksbee, secretary to the Royal Society, did, about the year 1710, begin, in conjunction with Mr. Whiston, who had then newly left the university, to give hydrostatical lectures, &c. in London; for the purpose of which he reprinted in a thin volume in 4to, in which are the schemes of his experiments, Mr. Cotes's table of specific gravities abovementioned. To which he added, from trials of his own, the weights of steel, soft, hard, and tempered, which are printed with his name in the following tables, as are also some other experiments, which he has since occasionally made, and communicated to Dr. D. Mr. Cotes's table, with the abovementioned additions of Mr. Hauksbee, was afterwards again published by Dr. Shaw, in his abridgment of Mr. Boyle's Philosophical Works, at London, 1725, 4to, vol. ii, p. 345.

John Freind, M. D. at the end of his Prælectiones Chemicæ, printed at London in 1709, 8vo, has published some new tables of the specific gravities both of solid and fluid bodies, entirely taken from his own original experiments. And as these tables contain an account of a very useful set of bodies, on which few or no other experiments have been made, it is great pity that this truly learned and elegant writer was not more accurate in his trials than he appears to have been. Many of his experiments having indeed been made in so lax and improper a manner, and so many errors having been committed in them, that we cannot with security depend on these tables, though containing otherwise facts one would so much desire to be truly informed about. Dr. D. has however here inserted the several particulars of his last two tables, which immediately concern specific gravities, after correcting such errors in calculation as he could certainly come at.

James Jurin, M. D. and several years secretary to the Royal Society, gave, in N° 361 of the Philos. Trans. anno 1719, some original and very accurate experiments made by himself, on the specific gravity of human blood, at several times during the 6 preceding years. These were accompanied with a very curious discourse, which has since been translated by himself into Latin, and reprinted in his Dissertationes Physico Mathematicæ, Lond. 1732, 8vo. This gentleman has also, in N° 369 of the same Transactions, obliged us with some very judicious and useful remarks, relating to the caution to be used in examining the specific gravity of solids, by weighing them in water; for want of attending to which, several sorts of bodies, such as human calculi, the substance of all woods, &c. have appeared, from their pores and small cavities filled up with air, to be considerably lighter than they really are.

John Woodward, M. D. and professor of physic in Gresham College, had, as he acquaints us in several places of his works, made a great number of experiments on the specific weights of mineral and other fossil bodies, but which being probably contained in those of his papers which he ordered to be suppressed, are thus lost to the world, to which they would doubtless have been very acceptable. All that Dr. D. has been able to pick up, are a very few mentioned in the Catalogue of the English Fossils in his collection, published since his decease in 8vo, at London 1729.

Mr. Gabriel Fahrenheit, F.R.S. communicated, in N° 383 of the Philos. Trans. A Table of the Specific Gravities of 28 several Substances, from hydrostatical experiments of his own, made with great care and exactness; to which he subjoined some observations on the manner in which his trials were performed, with a description of the instruments in particular which he made use of to examine the gravities of fluids. To some of his experiments, which he thought required a greater nicety, he has affixed an asterisk in his table, signifying such to have been adjusted to the temperature of the air, when his thermometers stood at the height of 48 degrees. This gentleman, who is well known by the reputation of his mercurial thermometers, which he made with great accuracy, and which are now generally used, was in England in the year 1724.

Professor Peter Van Muschenbroek, of Utrecht, published in his Elementa Physicæ at Leyden in 8vo, 1734, a large table of specific gravities, which he afterwards somewhat further enlarged in his Essai de Physique, in French, at Leyden 1739, 4to. This table contains almost all the preceding ones, but without the names of the authors from whom they were collected. Dr. D. has among those which follow, inserted under this author's name, such experiments as he had not before met with elsewhere, making use of the Latin edition as the more correct, except in such articles as are only to be found in the French.

Mr. John Ellicott, F.R.S. having an opportunity in the year 1745 to examine the weight of some large diamonds, he accordingly, with the utmost care, and with exquisite assay scales, which very sensibly turned with the 200th part of a grain, took the specific gravities of 14 of those diamonds, 4 of which came from the Brasils, and the other 10 from the East Indies. These experiments he communicated to the president of the Royal Society, who caused them to be read at one of their meetings, and afterwards published them in N° 476 of the Philos. Trans. Among these Brasilian diamonds, one was of the absolute weight of 92,425, another of 88,21; and among the East-Indian ones, one of 92,525 Troy grains. And as the size of these stones made them much fitter for these inquiries, than any others which had probably ever before been used for the same purpose, so the known accuracy of the author, the goodness of his instruments, and the consistency of all his experiments, sufficiently show that the specific gravities he has delivered in his paper, may entirely be depended on. The same curious person also communicated the specific gravities of fine and standard gold, published under his name in the following tables, and which were deduced from experiments he was so kind as to make on purpose at Dr. D.'s request.

Having occasion to mention diamonds, Dr. D. observes, it may possibly not be foreign to the purpose here to take some notice of the diamond carat weight, used among jewellers, which weight was originally the carat or 144th part of the Venetian ounce, equal to 3.2 Troy grains, but which is now, for want of an acknowledged standard, somewhat degenerated from its first weight. He had himself found it, on a medium of several experiments, equal to 3.17 Troy grains; and he had the rather taken notice of this weight here, because there happens to be a mistake about it, both in Dr. Arbuthnot's and Mr. Dodson's tables, who have set down as it seems the number of diamond carats in a Troy ounce, instead of the weight of the diamond carat itself. This carat is again divided into 4 of its own grains, and those into halves and quarters, commonly called the 8ths and 16ths of a carat; and thus the largest of the diamonds just abovementioned, weighed, in the jewellers' phrase, better than 29 carats and almost half a grain.

Mr. James Dodson, in his book called The Calculator, printed in 8vo at London in 1747, has inserted a useful table of specific gravities, in which he has by the first initial letter of their names distinguished the several authors he has quoted; and among these, are several new experiments marked with an L, which it is said were communicated from his own trials, by Mr. Charles Labelye, engineer, and which concern particularly the weights of several sorts of stone and other materials used in building. These Dr. D. has also distinguished by an L, as they stand in Mr. Dodson's book.

Mr. Geo. Graham, F.R.S. made for Dr. D. at the request of a friend, some accurate trials on the weight, of gold and silver, both when reported fine, and when reduced to the English standard; all which he has inserted under his name in the following tables. Where he has also reported some other single experiments which he occasionally met with, from Frederic Slare, M.D. John Keill of Oxford, M.D. Stephen Hales, D.D. and Edward Bayley of Havant in Hampshire, M.D.

Richard Davies M.D. I have lastly to this collection of experiments added some of my own, which I endeavoured to make with as much accuracy as the instruments I was provided with would allow of. My hydrostatical balance was one constructed several years since by Mr. Francis Hauksbee, which I have constantly found to turn sensibly with half a grain: and the bodies on which I made most of my trials, were taken from a collection of the materia medica formerly made by Signor Vigani, and still preserved in the library of Queen's College in Cambridge.

TABLE I.—*Of Metals.*

☉ GOLD, fine. Ward, C.	19.640
A medal esteemed near fine gold. J. C.	19.636
Or d'essai, ou de Coupelle. Musch.	19.238
Fine gold hammered. Ellicot	19.207
D° an ingot, and refined with Antimony. Ellicot	19.184
D° that ingot itself. Ellicot	19.161
A medal of the R. S. esteemed fine gold. Graham	19.158
A gold medal of Queen Eliz. J. C.	19.125
D° of Queen Mary. J. C.	19.100
Aurum. Fahrenheit	19.081
Id. Ghetaldus. Aurum purum. Bacon (ex hyp.)	19.000
A gold coin of Alexander's. J. C.	18.893
Gold. Reynolds	18.806
Aurum. Villalpandus. Petit.	18.750
Standard gold (by which is understood gold of 22 carats, or such of which our guineas are intended to be coined). J. C. Ward. C.	18.888
An old Jacobus. Probably the sceptered broad piece. Harris	18.375
A Mentz gold ducat. J. C.	18.261
Aureus Ludovicus. Muschenbr.	18.166
A 5-guinea piece of K. James II. 1687, with an elephant. Graham	17.933
A Portugal piece of 3l. 12s. 1731, supposed to be nearly the same as standard. Graham	17.854
Guineas, 10 weighed together. Davies	17.800
D° on a mean of 7 trials on those of different reigns. Ellicot	17.726
A piece of gold coin of the commonwealth. Harris	17.625
Guineas, 2 new ones. Hauksbee	17.414
A grain of Scotch gold, such as nature had made it. Boyle V. 30, *b*. 12 2/7	12.286
Electrum, a British Coin. J. C.	12.071
☿ QUICKSILVER. Mercurius crudus. Freind	14.117
Mercury Spanish. Boyle V. 10, *b*.	
Mercury sublimed 11 times. Musch.	14.110
Quicksilver. Oxford Soc.	14.019
D° Ward. C. revived from the ore. Boyle	14.000
Fine mercury. L.	13.943
Quicksilver, another parcel. Oxf. Soc.	13.593
Mercury amalgamated with silver, refined and sublimed 100 times. Muschenb.	13.580
Mercurius. Fahrenheit.	*13.575
Argentum vivum. Ghetaldus. 13 4/7	13.571
Mercure amalgamé avec de l'or, affiné et sublimé 100 fois; le même mêlé avec du plomb, ensuite converti en poudre et revivifié. Musch.	13.550
Coarse mercury. L.	13.512
Mercurius. Petit.	13.406
Quicksilver. Reynolds	13.147
♄ LEAD. Reynolds.	11.856
Plumbum. Villalpand	11.650
Id. Ghetaldus 11½	11.500
Id. Bacon	11.469
Lead. Harris	11.420
Hardest lead. L.	11.356
Plumbum. Fahrenheit.	11.350
Lead. Oxford Soc. Ward	11.345
Plumbum. Petit.	11.343
Lead. Harris. (an ordinary piece).	11.330
D° Cotes	11.325
Plumbum Germanicum. Musch.	11.310
Cast lead. L.	11.200
☽ SILVER, fine. Ward. C.	11.091
A medal of the R. S. reported fine silver. Graham	10.484
Argentum. Fahrenheit	10.481
Silver. Reynolds	10.432
Argentum. Villalpandus	10.400
Id. Ghetaldus. 10⅓	10.333
Id. Bacon	10.331
Id. Petit.	10.219
Sterling or standard silver (that is, silver 11 oz. 2 dwt. in the pound fine) A half-crown of K. William's coin. Harris	10.750
D° struck into money. L.	10.629
D° J. C. Ward. C.	10.535
D° Cast. L.	10.520
A new crown-piece, 1746. Lima under the head, Graham	10.284

♀ COPPER. Reynolds	9.127
Cuprum. Villalpandus	9.100
Æs. Ghetaldus. Rose-copper. Ward. C. Fine copper. L. An old copper halfpenny, Charles 2d's coin. Harris	9.000
Copper in halfpence. L.	8.915
Æs; cuivre. Petit	8.875
Cuprum. Bacon	8.866
Copper. Oxford Soc.	8.843
Cuprum Suecicum. Fahrenheit	8.834
Id. Japonense. Fahrenheit	8.799
Id. Suecicum. Muschenbr.	8.784
Common copper. L.	8.478
BRASS. An old brass gold weight marked XXXIII. Harris	8.830
Aurichalcum. Bacon	8.747
A piece of hammered brass. Harris	8.660
Æs, airain, calaminæ mixtum. Petit.	8.437
Aurichalcum. Fahrenheit	8.412
Brass hammered. J. C. Plate brass. Ward	8.349
Wrought brass. J. C.	8.280
Cast brass. L.	8.208
D° J. C. Ward	8.100
D° Cotes	8.000
Brass hammered. Reynolds	7.950
D° cast. Reynolds	7.905
A piece of cast brass. Harris	7.666
♂ IRON. Ferrum. Villalpandus	8.086
Id. Ghetaldus	8.000

Iron, forged. Reynolds	7.906
Ferrum. Petit	7.875
Id. Bacon	7.837
Spanish bar-iron. L.	7.827
Swedish D°. L.	7.818
Ferrum. Fahrenheit	7.817
Iron. Cotes	7.645
D° of a key. J. C. Com. iron. Ward	7.643
A piece of hammered iron, perhaps part steel. Harris	7.600
Iron cast. Reynolds	7.520
D° cast. L.	7.135
Softest cast iron or Dutch plates. L.	6.960
STEEL. J. C. Ward	7.852
D° Cotes	7.850
.. Spring temper. Hauksbee	7.809
.. Nealed soft. L.	7.792
.. Soft. Hauksbee	7.738
.. Hard. Hauksbee	7.704
.. Hardened. L.	7.696
♃ TIN. Reynolds	7.617
Stannum. Bacon	7.520
Id. Villalpandus. Freind	7.500
Etain d'Angleterre. Muschenbr.	7.471
Stannum. Ghetaldus. 7⅖	7.400
Id. Provinciæ Indiæ or Malacca. Fahren.	7.364
Block tin. Oxf. Soc. Ward. C.	7.321
Stannum Anglicanum. Fahrenheit	7.313
Id. commune. Petit	7.312
Id. purum. Petit	7.170
Block or Grain tin. L.	7.156

TABLE II.—*Of Minerals, Semimetals, Ores, Preparations and Recrements of Metals, &c.*

BISMUTH. J. C.	9.859
D° Cotes	9.700
D° or Tinglass. Boyle	9.550
Tinglass. Reynolds	7.951
Marcasita alba. Fahrenheit	9.850
Mineral, Cornish, shining like a marcasite. Boyle	9.060
Calx of lead. Boyle	8.940
Spelter solder. J. C.	8.362
Spelter. J. C.	7.065
Cinnabar common. Boyle	8.020
Cinnabaris factitia. Muschenb. (if not a mistake for the last experiment)	8.200
Cinnabar native, breaking in polished surfaces like talc. Davies	7.710
D° Persian, breaking rough. Davies	7.600
D° native. Boyle	7.576
Cinnabaris nativa. Muschenb.	7.300
Cinnabar native, very sparkling. Boyle	7.060
D° native from Guinea. Davies	6.280
Cinnabar of antimony. Harris	7.060
D° another piece. Harris	7.043
D° Boyle	7.030
Cinnabar antimonii. Freind	6.666
Cinnabre d'antimoine. Muschenb.	6.044

Lead ore, rich, from Cumberland. Boyle	7.540
D° Boyle	7.140
The reputed silver ore of Wales. J. C.	7.464
The metal thence extracted. J. C.	11.087
Regulus antimonii. Item Martis et Veneris. Freind	7.500
Id. Fahrenheit	6.622
Id. Harris	6.600
Id. per se. Davies	4.500
Silver ore, choice. Boyle	7.000
D° another piece from Saxony. Boyle	4.970
Lithargyrus argenti. Freind	6.666
Lithargyrium argenti. Muschenb.	6.044
Id. Auri. Freind	6.316
Id. Auri. Muschenb.	6.000
Minera antimonii. Davies.	5.810
Cuprum calcinatum. Freind	5.454
Glass of antimony. Newton. C.	5.280
Vitrum Antimonii. Freind	5.000
Id. per se. Boyle	4.760
Tin ore, choice. Boyle	5.000
D° black, rich. Boyle	4.180
New English tin ore, Mr. Hubert's. Boyle	4.080
Tutty, a piece. Boyle	5.000

Tutia. Muschenb.	4.615
Lapis Calaminaris. Freind. Lapis cæruleus Namurcensis. Muschenb.	5.000
Id. Boyle	4.920
Loadstone. Boyle V. 6, b.	4.930
Magnes. Petit.	4.875
A good loadstone. Harris	4.750
Marcasites, one more shining than ordinary. Boyle	4.780
A golden marcasite. J. C.	4.589
Marcasites from Stalbridge. Boyle	4.500
D° Boyle	4.450
Antimonium Hungaricum. Muschenbr.	4.700
Antimony, good, and supposed to be Hungarian. Boyle	4.070
D° crude, which seemed to be very good. Harris	4.058
Antimonium crudum. Freind	4.000
Id. Davies	3.960
Black sand, commonly used on writing. Boyle. V. 33, *b*.	4.600
Crocus metallorum. Muschenb.	4.500
Id. Freind	4.444
Hæmatites. Muschenb.	4.360
Id. Boyle. V. 6, *a*.	4.150
D° English. Boyle	3.760
Copper ore, rich. Boyle	4.170
D° Boyle	4.150
Copper-stone. Boyle	4.090
Emeri. Boyle. V. 26, *b*.	4.000
Manganese. Boyle	3.530
A blue slate with shining particles. J. C.	3.500
Iron ore, a piece burnt or roasted. Harris	3.333
Cerussa. Item chalybs cum sulphure. pp. Freind.	3.158
Lapis lazuli. J. C.	3.054
D° Boyle. V. 6, *b*.	3.000
D° Boyle	2.980
Gold ore. Boyle. V. 29, *b*.	2.910
D° not rich, brought from the East Indies. Boyle	2.652
Another lump of the same. Boyle	2.634
A mineral stone, yielding 1 part in 160 metal. J. C.	2.650
The metal thence extracted. J. C.	8.500
Pyrites homogenea. Fahrenheit	2.584
Black lead. Boyle. V. 27, *a*.	1.860
Æs viride. Freind	1.714
Plumbum ustum. Freind	1.666

TABLE III.—*Of Gems, Crystals, Glass, and Transparent Stones.*

GRANATE, Bohemian. Boyle	4.360
Granate. J. C.	3.978
Granati minera. Boyle	3.100
A pseudo topazius, being a natural pellucid, brittle, hairy stone, of a yellow colour. Newton. C.	4.270
Sapphires. Davies	4.090
A sapphire very perfect, but rather pale. Hauksbee	4.068
Glass, blue in sticks from Mr. Seale. Hauksbee	3.885
D° whitest, from Mr. Seale. Hauksbee	3.380
.. clear crystal. Cotes	3.150
.. blue plate, old. Hauksbee	3.102
.. plate. I.	2.942
.. old looking-glass plate of a light colour. Hauksbee	2.888
.. green. Freind	2.857
.. green bottle. Hauksbee	2.746
.. of a bottle. Oxf. Soc. It. a blue paste. Hauksbee	2.666
.. common green. Hauksbee	2.620
.. deep green old. Hauksbee	2.587
.. vulgar. Newton. Ward	2.580
Vitrum venetum. Freind	1.791
An oriental cat's-eye, very perfect. Hauksb.	3.703
A diamond, yellow, of a fine water, somewhat paler than the jonquille. Hauksb.	3.666
D° white of the second water, eau celeste. Hauksbee	3.540
A diamond, East Indian, the heaviest of many. Ellicot	3.525
.. the lightest of many. Ellicot	3.512
.. Brasilian, the heaviest of many. Ellicot	3.521
.. the lightest of many. Ellicot	3.501
.. the mean of all his experiments. Ellic.	3.517
.. Newton. C.	3.400
Diamond bort, of a bluish black, with some little adhering foulness. Hauksb.	3.495
A jacinth of a fine colour, but somewhat foul. Hauksbee	3.637
A chrysolite. Hauksbee	3.360
Crystal cubic, supposed to contain lead. Woodward	3.100
Crystal from Castleton in Derbyshire, having the double refraction. Hauksb.	2.724
Crystal of Island. Newton. C.	2.720
Crystallum disdiaclasticum. J. C.	2.704
Crystallus de rupe. Fahrenheit	2.669
Crystal rock. J. C. Boyle III. 229, *b*.	2.659
D° a large shoot. Hauksbee	2.658
.. of the rock. Newton. C.	Id.
Crystal in the lead mines near Worksworth. Woodward	2.650
.. Hauksbee	2.646
.. Pure pyramidal, supposed to contain tin. Woodward 2.5, or	2.400
Crystallus. Petit	2.287
Crystal. Boyle	2.210
Talc, Jamaican. Boyle	3.000

Talc, Venetian. Boyle	2.730
D° J. C.	2.657
.. English. Woodward	2.600
.. a piece like lapis Amianthus. Boyle	2.280
A red paste. J. C.	2.842
A Brasil pebble, foul and feathered. Hauks.	2.755
D° a fragment uncut. Hauksbee	2.676
.. cut. Hauksbee	2.591
Jasper, spurious. J. C.	2.666
A Cornish diamond cut. Hauksbee	2.658
A water topaz, very perfect, but said not to be oriental. Hauksbee	2.653
Pebble, pellucid. J. C.	2.641
Bristol stone. Davies	2.640
Hyacinth, spurious. J. C.	2.631
Selenites. J. C.	2.322
D° Newton	2.252

TABLE IV.—*Of Stones and Earths.*

Sardachates. J. C.	3.598
Lapis scissilis cæruleus. Muschenb. (qu. if not the same experiment mentioned before in tab. 2, a blue slate with shining particles. J. C.)	3.500
Cornelian. Boyle	3.290
D° J. C.	2.563
A hone. J. C.	3.288
D° to set razors on. Harris	2.960
Marmor. Petit. (probably some mistake in the experiment.)	3.937
Marble. Reynolds	3.026
D° white. Hauksbee	2.765
.. white Italian	2.718
.. white. Boyle. fine. Ward. C.	2.710
.. white Italian. Oxford Soc.	2.707
.. black Italian. Oxf. Soc. veined L.	2.704
.. black. Hauksbee	2.683
.. Parian. L.	2.560
Lapis Amianthus, from Wales. J. C.	2.913
Turquoise, one of the old rock, very perfect. Hauksbee	2.908
Turcoise stone. J. C.	2.508
Lapis Nephriticus. J. C.	2.894
Corallium rubrum. Freind	2.857
Corall. J. C.	2.689
D° red. Boyle V. 7. *a*	2.680
.. Boyle	2.630
.. white, a fine piece. Boyle	2.570
.. white, another piece. Boyle	2.540
Emeril stone, a solid piece. Hauksbee	2.766
Paving stone. Reynolds	2.708
D° a hard sort from about Blaiden. Oxf. Soc.	2.460
A whetstone, not fine, such as cutlers use. Harris	2.740
Pellets, vulgarly called alleys, which boys play with. Hauksbee	2.711
English pebble. L.	2.696
Lapis Judaicus. Boyle	2.690
Id. Freind	2.500
Maidstone rubble. L	2.666
Marbles, vulgarly so called, which boys play with. Hauksbee	2.658
Moor-stone. L.	2.656
Agate. Boyle	2.640
D° German, for the lock of a gun. Hauksbee	2.628
D° English. J. C.	2.512
Lapis Petit.	2.625
Flint, black, from the Thames. Hauksb.	2.623
Flint Stone. L.	2.621
A round pebble stone within a flint. Harris	2.610
East Indian, blackish. Item, an English one. Boyle. III. 243, *a*	2.600
D° Oxford Soc.	2.542
Corallachates. J. C.	2.605
Purbeck stone. L.	2.601
Freestone. Reynolds	2.584
Portland stone. L.	2.570
D° white for carving. L.	2.312
Grammatias lapis. J. C.	2.515
Onyx stone. J. C.	2.510
Slate, Irish. Boyle. Lapis Hibern. Davies.	2.490
Wood petrified in Lough Neagh. J. C.	2.341
Osteocolla. Boyle	2.240
Heddington stone. L.	2.204
Alum stone. Boyle	2.180
Bolus Armena. Freind	2.137
Hatton stone. L.	2.056
Burford stone. Oxford Soc.	2.049
Heddington stone, soft. Oxford Soc.	2.029
Terra Lemnia. Freind	2.000
Brick. Cotes	2.000
D° Oxford Soc.	1.979
A gallypot. J. C.	1.928
Alabaster. Ward. C.	1.874
D° Oxford Soc.	1.872
A spotted factitious marble. J. C.	1.822
Stone Bottle. Oxford Soc.	1.777
A piece of glass (perhaps glazed) coffee-dish of a brown colour. Harris	1.766
Barrel clay. L.	1.712
Lapis de Goa. Davies	1.710
Lapis ruffus Bremensis. Muschenb.	1.666
An icicle broken from a grotto (probably stalactites), Dr. Slare, in Harris	1.190
Chalk, as found by Dr. Slare. Harris	1.079

TABLE V.—*Of Sulphurs and Bitumens.*

SULPHUR. Petit	2.344
D°. a piece of roll. Hauksbee	2.010
.. vive. Boyle	2.000
.. German, very fine. Boyle	1.980
.. transparent, Persian. Davies	1.950
Sulphur minerale. Freind	1.875
Brimstone, such as commonly sold, J.C.	1.811
D°. Cotes	1.800
Asphaltum. Boyle, III 243, *a*	1.400
Scotch coal. Boyle, III 243, *a*	1.300
Coal, of Newcastle. L.	1 270
Coal, pit, of Staffordshire. Oxf. Soc.	1.240
Jet. J. C.	1.238
D°. Davies	[illegible]
.. Davies	1.020
Succinum citrinum. Davies	1.110
Id. pingue. J. C.	1.087
Id. flavum. Davies	1.080
Id. pellucidum. J. C.	1.065
Id. album, item pingue. Davies	1.060
Amber. Boyle. Newton. C.	1.040
Fine gunpowder. Reynolds	0.698

TABLE VI.—*Of Gums, Resins, &c.*

GUM Arabic. Freind	1.430
D°. Newton. C.	1.375
Opium. Freind	1.360
Gum tragacanth. Freind	1.330
Myrrh. Freind	1.250
Gum guaiac. Freind	1.224
Resina Scammonii. Freind	1.200
Aloes. J. C. (qu. whether the resin or the wood)	1.177
Asafætida, a very fine sample. Hauksbee	1.251
D° from Dr. John Keill's Introd. ad veram Physicam	1.143
Pitch. Oxf. Soc. C.	1.150
Thus. Freind	1.071
Camphor. Newton. C.	0.996
Bees-wax. Cotes	0.955
Cera. Ghetaldus	0.954
Wax well freed from the honey. Davies	0.938
Cera. Petit	0.937
D° the same lump 2 years after. Davies	0.942
Balsamus de Tolu. Muschenbroek	0.896
Mastic. J. C. (qu. whether the gum or the wood)	0.849

TABLE VII.—*Of Woods, Barks, &c.*

Coco shell. Boyle	1.345
Bois de Gayac. Muschenbroek	1.337
Lignum guaiacum. Freind	1.333
Lignum vitæ. Oxf. Soc.	1.327
Speckled wood of Virginia. Oxf. Soc.	1.313
Cortex guaiaci. Freind	1.250
Lignum nephriticum. Freind	1.200
Lignum asphaltum. J. C.	1.179
Ebony. J. C. Item aloes. J. C.	1.177
Santalum rubrum. J. C.	1.128
Id. album. J. C.	1.041
Id. citrinum. J. C.	0.809
Lignum rhodium. J. C.	1.125
Radix chinæ. Freind	1.071
Dry mahogany. L.	1.063
Gallæ. Freind	1.034
Red wood. Oxf. Soc. It. box wood. Oxf. Soc. Ward. C.	1.031
Log wood. Oxf. Soc.	0.913
Oak, dry. close texture. Oxf. Soc.	0.932
D° tried another time. Oxf. Soc.	0.929
.. sound, dry. Ward	0.927
.. dry. Cotes	0.925
.. dry, English. L.	0 905
Oak of the outside sappy part, felled a year. Oxf. Soc.	0.870
D°. Reynolds	0.801
D° very dry, almost worm-eaten. Oxf. Soc.	0.753
Dry wainscot. L.	0.747
Beech meanly dry. Oxf. Soc.	0.854
Mastic (qu. if the wood or gum). J. C.	0.849
Ash, dry, about the heart. Oxf. Soc.	0.845
D° dry. Cotes	0.800
.. meanly dry, and of the outside lax part of the tree. Oxf. Soc.	0.734
Elm dry. L.	0.800
D°. Reynolds	0.768
.. Oxf. Soc. C.	0.600
Rad. gentianæ. Freind	0.300
Cortex Peruvianus. Freind	0.734
Crabtree, meanly dry. Oxf. Soc.	0.765
Yew, of a knot or root 16 years old. Oxf. Soc.	0.760
Maple dry. Oxf. Soc. C.	0.755
Plumtree dry. J. C.	0.663
Fir, dry yellow. L.	0.657
Dry white deal. L.	0.569
Lignum abietin. Freind	0.555
Fir, dry. Cotes	0.550
D°. Oxf. Soc	0.546
Walnut tree, dry. Oxf. Soc.	0.691
Cedar, dry. Oxf. Soc.	0.613
Juniper wood, dry. J. C.	0.556

Sassafras wood. J. C.	0.482
Cork. Cotes	0.240
Cork. J. C.	0.237

TABLE VIII.—*Of Animal Parts.*

MANATI lapis. Boyle	2.860
D° another. Boyle	2.330
.. a fragment of. Boyle	2.290
.. J. C. another from Jamaica. Boyle	2.270
Pearl, very fine seed, orien. Boyle, V. 12, *a*	2.750
D° a large one, weighing 206 grains. Boyle V. 7, *b*	2.510
Murex shell. J. C.	2.590
Crabs' eyes, artificial. Boyle	2.480
D° native. Boyle	1.890
Os ovinum recens. Freind	2.222
Oyster shell. J. C.	2.092
Calculus humanus just voided. Davies	2.000
D°. Boyle V, 7, *b*	1.760
.. Boyle	1.720
.. Cotes	1.700
.. Boyle V. 7, *b*	1.690
.. J. C.	1.664
.. Davies	1.650
.. Boyle	1.470
.. J. C.	1.433
.. Davies	1.330
.. J. C.	1.240
Rhinoceros horn. Boyle	1.990
The top part of one. J. C.	1.242
Ebur. Freind	1.935
Ivory. Boyle	1.917
D° dry. Oxford Soc. C.	1.826
.. Ward	1.823
Unicorn's horn, a piece. Boyle	1.910
Cornu cervi. Freind	1.875
Ox's horn, the top part. J. C.	1.840
Blade bone of an ox. J. C.	1.656
A stone of the bezoar kind found with 4 others in the intestines of a mare. Edw. Bailey, M. D. of Havant in Hampshire. See Philos. Trans. N° 481	1.700
Bezoar stone. Boyle	1.640
D° a large one. Davies	1.570
.. being the kernel of another. Boyle V. 8, *a*	1.550
.. a fine oriental one. Boyle	1.530
.. Davies	1.504
.. Cotes	1.500
.. Boyle	1.480
.. Boyle	1.340
A stone from the gall-bladder. Hales	1.220
Blood human, the globules of it. Jurin by calculation	1.126
D° the crassamentum of. Jurin from experiments	1.086
.. Davies	1.084
.. from another experiment. Jurin	1.082
Sanguinis humani cuticula alba. Davies	1.056
Human blood when cold. Jurin	1.055
The same as running immediately from the vein. Jurin	1.053
The serum of human blood. Jurin	1.030
D° Davies	1.026
Ichthyocolla. Freind	1.111
A hen's egg. Davies	1.090
Milk. J. C. C.	1.031
Lac caprinum. Muschenbroek	1.009
Lac. Freind	0.960
Urine. J. C. C.	1.030
Id. Freind	1.012

TABLE IX.—*Of Salts.*

MERCURIUS dulcis bis sublim. Musch.	12.353
Mercurius dulcis. Freind	11.715
Id. ter sublim. Muschenbroek	9.882
Id. tertio sublim. Id. panacea rubra. Fr.	9.372
Id. quater sublim. Musch. Item Turpethum minerale	8.235
Id. 4to sublim. It. Turp. mineral. Frei.	7.810
Sublimat. corrosiv. Muschenbroek	8.000
Id. Freind	6.045
Cinis clavellatus, sordibus saleque suo neutro quodam (quod fere semper magis vel minus in cinere illo reperitur) depurgatus. Fahrenheit	3.112
Sal illud neutrum. Fahrenheit	2.642
Saccharum saturni. It. sal nitri fix. Musc.	2.745
Eadem. Freind	2.600
Magisterium coralli. Item pulvis sympatheticus. Freind	2.231
Tartarum vitriolatum. Muschenbroek	2.298
Id. Freind	2.186
Sal mirabile glauberi. Muschenbroek	2.246
Id. Freind	2.132
Tartarum emeticum. Muschenbroek	2.246
Id. Freind	2.077
Sal gemmæ. Newton. C.	2.143
Nitrum. Fahrenheit	2.150
Nitre. Newton. C.	1.900
Id. Freind	1.671
Sal guaiaci. Item sal enixum. Item sal prunellæ. Item S. polychrest. Musch.	2.148
Eadem omnia. Freind	2.030
Sal maritimum. Fahrenheit	2.125
Cremor tartari. Item vitriol. alb. Item vitriol. rubefact. Item s. vitriol. Musch.	1.900
Cremor tart. Item vitriol. alb. Freind	1.796
Vitriol English, a very fine piece. Boyle	1.880

Vitriol Dantzic. Newton. C.	1.715
Alumen. Fahrenheit	1.738
Alum. Newton	1.714
Sal chalybis. Freind	1.733
Borax. J. C.	1.720
D° Newton. C.	1.714
Vitriolum viride. Item calcanth. rubefact. Item s. vitriol. alb. Freind	1.671
Saccharum albiss. Fahrenheit	1.606½
Mel. Villalpandus	1.500
Id. Ghetaldus. Honey. Cotes.	1.450
Sal volatile cornu cervi. Muschenbroek	1.496
Id. Freind	1.421
Sal ammoniac. purum. Item ens martis semel sublimat. Muschenbroek	1.453
Eadem. Freind	1.374
Ens martis ter sublimat. Muschenbroek	1.269
Id. Freind	1.233

TABLE X.—*Of Fluids.*

MERCURY. Ward. C.	14.000
Oleum vitrioli. Fahrenheit	1.8775
Oil of vitriol. Newton. C.	1.700
Spiritus nitri hermeticus. Freind	1.760
Id. Muschenbroek	1.610
Lixivium cineris clavellati, sale quantum fieri potuit impregnatum. Fahrenheit	1.5713
Id. alio tempore præparatum. Fahren.	1.5634
Oil of tartar. Cotes. Ol. tartari per deliquium. Muschenbroek	1.550
Spiritus nitri, cum ol. vitrioli. Freind	1.440
Id. Muschenbroek	1.338
Spiritus nitri communis. Item, bezoardicus. Freind	1.410
Spirit of nitre. Cotes. Item spirit. nitri bezoardicus. Muschenbroek	1.315
Spirit nitri. Fahrenheit	*1.2935
Spirit nitri dulcis. Muschenbroek	1.000
Aquafortis melioris notæ. Fahrenheit	*1.409
Eadem, duplex. Freind	1.340
Aquafortis. Cotes	1.300
Eadem, simplex. Freind	1.100
Solutio salis comm. in aqua saturata. Da.	1.244
Eadem. Davies	1.240
Eadem 1 in aquæ 3 part. Davies	1.217
Eadem, 1 in aquæ 3 part. Freind	1.146
Eadem, 1 in aquæ 12 part. Davies	1.050
Soap lees the strongest. Jurin	1.200
D° capital. Jurin	1.167
Spirit of vitriol. Freind	1.200
Spiritus salis cum ol. vitriol. Muschenb.	1.154
Idem, &c. Freind	1.146
Spirit of salt. Cotes. Sp. salis marini. Muschenbroek	1.130
Sp. salis communis. Freind	1.037
Sp. salis dulcis. Muschenbroek	0.951
Id. Freind	0.890
Sp. salis ammoniaci succinat. Item, cum ciner. clevellat. Freind	1.120
Sp. salis ammoniac. cum calce. Musc.	0.952
Idem cum calce viva. Freind	0.890
Sp. cornu cervi non rectific. Freind	1.073
Sp. serici. Muschenbroek	1.145
Sp. urinæ. Cotes	1.120
Solutio salis enixi, 1 in aquæ 5 part. Fr.	1.100
Oleum sassafras. Muschenbroek	1.094
Decoctio gentianæ. Freind	1.080
Sp. tartari. Freind. Muschenbroek	1.073
Decoctio bistortæ. Freind	1.073
Decoctio sarzæ. It. Chinæ. Freind	1.049
Decoctio ari. It. Sp. salis comm. Freind	1.037
Oleum cinnamomi. Muschenbroek	1.035
Ol. caryophyllorum. Muschenbroek	1.034
Beer vinegar. Oxford Soc.	1.034
Acetum vini. Muschenbroek	1.011
Id. distillatum. Muschenbroek	0.994
Acetum. Freind	0.976
Sack. Oxford Soc.	1.033
Sp. ambræ. Muschenbroek	1.031
Sea-water. Cotes	1.030
D° settled clear. Oxford Soc. Ward	1.027
College plain ale. Oxford Soc.	1.028
Solutio aluminis, 1 in aquæ 5.33 part. Item solutio sal. amm. purif. 1, et vitriol. alb. 1, in aquæ 5 part. Freind	1.024
Laudanum liquidum Sydenhami. It. panacea opii. Freind	1.024
Decoctio cort. Peruv. Item, granatorum. Freind	1.024
Moll cyder, not clear. Oxford Soc.	1.017
Aqua fluviatilis. Muschenbroek	1.009
Tinctura aloes cum aqua. Item, decoctio santali rubri. Freind	1.000
Rain water. Newton, Reynolds. Common water. Cotes. Common clear water. Ward. Pump water. Oxford Soc. J. C. Aqua. Ghetaldus. Aqua pluviatilis. Fahrenheit, Muschenb. &c.	1.000
Aqua vel vinum. Villalpandus	1.000
Aqua putealis. Muschenbroek	0.999
Oleum fœniculi. Muschenbroek	0.997
Oleum anethi. Muschenbroek	0.994
Aqua distillata. Muschenbroek	0.993
Wine, claret. Oxford Soc.	0.993
D° red. Ward	0.992
Vinum. Petit	0.984
Id. Ghetaldus	0.983
Id. Burgundicum. Muschenbroek	0.953
Oleum sabinæ. Item hyssopi. Muschenb.	0.986
Ol. ambræ. Item pulegii. Muschenbroek	0.978
Ol. menthæ. Item cumini. Muschenb.	0.975
Decoctio sabinæ. Freind	0.960
Infusio marrubii. Item menthæ. Item absynth. Freind	0.950

Ol. nucis moschatæ. Muschenbroek	0.948
Ol. tanaceti. Muschenbroek	0.946
Ol. origani. Item carvi. Muschenbroek	0.940
Elixir propr. cum sale volat Item infusio theæ. Freind	0.940
Ol. spicæ. Muschenbroek	0.936
Ol. rorismarini. Muschenbroek	0.934
Linseed oil. Newton. C.	0.932
D° Ward	0.931
Spirits of wine, proof, or brandy. Ward	0.927
Sp. of wine well rectified. Newton. C.	0.866
Alcohol vini Fahrenheit	0.826
Id. magis dephlegmatum. Fahrenheit	0.825
Sp. vini. Freind	0.818
Id. rectific. Freind	0.781
Esprit de vin etherè. Muschenbroek	0.732
Spiritus croci. Freind	0.925
Lamp oil. Reynolds	0.924
Oleum. Ghetaldus	0.916
Oil olive. Newton. C.	0.913
D°. Ward	0.912
Sallad oil. Reynolds	0.904
Oleum. Villalpandus	0.900
Id. Petit	0.891
Ol. raparum. Fahrenheit	0.913
Id. it. tinct. chalyb. mynsicht. It. tinct. sulphur cum sp. terebinth. Freind. It. huile de semences de navets. Mus.	0.853
Sp. mellis. Muschenbroek	0.895
Sp. salis ammoniaci cum calce viva	0.890
Oleum aurantiorum. Muschenbroek	0.888
Spirit of turpentine. Newton. C.	0.874
Tinct. castorei. Item sp. vini camphorat Freind	0.870
Oil of turpentine. Boyle V. 22, *a*	0.864
Ol. terebinth. Freind	0.793
Ol. ceræ. Muschenbroek	0.831
Tinctura corallii. Freind	0.828
Aqua cocta. Freind	0.750
Air. Newton. C.	0.00125
Aer Princip. edit. 3. p. 512. Aer juxta superficiem terræ occupat quasi spatium 850 partibus majus quam aqua ejusdem ponderis	0.00118
The same, by an experiment made by the late Mr. Fr. Hauksbee, F.R.S. when the barometer stood at 29.7 inches. See Physico Mathem. Exp. p. 74.	0.00113

As to the absolute weight of water, with which all the other bodies are compared in these tables, Mr. Boyle tells us, in his Medicina Hydrostatica, that he had found by his own experiments, that a cubic inch of clear water weighed 256 Troy grains. And Mr. Ward of Chester, who afterwards pursued this affair with great accuracy, determined that a cubic inch of common clear water weighed by his trials 253.18 like Troy grains, or 0.527458 decimals of the Troy ounce, or 0.578697 of the ounce avoirdupois, agreeable to what Mr. Reynolds had formerly delivered, who found the cubic inch of rain water to weigh by his experiments 0.579036 decimals of the same avoirdupois ounce, differing from the other only 0.000339 parts.

The heaviest fluid we are acquainted with, next to mercury, is oil of vitriol, or water impregnated with the vitriolic acid in the highest degree we can obtain it, being almost double the weight of water. The next is probably the saturated solution of the fixed salt of vegetables; being a ponderous salt, and dissolving freely in water. The next to this is spirit of nitre. Spirit of salt is lighter, and inferior in weight to the saturated solution of salt itself. It is observable, that marine or common salt, and nitre, differ little in gravity, contrary to the nature of their spirits.

The several solutions of common salt, if accurately repeated, would show in what proportion the gravities of fluids increase, on the addition of salt; and that sea water does not contain $\frac{1}{24}$ part of salt.

Since the density of the air is as the force by which it is compressed, it follows that the weight of any portion of air must vary in the same proportion with the weight of the whole atmosphere: which in our climate is not less than $\frac{1}{16}$ of the whole weight, allowing the barometer to vary from 28 to 31 inches. Again, by an experiment of the late Mr. Hauksbee's in his Phys. Mechan. exp. p. 170, the density of the air varies $\frac{1}{8}$ part between the greatest degree of heat in summer, and that of cold in the winter season. So that the air, in a hard frost, when the mercury stands at 81 inches, is near a 5th part specifically heavier than it is in a hot day, when the mercury stands at 28 inches.

TABLE XI.—*From Mons. Homberg and John Caspar Eisenschmid, of the Proportion of the Specific Weights of certain Fluids in the Winter, to the Weights of the same in the Summer Season.*

Mercurius	1.00479	Acetum	1.01600
Aqua pluvialis	1.00809	Ol. vitrioli	1.02131
Aqua fluviatilis	1.00811	Ol. terebinth	1.02141
Aqua distillata	1.00815	Aquafortis	1.02637
Spirit. vitriol.	1.01272	Ol. tartari	1.03013
Lac bubulum	1.01316	Spir. vini	1.03125
Aqua marina	1.01351	Spir. nitri	1.04386
Spir. salis	1.01467		

According to this table, the increase of the specific weight of common water in the winter above its weight in the summer, is not more than about the 124th part of the whole; which is little more than half of what Professor Muschenbroek has elsewhere accounted the same; but sure this difference is much too great. Though all fluids are condensed by cold, it is only till such time as they are ready to freeze: for on the freezing they immediately expand again, so as for the ice to be specifically lighter than the fluid of which it is formed, and to swim in it; Muschenbroek gives the specific weight of ice to be to that of water commonly as 8 to 9.

Of the Experiments made by some Gentlemen of the Royal Society, to Measure the Absolute Velocity of Electricity. By Mr. W. Watson, F. R. S. N° 489, p. 491.

Mr. W. laid before the R. S. and printed in N° 485, an account of what had been done by some gentlemen, in order to ascertain the respective velocities of electricity and sound; from which it appeared, that through a space measuring 6732 feet, the electricity was perceptible in a quantity of time less than $\frac{837}{1000}$ of a second. But the gentlemen concerned were desirous, if possible, of ascertaining the absolute velocity of electricity at a certain distance; and a method had been thought of, by which this might be determined with great exactness.

Accordingly, Aug. 5, 1748, there met at Shooter's Hill for this purpose, the president of the R. S. the Rev. Mr. Birch, the Rev. Dr. Bradley, astron. royal; James Burrow, Esq., Mr. Ellicot, Mr. George Graham, Rich. Graham, Esq. the Rev. Mr. Lawrie, Charles Stanhope, Esq. and Mr. W. who were of the

Royal Society; also Dr. Bevis, and Mr. Grischow, jun. a member of the Royal Academy of Sciences at Berlin.

It was agreed to make the electrical circuit of 2 miles; in the middle of which an observer was to take in each hand one of the extremities of a wire, which was a mile in length. These wires were to be so disposed, that this observer being placed on the floor of the room near the electrical machine, the other observers might be able in the same view to see the explosion of the charged phial, and the observer holding the wire; and might take notice of the time lapsed between the discharging the phial and the convulsive motions of the arms of the observer in consequence of it; as this time would show the velocity of electricity, through a space equal to the length of the wire between the coated phial and the observer.

The electrifying machine was placed in the same house as it was last year. We then found ourselves, says Mr. W. greatly embarrassed by the wire's being conducted by the side of the road, which we were compelled to, on account of the space necessary for the measuring of sound; but so great a distance from the machine was not now wanted, though the circuit through the wire was intended to be at least 2 miles. We had discovered, by our former experiments, that the only caution now necessary was, that the wires conducted on dry sticks should not touch the ground, nor each other, nor any non-electric, in a considerable degree, in any part of their length; if they did not touch each other, the returns of the wire, be they ever so frequent, imported little, as the wire had been found to conduct electricity so much better than the sticks. It was therefore thought proper to place these sticks in a field 50 yards distant from the machine. The length of this field being 11 chains, or 726 feet, 8 returns of the wire from the top to the bottom of the field, made somewhat more than a mile, and 16 returns more than 2 miles, the quantity of wire intended for the electricity to pass through to make the experiment.

We had found last year, that on discharging the electrified phials, if 2 observers made their bodies part of the circuit, one of which grasped the leaden coating of the phial in one hand, and held in his other one extremity of the conducting wire; and if the other observer held the other extremity of the conducting wire in one hand, and took in his other the short iron rod with which the explosion was made; on this explosion, they were both shocked in the same instant, which was that of the explosion of the phial. If therefore an observer making his body part of the circuit, was shocked in the instant of the explosion of the charged phial in the middle of the wire, no doubt would remain of the velocity of electricity being instantaneous through the length of that whole wire. But if, on the contrary, the time between making the explosion, and seeing the convulsions in the arms of the observer holding the conducting wires, was great enough to be measured, we then should be able to ascertain its velocity to the distance equal to half the quantity of wire employed only, let the manner of the electricity's discharging itself be what it would.

To make the experiment, the same phial filled with filings of iron, and coated with sheet-lead, which was used last year, was placed in the window of the room near the machine, and was connected to the prime conductor by a piece of wire. To the coating of this phial a wire was fastened; which being conducted on dry sticks to the before-mentioned field, was carried in like manner to the bottom; and being conducted thus from the bottom of the field to the top, and from the top to the bottom 7 other times, returned again into the room and was held in one hand of an observer near the machine. From the other hand of this observer, another wire, of the same length with the former, was conducted in the same manner, and returned into the room, and was fastened to the iron rod with which the explosion was made. The whole length of the wires, allowing 10 yards for their turns round the sticks, amounted to 2 miles a quarter and 6 chains, or 12276 feet.

When all parts of the apparatus were properly disposed, several explosions of the charged phial were made; and it was invariably seen, that the observer holding in each hand one of the extremities of these wires was convulsed in both his arms in the instant of making the explosions.

Instead of one, 4 men were then placed, holding each other by the hand near the machine, the first of which held in his right hand one extremity of the wire, and the last man the other in his left. These also were all seen convulsed in the instant of the explosion. Every one who felt it complained of the severity of the shock.

On these considerations we were fully satisfied, that through the whole length of this wire, being 12276 feet, the velocity of electricity was instantaneous.

Of Double Fetuses of Calves. By M. le Cat, M.D., F.R.S., &c. Dated at Rouen, August 20, 1748, N.S. N° 489, p. 497.

M. le Cat had since the month of January 1735, been in possession of a child, born in the city of Rouen, which had 2 heads, 4 arms, 4 lower extremities, and 2 trunks united, and as it were blended together. About that time he published a Description * of the internal parts of this monster, which had but one heart; but he did not cause drafts to be taken of those parts: and it was afterwards a difficult matter to have them drawn so as to exhibit a good representation of the state in which they then were. This negligence, through which he was deprived of those curious and instructive figures, which this monstrous birth would

* Journal de Verdun for March 1735, p. 194. Orig.

have afforded, made him wish for a like opportunity, in some measure at least to make amends for that fault. This opportunity presented itself in Jan. 1748, not in a human fetus indeed, but in a calf, which the butchers of the hospital cut out of a cow. The description of which monster is as follows.

The integuments of the breast being raised, there appeared the union and reciprocal insertion of the pectoral muscles of each subject into one common linea alba. None but the inmost plans were attached to the bones.

The muscles being removed, one sternum, common to both subjects, appeared in sight. There was a sternum entirely similar to this, on the other or opposite side. The heart was common to both.

A trunk was formed by the reunion of the carotids, and the subclavians; which trunk commonly constitutes the superior aorta; but in this subject it only sends a small canalis arteriosus into the inferior aorta. There was a thick common trunk of the pulmonary artery and the inferior aorta. The latter plainly appears a continuation of this trunk; whereas it is commonly a continuation of the aorta: and the pulmonary artery only furnishes the aorta, which makes but one canal in ordinary subjects, with a canalis arteriosus, or canal of communication. And indeed he was of opinion, that this structure, which seems extraordinary, is natural to every fetus that is not far advanced, as he explains it in his course of physiology under the article of the fetus; and that it is a consequence and proof of the mechanical and successive formation of the organs of its circulation, which begins by the lower circle made by the umbilical vein, as the first mover; the trunk of the vena cava, the inferior aorta, and the branches of the vena cava, which correspond with it. Now the one subject A had several marks, which demonstrated that its formation was less advanced than that of the other subject B.

The umbilical vein of the subject B received a large branch of the umbilical vein of the subject A: and which branch seemed to supply the place of the venal duct that was wanting. Having thrown in the injection through this vein, the heart and vessels of the 2 subjects were injected.

The orifices of the umbilical arteries were but 2 in number, one for each subject; the one and the other issuing from the right iliac of each subject.

The heart had only 2 cavities, as usual; but the right cavity or ventricle belonged to the subject B; and the left ventricle to the subject A. Into each of the cavities there opened 4 orifices; viz. two arterial, which were those of the pulmonary arteries, and of the aortas; and two venal orifices, or those of the right and left auricles, for the blood of the cavæ, and of the pulmonary veins.

He gives the name of aorta to the superior arterial trunk of the subject A, in conformity with the usual appellations, and because in common subjects this trunk alone deserves that name; though in this case the pulmonary artery visibly constitutes the principal part of the inferior aorta.

Concerning a Wether giving Suck to a Lamb; and of a Monstrous Lamb. By the Rev. Dr. Doddridge. * N° 489, p. 502.

D. D. had this remarkable fact from a member of the church of which he was pastor, and in whom he could entirely confide. The person told him that he had in Upper Heyford field, about 4 miles from Northampton, a wether sheep, which then sucked a lamb. The lamb sometime before ran after it, and fixed on its teats: drawing hard, milk followed. The lamb had subsisted very well on what it sucked from him, and at the late shearing time he himself pressed the teats, and milk came out in a considerable quantity.

This reminded the Dr. of what Mr. Ray tells us from Boccone, that a countryman in Umbria nourished his child by his own milk, and Florentini and Malpighi are quoted on the same occasion. Bartholin, in his Anatomy, p. 215, has some remarkable passages to this purpose: he quotes a passage in Aristotle concerning a he-goat in Lemnos, which had a great quantity of milk.

Dr. D. adds a short account of a monstrous lamb, which was weaned in a field near Newport Pagnel about the middle of March, and was brought to him soon after it died. It had 2 perfect heads, and 2 long necks, each as large as that of a common lamb, but sucked only with that on the right side. So far as he could learn, the organs of both were compleat. It walked only on 4 legs, but had a fifth hanging down between the 2 necks, rather longer than the other 4; the bones and hoof were double, and had 4 claws: the concave side of it was turned upwards, and whenever the creature walked, this leg moved up and down as it seemed spontaneously, and in a manner answerable to the motion of the other 4: it had 2 tails, but no vent behind: it had also 2 distinct spines, but they met about 5 inches above the tail, and then divided again; but where they met they were not as one entire spine, but as 2 adhering to each other. There

* Dr. Philip Doddridge, an eminent English divine, was born at London in 1702. After completing his education at an academy at Kilworth in Leicestershire, kept by Mr. Jennings, he became a minister in that town, and on the death of Mr. Jennings he succeeded him in the academy; but shortly after he accepted a call from the dissenting congregation at Northampton, where his academy soon became flourishing. Here Dr. D. laboured with great assiduity as a minister and instructor, generally admired and esteemed, by men of every persuasion, for the extent of his learning, the amiableness of his manners, and the piety of his disposition. His continued exertions however were too much for his constitution, and soon destroyed his health; for the recovery of which he repaired to Lisbon, where he died in 1751, at 49 years of age.

Dr. D.'s chief publications were, 1. The Family Expositor; being an exposition of the New Testament, in 6 vols. 4to. 2. The Rise and Progress of Religion in the Soul, 8vo. 3. Various Sermons and Tracts, which were collected in 3 vols. 12mo.

were 2 sets of ribs, only those which met upwards, where the spine should regularly have been placed, were rather shorter than the other: and it seemed that the blade-bone belonging to the doubled leg, that grew between the necks, was larger than the rest, and seemed to be 2 bones, but not entirely distinct: it had 2 hearts of equal size, lying over each other, almost like a St. Andrew's cross. There were 2 œsophagi, and 2 asperæ arteriæ: 4 small lobes of lungs, but the 2 gullets were inserted into 1 common stomach. There was nothing preternatural in the formation of the intestines, but the tails grew so near, that the return of both seemed to point to one vent, though as before observed, the anus was deficient. It had 3 kidneys, 1 of them very large in proportion to the other 2; so that he apprehends there was a conjunction.

Abstract of a Letter from Mons. Buffon, Member of the Royal Academy of Sciences at Paris, &c. concerning his Re-invention of Archimedes's Burning Specula.* N° 489, p. 504.

The speculum already constructed, and which is but 6 feet broad, and as many high, burns wood at the distance of 200 feet; it melts tin and lead at the

* Georges-Louis Leclerc, Count de Buffon, was born at Montbard, Sept. 7, 1707. In his youth he was chiefly attached to pleasure and amusements, without any particular attention to study; till chance brought him acquainted with the young lord Kingston, then on his travels, whose travelling tutor, being a man of scientific pursuits, gave a turn to young Buffon's disposition. Hence he lived with them at Paris and at Saumur, he followed them to England, and accompanied them to Italy. From this beginning Buffon continued to cultivate, with extraordinary success, most of the sciences, but chiefly natural history. Being of a strong and robust frame, an ardent mind, a sanguine disposition, his pursuits and discoveries were rapid, ingenious, and important. The variety and excellence of his compositions soon introduced him to respect and literary honours. He was admitted of the Academy of Sciences in 1739, and the same year named intendant of the king's botanical garden; an office which he filled with the greatest honour to himself. His articles in the Memoirs of the Academy of Sciences were very numerous, and of considerable importance. Besides these, he published several other separate works on different subjects; the chief of which were, a Translation into French of Hales's Vegetable Statics, in 1 vol. 4to; and his grand work, the Hist. Naturelle, in 19 vols. 4to, by which he rendered his name immortal. Here the general theory of the globe we inhabit; the disposition, nature, and the origin of the substances it offers to our notice; the grand phenomena which operate on its surface, or in its bosom; the history of man, and the laws which take place at his formation, in his developement, in his life, at his dissolution; the nomenclature and the description of quadrupeds and of birds, the examination of their faculties, the description of their manners; such are the objects that Buffon has treated in this grand work. The philosophical reflections mixed with the descriptions, with the exposition of facts, and description of manners, add at once to the interest, to the charms, and utility of the composition. The strength of his constitution, and activity of his mind, enabled Buffon to continue his studious and useful labours, to the very last years of a long life, though often embittered by a painful disorder so frequently the consequence of a studious and sedentary occupation; by which his valuable life was at length terminated in 1788, at 81 years of age.

The particular list of Buffon's writings may be seen in Rozier's large index to the articles in the distance of above 120 feet, and silver at 50. The theory which led him to this discovery, is founded on 2 important remarks, the one that the heat is not proportional to the quantity of light, and the other that the rays do not come parallel from the sun. The first of these, which appears to be a paradox, is nevertheless a truth, of which we may easily satisfy ourselves, by reflecting that heat propagates itself even within bodies; and that when we heat at the same time a large superficies, the firing is much quicker than when we only heat a small portion of the same.

An Essay on Quantity: occasioned by reading a Treatise, in which Simple and Compound Ratios are applied to Virtue and Merit. By the Rev. Mr. Reid. Communicated in a Letter from the Rev. Henry Miles, D.D., F.R.S. N° 489, p. 505.

Since it is thought that mathematical demonstration carries a peculiar evidence along with it, which leaves no room for further dispute, it may be of some use, or entertainment at least, to inquire to what subjects this kind of proof may be applied.

Mathematics contain properly the doctrine of measure; and the object of this science is commonly said to be quantity; therefore quantity ought to be defined, what may be measured. Those who have defined quantity to be whatever is capable of more or less, have given too wide a notion of it, which it is apprehended has led some persons to apply mathematical reasoning to subjects that do not admit of it. Pain and pleasure admit of various degrees, but who can pretend to measure them?

Whatever has quantity, or is measurable, must be made up of parts, which bear proportion to each other, and to the whole; so that it may be increased by addition of like parts, and diminished by subtraction, may be multiplied and divided, and in short, may bear any proportion to another quantity of the same kind, that one line or number can bear to another. That this is essential to all

volumes of the Royal Academy of Sciences, in 4 vols. 4to. And a general analysis of his works and life in the History of the Academy for the year 1788. An account of his life and writings has also been published separately by M. Cuvier.

It must not be dissembled however that the natural history of the Count de Buffon, though often distinguished by peculiar eloquence, is of too diffuse and rambling a cast to be considered as a proper model of good writing. His theories are bold and ingenious, but at the same time highly absurd. In his history of quadrupeds he seems to take a particular delight in railing at methodical distribution and minute exactness of description, and affects to be particularly severe against the arrangements of Linneus. The work is in reality much more valuable for the anatomical descriptions of Daubenton annexed to each article than for the declamatory harangues of the Count de Buffon himself, who seems not to have perceived, that a natural history conducted on his own plan and principles, must inevitably have sunk under its own weight.

mathematical quantity, is evident from the first elements of algebra, which treats of quantity in general, or of those relations and properties which are common to all kinds of quantity. Every algebraical quantity is supposed capable not only of being increased and diminished, but of being exactly doubled, tripled, halfed, or of bearing any assignable proportion to another quantity of the same kind. This then is the characteristic of quantity; whatever has this property may be adopted into mathematics; and its quantity and relations may be measured with mathematical accuracy and certainty.

There are some quantities which may be called proper, and others improper. This distinction is taken notice of by Aristotle; but it deserves some explanation. That properly is quantity which is measured by its own kind; or which of its own nature is capable of being doubled or tripled, without taking in any quantity of a different kind as a measure of it.

Improper quantity is that which cannot be measured by its own kind; but to which we assign a measure by the means of some proper quantity that is related to it. Thus velocity of motion, when we consider it by itself, cannot be measured. We may perceive one body to move faster, another slower; but we can have no distinct idea of a proportion or ratio between their velocities, without taking in some quantity of another kind to measure them by. Having therefore observed, that by a greater velocity a greater space is passed over in the same time, by a less velocity a less space, and by an equal velocity an equal space; we hence learn to measure velocity by the space passed over in a given time, and to reckon it to be in exact proportion to that space: and having once assigned this measure to it, we can then, and not till then, conceive one velocity to be exactly double, or half, or in any other proportion to another; we may then introduce it into mathematical reasoning without danger of confusion or error, and may also use it as a measure of other improper quantities.

All the kinds of proper quantity we know, may perhaps be reduced to these 4, extension, duration, number, and proportion. Though proportion be measurable in its own nature, and therefore has proper quantity, yet as things cannot have proportion which have not quantity of some other kind, it follows, that whatever has quantity must have it in one or other of these 3 kinds, extension, duration, or number. These are the measures of themselves, and of all things else that are measurable.

Number is applicable to some things, to which it is not commonly applied by the vulgar. Thus, by attentive consideration, lots and chances of various kinds appear to be made up of a determinate number of chances that are allowed to be equal; and by numbering these, the values and proportions of those which are compounded of them may be demonstrated.

Velocity, the quantity of motion, density, elasticity, the vis insita and impressa, the various kinds of centripetal forces, and different orders of fluxions, are all improper quantities; which therefore ought not to be admitted into mathematics, without having a measure of them signed. The measure of an improper quantity ought always to be included in the definition of it; for it is the giving it a measure that makes it a proper subject of mathematical reasoning. If all mathematicians had considered this as carefully as Sir Isaac Newton appears to have done, some labour had been saved both to themselves and to their readers. That great man, whose clear and comprehensive understanding appears, even in his definitions, having frequent occasion to treat of such improper quantities, never fails to define them, so as to give a measure of them, either in proper quantities, or in such as had a known measure. This may be seen in the definitions prefixed to his Princip. Phil. Nat. Math.

It is not easy to say how many kinds of improper quantity may, in time, be introduced into mathematics, or to what new subjects measures may be applied: but this I think we may conclude, that there is no foundation in nature for, nor can any valuable end be served by applying measure to any thing, but what has these 2 properties. First it must admit of degrees of greater and less. Secondly, it must be associated with or related to something that has proper quantity, so as that when one is increased the other is increased, when one is diminished, the other is diminished also; and every degree of the one must have a determinate magnitude or quantity of the other corresponding to it.

It sometimes happens, that we have occasion to apply different measures to the same thing. Centripetal force, as defined by Newton, may be measured various ways, he himself gives different measures of it, and distinguishes them by different names, as may be seen in the above-mentioned definitions.

In reality Dr. M. conceives that the applying of measures to things that properly have not quantity, is only a fiction or artifice of the mind, for enabling us to conceive more easily, and more distinctly to express and demonstrate, the properties and relations of those things that have real quantity. The propositions contained in the first two books of Newton's Principia might perhaps be expressed and demonstrated without those various measures of motion, and of centripetal and impressed forces which he uses: but this would occasion such intricate and perplexed circumlocutions, and such a tedious length of demonstrations, as would frighten any sober person from attempting to read them.

From the nature of quantity we may see what it is that gives mathematics such advantage over other sciences, in clearness and certainty; namely, that quantity admits of a much greater variety of relations than any other subject of human reasoning; and at the same time every relation or proportion of quantities may by the help of lines and numbers be so distinctly defined, as to be easily distinguished from all others, without any danger of mistake. Hence it is that we are able to trace its relations through a long process of reasoning, and with a perspicuity and accuracy which we in vain expect in subjects not capable of mensuration.

Extended quantities, such as lines, surfaces and solids, besides what they have in common with all other quantities, have this peculiar, that their parts have a particular place and disposition among themselves: a line may not only bear any assignable proportion to another, in length or magnitude, but lines of the same length may vary in the disposition of their parts; one may be straight, another may be part of a curve of any kind or dimension, of which there is an endless variety. The like may be said of surfaces and solids. So that extended quantities admit of no less variety with regard to their form, than with regard to their magnitude: and as their various forms may be exactly defined and measured, no less than their magnitudes, hence it is that geometry, which treats of extended quantity, leads us into a much greater compass and variety of reasoning than any other branch of mathematics. Long deductions in algebra for the most part are made, not so much by a train of reasoning in the mind, as by an artificial kind of operation, which is built on a few very simple principles: but in geometry we may build one proposition on another, a third upon that, and so on, without ever coming to a limit which we cannot exceed. The properties of the more simple figures can hardly be exhausted, much less those of the more complex ones.

It may be deduced from what has been said above, that mathematical evidence is an evidence sui generis, not competent to any proposition which does not express a relation of things measurable by lines or numbers. All proper quantity may be measured by these, and improper quantities must be measured by those that are proper.

There are many things capable of more and less, which perhaps are not capable of mensuration. Tastes, smells, the sensations of heat and cold, beauty, pleasure, all the affections and appetites of the mind, wisdom, folly, and most kinds of probability, with many other things too tedious to enumerate, admit of degrees, but have not yet been reduced to measure, nor perhaps ever can be. I say, most kinds of probability, because one kind of it, viz. the probability of chances, is properly measurable by number, as observed above.

Though attempts have been made to apply mathematical reasoning to some of these things, and the quantity of virtue and merit in actions has been measured by simple and compound ratios; yet Dr. M. does not think that any real knowledge has been struck out this way: it may perhaps, if discreetly used, be a help to discourse on these subjects, by pleasing the imagination, and illustrating what is already known; but till our affections and appetites shall themselves be reduced to quantity, and exact measures of their various degrees be assigned, in vain shall we essay to measure virtue and merit by them. This is only to ring changes on words, and to make a show of mathematical reasoning, without advancing one step in real knowledge.

Dr. M. apprehends that the account given o the nature of proper and improper quantity, may also throw some light on the controversy about the force of moving bodies, which long exercised the pens of many mathematicians, and perhaps is rather dropped than ended; to the no small scandal of mathematics, which has always boasted of a degree of evidence, inconsistent with debates that can be brought to no issue.

Though philosophers on both sides agree with each other, and with the vulgar in this, that the force of a moving body is the same, while its velocity is the same, is increased when its velocity is increased, and diminished when that is diminished. But this vague notion of force, in which both sides agree, though perhaps sufficient for common discourse, yet is not sufficient to make it a subject of mathematical reasoning: in order to that, it must be more accurately defined, and so defined as to give us a measure of it, that we may understand what is meant by a double or a triple force. The ratio of one force to another cannot be perceived but by a measure; and that measure must be settled, not by mathematical reasoning, but by a definition. Let any one consider force without relation to any other quantity, and see whether he can conceive one force exactly double to another; I am sure I cannot, says he, nor shall, till I shall be endowed with some new faculty; for I know nothing of force but by its effects, and therefore can measure it only by its effects. Till force then is defined, and by that definition a measure of it assigned, we fight in the dark about a vague idea, which is not sufficiently determined to be admitted into any mathematical proposition. And when such a definition is given, the controversy will presently be ended.

Of the Newtonian Measure of Force.—You say, the force of a body in motion, is as its velocity: either you mean to lay this down as a definition, as Newton himself has done; or you mean to affirm it as a proposition capable of proof. If you mean to lay it down as a definition, it is no more than if you should say, I call that a double force which gives a double velocity to the same body, a triple force which gives a triple velocity, and so on in proportion. This he entirely agrees to; no mathematical definition of force can be given that is more clear and simple, none that is more agreeable to the common use of the word in language. For since all men agree, that the force of the body being the same, the velocity must also be the same; the force being increased or diminished, the velocity must be so also, what can be more natural or proper than to take the velocity for the measure of the force?

Several other things might be advanced to show that this definition agrees best with the common popular notion of the word force. If 2 bodies meet directly with a shock, which mutually destroys their motion, without producing any other sensible effect, the vulgar would pronounce, without hesitation, that they met with equal force; and so they do, according to the measure of force above laid down: for we find by experience, that in this case their velocities are reciprocally as their quantities of matter. In mechanics, where by a machine 2 powers or weights are kept in equilibrio, the vulgar would reckon that these powers act with equal force, and so by this definition they do. The power of gravity being constant and uniform, any one would expect that it should give equal degrees of force to a body in equal times, and so by this definition it does. So that this definition is not only clear and simple, but it agrees best with the use of the word force in common language, and this is all that can be desired in a definition.

But if you are not satisfied with laying it down as a definition, that the force of a body is as its velocity, but will needs prove it by demonstration or experiment; I must beg of you, before you take one step in the proof, to let me know what you mean by force, and what by a double or a triple force. This you must do by a definition which contains a measure of force. Some primary measure of force must be taken for granted, or laid down by way of definition; otherwise we can never reason about its quantity. And why then may you not take the velocity for the primary measure as well as any other? You will find none that is more simple, more distinct or more agreeable to the common use of the word force: and he that rejects one definition that has these properties, has equal right to reject any other. I say then, that it is impossible, by mathematical reasoning or experiment, to prove that the force of a body is as its velocity, without taking for granted the thing you would prove, or something else that is no more evident than the thing to be proved.

Of the Leibnitzian Measure of Force.—Let us next hear the Leibnitzian, who says, that the force of a body is as the square of its velocity. If he lays this down as a definition, I shall rather agree to it, than quarrel about words, and for the future shall understand him, by a quadruple force to mean that which gives a double velocity, by 9 times the force, that which gives 3 times the velocity, and so on in duplicate proportion. While he keeps by his definition, it will not necessarily lead him into any error in mathematics or mechanics. For however paradoxical his conclusions may appear, however different in words from theirs who measure force by the simple ratio of the velocity; they will in their meaning be the same: just as he who would call a foot 24 inches, without changing other measures of length, when he says a yard contains a foot and a half, means the very same as you do, when you say a yard contains 3 feet.

But though I allow this measure of force to be distinct, and cannot charge it with falshood, for no definition can be false, yet I say in the first place, it is less simple than the other; for why should a duplicate ratio be used where the simple ratio will do as well? In the next place, this measure of force is less agreeable to the common use of the word force, as has been shown above; and this indeed is all that the many laboured arguments and experiments, brought to overturn it, do prove. This also is evident, from the paradoxes into which it has led its defenders.

We are next to consider the pretences of the Leibnitzian, who will undertake to prove by demonstration, or experiment, that force is as the square of the velocity. I ask him first, what he lays down for the first measure of force? The only measure I remember to have been given by the philosophers of that side, and which seems first of all to have led Leibnitz into his notion of force, is this: the height to which a body is impelled by any impressed force, is, says he, the whole effect of that force, and therefore must be proportional to the cause: but this height is found to be as the square of the velocity which the body had at the beginning of its motion.

In this argument I apprehend that great man has been extremely unfortunate. For, 1st, whereas all proof should be taken from principles that are common to both sides, in order to prove a thing we deny, he assumes a principle which we think farther from the truth; namely, that the height to which the body rises is the whole effect of the impulse, and ought to be the whole measure of it. 2dly, his reasoning serves as well against him as for him: for may I not plead with as good reason at least thus? The velocity given by an impressed force is the whole effect of that impressed force; and therefore the force must be as the velocity. 3dly, Supposing the height to which the body is raised to be the measure of the force, this principle overturns the conclusion he would establish by it, as well as that which he opposes. For, supposing the first velocity of the body to be still the same; the height to which it rises will be increased, if the power of gravity is diminished; and diminished, if the power of gravity is increased. Bodies descend slower at the equator, and faster towards the poles, as is found by experiments made on pendulums. If then a body is driven upwards at the equator with a given velocity, and the same body is afterwards driven upwards at Leipsic with the same velocity, the height to which it rises in the former case will be greater than in the latter; and therefore, according to his reasoning, its force was greater in the former case; but the velocity in both was the same; consequently the force is not as the square of the velocity any more than as the velocity.

Reflections on this Controversy.—On the whole, I cannot but think the controvertists on both sides have had a very hard task; the one to prove, by mathematical reasoning and experiment, what ought to be taken for granted; the other by the same means to prove what might be granted, making some allowance for impropriety of expression, but can never be proved.

If some mathematician should take it in his head to affirm, that the velocity of a body is not as the space it passes over in a given time, but as the square of that space; you might bring mathematical arguments and experiments to confute him; but you would never by these force him to yield, if he was ingenuous in his way; because you have no common principles left you to argue from, and you differ from each other, not in a mathematical proposition, but in a mathematical definition.

Suppose a philosopher has considered only that measure of centripetal force which is proportional to the velocity generated by it in a given time, and from this measure deduces several propositions. Another philosopher in a distant country, who has the same general notion of centripetal force, takes the velocity generated by it, and the quantity of matter together, as the measure of it. From this he deduces several conclusions, that seem directly contrary to those of the other. Thereupon a serious controversy is begun, whether centripetal force be as the velocity, or as the velocity and quantity of matter taken together. Much mathematical and experimental dust is raised, and yet neither party can ever be brought to yield; for they are both in the right, only they have been unlucky in giving the same name to different mathematical conceptions. Had they distinguished these measures of centripetal force as Newton has done, calling the one vis centripetæ quantitatis acceleratrix, the other quantitatis motrix; all appearance of contradiction had ceased, and their propositions, which seem so contrary, had exactly tallied.

Concerning a large Piece of a Lath being thrust into a Man's Eye, who Recovered of it. By Rich. Hassel, Esq., F.R.S. N° 489, p. 520.

Henry Halsey, of South Mims, labourer, thrust a long lath with great violence into the great canthus of the left eye of Edward Roberts of the same place, labourer, which broke off quite short; so that a piece near 2¼ inches long, half an inch wide, and above a quarter of an inch thick, remained in his head, and was so deeply buried there, that it could scarcely be seen, or laid hold of. He rode thus with the piece of lath in his eye from Kick's end, where the thing was done, to Barnet, which is above a mile, to the house of Mr. Morse, a surgeon there, who extracted it with difficulty; it sticking so hard, that others had been baffled in attempting to extract it. Roberts continued dangerously ill a long time; but at last recovered entirely, and has the sight of the eye, and the use of the muscles. But some time after he seemed well, he told Mr. H. that, on leaning down forward, he felt great pain in his head. The piece is supposed to have passed behind the right eye.

The Sun's Eclipse of July 14, 1748. *Observed at Marlborough House, with the Twelve-Foot Refracting Telescope, fixed as a Finder to the Tube of the Great Twelve-Foot Reflector. By John Bevis, M.D.* N° 489, p. 521.

Apparent Time.

July $14^d\ 9^h\ 3^m\ 50^s$ The beginning, which perhaps might be 2^s or 3^s sooner.

The end could not be precisely observed for flying clouds; at $12^h\ 9^m\ 15^s$ it was not quite over; but at $12^h\ 9^m\ 35^s$ the sun was clear, and nothing of the eclipse left.

The Moon's Eclipse of July 28, 1748, *observed at the same Place. By the same.* N° 489, p. 522.

July 28^d	10^h	13^m	28^s	The penumbra discernible.
		16	30	The beginning, as most of the company judged.
	12	24	30	The end.
		27	40	The penumbra quite gone.

About the middle of the eclipse, the moon's diameter, perpendicular to the equator, measured in a 5-foot telescope, was 33′ 50″; perhaps 15″ or 20″ greater than it would have been found to be with a 12-foot tube.

An Observation of an Extraordinary Lunar Circle, and of two Paraselenes; made at Paris, Oct. 20, 1747, N. S. *and of the Eclipse of the Sun, July* 14, 1748, O. S. *By Augustine Nathaniel Grischow, Memb. of the Royal Acad. of Sciences at Berlin, &c.* N° 489, p. 524.

October 20, at night, the sky was darkened by a slight fog, through which the moon appeared of a fiery red colour, till $8^h\ 40^m$, when the fog was quite dispersed, and the heavens were overcast with a whitish streaky cloud. At the same time there appeared round the moon a halo, ABCD, fig. 6, pl. 9, accompanied with 4 other segments of circles, 2 of which EAF and GH of 10°, were concentric, so as to have their common centre at the zenith. The segment or arch IPL, on the north side, of 7°, was concentric with the great lunar circle, and consequently had the moon for its centre; and lastly the arch MCN, which faced the horizon, was of 12°. Besides these 4 segments, what was most remarkable, was a mock-moon or paraselene B, shaped like a mock-sun or parhelius. The diameter of this mock-moon, though ill determined, was of 35 minutes at least, with a tail BP opposite to the moon, as the tail of a comet is opposite to the sun. This tail varied in its degree of light from time to time, extending as far as the arch IPL, which, as well as the arch GH, was 4° distant from the lunar circle ABCD. The paraselene B had the same colours with a common parhelius, excepting that they were not so lively, but they were much inclined to the tawny, especially on the side which faced the moon. This paraselene was in the same altitude as the moon. Its tail was much more faint and transparent; so that Capella appeared through this luminous tail. The lunar circle ABCD was much weaker to the south, and there appeared no paraselene on that side. This meteor did not seem to undergo any alteration till 9^h 18^m, when the atmosphere was covered with thick clouds.

The clouds being diminished at 9^h 32^m, the meteor appeared again, but very different from what it was before; for, instead of seeing a lunar circle with 4 other arches of circles, appeared the lunar circle DABD, fig. 7, and on the south side a faint arch QR of 4°, having the moon for its centre in common with the great lunar circle. There were likewise 2 paraselenes, one of which B was to the north, and the other D to the south. These however did not cast so strong a light as that which had appeared before, nor were they so distinctly formed. On the contrary, the lunar circle was very beautiful, and remarkably bright, till 9^h 50^m, when the whole phenomenon disappeared, and the sky grew clear by degrees. The moon's diameter was 30′ 30″. On the same night a very beautiful lunar circle was observed at Berlin, but without paraselenes.

The following is the observation that was made of the last eclipse of the sun, at the Observatory Royal at Berlin.

1748, July 25, N.S. The beginning of the eclipse was not observed, the sun having been covered with clouds.

The annulus was completed at	11^h	52^m	51^s	a. m.
.... broken	11	54	13	
The end of the eclipse	1	25	9	p. m.
The diameter of the sun was	31′ 43″.			

This eclipse was likewise observed annular at Frankfort on the Oder, but not so exactly as at Berlin.

Of a Preternatural Conjunction of Two Female Children. By James Parsons, M.D., F.R.S. N° 489, p. 526.

About the middle of September last, [1748] a woman in Holborn was delivered with much difficulty of 2 girls, joined together by the bellies in so singular a manner, as to deserve a particular description to be laid before the Society, as a very curious subject.

The care of preparing these children for keeping in spirits was committed to Mr. James Sherwood, surgeon, who sent for Dr. P. to observe them with him; and it was resolved to inject them, in order to make their anatomical examination the more accurately, which was executed by Mr. Sherwood, and the state of the children was as follows:

The skin of part of the breast and belly was continued to each child, from the lower part of the sternum, down to the insertion of a single * funis umbilicalis, which, instead of one to each, serves in common to both.

Each child had its peculiar muscles of the abdomen; but the straight muscles were so divided, as that the rectus on the right side of the one child had the linea alba between it and the rectus on the left side of the other, and vice versa; so that the line of each lying directly on each other, was colliquated and opened, and the conjunction of the musculi recti, thus formed but one common abdominal cavity up to the diaphragms of each child; above which each had its own proper thorax, even evident from their external appearance; whereas, had their junction been but ever so little in a lateral way, each would undoubtedly have had its own separate abdomen, since they would not have been so closely pressed forwards, as to occasion that intimate coalescing of parts; which is manifest in the dissections of several of these kinds of monstrosities, some of which have been joined by the hips, some by the backs, some partly by the sides, and one or two cases mentioned by Parée and Tulpius joined by the bellies.

None of these uncommon subjects ought to be touched with a knife, till it is well injected, because the vascular system, where there are any preternatural adhesions or distortions, can never be understood nor traced without it; and therefore Tulpius, whose account of his subjects is very inaccurate, and who certainly did not inject it, confesses he could make no distribution of the vessels, nor find out any thing of them distinctly.

But in the present case, a complete injection of the children being made by the vessels of the umbilical cord, they were enabled to give the following exact account of the vascular system and other parts; to which however they premise a description of the intestinal canal of both.

When they came to examine the intestines, the only proper means for laying them fairly to view, before they were taken out of the body, was to inflate them: which was accordingly done, and thereby every part of them was rendered conspicuous. Each child had its own peculiar œsophagus, stomach, and pylorus, in a natural state; from each of which the duodenum descended about 3 inches, and then united into one common duct, which they call the beginning of the jejunum, and which was near 4 inches long: this was inserted into the upper part of a large sacculus, formed out of the very coats of the intestines, and differing in nowise from them in colour, density, or any other quality, but the form and extension.

Its horizontal diameter was about 5 inches, and its vertical about 4, and it was formed out of the jejunum, which, in some subjects, is as long as the ileum, in most near that length, and no doubt was an attempt of nature to supply the

* See a similar case in these Trans. N° 65.—Orig.

want of two regular jejuna; for if these children had lived, each having its own proper stomach, would probably have eaten a due quantity of food for its sustenance; and the office of the stomachs might have been well enough performed; but each requiring a separate system of intestines to dispose naturally of the digested chyle, and this preternatural conjunction happening between them, the jejuna of both were confused together; and having room in the abdomen, now large and common to both, these parts of their organizations, that ought to have grown into two guts of a considerable length, being hindered from a regular accretion, the joint growing powers of both formed the sack of communication; which is proportionably capacious enough to answer the purposes of two natural jejuna; below which the rest of the intestines of each child were sufficient to do their several offices.

In the lower part of this sacculus there was an outlet on each side, which were the origins of their separate ilea; these were in a good state, and regularly inserted each into its cœcum; and this in each had its natural appendicula; these were regularly succeeded by their colons, and terminated by their proper recta intestina to their natural outlets; with this difference only, that the colons were out of their natural situation, and were convoluted in each child, by as narrow portions of the mesocolon, as any part of the ileum is by its mesentery, and that as low as the going off of the rectum.

Of the vascular system.—As these children had but one abdominal cavity between them, so it contained in appearance only one liver, of a considerable size, and an irregular form; but this consisted of 2 in reality preternaturally joined, as there were 2 gall-bladders. The umbilical vein is inserted into this, pretty nearly in the usual manner, and afterwards this canal is divided into 2 branches, which carry the blood into the vena cava of each child, whence it falls naturally into the right auricles of their hearts.

The heart of the larger child is but small, has a bifid apex, and from the division has a vestige of the septum, on both the upper and under sides; which forms a sulcus in a longitudinal direction, from between the apices to the basis; from whence arises a pericardium which extends itself over each side from the sulcus, and so forms a separate capsula over each ventricle of this heart, and may therefore be called a double pericardium.

The ascending vessels are distributed according to the standard of nature; but the descending trunk of the vena cava rides over that of the aorta, above the going off of the emulgents, and sinks back again behind the external iliac artery, before it is itself divided into iliac veins, descending naturally to the lower extremities, as do the arteries from thence also.

The kidneys, urinary and uterine parts were in a natural state, and the lungs appeared well, and seemed as if this child had breathed.

The heart of the smaller child was single, but above a third larger than it naturally ought to be; out of which the ascending arteries are very regular and natural; but there was scarcely any vestige of lungs in this child on the right side of the thorax, and but a small portion of the pulmonary substance in the left. The descending trunk of the aorta is very small in comparison of the other; yet goes down regularly towards the extremities, except the internal iliac arteries, which were obliterated and degenerated into ligaments, while the externals continued down, as abovesaid; for only the iliacs of the larger fœtus took place in the umbilical cord, which was the reason that there were only 2 arteries in it; so that, though both children received nourishment by the division of the canal from the liver to the venæ cavæ, yet the superfluous blood of both could be sent back to the placenta no other way than by the internal iliacs of the larger child.

The descending branches of the vena cava entered as usual, on the right side, into the auricles; but those of the left join in one trunk, pass round the left auricle, and enter into the right close by the cava ascendens, which is of a natural size, and very regular up to the diaphragm, from which it extends a full inch before it reaches the auricle. The kidneys differ a little in size from each other; yet these, with the other urinary and also the uterine parts in general, are in good order; but the most remarkable lusus of nature in these subjects, is an artery which arises from the aorta about the place of the cœliac of the one child, running along before the liver, and is inserted into the same place of the aorta of the other. It was much larger than any other artery in either child, and bestowed branches on the stomach, mesentery, and mesocolon, being about 5 inches long; for there were neither cœliac nor mesenteric arteries, according to nature in either child.

[Then follow some speculations on the formation of monsters, which are deemed not of sufficient importance to be retained.]

Explanation of the drawings representing the two children joined together.— Fig. 8, pl. 9, shows a back view of the intestinal canal of each child, from the stomach to the anus; a, a, the stomach of each; b, b, the duodenum; c, c, part of the jejunum which is common to both children; d, the remaining part of the jejunum formed into a sacculus, out of which arises e, e, the ileum of each child; f, f, the cœcum of each; g, g, the colon of each; h, h, the rectum of each.

Fig. 1, pl. 10, is a fore view of the other viscera, and vascular system of both children: a, the umbilical vein entering into the liver, which is formed of that of each child preternaturally conjoined; b, b, the lungs of the larger child; c, the heart, which has a bifid apex, and of which each ventricle has its particular pericardium, from the sulcus that divided it; f, the great vessels arising out of the heart; g, g, the kidneys, rather mis-shapen; h, h, the trunks of the large vessels descending to the lower extremities; the vein running before the artery, and sinking behind it again where it divides; i, the right ureter; k, the bladder; and l, the umbilical arteries, with the urachus turned down, to show m, the uterus, &c.; n, n, is an artery communicating with, and entering into, the aorta of each, near the going off of the emulgents; o, the heart of the smaller child, much too large in proportion, with the right auricle; p, part of the lungs, which were rendered much too small, in proportion, by the compression of the large heart upon them; q, the aorta and pulmonary artery, as they are connected by r, the canalis arteriosus; s, t, the descending trunks of the veins; the latter of which was preternatural, running round the left, and entering into the right auricle in its posterior part.

The other parts were much as those of the former child in general; except the aorta, which was much smaller, as the figure shows.

On the Preparation and Uses of the various kinds of Potash. By John Mitchel, M. D. and F. R. S.* N° 489, p. 541.

It is well known, that the ashes of all kinds of vegetables whatever afford potash in some measure or other; though some are much more fit for that purpose than others, which may be determined from the experiments of Redi in the Philos. Trans. N° 243, Boerhaave, Merret, and others; so that we need not insist on them here.

Most of the trees and herbs that are common in the American woods are known to be fit for this purpose, as the ashes of them all, burnt promiscuously in their houses, make a very strong lee fit for soap. Of these, the fittest for that purpose is the hiccory, the most common tree in their woods, which makes the purest and whitest ashes, of the sharpest taste, and strongest lee, of any wood. Their stickweed is said to do the same, which is as common a weed. For this reason the ashes of both these plants were used by the Indians there, instead of salt, before they learnt the use of common salt from the Europeans. The ashes of damaged tobacco, or its stalks, stems, and suckers, of which great quantities are thrown away, and rot and perish, are very fit for potash, as they contain a great deal of salts, and are well known to make a strong lie.

On the other hand, pines, firs, sassafras, liquid amber, or sweet gum, or all odoriferous woods, and those that abound with a resin or gum, are unfit for potash, as their ashes are well known to make a very weak lie, unfit for soap.

Besides these, that contain little or no salt, there are some other vegetables

* See an interesting paper on the same subject by Mr. Kirwan, in the Memoirs of the Royal Irish Academy.

that afford a large quantity of it, but make a bad kind of potash, at least for many purposes, on account of a neutral salt with which they abound. This seems to have been the case of the potash made in Africa, in a manufacture of that commodity set up there by the African company, which Mr. Houston, who was chiefly concerned about it, tells us, in his Travels, proved so bad, on account of a neutral salt it contained, that the manufacture was left off on that account; or perhaps from their not knowing how to make it rightly. What those vegetables are that afford this kind of ash, is not well known, if it be not fern, and some sea-plants.

Whatever vegetables are used, should be fresh or green, and nowise rotten, dried, or decayed. They should likewise be burnt to ashes by a slow fire, or in a close place; otherwise, when they are burnt in the open air by a strong fire, great quantity of the ashes is consumed in smoke, by the saline and terrestrial parts being carried up in fumes, before they are separated from these exhalable parts by the action of the fire. For the difference between burning wood in a close place and the open air, is so great, that the quantity of ashes obtained from one is more than double the other. This we learn from the experiments of Lundmarck hereafter mentioned, who tells us, he burnt a quantity of birch in a close stove, from which he obtained 5 pounds of ashes; whereas the same quantity of the same wood burnt in the open air, yielded only 2 pounds.

It is for this reason, that most people who make potash, burn their wood in kilns, or pits dug in the ground; though the Swedes burn it in the open air, as the author abovementioned informs us. This first step, or the burning the wood to ashes, seems to be taken by many for the whole process of making pot-ash; for those who pretend to have learned this art in Russia, as well as Lemery and some other authors, hardly give us any other account of it. But, in order to convert the ashes, prepared in this or any other manner, to what is called pot-ash, there are many different ways practised in different countries, which make as many different kinds of potash, that are all to be found in our markets, and have all their respective uses.

1. The first of these is commonly called pearl-ashes by our people, who import great quantities of it from Germany. This is no other than the lixivial salt of wood-ashes, extracted by making a strong lie of them, and by evaporating it to dryness, in a manner that is well known, and sufficiently explained by Kunkel in his art of making glass, also by Boerhaave, and many others.

2. But the art of converting these wood-ashes into potash, without this tedious process of elixiviation, is only practised in Russia, Sweden, and other northern countries, where it has been lately disclosed by one Lundmarck, who tells us he had often made it himself, in the manner he now describes. This account is contained in an academical dissertation on this subject at Aboe in Sweden, and

was communicated to Dr. M. by Dr. Linneus, professor of botany at Upsal, as a genuine account of this art; which he thinks has hitherto been little known.

This author tells us, "They have many large woods of beech in Smoland, and other parts of Sweden, in want of which they take alder; of these they are allowed to use only the old and decaying trees for this purpose, which they cut to pieces, and pile in a heap, to burn them to ashes, on the ground, by a slow fire. They carefully separate these ashes from the dirt or coals in them, which they call raking them; after which they collect them in baskets of bark, to carry them to a hut built in the woods for this purpose. This they continue to do till they have a sufficient quantity of the ashes. Then their whole art follows: for which they choose a convenient place, and make a paste of these ashes with water, by a little at a time, in the same manner, and with the same instruments, as mortar is commonly made of clay or lime. When this is done, they lay a row of green pine or fir-logs on the ground, which they plaster over with this paste of ashes; over this they lay another layer of the same straight logs of wood, transversely or across the others, which they plaster over with the ashes in the same manner; thus they continue to erect a pile of these logs of wood, by layer over layer, and plastering each with their paste of ashes, till they are all expended; when their pile is often as high as a house. This pile they set on fire with dry wood, and burn it as vehemently as they can; increasing the fire from time to time, till the ashes begin to be red-hot and run in the fire. Then they overset their pile with poles, as quickly as they can; and while the ashes are still hot and melting, they beat and clap them, with large round flexible sticks made on purpose, so as to incrust the logs of wood with the ashes; by which the ashes concrete into a solid mass, as hard as stone, when the operation has been rightly performed. This operation they call walla, i. e. dressing. At last they scrape off the salt thus prepared, with iron instruments, and sell it for potash; which is of a bluish dark colour, not unlike the scoriæ of iron, with a pure greenish white salt appearing here and there in it."

All the potash we have from Russia, Sweden, and Dantzic, is exactly like what our author here describes, and seems to be made in this manner. It is however generally observed, that the Russian is the best of these, on account of the greater quantity of salt in it. Now if, in the preceding process, we make our paste of the ashes with lie, instead of water, it is plain the potash will be impregnated with more salt, and make all the difference found so between these sorts of potash. This then is likely to be the practice in Russia; where their wood may also be better for this purpose, and afford more salt. This is well known to be the case of different kinds of wood; thus, our author abovementioned tells us, he obtained 2¼¾ lb. of salt out of 8 cubic ells of poplar, which was very sharp and caustic; but the same quantity of birch afforded only 1 lb. of salt, and that not so strong; and fir hardly yielded any at all.

The way of making potash above described may be the more easily understood by our people in America, for whom this is chiefly intended, as it is the same with their way of making lime of shells, the only lime they use in most places. These shells they burn to lime between the layers of a pile of wood, instead of a kiln, till it is reduced to ashes, in the same manner as is here directed to be done with ashes, to make potash. The lime thus made is reckoned very good; but as it is impregnated with the ashes of the wood, and the marine salt that is often in the shells, it is apt to make the houses that are built with it very damp in moist weather; so that the water often runs down their walls in streams; which cannot but be very unwholesome in an air that is naturally close and damp; the only way to prevent which would be, to wash and dry their shells frequently, and burn them in dry pine, that afford little or no lixivial salt.

3. There is another way of making potash, practised chiefly in England, where they make it in the following manner.

With their ashes of fern, or wood of any kind, they make a lie, which they reduce to what they call potash, by burning it with straw. To do this, they place a tub full of this lie near a clean hearth of a chimney, in which they dip a handful of loose straw, so as to take up a quantity of lie with it. The straw thus impregnated with lie they carry as quickly as they can, to hold it over a blazing fire on their hearth, which consumes their straw to ashes, and at the same time evaporates the water from the salts of the lie. Over the blaze of the the first parcel of straw they burn another dipped in lie in the same manner. This they continue to do till their lie is all expended. By this means the coals and ashes of the straw, and salts of the lie, are left on the hearth, and concrete together into a hard solid cake of a greyish black colour, which they scrape off, and sell for potash.

This is an easy way of making potash, when in want of proper vessels to extract the salt of the lie by evaporation, or in want of wood to reduce the ashes to potash in the way abovementioned, for which it seems to be contrived, and for which it is only to be commended. For the potash made in this manner is full of the coal of the straw, and its salt is not so strong, nor so sharp and corrosive as the salt of the foreign potash, calcined in an open fire; besides other differences hereafter mentioned, which makes this potash unfit for some purposes, and not above half the value of the foreign.

4. They have a very different way in the north of England of reducing their kelp to potash, which they use for making alum. This is made of the different kinds of fuci, or sea-weeds thrown up on the shore, or gathered on the rocks; which they dry a little in the sun, and afterwards burn them in a kiln, built of

the stones they find on the shore, in a cylindrical form, and about 2 feet or less in diameter. In this they first burn a small parcel of the herb, and before it is reduced to ashes they throw on more, till the kiln is full, or their materials are expended. This is said to reduce the ashes to a hard and solid cake, by the heat of the kiln, and quantity of salt in the herb, which makes what is commonly called kelp-ashes.

There are some other ways of making potash, suggested by several persons, which appear to be more easy and ready than any of the abovementioned; for which reason they are apt to be tried by those who make attempts of this kind. These are deduced from what they reckon the nature and properties of this production; and there is no doubt, but if that was well understood, it might afford some insight in the way of making it. For this reason we made the following experiments with the best Russia potash, in order to discover its nature and properties, and how they are most probably communicated to it; that we might see what we are to make; in order to imitate the best, or to make what is accounted good potash.

1. Russia potash, as it is brought to us, is in large lumps, as hard as a stone, and black as a coal, incrusted over with a white salt, that appears in separate spots here and there in it. 2. It has a strong fetid sulphureous smell and taste, as well as a bitter and lixivial taste, which is rather more pungent than other common lixivial salts. 3. A lixivium of it is of a dark green colour, with a very fetid sulphureous smell, and bitter sulphureous taste, somewhat like gunpowder, as well as sharp and pungent like a simple lixivium. 4. Though it is as hard as a stone, when kept in a close place, or in large quantities together in a hogshead; yet, when laid in the open air, it turns soft, and some pieces of it run per deliquium; while most other kinds of potash only turn friable, and crumble in the open air. 5. It readily dissolves in warm water, but leaves a large sediment of a blackish grey colour like ashes, which is in a fine soft powder, without any dirt or coals in it, that are to be observed in most other kinds of potash. 6. As it is dissolving in water, there is scummed off from some lumps of it a dark purple bituminous substance, like petroleum or tar, which readily dissolved in the lixivium. 7. This, or any other true potash, or a lixivium made of them, will presently tinge silver of a dark purple colour, difficult to rub off; while a mere lixivial salt has no such effect.

8. Pieces of this potash boiling in water made a constant explosion like gunpowder; which was so strong as not only to throw the water to some height, but to lift up and almost overset a stone cup in which he boiled them. These explosions were owing not so much to the included air, which some perhaps may imagine, as to the sulphureous parts of the composition expanding and flying off; for this boiled lixivium had neither the green colour, nor fetid sulphureous smell

and taste; at least in any degree like what it has when made of the same potash by a simple infusion in warm water.

9. He evaporated some of the green lixivium, made only by infusion, and filtred through a double rag: as soon as it began to boil, a green powder, to which its colour is owing, fell to the bottom, and the lie became pale. After it was evaporated to a pellicle, and set in a cool place, a salt separated from it on the sides of the cup, in angular crystals like tartar. These crystals were soon formed, and in pretty large quantities, but were difficult to separate from the alkaline lie and salt, in which and the open air they were apt to dissolve: but from the pellicle he obtained some pieces of the same salt that would not dissolve in the open air.

10. Oil of vitriol makes a strong effervescence with this green precipitate, with a white fume, and a very strong sulphureous smell. It does the same with these white crystals, though the sulphureous smell is not so strong. But with the pure fixed alkali there was no such sulphureous smell to be discerned.

From these experiments we may determine something about the nature and contents of potash. This we are the better enabled to do, from the accurate experiments and reasonings of the learned Mr. Geoffroy, on a like substance made of charcoal and an alkali salt calcined together, in which he observed all the properties and contents of potash above mentioned, particularly related in the Memoirs of the Royal Academy, for the year 1717. This was made of the same materials, and had all the properties above related of our potash; particularly a green lixivium, a strong sulphureous smell and taste, a sulphureous green precipitate, crystallized salts, and sulphureous fumes with oil of vitriol. From hence this learned author concludes, that this substance contained the active sulphureous parts of the wood, blended with more active igneous particles. These, united with the alkaline salts, make a kind of soap, or sulphureous saponaceous salt, resembling soap of tartar, or hepar sulphuris. The crystallized salts he attributes to the acid of the wood, mixing with the alkaline salts. All these parts of the wood then are contained in our potash; and he observed the same in the common soda, or cineres clavellati; though they are in a less degree in that, than in the Russian potash.

Besides these, he shows that potash contains a metallic substance, which affords the Prussian blue. We may add further, that the combination of these principles makes many properties in potash, more than what result from them in a state of separation. The most remarkable of these seems to be its explosive quality; which we take to proceed from the crystallized salts approaching to the nature of nitre, and uniting with the sulphur and charcoal; by which they form, from all these ingredients of gunpowder, a kind of that explosive substance, whose parts are highly rarefied in an intense and confined heat, by which they readily explode in boiling lie.

By this we may perceive, that the difficulty in making potash aright, is, first to reduce the materials to cinders and ashes, and at the same time to preserve their volatile, sulphureous, and exhalable acid parts, that are totally destroyed in such a degree of heat; and secondly, to calcine these ashes still further, so as to flux their salts, and vitrify their terrestrial parts, and at the same time to keep them separate from each other, or prevent their running into an indissolvable glass. To give potash some of these properties, seems plainly to require a degree of heat that will totally deprive it of others.

The most likely way by which it comes to receive all these properties, is from the way of making it in Sweden above described. In that process, the green fir, in which the ashes are burnt, impregnates them with the acid saline parts of the wood or tar, which is well known to be in pretty large quantities, and is absorbed and fixed by the alkaline salts, and porous terrestrial parts of the ashes in this process; so that, besides the fixed alkaline salts of the ashes, the potash, thus made, must likewise contain the more volatile salts of the pine, which are exhaled in smoke by burning the pine alone in the open air. Besides these, it likewise contains the resinous parts and sulphureous fumes of the pine, that are hindered from exhaling by the heap of the mass.

At the same time the alkaline salts are fluxed in the open fire, and in a manner vitrified with the terrestrial parts of the ashes, which gives them their hard and solid consistence; while the sulphureous and acid parts of the green wood hinder them from turning to a perfect glass, or inert calx. All these parts united together in the fire, make that saponaceous substance we find in the potash thus made, which further hinders the vitrification of the mass, and endows it with many of its most peculiar and active properties.

From hence we may see how difficult it is to make a substance endowed with all these properties in any other manner. This is the reason why we could never before make potash equal to that of Russia, and the other northern countries, though we have much greater plenty of materials and perhaps better: for this way of making it has never before been thought of by the learned, or practised any where else, as far as known.

Somewhat of the same qualities are communicated to the English potash, by the way of making it above described; but in a degree as much inferior, as dry straw, used for that purpose, is to green wood: accordingly our workmen find that potash as much inferior to the foreign, for many purposes.

From this account of the contents and qualities of potash, and the way of making it, we may form some judgment of the other ways of making it, proposed by authors, and suggested by many. Thus Lemery and others tell us, potash is made in Russia, and all the northern countries, only by calcining the ashes in pits bricked within, and sprinkling them well with lie, till they become hard and solid. But such a calcination of ashes with a lixivial salt, must render them whiter, instead of black, and must further destroy the active sulphureous parts of the wood, which we find in potash rightly made. So that this only leaves the ashes in the state they were at first, or turns them into a kind of indissolvable glass, as we have found on trial.

This, and the like mistakes about the way of making potash, seem to proceed from a general error concerning the nature of it; for it is commonly supposed to be only a kind of inert calx, impregnated with nothing but a lixivial salt. Some such mistake seems to have frustrated all the attempts hitherto made of making potash in America; for, on trial, what they have made there was found to be no better than common ashes.

But the most general mistake about the way of making potash, seems to proceed from the accounts we have of making it, from glasswort, and some marine plants, which are said to be easily converted to this kind of substance, in the manner above mentioned. But we apprehend that the way of making it from wood must be very different: for these herbs are easily reduced to ashes by a small fire, that does not entirely consume their sulphureous parts, which wood is not. These ashes abound with a great quantity of alkaline and some neutral salts, that readily convert them to a hard and solid consistence, which wood does not. They have likewise few or no terrestrial parts, to run them into an indissolvable glass, when fluxed in the fire, as happens in wood ashes. Besides, these herbs have few or no sulphureous or acid parts, like most woods; and the potash made of them has few of these principles in it, like what is made of wood.

It is however generally said, if we burn our wood in a close place, as a kiln in which we burn lime, or make charcoal, or a pit dug in the ground, we may impregnate the ashes with the sulphureous fumes and acid parts of the wood, only by the closeness of the place, or by smothering the fire in it. If at the same time we impregnate them with a greater quantity of lixivial salt, it will flux the whole mass, and make it run into a solid hard consistence like potash. This is commonly directed to be done, by throwing fresh or green wood or herbs on the others, as they are burning, before they are quite reduced to ashes, or by smothering the fire, as in making charcoal; and at the same time to sprinkle the ashes, thus burnt, with a strong lye, from time to time, in the manner commonly practised with glasswort.

This would be a more ready way of making potash than any of the above-mentioned; but as those who give their advice about it, have neither tried it, nor seen it done; and those who have tried this or any other way, find more difficulty in it than they at first imagined, we shall suspend our judgment about it, till we see it fairly tried, lest we should deter some from making useful experiments of it, or lead others into fruitless and expensive attempts.

By the various ways of making potash above mentioned, and the different materials it is made of, there appear to be many different kinds of it, that have as different qualities. It would lead us too far beyond our present design, to give a particular account of each of these; but as they are used in many of our manufactures, it seems worthy inquiry, to know what sorts are generally used, and what are the fittest to be used in them.

The workmen in England make 2 general kinds of it, which they distinguish by the names of pearl-ash and potash. The first is a mere lixivial salt, which is supposed to be the only ingredient of any efficacy in potash; but on trial there is found to be a great difference between them, especially in making soap. The salt is so weak in the pearl-ash, that it does not entirely dissolve and unite with the fat. The reason seems to be, that these salts are dissolved in water, in order to extract them, by which they lose many of their caustic igneous parts; whereas in potash, the salts are calcined and fluxed in an open fire, with the ignited terrestrial parts of the ashes, which makes them more sharp and corrosive: they are likewise incorporated with the coal, and fuliginous parts of the vegetables they are made of, or with the resinous parts of fir, which gives them the sulphureous quality above mentioned, and makes a kind of soap of tartar, or hepar sulphuris, in all potash; which makes these salts so ready to dissolve, and incorporate with oil, or other pinguious substances.

This is perhaps the reason, why the cineres russici are ordered for this purpose, instead of a mere lixivial salt, by the College of Physicians in their late dispensatory. The soap made of them must be impregnated with their heating sulphureous quality, which will make it more aperient and detergent, but not so mild and soft as some others; by which it may be more fit for obstinate and indurated obstructions, but will be more offensive to the stomach; which is much complained of by some people, who take large quantities of the sharper kinds of soap.

But, to consider potash as a commodity in trade and manufactures, which is its chief use; it appears, that the people in England not only have it at a dear rate, but the worst sorts of it, at least for most purposes; which cannot but have a proportional influence on their manufactures: for it is generally of as great, and some sorts of a greater value in their markets, than a pure lixivial salt; notwithstanding the small quantity of such salt in ashes, and the trouble and expence of extracting it; which seems to be occasioned by their not knowing how to convert ashes into this commodity; for in Sweden, where this art is known, Lundmarck tells us, potash is sold for little more than a farthing a pound, which costs our workmen near 6 pence.

But this is not the only inconvenience we labour under for want of this commodity; the salts we are chiefly supplied with, are perhaps the worst of any, and unfit for many purposes for which potash is used. The only potash almost to be met with here, comes from Russia, Sweden, and Dantzic, or is made in England. These are all made either of wood or fern-ashes, whose salts are never so pure and white at the best, as some others: but by the way of making them, and the experiments on them above mentioned, they appear to be impregnated with coal, smoke, and soot, which renders them still more foul and impure, makes them of a black, brown, or green colour, and of a peculiar sulphureous quality. On this account they are entirely unfit for making white glass: they make a very coarse and strong kind of soap; they are too foul, sharp, and corrosive for bleaching, and are as unfit for dyeing, at least many colours.

It is perhaps for this reason, that the workmen here make all their white glass with salt-petre; which must not only be more costly, but Neri, Merrett, and others tell us it is not so good, at least for the better sorts of glass, as a sharper lixivial salt. What they use for dyeing he is not so well apprised of: it is said, they use the volatile alkali of urine; but the French potash, made of the lees of wine, is generally allowed to be the best for that purpose. So likewise the Alicant potash is reckoned much the best for bleaching, and making soap; as the Syrian and Egyptian is for making glass.

These purer kinds of potash are all made of herbs, that grow only in the more southern climates, whose salts are finer and whiter, and less acrid and corrosive than the salts of wood, or most other vegetables; and by the way of extracting them by calcination in a more open fire, they are more free of coal, smoke and soot, or any other heterogeneous mixture. On this account they are much better for the purposes above mentioned, than the coarse and foul kinds of potash that our people are supplied with.

All we have of these kinds of potash, it seems, comes only from Spain; for which reason our people were obliged to petition to allow the importation of potash from thence, during the late war; as appears by an order of the king and council of the 24th of June 1742; since they could not do without it in many manufactures: so that it may be worth our inquiry, to know what it is that produces so necessary a commodity.

This kind of potash is commonly called barrilha, from a herb of the same name, in Spain that produces it. The first account we have of this barrilha is, from Amatus Lusitanus, who leaves us much in the dark about it. It is generally said in England to be a plant pretty well known to the botanists by the name of ficoides Neapolitana, flore candido. Hort. Lugd. Bat.; but for what reason I cannot say. We have as little reason to believe with John Bauhin that it is what he calls kali vulgare: for Mr. de Jussieu has shown, that the true barrilha is a different plant from any of these, from his own observations of it in Spain, where it was cultivated; of which he has given a particular account, by the name of kali Hispanicum, supinum, annuum, sedi foliis brevibus. Mem. Academ. Anno 1717, p. 93, or Alicant glass-wort.

The potash made of this plant, he says, makes the best soap, the finest glass, and is the best for bleaching of any other; for which reason it is much sought after in all countries, where they value themselves for these manufactures. But Dr. M. questions very much, whether our workmen have it either pure and genuine, or in sufficient quantities for these purposes. All the use he finds made of it among them, is to make hard-soap; though they say what they have of it spoils their soft soap, by making it curdle. This is well known to be the effects of sea-salt; and Mr. de Jussieu and others say, that the true barrilha is often adulterated with sea-weeds, which contain such a marine salt; so that it is probably only this adulterated sort that they have. Accordingly, all the barrilha he had found here, was of a dark brown colour, and very foul and ponderous; whereas the true sort is said, by all who know it, to be more porous, pure, and of a bluish colour. It is for this reason, in all probability, that, notwithstanding all the barrilha our workmen have at so dear a rate from Spain, yet they can never make so good soap, as what comes from thence, and some other places.

The only way then, by which we are likely to have this commodity either pure and genuine, or in sufficient quantities at a reasonable rate, is from the herb itself that produces it. Whether or not it would grow in England is not known, as it has perhaps never been tried: but there is no doubt but it would grow very well in our colonies in America, as he was certainly informed it did in the Spanish colonies there, where they have great plenty of it; and a sort that is indigenous, particularly in Peru, which might probably be found in our colonies, if sought for by those who knew it. But wherever it will grow in any of the English dominions, there is no doubt but it would be a considerable improvement, where potash of all kinds is so valuable a commodity, and so much wanted; for it grows on the same ground with corn of any kind, which it does no harm to, as it is a small annual herb, that does not spread till the corn is ripe, or off the ground.

There are some other plants that are known to make a kind of potash, commonly called rochetta, which is said to be even preferable to the barrilha, especially for making glass. These are the first and second kinds of kali, described by Prosper Alpinus, in his account of the plants of Egypt. The first of which is the above-mentioned ficoides that grows in Italy, and all over the Levant, but the other is peculiar to Egypt. These would be fit improvements for our colonies in America, where we seem to want nothing more than some proper production for the vast tracts of land we are possessed of there. But these plants alone afford a commodity, which Pr. Alpinus and Rauwolfius tell us they saw many large ships yearly loaded with in Egypt, and which gives the excellency to the glass and soap that are made at Venice.

It would be worth while then at least to make a trial of a production, that is likely to improve both our trade abroad, and our manufactures at home. It was this that produced the above inquiry, as an improvement fit for our colonies.

*On the Cyprus * of the Ancients. By Dr. Laurence Garcin, of Neufchatel, F.R.S. Translated from the French by W. Watson, F.R.S.* N° 489, p. 564.

This plant is a shrub, which varies considerably in its size and figure, according to the nature and soil of the country where it naturally grows, as well in Asia as in Africa, where this plant is much used, both for its medicine, and for its agreeable odour.

The author has given the true characters of the fructification of the cyprus, after the method of the celebrated Linneus. 1. Its calyx is an expanded monophyllous cup, cut into 4 lobes, pointed at their extremities, and continuing attached to the fruit. 2. Its corolla consists of 4 oval petals, somewhat pointed and sinuous. They grow distant from each other, and are placed between the lobes of the calyx. 3. It has 8 erect stamina, ranged 2 by 2 almost horizontally, and parallel to the sides of the petals, and surpass them in length about half a line. They grow from the base of the embryo at a little distance from one another, and arise diminishing in their bulk to their extremities. Their antheræ or summits form each of them a little kind of purse.

4. Its pistillum is round, and occupies the middle of the calyx. Its style is erect, and terminated with a pointed stigma. Its length somewhat exceeds that of the stamina. 5. Its pericarpium is a round dry capsule, slightly four-cornered; each of which corners has a small prickle. It is divided into 4 compartments by an extremely delicate membrane, arising from a placenta, which occupies the centre of the capsule. 6. Its seeds are small and numerous: each of them is pyramidal, and somewhat quadrangular, of which the point is sometimes straight and sometimes crooked. Every seed is fastened by its point to the placenta, as to a common centre, and their bases are sustained by the sides of the capsule, all the cavity of which is filled by them.

There is but one species of this shrub generally known through all the East; and this is subject to vary according to the climate, the season, and the soil.

* This shrub is the *Lawsonia inermis* of Linneus.

Its Description.—The cyprus grows generally as a shrub of 10 or 15 feet in height; and has very much the appearance of privet. Its trunk grows sometimes as thick as a man's thigh, is sometimes straight and sometimes crooked, and produces a great number of branches irregularly. Its outward bark is ash-coloured, and much furrowed, and detaches itself from the trunk of the tree in long scales or pieces, by the heat and dryness of the climate, as in the Persian gulf. Its inner bark is reddish without, and whitish within. That of the branches is smooth and red, like that of the hazel-tree, and green within. Its young branches are straight, flexible, and moderately long. The wood of the trunk is hard and whitish.

Its leaves are disposed in different orders on the same twig. Sometimes they are placed opposite in pairs along the small branches, and this most generally cross-wise; sometimes by 3 and 3; but then the leaves are less, and this disposition generally takes place in the larger branches; sometimes they are alternate, but rarely, and then the leaves are largest. The least branches are most charged with leaves, the larger ones least. All these leaves are pointed at each end; the largest are 2 inches long, and about an inch broad in their middle; the smallest bear half the dimensions of the largest. their edges are even: they are smooth, shining, and of a beautiful green colour: their middle rib, which serves to each leaf as a short pedicle, is terminated in their point, but sends out, in its passage through the leaf, alternately 4 or 5 nervous filaments on each side. These leaves are much like those of privet.

The flowers grow in bunches at the extremities of the young branches, and are endowed with a very agreeable and singular odour. They are of a straw-colour; but as they grow old and wither, they become of the colour of a citron. The calyx is more pale than the corolla of the flowers. Its petals are turned up as much, if not more, than those small petals are which adorn the centre of a double rose. The stamina, which are white, transparent, and which grow from the base of the embryo of the fruit, form as it were a double cross, by their almost parallel situation and extension between the petals. The lobes of the calyx, being of the same length and form of the petals, seem to give to the entire flower an octagonal figure. The summits or antheræ are small, and of the same colour as the petals, each having a deep furrow in its bottom; the more these decay, the more yellow they grow, in the same manner as the petals. The furrow in the anthera, which at first is of a palish black, grows of a deeper hue as the flower fades. The pistillum, after the flower is gone, grows larger in the calyx, and becomes, when perfectly ripe, a dry, membranous, round fruit, of about 3 lines in diameter. But before it arrives to this state, it resembles very much a fleshy berry, green on one side, purplish, and sometimes black on the other, with very little juice. This false berry is the growing capsule, the side of which is soft, succulent, and very thick; which in proportion as it increases, becomes thin, membranous, dry, and brittle: in becoming thus capacious and thin, it gives room to a large number of pyramidal seeds, very close to each other, and fastened all by their points to a common centre, a kind of placenta. When this capsule is in its perfection, its outside is shining, and not unlike the seed of coriander in colour. The pericarpium is as it were divided into 4 loculi, by membranes so delicate, that they must be regarded with great attention, to be satisfied of their reality. The exterior form of this fruit sufficiently shows this division, by its roundness being interrupted by 4 slight ribs, like those of a melon, which shows as many cells. The membranes, which divide these cells, arise from the placenta, and are inserted into the sides of the capsule.

The seeds, which fill all the capsule, amount to about 4 or 5 dozen, according as they are more or less nourished; for the larger ones receiving more nourishment, make the smaller ones abortive. They are always so pressed in their apartments, that their pyramidal figure is owing only to this pressure, which arises from their reciprocal increase. The pyramidal points of these seeds are crooked in some, and bent in others, according to the direction given them in their growing. Their colour is red or brown, and always somewhat shining.

Remarks.—We find in the ancient writers of plants, such as Theophrastus, Dioscorides, and Pliny, who have all in their manner treated of vegetables, of how much esteem the cyprus was among the ancients. The historian Josephus, and St. Jerome, have mentioned it as a rare and precious plant, placing it in the same rank with the most valued spices. The fine smell, which its flowers send forth in the countries where they grow naturally, as in Egypt, Syria, Arabia, Persia, &c. has occasioned its use in the earliest time; and the same use still continues in those countries. Its being twice mentioned in Solomon's Song,* is a very great proof of its being much valued in the most ancient time. We there see it was accustomed to be cultivated even in their vineyards. The perfumers in old times made of it an oil or precious ointment, for various uses; but principally to give their anointings a grateful odour, and to make supple the limbs of the body.

Modern authors have taken great trouble to be thoroughly satisfied of the history of this plant. There have been great controversies among them concerning it, in endeavouring to settle its description; but it must be confessed they have made a very small progress in explaining its true characters. How many mistakes have the botanists of the last 2 centuries made, owing to the bad descriptions of this plant which the ancients have left us.

* Solomon's Song, chap. i. v. 14, ch. iv. v. 13. In both these places the English translation of the Bible has it camphor, instead of cyprus.—Orig.

Dioscorides, who, by describing the plants he treats of too briefly, always leaves their characters imperfect, says (perhaps after some other author more ancient than himself) that the leaves of the plant in question are like those of the olive-tree; that its flowers are in bunches, and that its fruit is black, like that of elder. This was enough to make the Latins conjecture, that the κύπρος of this author was the ligustrum or privet; and the more so as the cyprus was quite unknown to them, since it only grew in Egypt and in Syria, where it was always called henna, or alhenna, and by corruption alkanna.

There is some appearance that, as the Greeks received a good quantity of this drug from the Isle of Cyprus, as a species of merchandise, they would chuse to call it Cyprus, rather than give it any other denomination, on account of the quantity furnished to them from the isle of that name.

Pliny took it first for a kind of privet or ligustrum, which grew particularly in Egypt, and afterwards he thought it to be the common ligustrum of Europe: this shows how uncertain he was as to the plant in question. He judged ill in comparing the fruit of the cyprus with that of the jujube-tree; but was more happy in likening the fruit (capsule) to that of the coriander, as they agree in colour, though that of the cyprus was larger. Matthiolus, who thought himself greatly above his contemporaries in the theory of plants, asserts boldly, that this plant was the common privet: and in this he thinks himself justified, not only from the description of Dioscorides, but from the virtues attributed to the cyprus by Pliny. He even ridicules those who think that the ligustrum and cyprus are different plants. Fuchsius, who wrote before Matthiolus, had nevertheless reason to believe them of a different genus, by the account given of the Egyptian plant by Pliny; but he was wrong in confounding it with the phillyrea of Dioscorides, and in this mistake he has been followed by Dodonæus.

Bellonius, who had seen this plant in its place of growth, well knew that it was not the ligustrum or privet: he saw also how the commentators of the Arabian authors were deceived in taking it for such.

Rauwolf and Prosper Alpinus, who met with it in their travels, after having observed it in the places of its growth, believed, as Pliny had done, that it was a kind of ligustrum, which approached very near to that of Europe. They have each of them given a different figure; which made Caspar Bauhin believe that there must be 2 new species of ligustrum; but herein he was not followed by the ingenious Mr. Ray. In fact, we ought to acknowledge, by the characters here set down, that our cyprus is of a genus truly different, and the only one of its kind.

The Hortus Malabaricus has given a figure of this plant under the name of mail-anschi, which represents the end of a large branch ill chosen, and somewhat withered, without doubt by the fault of the designer, who has drawn it in its natural size; which is greater in Malabar than elsewhere, because of the rains which fall there in abundance half the year. This shrub is less in all its parts in Arabia, and to the south of Persia, because in those countries it rains seldom; but in recompence, its flowers have much more smell than in Malabar. It must be remarked here on this occasion, that the description just now given, and which contains the size of the parts, was made in a garden in the Persian gulf belonging to the Dutch factory, and situate about a league from the town of Gameroon, otherwise called Bender-Abassi, where there was one of these trees carefully preserved, which was the first he saw in the Indies; as it was complete in all its parts, having flowers and fruit; and as it appeared agreeable and curious, especially on account of the fine smell of the flowers, and as it was a new genus to be established in botany, he examined it with great exactness, and noted its characters, figures, and dimensions. He did not conceive it to be the cyprus, not then knowing what it was. He asked the people of the country the name of this beautiful shrub: they only called it henna, and he could learn no other name: they assured him it had no other name, either in Persia, or in Arabia. It was on the 1st of December, 1721, that he observed it, and described it under the name of frutex Persicus, foliis ligustri, flore et fructu racemoso, henna vulgo dictus. He thus characterized it, in expectation of finding it, if it had already been described among authors, after his return to Europe. When he returned in 1730, he had the satisfaction to find it in Mr. Ray's history, by the description which he has given of it, extracted from various authors, in the chapter of ligustrum, under the synonyma of Parkinson, and to see it in the other authors above mentioned, especially the figure given by Rauwolf, which is not a bad one, and which is copied by Clusius, Dodonæus, Parkinson, and Dalechamp.

The figure in the Hortus Malabaricus under the name of mail-anschi, does not so happily represent our cyprus, as that excellent work generally does the plants it treats of. The leaves of this plant there are half withered, and not in their natural disposition. Rauwolf's figure is much nearer the truth. The flowers are not much better represented than the leaves, in the Hortus Malabaricus; as, besides other things of less moment, the authors of that work have neglected to make the petals appear between the lobes of the calyx, as always happens in a natural state; by which disposition the flower appears of an octagonal figure. Rumphius, who has written a History of the Plants of Molucca, has given a description of this shrub, not different from mine.

By what is here laid down of the characters of this plant, we plainly see that it differs widely from the oxyacantha and rhamnus; of one of which the authors of the notes to the Hortus Malabaricus suspected the cyprus to be a species.

This occasioned Mr. Ray to range it under the last, supposing its fruit to be a berry, which it is not. This learned author could not think that the mail-anschi was the cyprus, because of the difference in the descriptions among authors, and of the imperfection of those of Rauwolf and Alpinus. Rumphius, just now quoted, has ill compared the colour of the leaves of cyprus to those of the olive-tree.

This shrub, so cherished among the Eastern nations, is cultivated in Africa, Asia, and all the Indies; that is, from near the equinoctial even to 35° of north latitude; where it is much used, as we shall find by the great commerce with it in the Levant, according to the relations of travellers of credit.

This plant does not love shade, even under the torrid zone, because of the violent rains there at the time of the western monsoon, no more than it does in cold countries, our author means those of the 5th climate; but towards the tropic, and even in Arabia, it grows best when a little sheltered from the sun. In hot and dry countries, as in the Persian gulf, where he first saw it, it produced a great number of boughs and branches very short, which gave it the appearance of white thorn. On the contrary, towards the equator, its branches are farther from each other, and longer, occasioned by the moisture from the rain. The back splits into scales, and detaches itself in pieces from the trunk, in those countries where it rains seldom; but in Malabar, in the isles of Ceylon and Sunda, the back continues entire and united almost all the year, because of the moisture of those places.

Rauwolf remarks, that the Turks and Moors cultivate this plant with care, and even keep it in pots, on account of the smell of the flowers, which somewhat resemble musk. They keep these pots in winter in chambers or caves, to preserve the plants from cold.

The Uses of Cyprus.—Bellonius, who was the first of the moderns who treated of this shrub under the name of alcanna, and spoke of its culture in Egypt, tells us, that the powder of its leaves is so great an article of commerce among the Turks, that they load several vessels from Alexandria for Constantinople, where the sale of it is so great, that the Grand Seignior's revenue from it amounts yearly to 18,000 ducats. According to him, the great consumption of this powder arises from its being used in beautifying the skin and nails, in making them red by a decoction made with it. The women, he says, generally use it all over Turkey, to dye the skin of those parts which are from the navel downwards, as well as their hands and their hair. Their children are served in the same manner. They consider this as a great ornament; and that the colour may hold longer, and penetrate deeper, they apply it usually when they go out of the baths. This practice of dyeing, to beautify the body, is extended even to their horses, of which they tinge the mane, the tail, and the hoofs. They often add alum to heighten the colour. This powder is sent from Constantinople to Russia. Let us now consider the other properties of cyprus.

It is not necessary here to take notice of what Dioscorides and Pliny attribute to this plant; they may be consulted, if, at the same time, they are regarded as being very little skilled in its true qualities. Our author contents himself with saying, that the Persians and Arabians, who appear to have been anciently the first that used this plant, frequently use at present not only its flowers to perfume their linen, their cloths, and their tables, but make a greater use of its leaves in a decoction, for the cure of all distempers of the skin, as the itch, scabs, and ring-worm, which the air of their country causes from its heat, and from the drought which often reigns there to a great degree. These disorders, if neglected to be cured as soon as possible in dry climates, easily degenerate into the leprosy; and it is on account of these disorders of the skin, that the eating of pork is forbidden to people of every religion in these countries; because that food there is known to occasion these distempers.

All the nations of the East Indies make use of it in medicine, for the same, as well as for several other disorders; but they particularly use the leaves to dye their nails; which our author thinks they had originally from the Arabians. In dyeing their nails, the Indians make use of the fresh leaves, which always grow in great plenty in their gardens, and apply them beaten on their nails, mixing with them sometimes a little lime and juice of citron. This colour lasts a great while on the skin, on account of sweating. A strong decoction of the leaves in water is sometimes used to tinge their nails, but more generally their skin and hair.

There is reason to believe, that this pretended beautifying of the skin, the hair and nails, which long custom has established among the Eastern nations, owes its origin to a quite different principle than that of beautifying. The ancients had no other view in the beginning, than the prevention of pruriginous and leprous disorders in the skin, to which their climate subjected them, as well as to preserve them from vermin, as the leaves of cyprus have that property. But as in using baths with these leaves in them, they dyed their skin either red or yellow, according to the preparation, they accustomed themselves to this colour by degrees, and afterwards regarded it as a salutary embellishment.

These baths, which are here constantly employed for the cleanliness and health of the skin, and which the necessity of using has established as a point of religion, and a duty, for the better prevention of these maladies, is certainly a true method to preserve both the body and the skin in a good state. These good effects are extended further by using the alcanna; because its colour, passing in the opinion of these people for a necessary ornament, and a mark of cleanliness, makes the practice of bathing better observed.

Of an Ancient Shrine, formerly belonging to the Abbey of Croyland. By Wm. Stukely, M.D. N° 490, p. 579.

This shrine is a great curiosity, few of this kind of antiquities having escaped the general ravage of the dissolution of abbeys. The shrine is made of oak, plated over with copper, on which the figures are chaced in gold: the ground is enamelled with blue; in the ridge along the top are 3 oval crystals set transparently; its dimensions are 12 inches long, 10¼ high, 4¾ broad.

It was found in the house of a gentleman near St. Neot's, who never showed it during his life-time; and who possibly might have given us some account, as to the history of it; but at present we have no means left of finding it out, but by conjecture.

Dr. S. conceives it came from Croyland-abbey. There was an intercourse between this abbey and St. Neot's priory; insomuch that St. Neot's body was carried hence to Croyland-abbey, and inshrined there.

These shrines were made for receiving relics of saints, in old abbeys, churches, and cathedrals. These were carried about in processions on their anniversary days; sometimes embellished with jewels of inestimable value. Besides these portable ones, there were others, built of stone, marble, and other materials; like that of St. Edward the Confessor in Westminster-abbey; one now in Chester cathedral of St. Werburga, on which the episcopal throne is set, adorned with sculptures of Saxon kings and saints: one of St. Thomas de Cantelupe bishop of Hereford, in that cathedral. These now remain. There was one in the church of Burton-Coggles, Lincolnshire; and of Heckington in the same county; and innumerable others, destroyed at the dissolution of monasteries.

The shrine before us, from the manner of drawing and workmanship, he concludes to be of Saxon antiquity, and that very high, now near 900 years old. He thinks it gives us the story of the murder of the abbot there, and his monks, perpetrated by the barbarous Danes, in the year 870. Sept. 25, that year, they rushed into the church of Croyland, while the religious were at divine service. Ingulphus, abbot of the place, in his History gives us this account. Lord Theodore was then abbot of Croyland; who at that time pontifically officiated at the high altar, expecting the barbarians. King Osketyl cut off his head on the altar. "Verus martyr et Christi hostia immolatur," says our author, "Ministri circumstantes omnes capitibus detruncati:" "Thus fell the true martyr and lamb of Christ, as a sacrifice on the altar. All the assistant ministers were beheaded likewise," says he. The 2 on our shrine, are friar Elfget the deacon, and friar Savin the subdeacon. Some days after, when the monks that fled returned, they found the body of the venerable abbot Theodore beheaded at the altar.

Above is represented his successor abbot Godric, with the ministers about him, putting the deceased abbot into his shroud; while angels are carrying his soul up to heaven. Perhaps some part of this martyr might be obtained, and kept in this shrine.

The famous old sepulchral stone in Peterborough Minster-yard is exactly of the same shape as this shrine. It was set up over the grave of the abbot and monks murdered by the same Danes, the day after those of Croyland abbey suffered, Sept. 26. It is carved on the sides with the images of our Saviour and the Apostles. It is now removed into the library.

An Eclipse of the Sun, July 14, 1748. Observed by the Earl of Morton, Mr. le Monnier, Royal Astronomer and Member of the Royal Academy of Sciences at Paris, and Mr. Ja. Short, Fellows of the Royal Society. By Mr. Short.* N° 490, p. 582.

These observations were made at Aberdour castle, belonging to the said earl, in latitude 56° 4′ N.

Mr. le Monnier having come over from France to go to Scotland, to observe the annular eclipse of the sun, July 14, 1748; Mr. S. and he accompanied earl Morton down to that country. On arriving at Edinburgh, they found that the meridian mark, which had been settled from observations, by the late Mr. Mac. Laurin, was lost, by the taking down of a chimney, on which it was fixed; and Mr. Matthew Stewart, the present professor, having no proper instruments, had not as yet re-established it: this they hoped to do by an instrument, which they expected from London; and Mr. Stewart having promised to make the best observation he could, they set out for Aberdour, a seat of the Earl of Morton's,

* Peter Charles le Monnier, (son of Peter le Monnier, Profes. of Philos. at Paris) was born at Paris, Nov. 20, 1715, and died at Lizieux in Normandy, April 2, 1799, in the 84th year of his age, and then the oldest astronomer in Europe. His observations and memoirs, to a vast number, are chiefly contained in the Memoirs of the Royal Academy of Sciences; besides which he published the Histoire Celeste, 1741, in 4to. In this work is twice found, but only as a fixed star, Dr. Herschel's new planet. From his earliest years he devoted himself to astronomy: when a youth of 16, he made his first observations, viz. of the opposition of Saturn. At 20, he was nominated a member of the Royal Academy of Sciences. In 1735 he accompanied Maupertius in the expedition to Lapland to measure a degree of the meridian. And he was the first astronomer who had the pleasure to measure the diameter of the moon on the sun's disk. In 1750 he drew a meridian at the royal chateau at Bellevue, where the king often made observations. Le Monnier was naturally of a very irritable temper; as ardently as he loved his friends, as easily could he be offended; and his hatred was then implacable. Lalande, who had been his pupil, had the misfortune to incur his displeasure; and he never after could regain his favour. At the time of Le Monnier's death, he had amassed a vast quantity of observations, which he could never be prevailed on to publish, but concealed them in a place, which it was feared he had forgotten; so that it has been supposed they were lost to the world, unless the place should happen to be known to the celebrated mathematician La Grange, who married one of his daughters in 1792.

about 8 miles nearly N.W. of Edinburgh; which place was chosen as being, by the computations of this eclipse, at or very near the southern limit of the annulus.

In the castle of Aberdour, lat. 56° 4′ N. and 25^s of time west of the college of Edinburgh, they set up a clock, July 9; but the weather being cloudy, and the equal-altitude instrument and transit not being yet arrived, they on the 11th made use of an equatorial telescope of Lord Morton's, to find corresponding altitudes of the sun, and at the same time set up a gnomon of 15 feet high.

The 13th being a clear day, they took equal altitudes with the equatorial telescope, and found the clock gained $1^m\ 46^s$ in 2 days, and that the sun passed the meridian at $12^h\ 7^m\ 6^s$ by the clock.

July 14th was an exceedingly bad morning, both for wind and rain; but about 8 in the morning, the clouds dispersed, and they had a very clear sun. To observe the eclipse, Lord Morton made use of a reflecting telescope, 12 inches focal length, magnifying about 40 times. Mr. Short made use of a reflecting telescope 4 feet focus, magnifying about 120 times; both belonging to Lord Morton. Mr. le Monnier made use of a refracting telescope, about 9 feet focus, which he brought with him from France, armed with a micrometer, made after the method of Mr. George Graham, by the late Mr. Sisson at London. Mr. le Monnier took his station in the garden, under the window of the room where the clock was placed; Lord Morton was in the room next that where the clock stood; and Mr. S. was at the window next the clock.

True Time.

$8^h\ 47^m\ 5^s$ The eclipse not yet begun. Clouds come on.

8 51 18 Beginning of the eclipse, found by the following chord.

8 52 47 First view of the eclipse, then considerably advanced.

8 54 35 Measured the chord of the part eclipsed; which was found equal to the field of the great reflector.

9 58 12 The illuminate part of the sun, measured by the micrometer, and found = 7′ 37″½.

10 37 0 Again measured, and found = 7′ 37″½.

Lord Morton judged the middle of the eclipse, or nearest approach to an annulus, at $10^h\ 17^m\ 54^s$ apparent time.

11 44 40 The same phase or chord observed as at the beginning, and measured both in the telescope, as at first, and by the micrometer, and found = 8′ 25″ of a great circle, as verified by a base after the eclipse was over, which gives the end as exact as the beginning.

11 48 18 End of the eclipse by the preceding chord.

Mr. le Monnier measured with the micrometer the apparent equatorial diameter of the moon, when she was on the sun; which he found = 29′ 47½″. He measured also the apparent vertical diameter of the sun at noon; which he found = 31′ 40″.

The eclipse was so nearly annular, that, at the nearest approach, the cusps seemed to want about ⅐ of the moon's circumference to be joined; yet a brown light was plainly observed to proceed or stretch along the circumference of the moon, from each of the cusps, about ⅓ of the whole distance of the cusps from each cusp; and there remained about ⅓ of the whole distance of the cusps not enlightened by this brown light; so that they were for some time in suspense whether they were to have the eclipse annular. Mr. S. observed, at the extremity of this brown light, which came from the western cusp, a larger quantity of light than at any other place, which at first surprised him; but afterwards he imagined it must have proceeded from some cavity or valley made by two adjoining mountains on the edge or limb of the moon. He had often formerly observed mountains on the circumference of the moon, more or less every where round it, but never saw them so plain as during the time of this eclipse; for the air was exceedingly clear, and free of all agitation, though it blew a perfect hurricane of wind, which began about the middle of the eclipse; and he remembered that in the annular eclipse of the sun in the year 1737, it did the same. The mountainous inequalities on the southern limb of the moon were particularly remarkable; in some parts mountains and valleys alternately; others extended a considerable way along the circumference, and ended almost perpendicularly like a precipice. Lord Morton was able to see them very easily through his small reflector.

A little after the middle of the eclipse, some clouds, that seemed stationary below the sun, appeared tinged on their upper extremities with all the colours of the rainbow.

During the greatest darkness, some people, who were in the garden adjoining to the castle, saw a star to the east of the sun; which, when they afterwards pointed to the place where they had seen it, showed that it must have been the planet Venus. This star, they were afterwards told, was seen also at Edinburgh, and other places, by a great number of people; but he did not hear of any other stars being seen. The darkness was not great, but the sky appeared of a faint languid colour. What is pretty remarkable, Mr. le Monnier found, that when he looked at the sun with his naked eyes during the middle of the eclipse, he could observe nothing on the sun, but saw the sun full, though faint in his light. This, Mr. S. imagines may be owing to his being short-sighted.

Mr. S. observed also, about the middle of the eclipse, a remarkable large spot of light, of an irregular figure, and of a considerable brightness, about 7′ or 8′ within the limb of the moon next the western cusp. He thought he lost this light several times; but whether this was owing to shutting his eyes, in order to relieve them, or not, he cannot tell. He was told, that the Rev. Mr. Irwin at Elgin observed the same. When Mr. S. first perceived it, he called to Lord Morton, who was in the next room, but he could not see it.

Before the eclipse began, and during the whole time of the eclipse, the air being exceedingly clear, Mr. S. saw through the 4-foot reflector, the surface of the sun covered with something which he had never observed before; it seemed to be all irregularly overspread with light, and a faint shade, especially towards his equatorial diameter. This appearance was so odd, that it is difficult to describe it, so as to give an adequate idea of what he saw; but if he may be allowed the expression, it seemed as if it were curdled with a bright and more dusky light or colour. This appearance was permanent, and regularly the same; and if in any degree seen before, may have given rise to faculæ having been seen in the sun; but to him the whole sun's body seemed to be more or less covered with it.

Mr. S. looked with all the attention possible, to see if he could observe the body or limb of the moon before she touched the sun, and also after she left it, and was entirely off the sun; but could see nothing at all of any such appearance. He mentioned it to satisfy Mr. Delisle, who publicly desired this might be attended to.

The weather being very bad at Edinburgh, Mr. Matthew Stewart, the professor of mathematics, could make no observations of the eclipse; he only saw the end at $11^h\ 50^m\ 34^s$ true time; and even then the sun was somewhat cloudy; he took however the sun's transit over the meridian, as then supposed, at $12^h\ 7^m\ 42^s$ by his clock. They afterwards, in a few days, examined his meridian mark, with a very exact equal altitude instrument, by 3 several correspondent observations; and found his mark $3^m\ 22^s$ of time to the west of the true meridian. The college is about 2500 feet distant from the castle eastward.

The Rev. Mr. Bryce, at Aldiston, about 6 miles to the west of Edinburgh, lat. 55° 55½′ N. observed with a reflecting telescope, 9 inches focus.

Beginning of the eclipse at	8^h	52^m	30^s
Middle of the eclipse, as near as he could judge	10	17	40
End of the eclipse, the sun being quite clear	11	48	40

Mr. Short has set down the following observations of this eclipse just as they came to hand when in Scotland, without making any other remark than that, from the disagreement among themselves, they do not all of them seem to have been made with due accuracy and attention; for want probably of sufficient practice in this kind of observations,

William Crow, Esq. at his house of Netherbyres near Haymouth, lat. 55° 51′ N. says,

The eclipse began at .... 8^h 55^m 0^s
Half of the sun eclipsed at 9 50 0
Middle of the eclipse, $\frac{1}{6}$ of the sun's limb covered by the moon at 10 25 0
End of the eclipse at 11 55 0

Mr. John Mair, at Air, lat. 55° 30′ N. says, the eclipse began at 8^h 45^m, but that, by reason of clouds, he could make no other particular observation; only that, by a view he had of the sun some little time before the end, he thinks the end of the eclipse might be about 11^h 48^m.

Mr. Mark, teacher of the mathematics at Dundee, lat. 56° 25′ N. observed,

The beginning of the annular appearance at .. 10^h 16^m 44^s
End of the annular appearance at 10 23 8

He says, the best observations make the annulus a small matter narrower on the upper than lower side; by which it appears the centre of the eclipse was to the northward of Dundee.

Mr. John Stewart, professor of mathematics at Aberdeen, writes, that by an observation made at Montross, lat. 56° 41′.

The annular appearance began at .... 10^h 20^m 0^s
Annulus ended at 10 24 30
End of the eclipse at 11 52 45

And that, by an observation made at a place about 18 miles S. W. of Aberdeen,

The eclipse began at .... 8^h 52^m 0^s
Middle at 10 21 0
End at 11 52 0

And that at Aberdeen, lat. 57° 11′ N.

The eclipse began at .... 8^h 55^m 33^s
Middle of the eclipse, and annular appearance,
as near as he could judge, at 10 23 3
End of the annular appearance at 10 24 48

He writes also, that he received an account from Mr. Reid, minister at New Macchar, about 7 miles N. W. of Aberdeen, who observed

The beginning of the annular appearance at .. 10^h 18^m 28^s
And the end of the eclipse at 11 49 3

Mr. Stewart says, that, by comparing his observation at Aberdeen with this of Mr. Reid's, he apprehends he is in a mistake as to his judging of the middle of the eclipse, and annular appearance; and reckons, that the annular appearance began at Aberdeen at 10^h 19^m, and ended as above. By which the total duration of the annulus was 5^m 48^s; and the end of the eclipse at Aberdeen was at 11^h 49^m 33^s.

The Rev. Mr. Irwin, at Elgin, lat. 57° 34′, says, the eastern limb of the moon touched or entered on the western limb of the sun at 8^h 57^m; though he suspects it began a little sooner, another having taken the telescope out of his hand; for when he looked, the moon was a little advanced on the disc of the sun about 30° from the zenith of the sun towards the west.

The eastern cusp in the zenith of the sun at	9^h	6^m	10^s
Eastern limb of the moon reached the centre of the sun at	9	39	0
The annulus began about 30° from the zenith of the sun westward at	10	20	0
The annulus appeared most perfect at	10	22	45

Though, as nearly as he could discern, he thought it a little narrower on the south-west limb of the sun, than it was on the opposite side. From hence it should appear, that the centre of the eclipse was to the southward of Elgin.

The annulus was observed to break on the south-east limb of the sun, about 30° from the Nadir, at $10^h\ 25^m\ 30^s$.

Before the joining of the cusps of the sun, as also at the breaking of the annulus, he says, he observed a quick tremulous motion, and several irregular bright spots between the cusps, which disappeared in a few moments; and he thought the moon's body passed quicker about the time of the annulus, especially as it was forming, than at any other time during the eclipse.

Before the western limb of the moon reached the centre of the sun's disc, the sun was hid under a cloud, and continued so till within some little time of the end of the eclipse, which happened at $11^h\ 50^m$.

There was no cloud all the time of the formation of the annulus, or the duration of it; and he thinks he is pretty right, as to the time of its continuance, for both the formation and breaking were very sensibly to be observed, and passed in a moment; affording a very pleasing sight, by the irregular tremulous spots of the sun.

He says, the darkness, during the annulus, was not so great as a little before and after; and, when greatest, was only somewhat duskish, but observable. Some saw a star to the east of the sun; but he saw it not, nor any present with him. He was told of it after his observation was over.

He says, that, by an observation taken of the sun that day at noon, he found that his clock was somewhat less than a minute faster than the sun. He says also, that he observed this eclipse with a telescope 3 feet long, and that he had a very good burning-glass; but that it had little force during the annulus, and some short time before and after.

Mr. Duncan Frazer writes to Mr. Monro, professor of anatomy at Edinburgh, that he went to the house of Culloden, lat. 57° 29′ N. on purpose to observe the eclipse; it having been said, that the centre of the eclipse would pass there; and after having adjusted his clock by the regulator clock of a watch-maker at Inverness, he observed the eclipse with a telescope 5 feet long, and found

The beginning precisely at	8^h	37^m	36^s
Beginning of the annulus at	10	0	10
End of the annulus at	10	5	10
End of the eclipse at	11	29	30

By comparing his observation with that sent him by Mr. Irwine at Elgin, he imagines his clock was not set to true time, since there is so great a difference, and more than the difference of longitude between the two places will allow, it being no more than 26 computed miles, and nearly in the same parallel of latitude.

Mr. Murdock Mackenzie, who has for some years past been making a survey of the islands of Orkney, and whose abilities for such an undertaking gives us hopes he will for the future free navigators of a great many melancholy disasters, which formerly happened in those seas, through the want of true charts, made the following observation at Kirkwall in the island of Pomona in Orkney, the latitude of which is 58° 58′ N.

Beginning of the eclipse about	8^h	40^m
End of the eclipse about	11	37

He says, that, by reason of clouds, he could not be perfectly exact, as to the precise time of beginning or ending; but adds, that the beginning cannot be more than 4^m wrong, nor the end more than 2^m. He says, he is sure he did not see it annular, but that there remained about $\frac{1}{4}$ or $\frac{1}{5}$ of the sun's circumference intercepted at the middle of the eclipse.

P. S. It having been an opinion pretty generally received, that the darker parts of the moon's surface are water, Mr. S. takes this opportunity to remark, that though those less lucid spaces are for the most part, to appearance, evenly extended surfaces, when telescopes of small magnifying powers are made use of; yet when they are examined with larger magnifiers, it is easy to discern on them many protuberances in a longitudinal direction; and that these risings are really elevated above the common plane surface, is past all question, from their projecting shadows, always opposite to the sun; moreover they are of the very same colour as the plane they arise from, of the like smooth surfaces, without any sensible asperities; and invariably the same, under the like positions of the sun to the moon, at least as far as he has been able to discover in 12 or 15 years frequent observations of them.

On two Extraordinary Belemnitæ. By Mr. David Erskin Baker. N° 490, p. 598.

Various have been the opinions of authors concerning the origin of the Belemnitæ, and as various the systems and hypotheses advanced by them in support of their opinions; some having imagined them vegetable productions; others have taken them for the different parts of animals, as teeth, horns, bones, &c. in which even these again have differed, as to the referring them to land or ma rine animals; and they have been by others supposed of mineral origin, or lapides sui generis. What they really are, will probably be still very difficult to determine; but as one principal objection to their being originally marine bodies, which supposition seems to carry the greatest colour of probability, has been, that no marine bodies have been found adhering to them, that objection will be obviated by no less than two specimens, from the same place, of belemnitæ, to which undoubted marine substances are found firmly affixed. These curious fossils were found in a chalk pit in Norfolk; whence they were sent not long before to his father, Mr. Henry Baker, F.R.S.

Fig. 2, pl. 10, is a belemnites, whose apex is perfect; the conic cavity, and the longitudinal seam, evidently distinguishable; which, as well as the contexture of its substance, show it to be a true belemnites; but on its surface are placed, in their natural condition, by which he means, not at all seemingly petrified, or otherwise altered, two of those vermiculi so frequently found sticking to oysters, scallops, and many other kinds of shells, when taken out of the sea.

Fig. 3, a frustum of another belemnites, the apex broken off, but the conic cavity is still remaining, and shown at a. To this belemnites adheres a shell of the oyster kind, which is fastened so strongly, that they are not to be separated without breaking; which shell, as well as the before-mentioned vermiculi, seems not altered in its substance, but appears like a recent one, of which many are to be met with in the cabinets of the curious.

Fig. 4, shows the other side of the said shell, where the cardo or hinge at b is plainly discernible; at c appears the broken end of the belemnites, where the radiated contexture, well known to belong to their bodies, is represented, as also the longitudinal seam at d.

As these specimens are undeniable proofs of marine bodies adhering to belemnitæ, several of the curious who have seen them, are of opinion, that they tend likewise to prove the belemnitæ to be marine productions. It may probably be objected, that these shells might have been brought and deposited near the belemnitæ to which they are affixed, by whatever mighty change it came to pass that productions of the sea are discovered in most countries at great depths in the earth, and in the bowels of mountains at great distances from the sea, even supposing the belemnitæ to be lapides sui generis, and produced in the earth, and that these shells might be cemented to them afterwards by some mineral, stony, or other matter. But the following observations will render this improbable; for,

1. The vermiculi of fig. 2 are not any species of the tubuli marini, found

sometimes recent, and sometimes fossil, detached entirely from every other body; but are of that sort, which is perhaps never seen separate, or in any other manner, when recent, than attached and fastened to other shells or stones, and they are placed on this belemnites exactly in the same manner as they are commonly found on other marine bodies, viz. lying on their broadest side, with their ridge upwards, and glued as it were by a shelly substance.

2. In fig. 4, at e is plainly to be distinguished, that the shell has been fashioned thus by the convex surface of the belemnites, in the same manner as these shells commonly receive a form from whatever substance they adhere to; which plainly implies, that this shell was fastened to the belemnites when itself was very small, and in a growing state; and that the shell in its growth was formed according to the figure of the body on which it was affixed; but such growth could not possibly have proceeded any where but in the sea; and therefore these two bodies must necessarily have been in the sea at one and the same time.

There is now but one way more, by which these shells, supposing the belemnitæ to be stones sui generis, could possibly become affixed to them; which is, that the belemnitæ might have been by some accident thrown on the sea-shore; and that there the shells might fasten themselves to them, as well as to any other stone. But as this must imply some former convulsion in nature, by which they were cast out of their natural beds on the sea-shore; and again a second convulsion to carry them to the chalk pit where they were found: so far-fetched an objection will, he believes, carry but little weight.

Of a mixed Breed of Apples; from the Mixture of the Farina. By Mr. Benj. Cooke, F. R. S. N° 490, p. 602.

Mr. C. sent last year a specimen of the effect of the farina of a rough coat apple striking on the flower of a smooth coat; he has now sent an example of the farina of the latter changing the former into its own dress and likeness.

The situation of the russeting was such, that it was surrounded by winter pippins, pearmains, and such-like; and they put the master fruit together with several of the changelings, as they grew on the same branches mixed together.

This instance will show what alterations may be expected in cognate species; and he should have given an example of a kind of antipathy between the pear and the apple in like circumstances, but was disappointed.

A Description of the Town of Silchester in its Present State. With a Short Account of an Ancient Date in Arabian Figures at Walling, near Aldermarston in Berkshire. By John Ward, F.R.S., P.R.G. N° 490, p. 603.

In a former paper,* Mr. W. attempted to explain a Roman inscription cut in

* Phil. Trans. N° 474.—Orig.

a stone, then lately found at Silchester in Hampshire; by which it only appeared, that this town was the ancient Vindomis, but also that it was situated within the limits of the Segontiaci: as to both which circumstances our best antiquaries have been at an uncertainty, and differed in their sentiments concerning them. He took notice also, that the traces of the ancient town are yet often visible in the summer; and that the ruins of an amphitheatre still remain without the wall. But being since in that country, he had an opportunity of visiting the place himself, and getting a more perfect account of it than he expected, by the assistance of two persons in the neighbourhood, the late Mr. John Wright, junior, and Mr. John Stair, junior, who were both well acquainted with it, and accompanied him thither. The former, who was an experienced surveyor, measured the whole circuit of the wall, with the height of it in several places, as also the dimensions of the amphitheatre, while they were on the spot. And the other traced out the several streets, and other parts of the town, to a considerable exactness.

The circuit of the wall on the outside, as given by the scale, contains near one English mile and a half; and the several parcels of land contained within it, amount together to 100 acres, or upwards. Indeed Leland says, that the compass of the wall is about 2 miles, and containeth 80 acres.* And Camden says the same, except that he calls them Italian miles.† But neither of them acquaint us, from whom they had their measurement. The wall consists of 9 sides, but very unequal; which might perhaps be occasioned by the different situation of the ground, which in some parts is uneven.

The materials composing the wall, are large flints, and rough stones of different sorts, cemented together with very strong mortar. And as to the manner of building it, the foundation is generally made of a row or two of stones laid flatwise; and over them 4 or 5 rows of flints; then usually a double row of stones, sometimes 3 rows, and at other times one only, laid in the same position; over these a like number of rows of flints, as before; and so alternately upwards. And a little to the westward of the south gate are yet to be seen 7 of these ranges of stone, with 6 of flint between them; where the height of the wall measured on the outside is about 18 feet. And about 50 yards eastward of the same gate are 6 ranges of stone, with 5 of flint between them; where a small part of the facing seems yet to be nearly entire. But there is no appearance either of copings, or battlements, on any part of the wall. Though the ranges of stone in the front of the wall are placed horizontally, yet those within it often stand edgewise and somewhat obliquely, like the wall of Severus in the north of

* Itinerary, vol. vi. p. 48, edit. 1744.—Orig. † Britann. p. 196, edit. 1607.—Orig.

England.* And at the south gate the thickness of the wall measured about 5 yards. From this account therefore it seems not improbable, that in the passage of Leland, given by Mr. Hearne from Stowe's transcript, where it is said 'the wall without is in some place 6 or 7 feet high,'† for the numbers 6 or 7 should be read 16 or 17.

The wall is not any where entirely demolished, except that 2 breaches have been made of late years on the north-west side, to open a passage for waggons. And the ditch without the wall is in some places 10 or 12 yards over, but in others at present not visible; where probably it may have been filled up by the earth thrown into it from the vallum, that incompassed the city between that and the wall, and which is yet in several places of a considerable height above the ditch. There is little appearance of the vallum, or military way, within the circuit of the wall, the ground being now more generally raised pretty near the top of the wall, on which grow many large oaks, and other timber trees. From the south gate towards Winchester has lain a military road, which, when broken up, appears to have been pitched with flints.

The amphitheatre stands without the wall, at the north-east corner, and distant from it upwards of 100 yards. Both the wall and seats, which are made in it, consist of a mixture of clay and gravel. The wall is about 20 yards thick at the bottom below the seats, and decreases gradually to the thickness of about 4 yards at the top. There are 5 ranges of seats above one another, at the distance of about 6 feet on the slope. It has two passages into it, one towards the town, and the other opposite to it. The diameter of the area is 50 yards by 40, and the area itself now serves for a pond to a farmer's yard. The design of this amphitheatre might possibly be for the baiting of wild beasts, or other athletic diversions, agreeably to the customs of those times. Though at present no appearance of a cavern, or any other place proper for the reception of such animals, is to be discovered.

The area of the town within the walls contains at present only corn fields, except a small quantity of meadow land, with an ancient church, and farm house, near the east gate. The method taken by Mr. Stair, in order to discover where the streets formerly lay, was by observing for several years before harvest those places in which the corn was stunted, and did not flourish as in other parts. These were very easily distinguished in a dry summer, and run in straight lines crossing one another. Also by spitting the ground, and often digging it up, he found a great deal of rubbish, with the plain ruins and foundations of houses on each side of these tracts. Whereas in the middle of the squares nothing of that nature appeared, and the corn usually flourished very well. The

* See Horsley's Britann. Rom. p. 123.—Orig. † Ubi supra.—Orig.

ploughmen also confirmed the same, who found the earth harder, and more difficult to be turned up, in these tracts and near them, than elsewhere. And it is further observable, that 2 of these streets, which seemed rather wider than the rest, lead to the 4 gates of the city, one of them running in a direct line from the north to the south gate, and the other from the east to the west, which latter measured at least 8 yards across.

By digging also in different places, Mr. Stair at length discovered the ruins of a number of buildings in the form of a long square. The foundations of some of these buildings were still pretty entire, and their depth from wall to wall was found to be about 27 feet, and the breadth about 16, which it is not improbable may be the remains of the ancient forum. What remained was about 3 feet in height, 4 in length, and 3 in breadth. It consisted of large Roman bricks, one of which dug up entire, is 17¼ inches long, 12¼ broad, and 2¼ thick; which accompanies this paper.

On further search where was found the stone with the inscription on it mentioned above, Mr. Stair has since dug up, about 4 feet under ground, a square copper frame, composed of several mouldings and its sides soldered together; 3 of which are yet entire, but part of the 4th is broken. This frame inclosed a border of the same metal, one side of which is still preserved. Their weight together is 47lb; but the thickness of the frame varies in different parts, from ¼ of an inch to much less, and the border is more than ¼ of an inch thick. Each side of the frame at the outer edge is about 33 inches long. And from the size of the inner edge of the border, each side of which is 20¼ inches in length, it is supposed that the stone, which contains the inscription, was at first placed behind it, and supported by it. And as that inscription was erected in honour of Hercules, it might originally belong to the stone building, whose foundation is yet visible, and which might have been a temple consecrated to that deity, by the persons named in the inscription; as was conjectured in the former account given of it. There was likewise a considerable number of brass Roman coins found near the same place.

The most valuable coin, represented fig. 5, pl. 10, which has been discovered in the ruins of this ancient Roman town, is a gold one of Allectus in fine preservation, and very remarkable for a peculiar attribute of the deity on the reverse. The front side represents the head of Allactus crowned with laurel, round which is this legend, IMP C ALLECTVS P F AVG. On the reverse is placed the figure of Apollo with a radiated crown; his left hand, which holds a globe, has over it a whip; his right arm is raised in a forbidding posture, and supports a chlamys, which crossing his breast descends on both his sides; at his feet sit two captives, whose hands are tied behind them; and the legend round it is

ORIENS AVG, with ML in the exergue.* These several attributes may be found in some or other of the imperial coins between the time of Gordian the younger, and the Constantines, or later. But the singularity of this coin is this; that in the figure of Apollo the eyes seem plainly to be covered by a fillet, which goes across the forehead; the reason of which he cannot account for. But we are told by Suetonius, that among other reflections thrown upon Augustus for a secret entertainment made by him, at which the persons present were dressed in the habit of deities, and this at a time of great scarcity in Rome, acclamatum est postridie frumentum omne deos commedisse; et Cæsarem esse plane Apollinem, sed tortorem. To which the historian adds, quo cognomine is deus quadem in parte urbis colebatur;† which being the place where criminals were punished, is thus described by Martial:

Cruenta pendent qua flagella tortorum.‡

It was not unusual for the ancients to apply the attributes of one deity to another on particular occasions. From whence one might be led to interpret this representation of Apollo, or the sun, with a whip, and a bandage over his eyes (the emblems of justice,) together with the two captives, as descriptive of the punishment denounced against all, who should attempt to oppose the government of Allectus. The place where it was found, of late years has gained the name of the Silver hill, because more silver coins have been found there, than in any other part of the city. And by the remaining ruins, which discover themselves on turning up the ground, it is supposed, that some large building stood anciently on that spot. But great numbers of coins in all metals, and of all sizes, have also been found in several other places; so that Mr. Stair is now possessed of several hundred, which have been all collected from this Roman settlement; among which are the emperors Valentinian and Arcadius in gold; with most of the imperial coins from Augustus to that time, either in silver or brass; many of which are exceedingly well preserved.

Mr. Ward subjoins here a brief account of an ancient date in Arabian figures, which yet remains at Walling, near Aldermarston in Berkshire. It is impressed in relievo on a brick, near the top of a large and high chimney, on the outside of a farmhouse belonging to William Wollascot, Esq. of Woolhampton, in that

* Some very skilful antiquaries have thought, that those letters on the reverse of many coins of the lower emperors, which are put at the bottom, often denote the place, where those coins were struck. And therefore, as he met with ML by themselves on no others, but those of Carausius and Allectus, who both ruled in Britain; it seems not improbable, that they may stand for moneta Londinensis, or Londini, supplying signata or incusa. As on some coins of Constantinus Magnus, who was first proclaimed emperor in Britain, we find MSL and MLI, which may also be so interpreted. Indeed the letters MLS and MLP occur on the coins of some other emperors, where they have been read, and perhaps justly enough, moneta Lugduni signata and percussa.—Orig.

† In vit. August. cap. 70.—Orig. ‡ Lib. II. epigr. 17.—Orig.

neighbourhood. This date had always hitherto been read 1182, the first two figures, as they are seen from the ground, having both the appearance of a one; with this difference only, that the second seems pretty much thicker than the first. And this led Mr. W. on viewing it in that situation to suspect it might be a 3, like that in the Cambridge date, published in the Philosophical Transactions, N° 474. And accordingly having by means of a long ladder an opportunity of going up to it, he found on a near inspection, that it was really so, as he had apprehended. For the small curves in the second figure being filled up with moss gave it the appearance of a broad and straight line, when seen at a considerable distance. The house, where this date remains, is by tradition said to have belonged anciently to a knight templer; but however that might be, the date must have been placed there long afterwards; as that order of knights was destroyed on the 7th of January, in the year 1307.

A Summary of some Observations on the Generation, Composition, and Decomposition of Animal and Vegetable Substances. By Mr. Turberville Needham, F.R.S. N° 690, p. 615.

After a review of the hypotheses entertained by different philosophers on the subject of generation, Mr. N. proceeds to give an account of experiments made by Mr. de Buffon and himself, on seed infusions. The first 4 infusions, among them one of almond-germs carefully picked out from between the two lobes and kernel, he mixed up at his own lodgings, and then closed them in phials with corks. The observations that occurred, were, first, a separation or digestion of the parts of these substances, and a continual flying off of the most volatile. These offuscated his glasses at every instant, and, according to the mixture yielded a fetid or an agreeable odour; particularly that of the almond-germs, one strongly spirituous. Eight days after they had been infused, he began to perceive a languid motion in some of the seed particles, that before seemed dead; such as gave him encouragement to prosecute his inquiry. It was visible, that the motion, though it had then no one characteristic of spontaneity, yet sprung from an effort of something teeming as it were within the particle, and not from any fermentation in the liquid, or other extraneous cause. A distinct atom would often detach itself from others of the same or less dimensions; and while these others remained absolutely unmoved, advance progressively for the space of 8 or 10 of its own diameters, or move in a little orbit, then fall off languid, rest between 2 others, and detach itself again and again, with a continuation of the same phenomena. The consequences of these were obvious, the motion was not spontaneous; for these atoms avoided no obstacle, nor had any other characteristic of spontaneity. It was not from any commotion in the fluid, fermentation, or the flying off of volatile parts; because a large atom would frequently move and detach itself from a much less absolutely quiescent: they did not seem to be enascent embryo animals, from a deposition of any extraneous spawn; for the phials had been closed with corks; nay, they were in the very seed, or the almond-germ particles themselves.

These same observations Mr. de Buffon made himself; for they examined these infusions together a 2nd time at his house; and then it was that he ordered 15 seed infusions to be made up, which they continued regularly to examine twice a week, till Mr. N. proposed to him to take them home, and follow them more closely by a daily or hourly inspection, if necessary. The result of the 1st observations was, that though the phials had been close stopped, and all communication with the exterior air prevented, yet, in about 15 days time, the infusions swarmed with clouds of moving atoms, so small, and so prodigiously active, that though he made use of a magnifier of not much above half a line focal distance, yet he was persuaded nothing but their vast multitude rendered them visible. It seemed therefore as if the first teeming languid particles they had observed, vast in their dimensions, if compared with those they saw, had broken and divided into this immense multitude of microscopical active atoms. Then it was that they began to lay down a distinction between animated and mere organized bodies; which, though far from being at this time groundless, yet afterwards proved to be false. These, and the spermatic animals, they supposed to be of the latter kind; and to be produced in their respective fluids, by a coalition of active principles, much as he had seen the calamary machines formed by hundreds, though absolutely detached, and swimming at liberty in the milt of the fish: while they thought, on the contrary, that the ordinary microscopical animalcules, with strong characteristics of spontaneous motion and animation, were to be classed among animals, and imagined them to proceed from parent individuals of their own species. It was not till some time after this, that, determined to convince himself and others, without any possibility of doubt, whether these moving atoms were really produced from without, or from the very substance infused: he discovered all the common microscopical animalcules, the spermatic ones not excepted, were to be ranged in the same class, and that their generation was very different from that of all other animated beings.

For his purpose therefore, he took a quantity of mutton gravy hot from the fire, and shut it up in a phial, closed up with a cork so well masticated, that his precautions amounted to as much as if he had sealed the phial hermetically. He thus effectually excluded the exterior air, that it might not be said the moving bodies drew their origin from insects, or eggs floating in the atmosphere. He would not instil any water, lest, without giving it as intense a degree of heat, it might be thought these productions were conveyed through that element. Seeds or plants were for this reason improper, because they might have been judged to have been previously adhering to these plants or seeds: he neglected no precaution, even as far as to heat violently in hot ashes the body of the phial; that if any thing existed, even in that little portion of air which filled up the neck, it might be destroyed, and lose its productive faculty. Nothing therefore could answer his purpose of excluding every objection better than hot roast meat gravy secured in this manner, and exposed for some days to the summer heat: and as he was determined not to open it, till he might reasonably conclude, whether, by its own principles, it was productive of any thing, he allowed sufficient time for that purpose to this pure unmixed quintessence, if it may be so called, of an animal body. From this time he took corruption entirely in a philosophical sense, for the rising of a dead substance, by a new kind of vegetation, into life: and no axiom, how much soever it may have been exploded, is more true than that of the ancients, corruptio unius est generatio alterius; though they drew it from false principles, and so established it as to render generation equivocal, and never penetrated sufficiently into nature by microscopes, to discover this class of beings, that are neither generated nor generate in the common way, yet furnish a key to lead to the generation of all others. His phial swarmed with life, and microscopical animals of most dimensions, from some of the largest ever seen, to some of the least. The very first drop he used on opening it, yielded multitudes perfectly formed, animated, and spontaneous in all their motions: and thus was he obliged to abandon not only the notion preconceived of a distinction to be made in this class of animals, between those that appeared under a sensible angle in the microscope, and the atomical ones; but even that hypothesis also which he had advanced as probable, in the little essay published in 1745, that spermatic animals were no more than multitudes of such machines as those of the calamary; for now it was plain of what kind they were, and whence they derived their origin.

He would not then trouble the reader with a detail of observations on 3 or 4 scores of different infusions of animal and vegetable substances, posterior to these on mutton gravy; all which constantly gave him the same phenomena, with little variation, and were uniform in their general result: these might better appear at length on some other occasion; let it suffice for the present to take notice, that the phials, closed or not closed, the water previously boiled or not boiled, the infusions permitted to teem, and then placed on hot ashes to destroy their productions, or proceedings in their vegetation without intermission, appeared to be so nearly the same, that, after a little time, he neglected every precaution of this kind, as plainly unnecessary. He would take no notice yet of their manner of being generated and generating; in relating these discoveries, as he believed he should be more intelligible, if he followed the order of time:

it was a justice moreover he owed both to Mr. de Buffon and himself; for some were made by M. de B. alone, some by him, and some of them in concert together: his system, the detail of his system, his experiments, his own discoveries, his thoughts in consequence of these discoveries; all these were reciprocally communicated; they made a secret of nothing to each other. Thus where one truth seems to lead to, or is the natural consequence of another, it will be easy, from the order he had observed, to see how much he had been obliged to his penetration and foresight. But this would be more distinctly seen, when their several essays on this subject shall appear; and in the 2nd volume of M. de B.'s Natural History, which would very soon be published, he would declare for a fact, that all which precedes his accounts of the experiments, begun March 16, N. S. of the year 1748, was previous either to his own experiments or Mr. N.'s, and was read to Mr. de B. by himself.

In this order of time therefore, Mr. de Buffon not only repeated the experiment he had taken notice of, and added particular observations of his own, but made some entirely new in every respect, peculiar to himself. Among these, that never to be forgotten by naturalists, which at once destroys the opinion of eggs in viviparous animals, and shows the real use of those reddish glandulous bodies observed by Vallisnieri on the testicles or ovaries, as hitherto called of cows. Every anatomist knows, that the whitish specks, near each of which a hydatid is placed on all female ovaries, were hitherto either considered as containing the real female eggs, or to be the remaining scars of eggs fecundated and dislodged. Vallisnieri, nearer the truth, thought the large reddish glandulous bodies, which he calls cherries, and found on the ovaries of cows and other females, in the time of their heat, if the animal is confined to any particular season, or at any time, in those females which are unconfined in this particular, were the real productive organs contributory alone to generation; yet still with a view to the ancient opinion of eggs, for he supposed these glandulous excrescences to be real oviparous productions. Mr. de Buffon, on the contrary, long before observation had realized his conjectures, rightly thought these to be no more than temporary blossoms, so to term them, not containing in their cavity, which they have distinct when they are ripe, an egg, but the real female seed; that the whitish specks, scattered on the surface of female ovaries, were partly the remaining scars of some of these temporary blossoms now faded, as having performed their destined office, or embryo blossoms not yet expanded; that the hydatid annexed to each of these, contained a quantity of imperfect indigested seed; and that, if we took the blossom in time, when it should be entirely ripe for action, as when a female is in heat, or not barren, these red glandulous excrescences would furnish a fluid as really productive of true spermatic animals, or organical parts, as he calls them, as that of any male observed by Hartsocker, Lewenhoeck, or any other. The result of these conjectures was, that, ordering a bitch in heat to be strangled, and dissected immediately, they found two of these red excrescences florid and ripe, one on each ovary, these, from their respective cavities that ran obliquely under these productions for near an inch in length, furnished a tea spoonful of a thick turbid fluid; and this fluid, observed in the microscope with the most powerful magnifier, after some little time exhibited numbers of spermatic animals, in every respect like those hitherto observed by other naturalists, animated, and moving spontaneously. Thus was Mr. de Buffon's conjecture verified in every particular.

About this time, or some few days after, Mr. de Buffon in his presence examined several sorts of male semen; and then it was that, for the first time, they fairly saw the spermatic animals enascent. Those kinds which satisfied them in this particular were extremely viscid, and contained in a certain quantity in the crystal of a watch. These precautions are not unnecessary; for if a viscid kind be not chosen, and that in a good quantity together, such as that of stags, &c. or any seed of the least exalted sort, so to term it, as they found some to be more so than others; it will alter in the atmosphere by an evaporation of its volatile parts, which serve to hold it though but gently together, after which it will liquefy, vegetate, ramify into filaments, and these filaments again break into moving globules, especially if the weather be hot, before a small portion can be adjusted to the microscope: by which an observer may easily be imposed on, and think the spermatic animals original and pre-existent, because he could not discern that action which produced them. This deception takes place in all semen of the more exalted kinds, such as particularly the milt of fish, when it is in a state of immediate impregnation, and many others: for it is to be observed, that the semen of animals is not at all times in an equal state of exaltation; and consequently that some sorts, or even the same at different times, will at some times give the spermatic animals immediately, but at others not so soon, and perhaps not under some hours: which is the reason why they have often been said by naturalists, and even by Lewenhoeck himself, not to have been found on inspection. By this it will appear, that they had tried many sorts, before they had the good fortune to meet with one, in that exact degree of exaltation necessary to exhibit the whole process of this vegetation; and so may others who shall be desirous of trying these experiments after: yet, when they shall at last have obtained a proper subject, one accurate view will be sufficient, and found to give the key to the whole secret.

When they had seized this favourable opportunity, they saw a small portion of male semen placed on the microscope, first, as it were to develope and liquefy, then shoot out into long filaments, ramify on every side, these open and divide into moving globules, and trailing after them something like long tails; these tails were so far from being members given them to swim and steer by, that they evidently caused in them an instable oscillatory motion; and were in effect nothing more than long filaments of the viscid seminal substance which they necessarily trailed after them; they were of various lengths in various animals, and they insensibly, by the continual progressive motion of those animals, grew shorter and shorter, till some of them appeared without any at all, swimming equably in the fluid. It was then plain how these animals were to be classed; their origin was clearly to be derived from principles contained in this matter, either by an evolution of organical parts, as M. de Buffon supposed, or by a real vegetation, as Mr. N. thought, of the same kind with those he had before observed in his infusions; though more prompt, because the matter was more exalted: consequently the spermatic animals were of the same kind as all other microscopical animals, their origin the same, their influence nothing more in generation, nor any otherwise conducting to its cause, than as effects of those principles in the semen, which alone are the true and adequate cause of it. See fig. 5, pl. 10.

These vegetative powers, which, from the very beginning of his observations, he had found to reside in all substances, animal or vegetable, and in every part of those substances, as far as the smallest microscopical point, he had at this time certain proofs of; though not so plain and incontestible as those he had procured a few days before M. de Buffon left Paris for the country, and which he had prosecuted after his departure. These he communicated to him in a few words the night before he began his journey, yet he was not at that time acquainted with any special detail of the many singularities that attend these latter vegetations, for he had but just then made and entered on the discovery of them himself. He was obliged the more particularly to observe this, because the many consequences M. de. B. has since drawn, as well as himself, and which, without any mutual communication, happened to tally with and seemingly to flow from the discoveries, were not in fact deduced from a circumstantiated knowledge of these new phenomena, which M. de B. had not, but from this one principle, that there is a real productive force in nature; in which they had both long since agreed, however they may have differed in explaining that action: for whether it be by an evolution and combination of organical parts, as M. de Buffon supposes, or by a real vegetating force residing in every microscopical point, may be probably far beyond the power of microscopes to determine. But as the principle from which they depart is entirely the same, it must necessarily lead to similar thoughts, and similar consequences.

Mr. N.'s first proofs therefore were drawn from a close attendance to all the common infusions, particularly that of wheat pounded in a marble mortar. It was plain from them all, that after some time allowed to the water to call off the salts and volatile parts, which evaporated copiously, the substance became softer, more divided, and more attenuated: to the naked eye, or to the touch, it appeared a gelatinous matter, but in the microscope was seen to consist of innumerable filaments; and then it was that the substance was in its highest point of exaltation, just breaking, as it were, into life. These filaments would swell from an interior force so active, and so productive, that even before they resolved into, or shed any moving globules, they were perfect zoophytes teeming with life, and self-moving.

If any particle was originally very small and spherical, as many among those of the pounded seeds were, it was highly agreeable to observe its little starlike form with rays diverging on all sides, and every ray moving with extreme vivacity. The extremities likewise of this gelatinous substance exhibited the same appearances, active beyond expression, bringing forth, and parting continually with, moving progressive particles of various forms, spherical, oval, oblong, and cylindrical, which advanced in all directions spontaneously, and were the true microscopical animals so often observed by naturalists. This brought to his mind a phenomenon often taken notice of, and seen with surprize, particles detached by the reaction of the water from the extremities of the fins of muscles, which yet continue to move progressively. He thought it sufficiently explained by these observations; and that it is more than probable, that muscles, polypes, and other kinds of this nature, vegetate in a manner analogous to this gelatinous matter. See fig. 6.

In the infusion of pounded wheat, the first appearances, after an exhalation of volatile parts, as in every other infusion, were the second or third day clouds of moving atoms, which he supposed to have been produced by a prompt vegetation of the smallest and almost insensible parts, and which required not so long a time to digest as the more gross. These in a day or two more entirely disappeared; all was then quiet, and nothing to be seen, but dead irregularly formed particles, absolutely unactive till about 14 or 15 days after. From these uniting into one mass sprung filaments, zoophytes all, and swelling from a force lodged within each fibre. These were in various states, just as this force had happened to diversify them; some resembled pearl necklaces, and were a kind of microscopical coralloids; others were uniform throughout their whole length, except just the very extremity, which swelled into a head like a reed, if the force had acted equally on all sides, or like the head of a bone at its joint, if the matter in its expansion had bore to either side. These filaments were all zoophytes, so teeming with life, that whenever on taking a drop from the surface of this infusion, he had separated the extremity of a filament so short as not to consist of above 4 or 5 globules chaplet-wise; they would advance progressively and in concert, with a kind of vermicular motion, for a little way, then fall off irregularly to one side, as if not yet fitted for progressive motion, languidly turn their extremities, and then again lie quiet for some little time. It was his fortune however, not in this infusion only, but in many others, to find some of these chaplet-like animals much smaller indeed than those of the wheat infusion; but quite regular, constant in their vermicular motion, and which were consequently arrived to a higher degree of maturity and perfection. He owned he could not but wonder to this day at what he saw; and though he had now seen them so often, he still looked upon them with new surprize. Yet had these phenomena served him to very good purpose, and cleared up many difficulties in his former observations.

The origin of blight in wheat, rye, and other vegetables, was no longer mysterious: an atmosphere charged to an extraordinary degree with humidity, now plainly appeared sufficient, particularly while the grains were tender and replete with a milky juice in a certain degree of exaltation, to produce in them this new kind of vegetation, and to form their interior substance into filaments, which are indeed those very eels he had observed some years before in blighted wheat.

This agrees perfectly with another observation made by the gentleman who translated his little Essay into French: some of this blighted wheat, 2 years after he had gathered it, he had given to Mr. Trembley, and he to this gentleman. In a note he has added, he observes, that these filaments not only recovered life and motion, after they had been so long dry, by macerating them in water; but many broke, and discharged from within them globules, which moved with extreme vivacity. The application of the foregoing observations to this case is easy and natural; nor is it now any wonder, that these filaments, the vegetative force still residing within them, should move and resolve into globules, or that they should have subsisted so long, full of that kind of life they are actuated with, though dry and without nourishment; for now they cease to be eels, as he formerly thought them.

Blighted rye, which is also so full of filaments of this nature, that the grains are swelled in their diameters, and extended to an extraordinary length by this new kind of vegetation, exhibited nearly the same phenomena when macerated, and is to be classed accordingly. He was told by some of the gentlemen of the Royal Academy of Sciences here, that in those provinces of France, where this blighted rye abounds, and is made up into bread, it produces very strange effects in the poor country people who feed on it, many of which are here found in the hospitals afflicted with a very singular kind of mortification, which causes their limbs to drop off.

There are 2 sorts of blight, in one of which the grain crumbles into a black powder; and the other is that which gives these moving filaments or eels. Mr.

Bernard de Jussieu said, that one is from a corruption of the flower, and the other of the grain.*

It may not here be amiss to hazard a few queries. Do not all mortifications, and other maladies in which there appears an extraordinary exuberance of matter in any one part, proceed from a weakness, a want of resistance, and from principles of union, which give to this vegetative force, found to reside in every point of animal or vegetable substances, more play in one part than in another? For if the resistance be not equal in all parts, the exuberant matter must break forth, and cause that part to decompose; and if the habit of body be extremely lax, the decomposition must continue; and that, in a certain extraordinary degree, he calls a mortification. To rub a wound, or any natural sore, with salt and spirit, is found to be salutary, and preventive of mortifications; and salt he knows by observation, will immediately put a stop to these microscopical vegetations, and cause the animals to subside motionless to the bottom: therefore it is probable, that salts and spirits are principles of union, and productive of a greater resistance in the ductile matter acted on by this vegetative force. High living, rich wines, &c. are preservatives against many contagious epidemical distempers: do not therefore these maladies arise from a laxer habit of body, and a more than ordinary action of the same vegetative force? And may not these, and many other phenomena of this kind, be reduced to the same principles?

The substance emitted from the globules of the farina fæcundans of all flowers, by an action he observed some years before, is also a substance of this nature, filamentous, and in a vegetating state: nothing can resemble it more than the fibres of most kinds of mould; resolving all, as they do in water, into others of a much finer contexture, when the vegetation, that had been before stopped by the nitrous salts of the atmosphere, begins by the assistance of the water to act again: and he knew, by observation, that all kind of mould is formed by a process of the same nature as the growth of these microscopical plants; and to be classed consequently with them, and reduced to the same principles.

He concludes that nothing can more perfectly than these wheaten filaments, represent in miniature corals, coralloids, and other sea plants, which have long been observed to be teeming also with life, and have been supposed to be the work of animals, as it will appear to any one, that but inspects the figure he had annexed, and recollects his description. Are not therefore all these in the same class, and is not their origin similar? See fig. 6.

* The subject of the blight in corn has been recently illustrated by Sir Joseph Banks, in a treatise accompanied with excellent engravings, showing the appearances as detected by the microscope. From these observations, it would appear that the blight is occasioned by a minute parasitical plant of the fungus kind.

But these instances from common infusions, of a vegetative force residing in every microscopical point of animal or vegetable matter, how strong soever and surprising, were neither so wonderful nor extraordinary as some others he observed after M. de Buffon's departure. From the wheaten filamentous zoophytes it was easy to infer, that they sprung from, and were productions of, the mass of matter that had subsided to the bottom of the phial. Yet this he could not obtain a sight of; nor was it possible in this way to observe them without separating them from their roots and from the mass, out of which they arose. The method the most natural therefore which occurred to him for viewing these zoophytes, without disturbing their vegetation, and for observing their whole process, from the origin of the plants to their last degree of maturity, was to take extreme thin slices of cork, and insert, through little holes which he made, 4 or 5 in each slice, grains of wheat or barley, or any other farinaceous seed, for these all nearly agree in the phenomena they exhibit, with the germ either turned upwards, or carefully picked out with the point of a penknife, to prevent their usual shooting.

These were permitted to swim on the surface of fresh spring-water, in a glass exposed to the sun, that the whole vegetating force might be determined downwards towards the inferior moiety of each grain, which alone could in these circumstances imbibe and be saturated with moisture. This answered his purpose entirely; his plants grew downwards into the water like corals, but appeared not till several days after the grains had been thus exposed; and were at last so large and strong, that he could see them with his naked eye.

When they became thus visible, he cut off with a small pair of scissars the vegetating extremity, and placed it in a concave object-glass with water. The plants then took a new direction, followed the expanse of the fluid, and continued to vegetate, while he supplied them with water, which he did from time to time, covering them after observation with another concave object-glass, to prevent the fluid from evaporating too fast. Thus he had for the subject of his observations what he might call a microscopical island, whose plants and animals soon become so familiar to him, that he knew every animal species, and every individual plant almost without any danger of mistake; an exactness so necessary, that it would not otherwise have been possible to follow the process of this vegetation without confusion. From this time he laid aside the use of large infusions, and provided a certain number of watch-crystals, or concave object-glasses, for every portion of animal or vegetable substance he was to macerate in water. The use of these is plain and easy; many fruitful little islands of various kinds with labels and dates affixed to each may thus be obtained, by placing the vegetating substances in these glasses; and this is the method he would recommend to all those who may be desirous to repeat or pursue his experiments.

He annexed a figure of his wheat-island and its productions, all which will be sufficiently intelligible without any more words; and he reserved a multitude of other observations he had by him in his journals, on infusions and other vegetating islands, for the Essay, which he hoped to publish in some months, if these few thoughts and discoveries shall meet with approbation. See fig. 7.

Then follow some observations on the generation of the paste-eel. The R. S. knows it to be viviparous; consequently perfect in this state, and such as may continue to generate in the common way, as long as it has an element and matter proper for its subsistence, yet is its own original generation, as far as he could learn by observation, as that of all these microscopical animalcules, from a ductile vegetating matter, the produce of wheat-flour and water; though it undergoes more changes than others, and lives in other conditions; ascending for some time before it enters its chrysalidal or egg-like state, whence it comes forth a perfect eel. He had added a figure of a group of these eel-chrysalids, but the detail of their metamorphosis he reserved for his little Essay, and not trouble you now with an account too circumstantiated of every observation he had made on them: besides that he was not yet thoroughly satisfied in the whole manner and process of their generation. See fig. 8.

He concludes with summing up his system in a few words: he supposes all semen of any kind to be an exalted portion of animal or vegetable matter, secreted from the aliment of every generating subject, when it is adult, and no further demand is made for its increase and growth; this he supposes to be endued with a proportionable vegetative force; to be various in various circumstances, and heterogeneous in different subjects; but to be uniform in its productions, when it falls into a proper matrix, where it finds matter to assimilate, of a quality and in a quantity sufficient to form that specific being; while in other circumstances, it will, if it extravasates, by the same vegetating force, yield all the several phenomena above noticed. And thus he supposes he has obtained what he first intended to make out, that the spermatic animals are not the efficient cause of generation, but only a necessary consequence of principles in the semen, which principles are necessary to generation.

Thus had he connected his system with our countryman Dr. Harvey's observation of that fine tissue, or web-like expansion, observed in the uterus of does, in the centre of which the embryo fetus, invested with its amnion and chorion, was found to be lodged: for let the vegetation begin from the semen, and continue to assimilate the effluent matter from the matrix in which it has taken root, and the fawn must come forth like any other specific animal or plant.

He only further observes, that Lewenhoeck had discovered this vegetating power in the semen, and had, like M. de Buffon and him (Mr. N.) seen the filaments from whence the spermatic animals spring; he even calls them nerves and arteries; and in one of his letters to Mr. Oldenburg says, that he saw more in one minute than the most accurate anatomist could discover by dissection in a day: but when he afterwards changed this system, false as it was, of nerves and arteries for another, he believes as false, that of pre-existing germs in the spermatic animals, he neglected to improve this observation as he might have done; nay he afterwards took no further notice of it, but barely to say, that it was to be neglected. This remark he had from M. de Buffon.

The difference therefore between Mr. Lewenhoeck and Dr. Harvey was, that the first had an hypothesis to maintain, and the latter nothing in view but to follow nature, without trusting too much to the first phenomena, as he hoped he should appear to have done in this his inquiry.

He recollects one remark that coincided with his system; that although animal and vegetable substances by a chemical analysis appear to differ, they are nevertheless found by a natural corruption to be reducible to the same principles. This had been observed long ago by many naturalists.

Fig. 5 represents the origin of the spermatic animals. Fig. 6, the wheat infusion. Fig. 7, what he has called an island in the wheat infusion. Fig. 8, a group of the chrysalids of the paste-eels. Fig. 9, is a draught of one of the first microscopical plants or zoophytes which he discovered; wherein A shows the figure of the plant throwing out its animals, and B the same again after the animals were discharged, again putting out a new shoot from the stem below, through the hollow transparent head, to form a new head, and produce another generation.

Astronomical Observations made at Paraguay in South America, from the Year 1706 to 1730. Communicated by James de Castro Sarmento, M.D., Col. Lond. Lic., and F.R.S. N° 490, p. 667.

These observations were made at the town of St. Ignatius in Paraguay, by Father Bonaventura Suarez, a jesuit missionary, with a 5-foot telescope, and a second's pendulum, rectified to true time by altitudes of the fixed stars. The latitude of the place being 26° 52′ south, and longitude from Paris 3^h 57^m 50^s.

The sun was eclipsed, anno 1706, Nov. 5. It began at 8^h 52′ in the morning, and ended at 11^h 15^m, the greatest quantity at 9^h 50^m being 4 dig. 0′.

The solar eclipse of March 11, 1709, began before sun-rise, which that day was at 5^h 53^m. The eclipse ended at 1^h 37^m 15^s. The greatest obscuration, at 6^h 15^m, was 9 dig. 20′.

The moon was observed eclipsed April 16, 1707. The beginning at 7^h 55^m afternoon; total obscuration at 8^h 58^m; and first emersion at 10^h 45^m. The end was not observed for clouds.

The moon was observed eclipsed, April 4, 1708, afternoon. A sensible penumbra at $12^h\ 18^m$; true eclipse began $12^h\ 30^m\ 29^s$; ditto ended $15^h\ 3^m$; penumbra ended $15^h\ 12^m$.

The sun was observed eclipsed Jan. 18, 1730, in the afternoon. The beginning at $2^h\ 52^m\ 30^s$; the greatest obscuration was 8⅓ digits: the end not observed for clouds: it seemed to be about $4^h\ 52^m$; as at $4^h\ 55^m$, the moon was quite clear of the sun.

Anno 1729, Aug. 8, in the total lunar eclipse, because of clouds, it could only be observed that the first emersion was at $10^h\ 1^m$ afternoon; and 6 dig. were eclipsed at $10^h\ 33^m\ 2^s$.

The same year 1729, Dec. 9, afternoon,

$11^h\ 3^m\ 5^s$ the moon occulted a satellite of Jupiter.
11 13 25 the moon touched the limb of Jupiter.
11 15 0 the moon covered Jupiter entirely.

Anno 1713, Dec. 1, afternoon, the moon was observed eclipsed, at town of St. Joseph, in longitude from Paris $3^h\ 52^m\ 30^s$. The beginning was at $10^h\ 33^m\ 31^s$; the end $12^h\ 56^m\ 57^s$; and the greatest obscuration at $11^h\ 45^m$, was 5 dig.

Anno 1717, March 26, afternoon, the moon was observed eclipsed, in the meridian of St. Cosma, longitude from Paris, $3^h\ 52^m\ 20^s$. A sensible penumbra at $9^h\ 40^m$; beginning of the eclipse $10^h\ 2^m\ 21^s$; end of it $12^h\ 45^m\ 40^s$; end of the penumbra $13^h\ 1^m$. Quantity eclipsed 7 dig. 18′.

Anno 1728, Feb. 24, the moon was observed eclipsed, at the town of St. Michael the Archangel, afternoon, with a 10-foot tube, the difference of longitude from Paris $3^h\ 48^m\ 50^s$. Beginning of the eclipse at $14^h\ 3^m\ 35^s$; the end at $17^h\ 0^m\ 37^s$; the quantity eclipsed 9 dig. 40′.

Anno 1700, March 4, afternoon, a total eclipse of the moon in the Fluentine College, commonly called de las Corrientes, its difference of longitude from Paris about $4^h\ 2^m$.

Beginning of the eclipse at $13^h\ 14^m$; total immersion $14^h\ 34^m$; first emersion $16^h\ 15^m$; end of the eclipse $17^h\ 15^m$.

END OF THE FORTY-FIFTH VOLUME OF THE ORIGINAL.

On Thermometers, and the Weather. By the Rev. Henry Miles, D.D., F.R.S. Nº 491, *Anno* 1749. *Vol. XLVI.* p. 1.

It has been often complained, that the theories of the air and weather are so imperfect, and that an unfinished one of Mr. Boyle, published since his death, is the best we yet have. Perhaps there is equal reason for complaint, that the thermometer first introduced into use in England, by the same excellent philosopher, has been so little improved for more than half a century, that it serves for little more than amusement.

For some years past, several eminent philosophers have applied themselves to bring this instrument to better condition, and to render it more useful; and among them the great Sir Isaac Newton did not think it unworthy his attention.

It seems now to be pretty generally agreed, that thermometers made with quicksilver are preferable to all others; that extravagant fluid, as Mr. Boyle calls it, being most easily susceptible both of heat and cold, and, when well purified, not liable to be obstructed in its motion.

Dr. M. had, by some years experience, found both the excellence of them, and the necessity of keeping them in the open shaded air, before he met with the learned and curious essays medical and philosophical of Dr. George Martine, in which he so much recommends their use; and it was no small satisfaction to find that gentleman had proved, by experiments, that quicksilver both heats and cools faster than any liquor we know; faster, I am sure, says he, than water, oil, or even spirit of wine, and never freezes, by any degree of cold hitherto observed.

There is another particular of great importance, which probably we may rather wish than hope to see made a general practice, recommended by the same gentleman; viz. the constructing all thermometers with one scale. But if this may not be expected, certainly no thermometer should be made without adjusting two determinate and sufficiently distant points of heat and cold; such, for instance, as those of boiling water, and of water just beginning to freeze, and the intervening space divided into a convenient number of equal degrees. By this means we should be able to know what is meant by any specified degrees of heat or cold, and a comparison might be easily made of the state of the air in distant places, provided the instruments were accurately made.

On Monday Nov. 21st, 1748, in the evening, the sky very clear, the wind N. and a smart frost, the barometer was 30 inches $\frac{2}{10}$. At near 9^h the thermometer without the window at 7° below 0, or freezing point. The thermometer within, of the same construction with it, and not a yard from it, (the room having had no fire in it this season) at 5° nearly above 0.

On Tuesday morning, at 4^h 20^m, the barometer was at $30\frac{26}{100}$ inch; the thermometer without at $14°\frac{1}{4}$ below 0; that within at $2°\frac{1}{4}$ above 0: which was surprising. At 7^h 40^m the same morning, the sky looked red and lowering; and the barometer was fallen to $30\frac{8}{100}$, the thermometer without risen to 5° below freezing point; but that within fallen to 1° above; the wind getting about to W. and S.W. and before 10 in the morning there was some rain, and this severe frost went off. At this last-mentioned hour the thermometer without had risen to 5° above 0; that within continuing at 1° as before. At $8^h\frac{1}{4}$ that evening, the thermometer without was at no less than 12° above 0, that within at 3° above 0: so that from $4^h 20^m$ in the morning to $8^h\frac{1}{4}$ at night, there was a change in the temperature of the air abroad of $26°\frac{1}{2}$; while the change within doors did not amount to more than $\frac{1}{2}$ a degree warmer.

The Case of a Clergyman's Lady, at Cotterd near Baldock, Herts, who had a Stone under her Tongue. By Wm. Freeman, Esq. F.R.S. N° 491, p. 5.

This substance, seemingly a concretion of stone or chalk, was voided in July 1748, from under the root of her tongue, just on the left side of the middle string among the blood-vessels. It was lodged in a cell formed by itself, the traces being left behind exactly answering. It was voided without pain, or effusion of blood.

The patient began to feel in the part affected some uneasiness about 18 months before the discharge. The pain extended itself sometimes along the jaw almost to the ear; the glands being at times swelled, and a salt rheum flowing into the mouth. The swelling of the part gradually increased to about the size of a large nutmeg; and felt hard to the finger.

About a fortnight before the discharge, some white specks appeared; on which it was supposed that matter was gathering; and being still hard, a common poultice of white bread and milk was applied; after which it presently dislodged itself, without any application, and left the patient ever since free from complaint.

On Glasses of a New Contrivance, for preserving Pieces of Anatomy or Natural History in Spirituous Liquors. By Claud. Nic. le Cat, M.D., F.R.S. &c. Translated from the French by T. S., M.D., F.R.S. N° 491, p. 6.

Having, in 1739, begun to make a collection of preparations in anatomy and natural history, M. le Cat soon felt the necessity of contriving some kind of vessel, or some way of closing the common glasses, which might prevent, or at least diminish the quantity of spirituous liquor lost by evaporation.

After trying several methods, he fixed on the glass, which is represented in figures 10, and 11, pl. 10. Its difference from the common glasses consists in having, quite round the edge of its orifice, a circular groove or channel, AA, fig. 11, an inch deep for the smaller sizes, and 2 inches for the larger. This circular groove is intended to receive a border, BB, of the same figure on the cover C: in the middle of the concavity of this cover is a double hook, on which the threads are to be fastened, which suspend the piece or preparation to be put into the glass.

In putting the piece into the glass, which contains the preserving liquor, care must be taken to place it on the hooks in the same position, which you intend it should keep in the glass: that done, you are to pour oil, or quicksilver, which is better, into the circular groove AA, so as to make it about half full. Then you are to let down the piece into the preserving liquor; and when it is entirely sunk down, the cover C ought to light upon the glass, and its circular border BB, enter into the circular groove AA, where it falls into the oil or quicksilver, which rises up and fills the whole groove; by which means the glass is in some measure hermetically sealed. The oil indeed permits some small evaporation; the quicksilver more completely answers the end of this invention.

As he had contrived this glass for his own private use, he neglected making it public; till, happening to be present at the public meeting of the Academy of Sciences of Paris, at Easter 1746, he heard M. de Reaumur read a memoir on this subject. His own glass seemed to him vastly preferable to the vessels proposed by that great academician. This incident roused his emulation, and gave him the better opinion of his glass, a pattern of which he sent to M. Morand. The same motive engaged him to send this description to the Royal Society.*

Astronomical Observations made in Paraguay. By J. de Castro Sarmento, M.D. From the Latin. N° 491, p. 8.

An eclipse of the moon took place Feb. 24, 1747, but the sky was so cloudy that very little of it could be observed besides the end, which happened at 15^h 16^m 4^s, at the town of St. Angelus, in lat. 28° 17′ south, and 323° 30′ longit. from the Ferro Isle.

Another lunar eclipse was observed Aug. 19, 1747, at the town of St. Mary the Greater, in lat. 27° 51′ south, and 322° 40′ longit. from the Ferro Isle.

The penumbra was sensible, at 14^h 44^m; beginning of the eclipse was at 14^h 55^m 44^s; 6 digits obscured at 15^h 24^m 6^s; the total obscuration at 15^h 53^m 16^s; beginning of emersion 17^h 34^m 48^s; 6 digits emerged 18^h 3^m 30^s.

* In using the bottle, M. le Cat found it was attended with one inconvenience. The circular groove, the edge of which was turned up on the inside of the bottle, retained a small quantity of the water, when he emptied and rinsed it; so that it was impossible to do it thoroughly. He therefore caused the vessel to be made, as represented in fig. 10, which is a section of it; and where it appears that the groove AA is placed on the outside of the edge of the bottle. By this means every drop of the contained fluid can be poured out. It is easy to see that the circular border BB of the cover is to enter into these grooves AA, which are filled with oil or quicksilver; and that the hook C, of the same cover, is to suspend in the liquor such pieces as are to be preserved in it.—Orig.

Mr. Carlisle's method of closing the openings of wide-mouthed vessels (for preserving anatomical preparations, &c.) is to have a glass jar with a groove ½ an inch deep round the outside of the top or mouth, and a glass lid, like that used by confectioners in their show-glasses. The lid fitting closely into the groove is rendered air-tight by hog's-lard, a substance which he finds never becomes quite fluid at the highest temperature of this climate, and is always soft enough in the cold season to admit of removing the lid or top. See the 6th volume of Nicholson's Journal of Nat. Phil.

Observations of a Solar Eclipse July 14, and of a Lunar July 28, 1748, at Madrid. By Don Antonio de Ulloa, F.R.S.* N° 491, p. 10. *From the Latin.*

With a 2-foot telescope, and an astronomical clock, the solar eclipse was observed to begin at $8^h\ 49^m\ 6^s$ true time. Some spots were seen on the sun, and the times of their eclipses observed.

The lunar eclipse of July 28, o. s. was observed, by the same telescope. The penumbra began at $9^h\ 45^m\ 42^s$ true time; the true eclipse at $9^h\ 50^m$; the end of the eclipse $12^h\ 10^m\ 22^s$; and the end of the penumbra $12^h\ 22^m\ 12^s$.

Remarks on the principal Paintings found in the Subterraneous City of Herculaneum, and at present in the Possession of the King of Naples. By ——— Blondeau, Esq. N° 491, p. 14.

The paintings found under-ground in Herculaneum near Portici, are all done on stucco in water-colours in fresco. They have been taken from the walls of an amphitheatre, a temple, and houses, and are in great variety, some exceedingly fine, and well preserved. They may be divided into 2 classes, the first of which contains the 4 following pictures.

The first is a large piece of 7 feet by 5, representing Theseus, after having killed the Minotaur. He is naked, at full length, holding a club or knotted stick in his left hand by the small end; a young woman by his side, holding the said club a little higher with her right hand, and looking up wishfully at him; three children of different ages: one kissing his right arm, which is extended: the second his left leg, which is a little raised: and the third grasping and kissing his left arm; all as it were wishing him joy, and caressing him after the victory; the Minotaur lying on his back dead at his feet, a human body with a bull's head and short horns. This piece has been a great deal larger. On the upper part is part of a naked arm with a trumpet.

The second is a noble piece of 10 feet by 7, entire, and seems to represent Rome triumphant; viz. a grand figure of a woman sitting, with a garland of flowers on her head, a majestic commanding countenance, a knotted club, exactly like that of Theseus, long and tapering, in her left hand, resting herself on her right elbow, with her hand to her temple; a young faun laughing over her shoulder, with a musical instrument of 12 pipes in his hand. At her side is

* This learned Spaniard (of whom some account has been already given at p. 316 of this vol. of these Abridgments) was born in 1716, and died in 1795. His progress in science was so rapid, that at the age of 18 he was associated with George Juan and la Condamine, at the instance of Lewis the 15th of France, and under the patronage of the king of Spain, to proceed to South America, to make observations for ascertaining the figure of the earth. He continued in America till 1744, when returning he was taken prisoner, and brought to England, where he was elected F. R. S. He was afterwards made governor of Louisiana. An Account of his Voyage was published at Madrid in 1748, in 5 vols. 4to.

a basket of fruit; over against her a naked figure of a man, robust and vigorous, with a beard; his back short, and his face seems turned to the left shoulder; a garland of flowers or laurels on his head; a quiver, a bow and arrows by his side; under his left arm something like part of a lion's skin, and one paw, but faintly expressed; a fine natural attitude; most exquisite proportion and drawing. A little higher, close by him, a genius or goddess of fame, with wings, a garland on her head, a sprig like ears of corn in the left hand, and pointing with the right; and both she and the man looking to a young infant below, a most beautiful figure, and natural attitude, sucking a doe, finely drawn and spotted, which is licking the child's knee. Under their feet an eagle with his claw on a globe, and a lion, both as large as life. Some reckon the man Hercules and the woman Pomona; but Hercules, I think, did not use the quiver; and Pomona has no such majesty, nor any business with a club, which is longer and smaller than that of Hercules.

The third is a piece of 4 feet square, representing the centaur Chiron, sitting, as it were, on his backside, and teaching his pupil Achilles, a young lad of about 12, to play on the harp. Part of the horse is a very difficult forced attitude; the whole body being in view; left fore foot extended; great expression and attention both in Achilles and Chiron, who is putting his right hand round the boy, and playing, by the help of a small instrument, on the strings, which are 10 in number. This is accounted a most masterly piece as ever was seen. Chiron has a mantle tied round his neck, made of the skin of some animal; and Achilles stands upright naked.

The fourth is a piece of 5 feet by 4, representing some very solemn and melancholy story of the Romans, and contains 7 figures, 3 men and 4 women: perhaps the story of Virginia, when Appius Claudius wanted to accuse her falsely, in order to gratify his lust. One man sitting in a pensive mood, his left elbow on his knee, and his hand up to his forehead; another sitting over-against him, setting forth something in a paper, which he holds to the breast of the first; a young woman sitting on the right side of the first, a figure expressing great concern; her left hand affectionately about his shoulder; and another young woman standing with great attention and surprise by her; behind both, the figure of a woman larger than the rest, with a quiver appearing above her shoulder, as Diana; an elderly woman in a suppliant bending posture, with her finger at her chin, as if she were listening with great grief, and her face to the first figure. Also an old man, in much the same attitude, in great grief, as if weeping. Perhaps the family of Virginia listening to the accusation against her, and fearful lest she should be delivered over to the brutal lust of the consul; to avoid which, when no other remedy was left, Virginius desired to speak with his daughter in private, and killed her.

These are the 4 capital pieces: and they are so extremely well executed, that Don Francesco de la Vega, a painter, whom the king of Naples sent for from Rome, as one of the best hands, to take draughts of these paintings, said, that if Raphael was now alive, he would be glad to study the drawings, and perhaps take lessons from them. Nothing can be more just and correct: the muscles are most exactly and softly marked, every one in its own place, without any of that preternatural swelling, which is so much over-done in some of the best Italian masters, that all their men are made to appear like Hercules. It is surprising how fresh all the colours of these pictures are, considering that they have been under-ground above 1650 years; besides the years they stood, before they were covered by the eruption, which cannot be exactly determined.

Theseus in the first, and the naked figures in the second piece, are a good deal on the red colour; but the women and children are of as soft and mellow flesh-colours as if painted in oil. The third and fourth are so highly finished, that you can scarcely discern whether they are done in water or oil colours. The last pleased him most; the composition is good; the attitudes natural, and of fine kinds; the different characters justly expressed; the drawing and drapery exquisite; and, though done in water, with only 2 or 3 colours at most, yet the light and shade are so artfully managed, that the figures are quite out of the surface. The connoisseurs prefer the third, or the centaur.

We now come to those of the 2d class, which are as follow:

1. A piece of 4 feet by 3, supposed to be the judgment of Paris. Three goddesses, with rays like circles of glory about their heads, which are very fine: the first sitting inclined: two standing naked; good drawing and natural attitudes. A figure of a shepherd at a distance above them, with a crooked staff in his hand, a garland on his head, his right hand grasping something, which is not distinctly seen, has not being so much finished as the rest.

2. A piece of 4 feet square, representing Hercules, when a child, tearing the serpent in pieces, with great vigour and fierceness in his eyes; an old man drawing a dagger, being startled at the danger, in order to kill the snake; a woman holding up her hands to heaven, an old woman holding a child in her arms. The whole natural and well drawn.

3. A piece of 4 feet by 3, an old man naked, sitting; a naked boy standing by his side, with a piece of a rod or twig in each hand: the old man is pointing with his finger, and teaching the boy something. Fine drawing, somewhat defaced.

4. A piece of 6 feet by 3, a half length of Jove with thunder in his hand; a little Cupid looking over his shoulder; a rainbow; an eagle; a bald old head; a figure like Venus coming from bathing, naked down to the thighs. Beautiful contour, great softness, and fine flesh-colours; seems to have the privy parts of a man, an hermaphrodite.

5. A small piece, about 14 inches square; 2 fine female heads, or half-lengths; one with a book in her hand; great expression; 2 muses.

6. A piece of about 18 inches square: 2 figures of women like graces, one naked to the middle, sitting: something like a quiver at her feet; another in a robe, standing, and leaning on her elbow: good attitude; drawing and drapery very fine: colours faint.

7 and 8. Two pieces of 3 feet square, of Egyptian sacrifices. First, the worshipping of an idol, which is placed above in the portico of a temple, and appears bloody: 7 figures bending and suppliant in the act of adoration: an altar in the middle; 2 birds, storks, standing one on each side; many other figures faint.—Second, a priest sacrificing on a flaming altar: a row of different figures on each side: 2 in the middle in the act of preaching. Attitudes very just and natural, finely done, great solemnity or horror: when looked at near, seems more daubing and unfinished; by virtuosi esteemed a great piece of antiquity, and of great study.

9. Is a half-length of a man like a priest, with a small water-pot, pouring it into a basin, seen by the light of a lamp.

10. Is Orpheus and Venus lying together, kissing and caressing, chained by the legs: a servant holding a harp. Finely drawn, but defaced.

11. An old man sitting, with a cup in one hand, a stick and garland in the other.

12. Is a half-length of a young woman.

13. Is a piece of 2½ by 2 feet, old Silenus, holding in his arms Bacchus a child; a satyr; a Baccante; Mercury sitting below; a tyger and ass lying. Finely drawn, and naturally expressed.

14. A sleeping nymph; a satyr lifting up her robe; three by-standers, who seem to be very curious. A small piece.

15 and 16. Two small pieces, of satyrs ravishing nymphs; well drawn, and natural attitudes, but faint and defaced.

17. A piece of 4½ feet by 1½, a figure of a Roman lady, almost full length, in attitude of great grief: her head a little inclined: her arms dropped down, and her fingers clasped; a sword, with the handle leaning in the hollow of her hand. Very just and natural expression, well finished.

18. The goddess Flora as descending from heaven. Fine contours; about 2 feet square.

19. Is a piece 8 feet square, a naked figure with a lance like a general; a woman sitting; a young man holding his horse; an old woman. Finely done, but defaced.

20. Orpheus with his harp, sitting on a rock by the sea side; a child or sea god riding on a dolphin, presenting him with a book.

21. Ten small pieces of Roman ceremonies, with many figures: some eating, dancing, making love; others tied like prisoners.

22. Eight small Cupids in different attitudes and different paces. Very good.

23. A pheasant and other birds; two small baskets, one tumbled down; a rabbit eating. Exquisitely done.

24. Two naked figures, with Cupid between.

25. A figure in the attitude of a warrior, with a sword in his right hand, a buckler in his left, and a cup with some jewels at his feet.

26. A large piece of architecture, which, looked at near, seems rough and daubing, at a distance very good perspective. You see quite through 2 porticos, one above another, into a palace or church. Very curious architecture, colours very lively and fresh.

27. A landscape with houses, ruins, a theatre. Good architecture; figures of pheasants, mules loaded, &c.

28. Another piece of architecture and perspective, very good.

A great many other figures of men and women, not easy to be described, because pretty much defaced: also many fancies of birds, beasts, chariots drawn by different animals, children driving; all in small.

Little pieces of landscapes, and other ornaments for the walls of their houses, which were painted mostly of a yellowish colour; divided into squares or panels; with those pieces of painting in the panel, and a border round it. There is a very good piece of ornament or cornice, that was on the picture of Theseus, of a very good taste, and finely finished.*

Of a New Invented Arithmetical Instrument called a Shwan-pan, or Chinese Account Table. By Gamaliel Smethurst. N° 491, p. 22.

The Chinese have for many ages piqued themselves on being the wisest nation in the world; but late experience and closer converse with them have found this pride to be ill-grounded. One particular, in which they think they excel all mankind, is their manner of counting, which they do with an instrument composed of a number of wires with beads on them, which they move backwards and forwards. This instrument they call a shwan-pan. But Mr. S. thinks he has formed one on the plan of our 9 numbers, that in no case falls short of the Chinese shwan-pan, but in many excels it.

The Chinese, according to the accounts of travellers, are so happy as to have their parts of an integer in their coins, &c. decimated, so can multiply or divide their integers and parts as if they were only integers. This gives them the advantage over Europeans in reckoning their money, &c. But then, as they have

* See accounts of this subterraneous city (Herculaneum) in these Trans. N° 456, 458.

no particular place set apart for the lesser denominations of coins, weights, measures, &c. their instrument cannot be used in Europe, nor can it be so universally applied to arithmetic as this, in which provision is made for the different divisions of an integer into parts.

This instrument has the advantage of our digits in many cases. First, the figures can be felt, so may be used by a blind man. If it had no other, this alone would be sufficient to gain it attention. Another advantage is, that when attained, this method is much swifter than by our digits, and less liable to mistakes: it is also less burdensome to the memory, in working the rules of arithmetic; as, by our digits, being obliged to carry the tens in the mind from one place to another, which are set down by the shwan-pan. One may work a whole night, without confusing the head, or affecting the eyes in the least.

It may be of great use to teach people the power of numbers; also to examine accounts by; for as the person will, by the shwan-pan,* work it a quite different way, it will serve as if another person had gone through the account; if it proves right with the written one, we may rest assured the work is true. It may be a very pretty lure, to lead young people to apply their minds to numbers.

A Proposal for Intirely Removing the only Real Defect in the Lateral Operation for the Stone. By Mr. John Mudge,† Surgeon, Plymouth. N° 491, p. 24.

Though the lateral method of cutting for the stone is now almost universally allowed to have greatly the advantage of any other hitherto discovered, yet it

* The inventor produced one of these instruments before the Society, and worked several questions in arithmetic on it. It much resembles the abacus of the ancients. C. M.—Orig.

† John Mudge, the author of this ingenious paper, was born at Biddeford, in the county of Devon, 1720, being the son of the Rev. Zachariah Mudge, a pious and learned divine. On the promotion of his father to the vicarage of St. Andrews, Plymouth, our author was articled to a surgeon there. In this line his talents were soon displayed on many occasions, which procured him the friendship and patronage of the celebrated Dr. Huxham, who then practised as a physician in that town. After returning to Plymouth, from walking the public hospitals in London, finding at that period but few surgical operations of difficulty were performed westward of Exeter, he determined to exhibit the character of his talents by some operations of consequence in the presence of the faculty. He accordingly sought out and found a proper subject, in a man afflicted with the stone. This subject he persuaded, by the aid of pecuniary arguments, to submit to the operation of lithotomy. And the patient was paid, cut, and cured.

From this time, till 1784, when Mr. Mudge received a diploma as M. D. and began to practise as a physician, he continued the profession of surgery, and with great success. With a capacious mind, assisted by experience and sound observation, Mr. Mudge in the course of his life made several improvements in the art of surgery, in which line he was not more eminent than in that of physic. His method of extracting the stone in the lateral operation, described above in these Transactions; as also his two publications, one on the safety of inoculation, and the other on a certain and speedy cure for a recent catarrhous cough, are proofs of his skill and philosophical judgment. His method of must be confessed, that the difficulty and hazard attending the extraction of large stones this way, renders it in some degree imperfect; for though the incision be made to the wish, quite through the prostate, and carried on to the neck of the bladder, if this be the case: (for it often happens otherwise,) as the bladder itself in general is not, nor in all probability can be, wounded in this way of operating, the real aperture after all for the exit of a large stone is so small, that the parts must suffer most violent lacerations, and a train of consequent evils must take place.

The old method indeed is much more liable to this misfortune, because the parts are torn to pieces by absolute violence, without any previous incision of any consequence, to prepare them for the egress of the stone; and this imperfection in the operation is so notoriously apparent, and so destructive in fact, that this method is almost universally discarded.

It would be well if the lateral method was intirely free from this imperfection; but it is to be feared an impartial inquiry would make it clear, that three-fourths of the accidents which have attended this operation, may truly be ascribed to excessive distensions and lacerations of the bladder; and that those few cases, which have miscarried from a mere symptomatic fever, will probably, on a strict disquisition, afford a shrewd suspicion, that this very fever itself arose from some violence offered to the bladder, in the forcible extraction of the stone.

rating in the case of the fistula in ano, as shown in a paper of his published in the Medical Journals, might also be adduced as an instance of the same.

The character of a physician Dr. Mudge sustained with great respectability. He possessed a liberal enlightened mind, and was distinguished not only for learning, skill, and discernment in his profession, but also for knowledge and judgment in general science. His mechanical turn, joined to his inclination for the study of optics, led him to the improvement of the reflecting telescope. In a paper presented to the Royal Society, and printed in the 67th vol. of the Philos. Trans. he suggested some improvements in the composition of the metals for that instrument, accompanied with a description of a process for grinding, polishing, and giving the true parabolic figure to the great speculum; and for this paper the Royal Society adjudged the prize of the gold medal. In discovering the means of this operation, Dr. M. received, what every well-grounded man must receive, in the investigations of nature, or the product of art, the assistance of mathematical learning. For he had, at intervals, acquired a very considerable knowledge of the mathematics in all its branches, and had written a treatise on forces, in great part corrected for publication. It may be farther remarked that, for coughs and colds, he was the inventor of the instrument called the inhaler, for introducing warm vapour into the lungs.

This respectable man died March 26, 1792, at 72 years of age; leaving behind him a most excellent character, both as a man and a scholar; as also a numerous family of sons and daughters, all of singular talents and conduct, respectable not only in private, but in public life; one having distinguished himself as a naval commander; and another, Colonel Mudge, of the Royal Artillery, as an able director of the present national trigonometrical survey of Britain; for which honourable office he was selected, at a very early age, by the discerning judgment of his grace the Duke of Richmond, when he so ably filled the office of master general of the ordnance.

Mr. M. enters not into a strict examination of those fatal symptoms which sometimes succeed the operation in grown subjects, in order to prove that they in general proceed from the bladder's being too roughly dealt with, because the case is of itself very evident; unless the habit be remarkably bad, to what else shall we attribute violent pain, and the successive inflammation, tumour, suppression of urine, mortification, &c.? These surely are not the attendants on a simple incision only; for constant experience evinces, that the bladder, though an organ of great importance, and essentially necessary to the animal economy, may be wounded with as little danger of any of the above-mentioned evils, as any other membranous part.

But probably we should not be at a loss for the true cause of all those mischiefs, if the state of the parts in the extraction of a large stone be closely considered. It may be observed, when a stone is laid hold of by the forceps, that both together, stone and forceps, from the screw-pin to the former, form a complete wedge; insomuch that a person in a forcible extraction, can hardly conceive the power applied to the bladder, or the force with which it is distended. If the diameter of the stone be equal to a third part of the length of the chops of the forceps, a force of 10 lb. applied to them, will be to the wound of the bladder as 30: but how shocking must be the case, when, either on account of the magnitude of the stone, or narrowness of the wound, a man uses his utmost force, and many such instances in adult bodies occur. The power is then augmented by the action of the lever to 2 or 300, a force no doubt sufficient to rend the bladder and neighbouring parts to rags. And there is too much reason to believe, that the want of success in subjects arrived at adult age, where the stones are almost always large, is owing intirely to this very circumstance.

When all this violence is insufficient, there is at present no other established method, but either to attempt the making a second incision on the stone, as it is held in the forceps, or to withdraw the latter, and to make it on the bladder, in the flaccid state as it then lies, without any guide at all. As to the first method, it is evident that the forceps, stone, and bladder in men, are so much in the dark, that the incision must be made with the utmost difficulty; indeed it is hardly possible to cut at all with any certainty. And the other way of cutting on the bladder, when the forceps is withdrawn, is much worse; for if it be remembered, that the bladder lies upon, and is contiguous with the rectum, and that they are both in the same flabby state, it will appear almost impossible to cut the one, without wounding the other.

This manifest defect in the operation would be intirely removed, if there always was a director for the knife left in the bladder; and this is so easily and completely to be done, that its great simplicity seems to be the reason it has not been attended to. If one limb of the forceps, from the joint to its extremity, be converted into a staff, by making a deep groove through its whole length, it will better answer the end desired, than if it were possible to suffer the staff itself to remain in the bladder during the operation; for, when a large stone is laid hold of, and the forceps of course much devaricated, insomuch that the extraction of the stone is found to be impracticable with safety, without a second incision on the parts on the stretch; you then slide the point of a narrow knife on the groove of the forceps quite on into the bladder, and it will divide the very part which needs it most; while the rectum, on account of its flaccid state, will most certainly escape wounding. If the wound is desired to be but little larger, the forceps must be but gently drawn forward when the incision is made; but if, on the other hand, the stone prove extremely large, the forceps should be then drawn forwards with a force sufficient to put the bladder on the stretch; and by this management the dilatation may be made as great or little as is required.

As the common knife is not so proper for this purpose on account of its breadth, Mr. M. got one made of a more convenient form, with a gorget-handle and button point, as in the bubonocele knife. This method, as it removes all occasion for violence to the bladder, will reduce the effects of the operation nearly to those of a mere incision only, which might, as now, sometimes prove fistulous, but he believed hardly ever mortal.

He would not be understood to mean that it should be used in common cases, and moderate sized stones; no, Mr. Cheselden has sufficiently demonstrated to the world, by experiment, the inconvenience of cutting beyond the prostate, when it can be avoided. He would only say, that where a large stone offers, and the circumstances are such, that the operator is under a necessity of tearing the parts to pieces, unless divided by an incision, the latter is more safe and eligible.

The principal advantages of this contrivance are, 1. That the degree of dilatation is in the operator's power, to the greatest exactness. 2. That it will infallibly divide those parts only which are on the stretch; and for this reason, 3. The rectum, if it is empty, and consequently flaccid, must certainly escape wounding. Its great simplicity is another circumstance much in its favour; for it seems allowed on all hands, that the less an operation is incumbered with instruments, the better. In common cases, where the assistance of this method is not wanted, the forceps is not the least incommoded as to its general use; but if the stone prove large, which can never be certainly known till it is laid hold of, the remedy is at hand.

These forceps and knife are represented in fig. 1, 2, 3, pl. 11. Fig. 1, the forceps, with the knife, ab, applied to one of its cheeks; c, the button; bfg, the handle. Fig. 2, the cheek of the forceps, with the groove in it de. Fig. 3, the knife; the blade ab; the button c, made to fit, and slide along the groove de;

bfg the handle; cab the sharp edge; chf the back of the knife, which must have the same curve as the back of the cheek of the forceps or groove to which it is to be applied, as in fig. 1.

Of the Locusts, which did vast Damage in Walachia, Moldavia, and Transilvania, in 1747-8; and of some Swarms of them which, in July and August 1748, passed into Hungary and Poland. By a Gentleman residing in Transilvania. N° 491, p. 30.

It is certain that the locusts came into Transilvania from Walachia and Moldavia, particularly through those narrow openings in the mountains, commonly called passes; the most considerable of which, in the neighbourhood of Clausenburg, is called the pass of the Red Tower; and through others not far from Karlstat, which are common roads from Transilvania into Moldavia and Walachia.

The first swarms entered into Transilvania in August 1747; these were succeeded by others, which were so surprisingly numerous, that when they reached the Red Tower, they were full 4 hours in their passage over that place; and they flew so close, that they made a sort of noise in the air, by the beating of their wings against each other. The width of the swarm was some hundreds of fathoms, and its height or density may be easily imagined to be more considerable, inasmuch as they hid the sun, and darkened the sky, even to that degree, when they flew low, that people could not know one another at the distance of 20 paces. But as they were to fly over a river that runs in the vallies of the Red Tower, and could find neither resting-place nor food; being at length tired with their flight, one part of them lighted on the unripe corn on this side of the Red Tower, such as millet, Turkish wheat, &c.; another part pitched on a low wood: where having miserably wasted the produce of the land, they continued their journey, as if a signal had been actually given for a march. The guards of the Red Tower attempted to stop their irruption into Transilvania by firing at them; and indeed where the balls and shot swept through the swarm, they gave way and divided; but, having filled up their ranks in a moment, they proceeded on their journey.

They were of different forms, according to their different ages; for when, in the month of September, some troops of them were thrown to the ground by great rains, and other inclemency of the weather, and thoroughly soaked with wet, they crept along in quest of holes in the earth, dung, and straw; where, being sheltered from the rains, they laid a vast number of eggs, which stuck together by a viscid juice, and were longer and smaller than what is commonly called an ant's egg, very like grains of oats. The females, having laid their eggs, die like the silk-worm; and the Transilvanians found by experience, that that swarm which entered into the fields by the Red Tower, did not seem to intend remaining there, but were thrown to the ground by the force of the wind, and there laid their eggs; a vast number of which being turned up, and crushed by the plough, in the beginning of the ensuing spring, yielded a yellowish juice.

In the spring of 1748, certain little blackish worms were seen lying in the fields and among the bushes, sticking together, and collected in clusters, not unlike the hillocks of moles or ants. As nobody knew what they were, so there was little or no notice taken of them; and in May they were covered by the shooting of the corn sown in the winter. But the subsequent June discovered what those worms were; for then, as the corn sown in the spring was pretty high, these creatures began to spread over the fields, and become destructive to the vegetables by their numbers. Then at length the country people, who had slighted the timely warning given them, began to repent of their negligence; for, as these insects were now dispersed all over the fields, they could not be extirpated without injuring the corn.

At that time they differ little or nothing from common grashoppers; having their head, sides, and back of a dark colour, with a yellow belly, and the rest of a reddish hue. About the middle of June, according as they were hatched sooner or later, they were generally a finger's length, or somewhat longer, but their shape and colour still continued.

Towards the end of June they cast off their outward covering; and then it plainly appeared that they had wings, very like the wings of bees, but as yet unripe and unexpanded; and then their body was very tender, and of a yellowish green: then, in order to render themselves fit for flying, they gradually unfolded their wings with their hinder feet, as flies do. And as soon as any of them found themselves able to use their wings, they soared up, by flying round, the others were provoked to join them: and thus their numbers increasing daily, they took circular flights of 20 or 30 yards wide, till they were joined by the rest; and, after miserably laying waste the native fields, they proceeded elsewhere in large troops.

Wherever those swarms happened to pitch, they spared no sort of vegetable; they eat up the young corn, and the very grass; but nothing was more dismal to behold than the lands in which they were hatched; for they so greedily devoured every green thing there, before they could fly, that they left the ground quite bare.

There was nothing to be feared in those places to which this plague did not reach before the autumn; for the locusts have not strength to fly to any considerable distance, but in the months of July, August, and the beginning of September; and even then, in changing their places of residence, they seem to tend to warmer climates.

Different methods of destruction are to be employed, according to the age and state of these insects; for some will be effectual as soon as they are hatched; others when they begin to crawl; and others again when they begin to fly. And experience has taught the Transilvanians, that it would have been of great service, to have diligently sought out the places where the females lodged; for nothing was more easy, than carefully to visit those places in March and April, and to destroy their eggs or little worms with sticks or briars; or if they were not to be beaten out of the bushes, dunghills, or heaps of straw, to set fire to them; and this method would have been very speedy, convenient, and successful; as it has been in other places. But in the summer, when they have marched out of their spring quarters, and have invaded the corn fields, &c. it is almost impossible to extirpate them, without thoroughly threshing the whole piece of land that harbours them, with sticks or flails, and thus crushing the locusts with the produce of the land.

Finally, when the corn is ripe, or nearly so, there is no other method of getting rid of them, or even of diminishing their numbers, but to surround the piece of ground with a multitude of people, who might fright them away with bells, brass vessels, and all other sorts of noise. But even this method will not succeed, till the sun is pretty high, so as to dry the corn from the dew; for otherwise they will either stick to the stalks, or lie hid under the grass. But when they happen to be driven to a waste piece of ground, they are to be beaten with sticks or briars; and if they gather together in heaps, straw or litter may be thrown over them, and set on fire. But this method serves rather to lessen their numbers, than totally destroy them; for many of them lurk under the grass or thick corn, and in the fissures of the ground, from the sun's heat: hence it is requisite to repeat this operation several times, in order to diminish their numbers, and consequently the damage done by them. It will likewise be of use, where a large troop of them has pitched, to dig a long trench, of an ell in width and depth, and place several persons along its edges, provided with brooms, and such like things, while another numerous set of people form a semicircle, that takes in both ends of the trench, and encompasses the locusts, and, by making the noise above-mentioned, drive them into the trench; out of which if they attempt to escape, those on the edges are to sweep them back, and then crush them with their brooms and stakes, and bury them, by throwing in the earth again.

But when they have begun to fly, there should be horsemen on the watch in the fields, who on any appearance of the swarm taking wing, should immediately alarm the neighbourhood by a certain signal, that they might come and fright them from their lands by all sorts of noise; and if, tired with flying, they happen to pitch on a waste piece of land, it will be very easy to kill them with sticks and brooms, in the evening, or early in the morning, while they are wet with the dew; or any time of the day in rainy weather; for then they are not able to fly.

It has been already noticed, that, if the weather be cold or wet in autumn, they generally hide themselves in secret places, where they lay their eggs, and then die: hence great care should be taken at this time, when the ground is freed of its crop, to destroy them, before they lay their eggs.

In the month of September 1748, certain intelligence was received, that several swarms of locusts came out of Walachia into Transilvania, through the usual inlets, and took possession of a tract of land in the neighbourhood of Clausberg, near 3 miles in length; where it was not possible to save the millet and Turkish wheat from these devourers.

The eggs of these animals, which have been preserved in dry mould, have produced nothing; but those that have been preserved in mould moistened with water from time to time, gave early in the spring of 1749 some of these grashoppers. The little ones were, soon after they came forth, of the size nearly of ordinary flies: They had already the form of grashoppers, but they had as yet no wings. This observation shows, that the author of the foregoing account was mistaken, when he says, "these insects had at first the form of grubs, or small worms." They change their skin several times, but they do not acquire wings till they have changed for the last time.

The grashoppers taken in England in 1748, have been compared with those that have been sent over from Hungary and from Poland that same year, and they have been found to be perfectly of the same kind. There are in Sir Hans Sloane's collection* some of the same sort of locusts or grashoppers, preserved in spirits of wine, and which were taken up here above 30 years before, and are exactly like those from Egypt and Barbary.

Concerning some Vertebræ of Ammonitæ, or Cornua Ammonis. Communicated by Mr. Henry Baker, F.R.S. N° 491, p. 37.

This curious fossil was sent to Mr. B. by Dr. Miles, of Tooting, F.R.S. It consists of 26 joints, which he calls vertebræ, and supposes to have been the joints of the back bone or tail of some animal; but, on considering them with attention, they will perhaps rather be judged to be the several articulated divisions that compose the body of some kind of nautilus, or of some one of the various species of the ammonitæ: which opinion is supported, not only by the spiral figure, which they form when put together, but also by the traces or markings of such articulations, found on some particular kinds of fossil nautili and ammonitæ.

All the parts of this uncommon fossil are converted into a kind of sparry sub-

* See Sir Hans Sloane's Hist. of Jamaica, vol. I. p. 29.—Orig.

stance, and that they are articulated with one another in an exact and beautiful order. They were fastened together in 2 divisions, that they might be examined more easily than when separate, and in confusion: and indeed Mr. B. was not quite satisfied that these 2 bundles belonged both to the same individual animal; if they did, some joints must be wanting that came between them, and united them together, as the two ends do not at present match: and what made him suspect they did not, was a different articulation to be observed on one side of that division made up of the largest joints: besides, the whole number appears rather too much, and the smaller joints seem to make up a body whose figure is nearly perfect.

The Case of Mr. Smith, Surgeon, at Sudbury, Suffolk, the Coats of whose Stomach were changed into an almost Cartilaginous Substance. By the Rev. Mr. Murdock. N° 491, p. 39.

Mr. Smith was in the vigour of life, being only 36 years of age; and, to all appearance, of a strong well set habit. His way of living was quite regular; but his practice of midwifery, which was pretty large, often forced him in severe weather from a warm bed into bad roads, and sometimes into raw uncomfortable houses.

He had for several years complained of uneasiness at his stomach; but it was not considerable, till about January 1746-7. From that time, he almost constantly threw up his food within an hour or 2 after taking it, and he felt violent pain about the scrobiculus cordis. Several physicians were consulted, but medicines availed him nothing; nor had he any ease, except from opiates, or spirituous liquors; and this was of short continuance.

It being in the September following, recommended to him to go to Bath, he for some weeks drank the waters, and afterwards bathed. The first had no remarkable effect; but he found himself worse after bathing. On his return home, new physicians were consulted, and new methods were tried, but to no purpose; and, to make life tolerable, he was forced to be very free in the use of spirituous liquors and opiates.

In Feb. 1747-8, he voided, by 2 or 3 stools, about a couple of ounces of matter. Some weeks before his death the pains went off, and his vomiting was at times stayed; but whenever that happened, whatever he took, ran directly through him. And indeed he was now and then, during the whole illness, subject to bilious dejections.

On this remission of the symptoms, his friends flattered themselves yet that all might do well; but his wasting, which had long begun, continued; and his legs, especially one of them, became œdematous. After growing gradually weaker, till nature was quite spent, he expired, with the utmost serenity of mind, on the 7th of August.

His body, pursuant to his request, being opened in the presence of Dr. Scarling, and 3 or 4 surgeons, the coats of the stomach were found changed into a uniform, white, inelastic, almost cartilaginous substance, which was $\frac{2}{10}$ of an inch in thickness. Besides this strange alteration in its coats, the stomach was so contracted, as to be incapable of holding more than 5 or 6 oz. and its inner surface was besmeared with a various coloured matter. The rest of the viscera seemed to be quite unaffected, and every thing was in its natural situation, except the omentum, which, besides being, as it is in all tabid bodies, vastly wasted, was necessarily drawn upwards by the contraction of the stomach. The following remarks are subjoined:

It is highly probable, that this gentleman's disorder, whether constitutional or acquired, was at first an obstruction in those glands, which separate the humour that serves to defend the villous coat from the acrimony of what is taken into the stomach, and to prevent its being stimulated by the aliment in digestion; for want of which it was so subject to irritation, that scarcely any thing would stay on it. The matter voided by stool was undoubtedly formed in the stomach, because he never complained of considerable pain in any other part; besides, had it been from an abscess in the intestines, or any other of the viscera, the seat of it would in all likelihood have been apparent. The looseness, which in the latter part of his illness, always attended him when the vomiting ceased, plainly shows, that the stomach had at that time acquired a great, if not its greatest degree of contraction; for which reason, as it could contain but little, any quantity of food must, if not thrown up, go immediately downwards. The going off of the pain some weeks before his death, was owing to the sensibility of the coats of the stomach being in a great measure, or quite destroyed. The bilious dejections, that frequently attended him, may be ascribed to want of digestion; which, as little or no chyle was sent into the duodenum, rendered the bile useless. The consequence of this was a non-secretion of that humour, an accumulation of it in the liver, or gall bladder; its being reconveyed into the blood; or its going off by stool. If the first or third had been the case, it would have shown itself in a jaundice; if the second, there would have been an abscess in the liver or gall bladder; so that of course it must run off by stool. Spirituous liquors might help to bring on this contraction, inelasticity, and insensibility of the stomach: but it seems pretty clear that they were not the sole cause; otherwise immoderate drinkers of them would generally be affected in the same manner.

A Catalogue of the Fifty Plants from Chelsea Garden, presented to the Royal Society by the Company of Apothecaries, for the Year 1746, *&c. &c.* N° 491, p. 43.

[This is the 25th presentation of this kind, completing to the number of 1250 plants.]

Concerning a Boy, who had a Calculus formed between the Glans and the Præputium. By the Reverend Mr. Robert Clarke. Dated Houghton Conquest, Jan. 21, 1749. N° 491, p. 45.

This boy from his infancy laboured under a difficulty of making water; for though he was 3 years of age, when put under the care of the person from whom this intelligence was received, yet he could not go alone. He was in the greatest agony on every motion; but was relieved by putting a great pin, the head foremost, an inch, at least, between the prepuce and the stone; when sometimes a mucous, sometimes a gritty matter, would first ooze out, and then came the urine with some violence.

During this time, the end of the penis was observed to grow in the form of, and as large as a hen's egg; occasioned, as may be imagined, by the concretion of the stone between the glans and the prepuce. At last there was a total suppression of urine for 48 hours at least, attended with an appearance of a sore in the side of the penis, out of which the stony concretion sent by Mr. C. dropped; and from that time, all the while the boy continued under the same person's care, which was about ½ a year, the urine was discharged at the side, with less and less pain. Last summer he came to see his quondam nurse, and told her that he was then perfectly well. His age then was about 23. His name John Blackhouse.

The Establishment of a New Genus of Plants, called Salvadora, with its Description. By Laurence Garcin, M.D., F.R.S. of Neufchatel in Switzerland.* N° 491, p. 47. *Translated from the French, by T. Stack, M.D.*

This plant is woody. It grows sometimes into a tree, sometimes into a shrub, and sometimes into a bush; spreading very tufted branches on all sides down to the ground. Its native countries are the parts adjacent to the Persic gulph, the north of Arabia, and the south of Persia. It does not appear that any author has known, or made the least mention of it.

Its characters are as follows: *Calix*, this is a monophyllous cup, divided into 4 lobes, which, as soon as they spread open, turn outward, and roll backward on themselves; then wither, grow whitish, and dry up.—*Corolla*, its flower is

* Salvadora persica, Lin. It belongs to the Tetrandria Monogynia.

void of petals.—*Stamina*, these are four in number, answering to the 4 lobes of the *calix*, and being likewise of the same length; they spring from the basis of the *pistillum*, and as they shoot up, tend outward. Their summits are round, with a furrow turning in on one side; which gives each of them the form of a purse.—*Pistillum*, it is round, its style single and short, and the *stigma* is blunt, and shaped like a navel.—The *pericarpium* is a round berry, of a middle size, with one cell or lodgement in it.—The *semen* is single, spherical, inclosed in a callous firm skin, beset with spots, forming a sort of husk like that of hemp.

As to the description: it is a plant which varies considerably in size; that of a larger sort of shrub, is what it most frequently grows to. It produces a number of boughs without order, and very tufted branches, which most commonly hang down to the ground. Its bark is moderately thick, sometimes smooth, sometimes full of cracks, of an ash colour, both in the trunk and branches, but green on the tender shoots. Its wood is every where brittle, and nearly of a straw colour.

The leaves are borne on young sprigs, which shoot out along the boughs. These sprigs are straight, generally short, but sometimes pretty long, like little wands. The leaves are thick set, and tufted on the former, but thin on the latter. They grow sometimes opposite to one another by pairs, crossing alternately; and sometimes by 3 and 3, disposed like rays; but this more rarely. Their length, which varies on the same stalk, is generally from an inch and half to two inches and half, and their width is from nine lines to an inch a little below the middle in each, which is the widest part. They are thick, pointed at their extremity, and rounded at their base, very even on their edges, somewhat succulent, but firm: their colour is a pale green, but somewhat yellowish, in those that are shooting out. The pedicles which support them are very short, each being but a half a line in length, and a quarter in thickness.

The flowers, which are stamineous, that is, without petals, are small, and disposed in clusters on the tops of the shoots. These bunches of flowers entirely resemble those of the vine blossoms. The empalement is small, green on the under side, having 4 segments almost pointed, which roll outward, and then dry up. Its diameter in this rolled state of its lobes, is but of one line. The stamina are of a straw colour. The hollow furrow in each of their summits is not easily discovered without a glass.

The pistil or embryo of the fruit, which is small, and yet occupies the whole inside of the calyx, is of the same colour with the bottom of this, viz. green. Afterwards it swells in all dimensions, and grows into a berry, of the shape and size of a gooseberry, of 3 or 4 lines in diameter. At first it is of a pale green, then a bright purple, and in its maturity of a dark red. Each berry is supported on a strong thick pedicle, attached to a small bunch. Its substance is a white transparent flesh, full of juice, much resembling jelly, which surrounds a single round grain, marbled with black or brown spots, as in the tortoiseshell, when ripe. This grain is as large as a grain of hemp seed, that is about 2 lines in diameter, but sometimes less. It is properly a kernel, or a shell with a cavity, which incloses a sort of little round almond, of a straw colour, yellowish on its outer surface, and pale in its inner substance, which is pretty firm.

As to its qualities: all the parts have an acid pungent taste and smell, vastly like our garden cresses, but more biting. The fruit is the most pungent part of the whole. The smell of the plant is perceptible at 7 or 8 paces distance, when a person is to leeward.

The natives of the country use it against the bite of the scorpion, by rubbing the wounded part with its bruised leaves. They also employ its warm infusion to wash the bodies of their children, in order to keep them healthy. And they feed camels with it, who love it naturally.

The name of Salvadora, here chosen for this shrub, is that of the late Mr. Salvador of Barcelona, a very skilful botanist, of whom Mons. de Tournefort makes mention in his introduction, which serves for a preface to his Institutiones Rei Herbariæ, where he styles him the phoenix of his nation; because he was really the richest naturalist, and the most expert in botanical matters that Spain ever produced.

A State of the English Weights and Measures of Capacity, as they appear from the Laws, as well Ancient as Modern. Being an Attempt to prove that the present Avoirdupois Weight is the legal and Ancient Standard for the Weights and Measures of this Kingdom. By Samuel Reynardson, Esq. F.R.S. N° 491, p. 54.

It is declared by Magna Charta, (c. 25,) that there should be, throughout the realm, one measure of wine, one of ale, and one of corn; viz. the quarter of London; and that it should be of weights as of measures.

This declaration has been repeated in many subsequent laws, and by several of them the treasurer is directed to provide standards of bushels, gallons, and weights, of brass; and to send them into every county; and all measures are to be made according to the king's standard; the assize which is established by several laws as follow: "the English penny, called a sterling round, without clipping, to weigh 32 grains of wheat, dry and taken from the midst of the ear. 20 pence make an ounce, 12 ounces a pound. 8 pounds make a gallon of wine; 8 gallons of wine make a London bushel, which is the 8th part of a quarter." And by other laws it is declared, "that the tun of wine, oil, and honey, should contain, of the English measure, according to the ancient assize, 252 gallons;

the pipe or butt 126; the tertian 84; the hogshead 63; and every barrel 31½, according to the old assize, and to be gaged by the king's gager."

In the reign of Edward the 3d, an act passed, to take away the weight called ancell, by which, and subsequent statutes, it is directed, that every sale and buying should be by the even balance.

In the 11th year of Hen. 7th, complaint being made to the parliament, that the ancient statutes and ordinances of the realm, relating to weights and measures, had not been observed and kept, it was therefore enacted, "that there should be delivered to the knights and citizens of every shire and city, one of every weight and measure, which the king had caused to be made of brass, according to his standard in the exchequer, to be delivered to the respective places mentioned in the act; and that the inhabitants of all cities, boroughs, and market towns, should make and use weights and measures made according to the weights and measures so delivered as aforesaid." In the next year another act passed, reciting, "that the king had made such weights and measures of brass, according to the old standard thereof remaining within his treasury; which weights and measures, on more diligent examination, had been found defective, and not made according to the statutes and old laws, and were therefore recalled, and ordered to be broken, and other new bushels and gallons were directed to be made and sised, according to a new bushel and gallon to be made according to the assize, to remain in the king's exchequer:" where we now find a bushel in the custody of the chamberlains called the Winchester bushel, and a gallon agreeing to it: on the bushel is the inscription, Henericus septimus Dei gratia Rex Angliæ et Franciæ.

In the last mentioned act, the assize for weights and measures is in substance the same as in the old statutes, only the pound is said to be the pound Troy of 12 ounces. But since by this, and the former assize laws, the pound is directed to be raised from 240 sterling pennies, it follows, that the gravity of the assize pound was always the same; but the dimensions of measures of capacity respectively raised from a pound of wine, and a pound of wheat, will be in proportion to each other as the specific gravity of wheat to that of wine or water.

Thus continued the laws relating to the English standard of weights and measures till after the restoration; when a duty of excise being laid on beer, ale, and other liquors, 36 gallons taken by the gage, according to the standard of the ale quart, (4 of which made the gallon remaining in the exchequer) were to be reckoned as a barrel of beer, and 32 such gallons a barrel of ale; and afterwards 34 such gallons of vinegar (and of beer or ale strong or small without the bills of mortality) were declared to be a barrel; and all other liquors liable to the excise duty were to pay according to the wine gallon.

We now find the officers of the revenue determining the contents of our measures of capacity with great exactness: for, on the 25th of May, 1688, 2 general officers of the excise, in the presence of the lord mayor, the commissioners of excise, Mr. Flamsteed, and others, on an exact trial, found that the old standard wine gallon, kept in Guildhall, contained only 224 cubic inches; yet at that time it was thought convenient to continue the former supposed content, being 231 cubic inches, as the standard wine gallon, and which has since been established by a law.*

In the year 1696, an experiment was made, in order to fix the true and exact contents of the brass standard bushel of Henry 7th, which being filled with common spring water, and the water measured out with great nicety and exactness; the bushel † was found to contain 2145.6 solid or cubic inches; and the water being weighed by the standard weights in the exchequer, was found equal to 1131 oz. 14 dwt. Troy; and at the same time and place the standard Troy weights were compared with the standard avoirdupois, and 15 pounds of the latter were found equal to 18 pounds 2 oz. 15 dwt. Troy; which fixes the pound avoirdupois at 7000 such grains, as the Troy pound weighs 5760; and on three several trials, made by the gentlemen of the council of the Royal Society, at the exchequer, on a medium the avoirdupois pound was found equal to $7000\frac{1}{4}$ Troy grains.

By the first malt act, which passed soon after the making the experiment on the Winchester bushel, it is declared, that every bushel $18\frac{1}{2}$ inches wide, and 8 inches deep, should be esteemed a legal Winchester bushel: and the coal bushel is directed to be made $19\frac{1}{2}$ inches diameter, and to contain the last bushel and one quart of water. The first contains 2150.42 cubic inches, the last 2217.47.

We now see different measures established by law; and under the excise laws two different gages or measures, used for taking the dimensions of wine and ale vessels. The wine gallon contains 231 cubic inches, and the ale gallon 282; but on what foundation this last measure was established, is difficult to determine.

Troy weights had for some time been established and used for the money affairs in the mint, and for weighing gold, silver, and some few commodities; and the avoirdupois were in general use for weighing all heavy and gross commodities. Wine measure was generally considered as equal to Troy weight: from hence the managers of the excise duty were perhaps led to fix the standard of the ale gallon, bearing the same proportion to the wine gallon, as the avoirdupois pound did to the Troy; and according to this conjecture, the two gallons answer

* 5 Ann. c. 27. § 17.—This act says, any cylinder 7 inches diameter, and 6 inches deep, or any vessel containing 231 cubical inches, and no more, shall be a lawful wine gallon.—Orig.

† Everard's Stereometry, p. 193.—Orig.

pretty exactly;* the ale gallon exceeding the proportion by somewhat more than one cubic inch and one quarter; but it exceeds the Winchester gallon, or 268.2 cubic inches by very near 14 cubic inches: and not one of these measures is agreeable to the words of the assize, which directs, "that the bushel shall contain 8 gallons of wheat, the gallon 8 pound of wheat of Troy weight, the pound 12 ounces of Troy weight, &c." according to the old laws of this land. It is very plain, that the law-makers in Henry the 7th's time took the Troy weight for the standard; and most authors, who have written on this subject, have followed their example.

The great difficulty in fixing on a standard pound, agreeable to the assize, arises from the uncertainty of the rule laid down in our laws of assize for raising the pound from 7680 grains of wheat; as these grains differ in weight, in different countries, and in different years, it might be said in the same field, and in the same year. The uncertainty of a pound so raised might with great probability occasion the variety in our weights and measures, so often complained of in our ancient laws, and for the prevention of which Edward the 3d, in his 14th year, ordered "standard weights and measures to be made of brass, and sent into every city and town in the kingdom."

Magna Charta points out the quarter of London as the only standard for measures and weights of that time; but we are left to guess of what measure or weight it was the quarter part. If we suppose it the quarter of a ton, or 2000 pound weight, then the quarter was 500 pounds, and the 8th part of that, or a bushel, was equal to a cubic foot, or 62¼ lb.; whence less measures and weights were easily deduced. Subsequent assize laws direct the greater measures to be raised from the less; that 8 pounds should make a gallon; 8 gallons a bushel; which was to be the 8th part of a quarter; and by this rule the quarter is raised to 512 pounds, and the ton to 2048 pounds. These measures and weights are raised with ease from known parts of the foot. For a cubic vessel, whose sides are equal to $\frac{1}{10}$ of a foot, will contain a cube of spring water equal to an ounce Avoirdupois; and hence, by a regular geometrical progression, we shall obtain cubes equal to 8, 64, 512 oz. or to 4, 32, 256, 2048 lb. Avoirdupois: and from a cubic vessel containing one such pound, we shall have other cubic vessels, equal in weight to 8, 64, 512 lb.; and in measure to the gallon, bushel, and quarter, according to the assize.

The gallon, bushel, and quarter, are called dry measures; and are used for ascertaining the quantity of corn, and other dry goods; the gallon is also a liquid

* For, as 144 : 175 :: 231 : 280.729.—And as 144 : 175 :: 224 : 272.222. This last comes very near the vulgar dry gallon.—Orig.

measure, raised from a pound, in liquids now called a pint; whence all the other liquid measures are raised; but with this difference in the proportion, that the liquid bushel is not 64, but 63 pounds or pints; 8 of which make the hogshead equal to 63 gallons; whence the contents, as well of the larger as smaller vessels, or measures of capacity, are settled.

The measures of capacity thus raised, are sufficiently convenient for common use, and are generally retained at this time; but for weights, there has been some variety from time to time, in the composition of the larger sort, used for determining the weight of merchandise and heavy goods, as will appear from the following extract from several old acts of parliament. The stone, for weighing lead, was settled at 12 lb; for wax, sugar, spices, and alum, at 8 lb; of which last, 13½, or 108 lb. made the hundred-weight: the sack of wool was to weigh but 26 stone, 14 lb. to each stone: a wey of cheese 32 cloves, each clove 7 lb. And for many years past, the hundred-weight has been fixed at 112 lb. Avoirdupois, and that by a general consent, and without any particular law to establish it.

These weights have been universally and immemorially used in England, with an exception to the weighing of gold, silver, and some very few commodities, for which the Troy weight has been used for a great many years. When it was first introduced into this kingdom, no where appears; but Mr. Folkes, in his tables of the English silver coins, tells us, it was not established or used at the mint before the 18th of Henry the 8th.

By reducing the liquid bushel, or one 8th of the hogshead, from 64 to 63 pints, it seems plain that our ancestors took the cubic foot for their model; the contents of such a vessel being 62½ pints or pounds: and hence it is not very unnatural to conclude, that at first our ancestors fixed and established, as well their weights as measures, from known parts of this model; taking always a whole number for each primary weight or vessel; and thence proceeding, by a regular geometrical proportion, to raise the greater weights or measures: so that the English foot (the undoubted and universal standard of all measures of length within this realm) is also the standard for the Avoirdupois * weights, and all measures of capacity.

* The very name Avoirdupois, by which our common weights are known, has by some been considered as a proof that they were of foreign extraction. The first time the word is used in our laws, is in an act of Ed. III. St. 1, where it is applied to wines as well as corn; as it is afterwards in 25 Ed. III. St. 3, c. 2, and 16 R. II. c. 1. And in an act 27 Ed. III. St. 2, c. 10, is the following clause: "Because we have perceived some merchants buy Avoirdupois merchandises by one weight, and sell by another, we will and establish, that one weight, one measure, and one yard be through all the land; and that wools, and all manner of Avoirdupois, be weighed by even balance." This king, in his 14th year, had directed standard weights to be made of brass, and sent into every city and town; and probably those standards, from the words of the foregoing clause, took the name of

On the whole therefore, it seems sufficiently proved, that a cubic vessel, whose sides are equal to an English foot, will contain 1000 oz. Avoirdupois, or very near that weight of spring-water: that weights and measures, deduced by a regular geometrical progression from such a vessel, or from cubic vessels, whose sides are equal to known parts of an English foot, bear an exact analogy to each other, and to weights and measures raised from a pound, according to the words of our most ancient assize laws. This being considered, and that the Avoirdupois weight is now in common use for determining the gravity of all heavy bodies; that this weight now is, and immemorially has been, used for settling the ancient duty of tonnage and poundage on all goods and merchandise taken by weight, except some few drugs, which are charged in the book of rates by the ounce Troy; and that there is not the least proof, either in our ancient or modern laws, to induce a belief that this duty was ever generally taken by the Troy weight, or that Troy weights were ever in general and common use in this kingdom, it must surely be allowed, that the weight mentioned in our old laws, or acts of parliament, was the Avoirdupois weight.

Bishop Cumberland, in his Treatise, says, "that our English Avoirdupois oz. is the same as the Roman ounce; and was probably introduced into this kingdom by the Romans, when they gave laws and planted colonies here, and has thence continued unchanged to this day; which is not commonly observed, because we use the Avoirdupois weights only about heavier commodities; not in weighing silver and gold, which are weighed by the Troy ounce; which I suppose was introduced by the Normans, because it takes its name from a French town, Troyes in Champagne." Most authors have been of this opinion. This leads us to compare our English foot with the Roman foot, which Mr. Greaves takes as equal to 967 such parts, as ours is 1000. The Roman amphora or quadrantal is generally allowed to be equal to a cubic Roman foot; and to contain 80lb. or 960 oz. Then the side of the amphora is equal to 986 parts of the English foot; agreeing exactly with the foot deduced by Villalpandus from the congius of Vespasian; and a cubic vessel, whose sides are equal to 967 parts of the English foot, will not contain quite $904\frac{1}{4}$ ounces; which, if true, reduces the Roman ounce to near $412\frac{1}{11}$ grains Troy.

Avoirdupois, and were the weights by which the merchants used to buy. What were the lighter weights by which they sold, does not appear; perhaps the pound Troy. That the former were the lawful weights, appears by an act 24 Hen. VIII. c. 3, where they are so called; and butchers, who before that time sold their meat by hand, were thereby obliged to provide themselves with beams, scales, and weights sealed, called Haberdepois (for Avoirdupois); and in the next reign the Avoirdupois weights, now remaining as standards in the Exchequer, were deposited there, as appears from the name and inscription on them.—Orig.

The Table of	Bushels in Cube Inch.	Gallons in Cube Inch.	Pints in Cube Inch.	Weight of the gallon in Avoirdupois pounds.
By the Coal Act	2217.47	277.183	34.648	10.025
By the Malt Act	2150.42	268.8	33.6	9722
Winchester bushel	2145.6	268.2	33.525	9.6
From, the wine gallon ..	1848	231	28.875	8.354
The Guildhall gallon	1792	224	28	8.101
16 oz. Avoirdupois	1769.472	221.184	27.648	8
12 oz. Troy	1456.0224	182.0028	22.75035	6.5826
The following are not supported by any law or authority.				
The vulgar dry measure ..	2178	272.25	34.0625	9.8468
The ale measure	2256	282	35.25	10.1995

On the Cure of Dry Gangrenes: with a Description of a New-invented Instrument for the Extirpation of Tumours out of the Reach of the Surgeon's Fingers. By Dr. le Cat, F.R.S. Translated from the French by Ph. H. Zollman, Esq. F.R.S. N° 491, p. 72.

From the year 1725 to the year 1733, when M. le Cat frequented the hospitals of Paris, he saw that a great many persons there died of a dry gangrene, and even that nothing was done to them. He knew that amputation had been attempted in vain; but he did not see any of the practitioners endeavour to cure that distemper by remedies; nor did he think that cure impossible. He suspected, that the cause of the dry gangrene, was the want of a flow of the arterial blood, and of the spirits, into the part affected: and this want, as it accounts for the deadness, which cannot miss befalling the part, and the dryness which attends this sort of mortification; whereas in the humid gangrene, it is chiefly the return of the blood which is hindered, and thereby the fluids are accumulated, that swell and distend the part.

Two causes so opposite must demand also very different cures. The distension which characterises the humid gangrene, shows the necessity there is for scarifications and evacuations; as the dryness of the other gangrene points out the uselessness of these operations in this sort.

In the humid gangrene, the solids are choaked up, and overflowed with the accumulated liquid; the nerves are there stupified and benumbed: what can be more proper then after scarifications, which disgorge and relax those regions, than to apply stimulating topics? tonics, which restore the spring, the tone of the solid parts; help them to expel those superfluous liquors, the spirits of which are as it were drowned and suffocated; and in short, the defect being local, if those succours do not suffice, it is very common to cut off a limb; the loss of which may bring on that of the whole person.

On the contrary, in the dry gangrene, the solids are void of fluids of all sorts. The neighbouring regions, which begin to share of that want, are affected with the most cruel pains: if you attack those parts with the cutting instrument, you increase the irritation of the solids, the constriction of the vessels, the want of fluids, the exsiccation, and hasten death.

On the contrary, the general indication, which this distemper affords us, is therefore to soften, to relax the vessels, to draw thither the liquors by topics, while inwardly all remedies must be given that are capable of bringing the blood and the spirits from the centre to the circumference. If this method be not successful, death is inevitable; for even supposing that the amputation was not liable to the dismal consequences just specified, there is no room for this operation in an internal defect, which depends on the whole habit, as the case is with the dry gangrene. And supposing that there are dry gangrenes purely local, as the critical depositums of certain malignant fevers, you may be sure that the very same nature which has caused this crisis, if you assist her but a little, will be able also to separate this mortification from the sound parts; and she will do it more gently and more dextrously than man.

These were the notions he had formed to himself of these 2 sorts of gangrenes; he only waited for opportunities to make the trials which this theory suggested to him. He did not find any before 1738, in the person of a wood merchant, one Mrs. Fournaise. She was then 65 years of age, extremely corpulent; the gangrene seized her at the heel, by a black and round blotch, of 2 inches diameter, without any tumour, with some small scorbutic spots, great pains, and a little fever.

The plethora made him begin with bleeding and purging; the latter he repeated every 8 days. He applied all over the foot and part of the leg, a poultice made of herbs and farinas, emollient, resolving, and aromatic, the suppurative ointment, and storax. He gave inwardly diaphoretic ptisans: in the morning, broths of vipers, of cray-fish prepared with proper herbs, and above all with water-cresses: in the evening a bolus of theriaca. In short, he followed entirely the theory he had formed to himself about the dry gangrene, and in 9 or 10 days he saw the suppuration formed; so that his patient was perfectly cured in about 2 or 3 months. This success was followed by many others, both in the town and hospital of Rouen.

The report of these cures having spread itself as far as Paris, he was sent for thither in February 1746, to attend M. Rondé, treasurer-general of the fortifications of France, who was at the last extremity, by a dry gangrene, which had sphacelated his foot; the cause of which had kept him in a languishing way for 4 years. M. Rondé was in a dreadful condition, and Mr. le C. was vexed at having been sent for in so desperate a case: he declared it to his relations, and to the eminent surgeons who attended him: he therefore applied his remedies to the patient, at the request of his relations, only by way of trial, which he publicly declared to be most doubtful. However, contrary to expectation, from the

3d day there appeared plainly a beginning of suppuration, and a separation of the eschars. On the 9th day there was a complete suppuration, and the regeneration of the flesh was even so far advanced, that the singularity of it raised the curiosity of some of the first surgeons of Paris. At last, on the 15th day, the patient found himself in a more favourable state; which, according to the testimony which M. Morand, an assiduous spectator of the cure, publicly gave of it, 'gave hopes of a recovery in a case that had hitherto been thought desperate.'

The suppuration, the separation of the eschars, and the regeneration of the flesh, being all that a surgeon can desire in a like cure, he then thought that his mission was fulfilled, and that he might return home, whither he was called by more material affairs. He had reason to repent of this journey: his patient having naturally a very voracious appetite, he had confined him to a spare diet; but he could hold out no longer; and, by the connivance of his nurse, he took various food, and that plentifully too. This conduct had soon ruined their progress. M. le Cat returned to Paris; his representations were useless: the patient had shaken off the yoke of the faculty, and of reason. Indigestions ensued one upon another. The looseness, which never left him after, totally suppressed the suppuration, and made him void the matter quite crude by stool, and at last he died.

The following are the particulars of this case:

From this, and some other cases, Dr. le Cat infers, that the common opinion, that it is impossible radically to cure the dry gangrene, is as false, as the ordinary method of treating it is bad. He does not establish his method as infallible; but asserts, that in 8 or 9 years practice it has not failed curing any persons who exactly followed it, and observed the regimen prescribed.

Then follows M. le Cat's description of a surgical instrument of his invention; with a forceps for the extirpation of tumours too remote from the surgeon's fingers. When the fingers can lay hold of an excrescency, the surgeon need not think of making use of machines for it; he will never find any convenient enough; but all tumours that are to be extirpated are not within the reach of the fingers: there are even many of them which the fingers can reach, but where they cannot lay hold of them, nor work as the extirpation requires. Such are the excrescences situated a little deep in the anus, in the vagina, in the throat, &c. For the like extirpations M. le Cat had been obliged to invent the forceps now described.

Fig. 4, pl. 11, represents the forceps shut, as they are when the instrument is closed, or when it holds a small excrescence: it is of silver, pliant as far as aa, to enable the cheeks to take the different figures which those of the tumours to be extirpated may require. The inside of these branches is lined with a slip of buff-skin, or close chamoys (as at KK, fig. 5.) to prevent the tumour's slipping when once it has been laid hold of; BD is the extremity of the forceps, to be held in the hand of the operator; EF, GH, are the pieces which connect the 2 parts of the instrument, instead of the common joints or rivets of the other forceps, in a very advantageous manner. These pieces make the principal utility of this invention.

Fig. 5 represents the pincers taken to pieces in 2 parts. The piece E is to go into the notch gg, so that the screw f may pass through its slit, and that the nut F, put on the screw f, is to keep the whole together: but one thing which the figure cannot show, is, that this piece E is moveable in the direction lengthwise of the forceps, to answer the different openings of the cheeks; at b, fig. 4, is seen the pin on which this piece turns. The piece G also receives the screw h into its slit; and both are stopped by the nut H. But an essential remark, with regard to the piece G, is, that it must have the figure of an arch of a circle, the radius of which is the instrument itself; to the end that in the small extremity of the pincers, the ends of the cheeks may find themselves over-against each other, whatever opening may be given to the larger extremity, or to the handle GH.

To explain the use of these forceps: suppose an excrescence is to be extirpated, a condyloma of 2 inches depth in the rectum. You cannot lay hold of this tumour with the fingers, nor make it come out; yet it is very troublesome to the patient, and it would be requisite to slit or lay open the anus, to make room for this extirpation. With this forceps you will avoid this cruel preparative, and will with great ease make the extirpation.

First, introduce the fore-finger of the left hand into the rectum over the tumour to be extirpated, to make sure of the situation: with the right hand take the part of the forceps CD, and thrust it into the rectum, under the finger which is already there, and make it slide along the right side of the tumour, which to you is the left side. With the fingers of the left hand support this instrument in its situation, while with the right hand you introduce the other part of the forceps AB, and let it slide along the left side of the tumour, which is over-against your right hand. Without taking the fore-finger of your left hand out of the rectum, put together the parts of the instrument; press between its cheeks the tumour by its root; after which draw the fore-finger out of the rectum; take with the left hand the handle of the forceps BD, fig. 4; thrust along the right side of their cheeks underneath the knife, fig. 6, the button A being turned against the cheeks, and the back CD towards the inside of the rectum. Push this instrument as far as beyond the tumour under the extremity of the cheeks of the forceps, of which you may make yourself sure with the fore-finger of the left hand. Then raise towards the upper part the cheeks of the forceps, to prolong as much as possible the root of the excrescence; and in this condition pull towards yourself with the right hand, the knife, which does not fail cutting the tumour.

There are cases, in which the instrument fig. 7, will be of more convenient use. This cuts only by its crescent ab, pushed forward, and moved alternately from one side to the other, to assist its cutting.

There are circumstances in which the knife fig. 8, may be preferable.

Forceps for extracting stones, and other foreign bodies, lodged in the parts where the common forceps are of no use, are shown in fig. 9. The same mechanism just now described in the foregoing forceps, may be applied with advantage to the forceps with which the stone is pulled out, and to other instruments designed for extracting bullets, splinters of granadoes, pieces of iron, and other foreign bodies.

There are several cases in the cutting for the stone, in which no use can be made of the common forceps: the most frequent is this; when a stone, laid hold of by the ordinary forceps, escapes from the instrument half-way, and so remains engaged in the incision. The expedient commonly taken, is to push the stone back into the bladder, in order to have again the necessary room for managing the forceps; but besides the cruel pain in thus pushing back the stone into the bladder, this foreign body may enter into the cellular texture which surrounds the bladder, and lodge itself there, and then the forceps not having any longer that play which was endeavoured to procure to them, the stone will remain in that fatal lodgment, without possibility of pulling it out, and the patient will die.

The stone having stopped in the passage of the incision, you slide along the body of it one of the cheeks of the forceps, A or C, well oiled, which will be done without much trouble; the other cheek afterwards will pass on the other side; after which you join them, as shown above, taking care to press close the extremity AC on the stone, and to leave the largest opening on the side of the handle BD, as in fig. 10, both to hinder the stone from escaping, and to widen its passage; then, having well secured the screw G, you leave the screw F almost at liberty. You grasp the instrument with both hands, as near the stone as you can, and you draw the body out, managing it as is usual with the common forceps.

A second case of cutting for the stone, where these new forceps will be of great use, is this; when the stone is exactly embraced by the internal coat of the bladder; whether it completely fills this whole organ, or that it fills part of it, which may have closed itself on the stone; or that the stone has made itself a lodgment or bed in the inside coat of the bladder, prolonging itself towards the cellular texture, which surrounds a small part of those inside coats. In short, every foreign body lodged in the substance of any part of the human body, be it of what nature it will, becomes the object of our instrument; and the extracting of it will become much more easy by the means of these forceps, than by the bullet-drawers, and most of the other instruments invented for that purpose.

On the Parabolic Paths of Comets. By Nicholas Struyck of Amsterdam, F.R.S. From the Latin. N° 492, p. 89.

That tracing the courses of comets belongs to the principal parts of the sublimer astronomy, has been established since Newton, 63 years ago, published a problem on finding the paths of comets by 3 observations, on the hypothesis that they describe a parabolic orbit about the sun. By this method, Dr. Halley determined, by calculation, the paths of 24 comets, in a table in the Philos. Trans. N° 297. There are indeed 21 different comets. In like manner Mr. S. has noted 18 other comets, not found in that table, in hopes that the periodic times may at length be found. Of these, the path of the comets of 1723 and 1737 was determined by Dr. Bradley; that of 1744, by Mr. Betts; of 1699, 1702, 1739, by La Caille; the 2d comet of 1743, by Mr. Klinkenberg; the 2d of 1746 by M. des Chezeaux; of the 1st of 1748 by Maraldi. Mr. Struyck gave the observations of the comets seen in 1533, 1678, 1718, 1729, to Mr. C. Downes to calculate; but the comets of 1706, 1707, 1742, the first of 1743, and the 2d of 1748, Mr. S. has calculated himself. He also thinks that in May 1748, both at Amsterdam and other parts of Europe, on the very same night, 3 comets were visible; of which there is no other certain instance in history. He has also added the comet seen at the end of 1680, and beginning of 1681; because in the last edition of Newton there are emendations, by which is determined the ellipse it described about the sun. He further remarks, that of the 31 observations he had of the comet seen in 1742, there are 22 whose longitudes hardly differ 1′, and 23 whose latitudes differ not so much as 1′.

The following table shows the paths of the 19 comets above mentioned.

Figure of the Perihelion. Equations at London. Anno						Ascending Node.				Inclination of Orbit.			Perihelion in the Orbit.				Peribel. dist. from the Sun.	Motion.
1533	June	16d	19h	30m	0	♌	5°	44′	0″	35°	49′	0″	♌	27°	16′	0″	20280	Retro.
1678	Aug.	16	14	3	0	♍	11	40	0	3	4	20	♒	27	46	0	123802	Dir.
1699	Jan.	2	8	22	19	♒	21	45	35	69	20	0	♏	2	31	6	74400	Retro.
1702	March	2	14	15	19	♎	9	25	15	4	30	0	♌	18	41	3	64590	Dir.
1706	Jan.	19	4	56	0	♈	13	11	23	55	14	5	♊	12	36	25	42686 ½	Dir.
1707	Nov.	30	23	43	6	♉	22	50	29	88	37	40	♊	19	58	9	85904	Dir.
1718	Jan.	4	1	14	55	♌	7	55	20	31	12	53	♌	1	26	36	102565	Retro.
1723	Sept.	16	16	10	0	♈	14	14	16	59	59	0	♉	12	15	20	96942	Retro.
1729	June	12	6	35	41	♒	10	35	15	77	1	58	♒	22	16	53	406980	Dir.
1737	Jan.	19	8	20	0	♏	16	22	0	18	20	45	♒	25	55	0	22282½	Dir.
1739	June	6	9	59	49	♈	27	25	14	55	42	44	♋	12	38	40	67358	Retro.
1742	Jan.	28	4	20	50	♎	5	35	45	67	4	11	♏	7	33	44	76555½	Retro.
....	Dec.	30	21	15	16	♊	8	10	48	2	15	50	♋	2	58	4	83811½	Dir.
1743	Sept.	9	21	16	18	♈	5	16	25	45	48	21	♐	6	33	52	52157	Retro.
1744	Feb.	19	8	17	0	♉	15	45	20	47	88	36	♎	17	12	55	22206 1/10	Dir.
1747	Feb.	17	11	44	38	♌	26	58	27	77	56	55	♑	10	5	41	229388	Retro.
1748	April	17	19	25	4	♏	22	52	16	85	26	57	♏	5	0	50	84066⅔	Retro.
....	June	7	1	24	15	♉	4	39	43	56	59	3	♑	6	9	24	65525½	Dir.
1680	Dec.	7	23	9	0	♑	2	2	0	61	6	48	♐	22	44	25	617	Dir.

A Letter from John Jas. Huber, M.D., concerning a Dead Body, in which the Gall-bladder was wanting, and concerning a Gibbosity of the Sternum. An Abstract from the Latin. N° 493, p. 92.

1. In the dead body of a woman, about 60 years of age, Dr. H. found the liver to be destitute of a gall-bladder; but its place was supplied by a preternatural enlargement of the hepatic duct, which was wide enough to admit the little finger. The coat of this duct (the hepatic duct) was nearly as thick as the coats of the arteries, not white, but yellowish, villous within, and dotted with a number of small spots, which Dr. H. supposes to have been so many simple folliculi. From the liver to the duodenum this duct was of the before-mentioned diameter, so that it might easily have been taken for the vena portarum: but it opened into the duodenum by an orifice of the usual form and dimension. In like manner the pori biliarii were enlarged to the size of arteries; they were all of a yellow colour externally, and were turgid with bile.

2. Not in a single subject, but in the bodies of many children, Dr. H. had observed a deformity of the chest, which he imputes to an improper mode of handling and nursing. This deformity was particularly striking in the case of an infant 8 weeks old, in whom the sternum was so exceedingly prominent, and the cartilages of the ribs (at their junction with the sternum) were so much pressed inwards (more so on the left than on the right side), as to give to the thorax the resemblance of a saddle. On opening the thorax, several of the depressed ribs were found to be gibbous on the surface next the lungs, thus contributing not a little to the straitening of the cavity of the chest. The lungs were found obstructed, and adhering in many places to the pleura. Hence (remarks Dr. H.) it is easy to comprehend how children thus deformed die consumptive.

Dr. H. suspects that this deformity is occasioned by a very common but very improper mode of nursing, which consists in grasping the child's buttocks, and letting its body, inclined forwards, rest on the right hand applied, with expanded fingers, to the chest, and in this posture repeatedly tossing the child up and down. During this exertion, the whole weight of the child's body falls on the nurse's right hand, whence the pressure is often so great, as to cause the cartilages of the ribs (at that early period of growth) to give way, leaving pits or depressions in the places where resistance was opposed by the nurse's fingers. This was particularly conspicuous in the infant above mentioned. Those therefore who have the care of children should be cautioned against handling them in this manner. Dr. H. adds that the deformity above described must not be confounded with rickets.

The Operation of Lithotomy on Women. By M. le Cat, M.D., F.R.S. N° 492, p. 97. Translated from the French by T. S.

The lateral way of cutting for the stone, which M. le Cat used on men since 1732, naturally led him, in the year 1735, to cut the widow Neel, a farmer near Yvetot in the Pais de Caux, in the same manner. In this operation the common grooved staff served as a director; and having made the incision, on the left side of the urethra, with the straight grooved knife, which he used that year in cutting men, he withdrew the grooved staff, and introduced the groove of the knife.

Immediately after this operation, he shortened the work, by reducing the 3 instruments to 2. For that purpose he contrived to add to the common gorget AA, fig. 11 and 12 pl. 11, a grooved staff BB. After placing and tying the patient, in the same manner as for cutting men, he at once introduced into the urethra the end B of the gorget, which constitutes the grooved staff, he turned the groove towards the patient's left buttock: on this groove he pushed the knife, fig. 13, which is not grooved, nor so broad as that which he used in 1735 on the widow Neel. Having laid open the neck of the bladder, he laid aside the knife, and thrust the gorget farther into the bladder; for example, as far as C. He then passed the fore-finger on the gorget into the bladder, to dilate the neck; which done, he introduced the gorget as far forward as is necessary, and on it the forceps. The rest of the operation was performed as on men.

The first of the sex, whom he cut in this last method, as appears by his journal, was Magdalen le Marchand of the Pais de Caux, aged 22, cut in May 1738. He extracted a large stone from her, and she was cured in 10 days.

Since that time he constantly practised this method, which had succeeded perfectly well. When the stones were small the patients were cured in a few days: but the following is one, whose speedy cure has somewhat surprising in it; as he really believed it the only one which had happened so.

Mary le Comte of Diepdal near Rouen, aged 12, cut the 24th of May 1740, had a stone of a middling size. In 3 hours she retained her urine, so as only to discharge it voluntarily. M. le Cat thought it was the common effect of the inflammatory swelling, which frequently happens after the operation; and that the suppuration would soon relax these parts, and open the wound, but he was mistaken. There was not the least suppuration. Mary le Comte performed all the functions of this organ, as usual; and being tired of the bed, to which she was confined against her will, she got up the 3d day, in good health, without any accident supervening.

At the same time that he was labouring to improve the manner of cutting women, and shorten the operation, he contrived another gorget, fig. 14, which, besides the grooved staff of the former, contained within it the cutting instrument, which was to make the incision or enlargement; viz. the 3 instruments in one: and this instrument was attended with this advantage, that it could serve for men as well as women. One hand is sufficient to perform the operation with this instrument.

A is a ring, for passing the middle finger of the right hand, which grasps the handle G. B another ring, which is slid by the fore-finger towards d, to push the blade ab, out of the groove EF. The figure represents the instrument in the state in which it is at the very instant when the incision is made into the neck of the bladder. The same fore-finger draws the piece B back towards A, when the operator intends to make the blades return into the groove F; where they lie hid, while he introduces the instrument from D, as far as Ff into the urethra. The groove FE is closed or covered from d to e, in order to secure the pieces a, b, c, d, e, in situation. The pins, which bind the hinges a, b, c, must not be in the centre of the pieces, but as they are expressed in the figure; where b is pretty near the outside edge, and the other two, a, e, on the opposite side; to the end that, when the ring B is thrust forward, the hinge b may bend, and issue out of the groove F, by making the angle abc.

On the Agreement of Barometers with the Changes of the Weather. By Samuel Christian Hollman. From the Latin, N° 492, p. 101.

Mr. H. observes, that it has hitherto been deemed a matter of great difficulty to explain the true cause of the alteration in the height of the quicksilver in the barometer, and the manner of its agreement with the subsequent changes of the weather. He then remarks on the hypothesis of Leibnitz on these points, which had for some time prevailed, viz. that by the drops of rain falling through the atmosphere, this is deprived of so much of its weight; showing that this notion is not authorised by either theory or practice, and that it had been shown by Desaguliers (Philos. Trans. N° 351) to be quite contrary to the laws of hydrostatics; by which it is found that solids, descending in fluids, add their weight to the ambient fluid.

Mr. H. remarks on the popular illusion, that the air is thought to be heavier when it becomes turbid and replete with thick vapours, and light when the air is clear and bright; which are contrary to fact, and the constant experience of the barometer. He argues, that the air being, by any means, rendered lighter, this levity is the cause of the formation of the contained vapours into a cloudy and thick appearance in the atmosphere, and the consequent descent of rain. But, besides that these phenomena are not constant concomitants; that a thick atmosphere does not always attend the descending barometer, nor rain follow it; the cause of the atmosphere's becoming lighter is still unexplained; though Mr. H. thinks it may be produced by various means; such as the alternate heating and cooling by the sun's presence or absence; or by tides in the atmosphere, occasioned by the moon's attraction, like the tides in the ocean; though it is acknowledged that the alleged effect is very far from uniformly following the regular pretended cause.

Though Mr. H. be not satisfied therefore with those ideas on the cause of the alteration in the weight and elasticity of the air, he contends at least that this alteration, not merely accompanies the turbid state of the atmosphere and the descent of rain, but is indeed the very immediate cause of them; though his illustration of the manner in which this is effected be not satisfactory, by noticing that exhausting the air out of a receiver by the air-pump, the inside becomes turbid or misty; since this phenomenon is otherwise accounted for.

Mr. H. concludes with these reflections: we find many are solicitous to foretell the weather from the rising or falling of the barometer, and endeavour to form certain rules for that purpose: so that this seems to be the only thing which persons, not otherwise versed in natural philosophy, may expect from this machine. But these things, which we would have so conjoined, do not seem to be uniformly connected. For the barometer cannot properly, and of itself, show any thing but the increase or decrease in the elastic force of the air; whereas the weather depends on various exhalations, existing at the same time, or together in the air, or not existing, or at least not present in the same quantity. But it may easily be, that the elasticity of the air is diminished, yet from a defect of a sufficient quantity of exhalations in it, little or no sensible change in the weather may take place. The same happens on the contrary, by too great a quantity of exhalations in the air, though the rising of the quicksilver may most evidently show that the elastic force of the air is increased. Hence then as these things coincide as it were by some accident, no certain prediction can be made of a change of weather, either by the rising or falling of the barometer. But we may safely enough make a kind of negative conclusion from it: for constant agreement of observations shows, that when foul weather has followed a descent of the mercury, then fair will not take place till the mercury begins to rise again; or if the ascent of the mercury has been attended with fair weather, then there will be clouds or rain till the mercury has begun to fall. Yet the descent of the mercury does more frequently predict foul than fair weather; and its rise more often fair than foul; as is shown by very manifold experience.

On the Effects of Lightning in Destroying the Polarity of a Mariner's Compass. By Capt. John Waddell. N° 492, p. 111.

On Jan. 9, 1748-9, the new ship Dover, bound from New York to London, being then in lat. 47° 30′ north, and longitude 22° 15′ west, from London, met with a very hard storm of wind, attended with thunder and lightning, as usual, most part of the evening; and sundry very large comazants, as they are called, overhead, some of which settled on the spindles at the top-mast heads, which burnt like very large torches; and at 9 p. m. a single loud clap of thunder with lightning struck the ship in a violent manner, which disabled Capt. W. and great part of the ship's company in the eyes and limbs; it struck the mainmast about $\frac{2}{3}$ up almost half through, and stove the upper deck, one carling, and quick-work; part of which lightning got in between decks, started off the bulk-head, drove down all the cabins on one side of the steerage, stove the lower deck, and one of the lower deck main lodging-knees. Another part of it went through the starboard side, without any hurt to the ceiling, or inside plank; and started off from the timbers 4 outside planks, being the wale upwards; one of which planks, being the second from the wale, was broke quite asunder, and let in: in about 10 or 15 minutes time 9 feet water in the ship.

It also took the virtue of the loadstone from all the compasses, being 4 in number, all in good order before, one in a brass and 3 in wooden boxes. The hanging compass in the cabin was not quite so much disabled as the rest; they were at first very near reversed, the north to the south; and after a little while rambled about so as to be of no service. The storm lasted 5 days; they lost the mainmast and mizenmast, and almost all the sails; and arrived at Cowes the 21st of January in a very shattered condition.

Of the Mariners' Compass, that was struck with Lightning, as related in the foregoing Paper; with some further Particulars relating to that Accident. By Gowin Knight, M.B., F.R.S. N° 492, p. 113.

On examining the compass struck with lightning, it appeared that the outward case was joined together with pieces of iron wire, 16 of which were found in the sides of the box, and 10 in the bottom. Mr. K. applied a small needle to each of these wires, and immediately perceived that the lightning had made them strongly magnetical; particularly those that joined the sides. All the heads of the wires on one side of the box attracted the north point of the needle, and repelled the south; while all the heads on the other side attracted the south and repelled the north; the wires at the bottom attracted the south and repelled the north; but it is not certain whether this polarity was anywise owing to the lightning; since it might be acquired by their continuing long in an erect position.

On examining the card, he found the needle was vigorous enough in performing its vibrations, but that its polarity was inverted; the north point turning constantly to the south. He then tried to take out the card, to examine the state and structure of the needle: but the junctures were every where well secured with putty, become so hard, that he was obliged to use some violence, and at last broke the glass. The needle consisted of 2 pieces of steel wire, each bent in the middle, so as to make an obtuse angle; and the ends of these wires applied together, forming an acute one, the whole appeared in the shape of a lozenge; in the centre of which was placed the brass cap on which the card turned. And so far was it from being made with any tolerable degree of exactness, that there was not the least care taken either to bend the wires in the middle, or to fix the cap exactly in the centre of the lozenge. The pin, on which it turned, was made of a slip of plate brass sharpened to a point.

Besides the particulars already communicated to the Society, the captain informed him, that he was obliged to sail above 300 leagues, after this accident happened, without a compass, till he arrived at Cowes in the Isle of Wight; where being provided with one, he placed it in the binacle, but was much surprised to find that it varied from the direction it stood at when out of the binacle nearly 2 points. He removed the binacle to different parts of the deck, but found that it always made the needle to vary after the same manner when placed in it. He repeated the same experiment lately in the river, with the like success; only that he observed, that the variation of the needle, when placed in the binacle, was rather less than at first. It was natural to inquire if there was any iron about the binacle; but the Captain said he had given strict charge to the maker not to put so much as a single nail in it; and that he firmly believed that there was not the least bit of iron about it.

Being willing to be satisfied of the truth of a circumstance so very extraordinary, the Captain was desired to send the binacle to a house in the city; where, in company with the Captain, Mr. Ellicot, and another gentleman, Mr. K. tried it with a large compass touched by his bars; but finding no sensible variation, they at that time desisted, thinking the fact quite improbable; but having discovered the effect which the lightning had produced on the wires which fastened the sides of the compass box, he was induced to examine the binacle a second time; which he did with a small compass, and with great care, in every part; and at last, about the middle of the binacle, he found it to vary very sensibly, but could not discover any nails or iron any where thereabouts; till, turning it up to examine the bottom, he there found 3 or 4 large nails, or rather spikes, driven through it to fasten the upright partitions in the middle of the binacle.

It would not be difficult to explain why any needles, under such circumstances, should be rendered useless by lightning, though the needles themselves had remained unhurt. So many iron wires made strongly magnetical would doubtless have effected it; and 3 or 4 large nails in the binacle, if made magnetical, would alone have been sufficient to have done it. But it has already been noticed that the polarity of the needle was inverted by this accident; and he further observes, that all needles constructed after this manner are liable to be rendered useless, not only by the lightning's destroying their virtue, but also by its placing it in a particular direction; ex. gr. if the lightning struck the needle in the direction of either of the two parallel sides of the lozenge, it must strike the other two sides very obliquely; by which the first two sides may have their polarity destroyed, and a very strong one given them in the contrary direction; while that of the other sides, if it be inverted, will be very weak; but it is probable that the virtue would be placed obliquely in the direction of the stroke; in either case, these two sides can contribute but very little, if any thing, in directing the card; and if the first two sides only be capable of acting on it, it will point in the direction of those sides, which will produce a variation of about 4 points.

It may further be observed that a needle would not continue long in this state, but would every day grow more and more regular; because if the virtue be placed obliquely, it generally turns itself in the direction of any piece of steel that is long and slender; and that may be the reason why this card is now become regular, except that it is inverted.

The wires that join the box seem weaker than when first examined; which makes it very probable that they might be vastly stronger when first struck with the lightning; and the same may be likewise true, in regard to the nails in the binacle; which may account for the experiments not answering exactly the same as at first.

From what has been said it appears, that this form of needles is very improper, and ought to be changed for that of one straight piece of steel; and then if a needle should be inverted, it might still be used. It also shows the absurdity of permitting iron of any kind about the compass-box, or the binacle. Whoever considers the whole description here given of this compass, will esteem it a most despicable instrument; how then must any one be shocked to hear, that almost all the compasses, made use of by our trading vessels, are of the same sort! the boxes all joined with iron wire, and the same degree of inaccuracy observed throughout the whole!

Of the pretended Serpent-stone called Pietra de Cobra de Cabelos, and of the Piĕtra de Mombazzo or the Rhinoceros Bezoar, with the Figure of a Rhinoceros with a double Horn. By Sir Hans Sloane, Bart. late Pr. R. S. N° 492, p. 118.

Sir H. S. here communicates to the Society an account of two pretended stones, said to be found in the head of the most venomous snake of the East Indies, called Cobra de Cabelo, with an account of what he has heard, and what he believes they really are. The first he has heard, and believes to be, a stone found in the stomach or intestines of the rhinoceros; not, that he knows, taken notice of by any natural historian, excepting Redi. The place where it is said to be found, is on the south-east coast of Africa, according to the information Redi had of it, and from which place he had the two horns figured in these Transactions, N° 470, by Dr. Parsons, which were tied together across, the better he believes to preserve the short skin that connected them on the nose of that animal, so that the straight and crooked horn might appear distinct, as they do in a very entire small brass medal of Domitian in his collection. Whether the rhinoceros, who bore these 2 horns, be a distinct species of that animal from that of Asia, future travellers must determine.

Dr. Waldo went into the East Indies, on purpose to search after and collect the natural productions and curiosities of those parts, especially such as related to the cure of diseases, which he sent from time to time to his sister he left in London, with directions to show them to the Earl of Pembroke, Sir Godfrey Kneller, and Sir Hans Sloane, to sell. The two former not caring to buy several of them, they fell into the hands of the latter. Among the rest which he purchased, were some of these stones, which were by Dr. Waldo called rhinoceros bezoars, which Sir Hans supposed were taken out of the stomach or guts of that large animal.

These productions or bezoars, as they are commonly called, consist of several coats made up of several parts attracted by their centres, such as the stones of fruits, and other indigestible substances swallowed with its food, after the manner of those found in the stomach and intestines of mankind, and other animals. The uppermost coat or layer of this bezoar is made up of several brown striated small knobs or tubercles, something like low warts, distant from each other, and making its outermost surface very unequal, different from the other bezoars whose surface is generally smooth. Those he had of this bezoar are of different magnitudes and diameters, the largest about the size of an orange, heavy, and as hard as stone, and capable of being polished.

Redi relates great virtues belonging to them, as told by the bringers of them from the East Indies; probably on no good grounds.

Dr. John Bateman, formerly Pres. of the College of Physicians of London, told Sir H. S. with great admiration, that he had seen the great effects, on the bite of a viper, of the snake-stone or serpent-stone, as it is called, before King Charles II, who was a great lover of such natural experiments; and that he knew the person possessed of the very stone he had seen tried, who he believed would part with it for money. On Sir H.'s desire and request to see him, he brought him the stone, which was round and flat, as the common ones brought by merchants and others from the East Indies, about the size of a milled shilling, but thicker, for which he asked 5 guineas, though it was broken.

Dr. Alex. Stuart, returning from the East Indies, brought, among many other curiosities, some of these snake or serpent-stones, with this account of them, which he had from a father missionary in the East Indies, viz. "that they were not taken out of a serpent's head, but made of the bones of the small buffalo in the Indies;" by which their coaches are drawn instead of horses; the bones being half calcined or charred by the dung of the same buffalo. He gave Sir H. several pieces, with some of the snake or serpent-stones made out of them, and which he had in his collection, of several shapes and colours.

Sir H. thinks the first who gave any account of them, was Francesco Redi at Florence, who had them from the Duke of Tuscany's collections, and who, in his Esperienze Nat. mentions great virtues of them, related by 3 Franciscan friars, who came from the East Indies in 1662; which were, that being applied to the bites of the viper, asp, or any other venomous animals, it sticks very fast till it has imbibed or attracted all the poison, as a loadstone does iron, as many people in the Indies believe, and then it falls off of itself; and being put into new milk, it parts with the poison, and gives the milk a bluish colour; of which Redi tells the success of those he figured.

Kempfer, in his Amœnitat. Exot. p. 396, speaking of this, says, it helps those bitten by vipers, outwardly applied; and that it is not found in the serpent's head, as believed, but by a secret art made by the Brahmans; and that, for the right and happy application of it, there must be two ready; that when one has fallen off filled with the poison, the other may supply its place. They are commonly, as he says, kept in a box with cotton, to be ready on occasion.

Biron says, that if the wound of the serpent has not bled, it must be a little pricked, so as the blood comes out, and then to be applied as usual. It comes from the kingdom of Camboya.

Fig. 1, pl. 12, is a coin of Domitian in small brass, having on the foreside, the figure of a rhinoceros with 2 horns growing out of his nose, the one above the other; which in the Numismata Pembrokiana, part 1, tab. xvi, n. 68, the engraver has made like a tusk or dens exertus of a boar, and in part 3, tab. 39, he has made the 2 horns on his nose like 2 tusks, and has likewise given him 2 horns close to his ears; so that he has made him a creature with 4 horns; and therefore it was thought proper to give an accurate copy of the medal, in order to clear up that famous passage of Martial, Lib. de Spectac. N° 22,

Namque gravem gemino cornu sic extulit ursum,
Jactat ut impositas taurus in astra pilas.

Which has for many ages puzzled the critics, all thinking that the rhinoceros was a real unicorn, or animal which never had any more than one horn. See these Trans. N° 470, and beside the double horns, or geminum cornu, in Sir Hans Sloane's museum, it is said Dr. Mead had another geminum cornu likewise from Africa.

Fig. 2 is the reverse of the same medal, with this inscription IMP DOMIT AVG

GERM, and in the middle sc. Fig. 3 is the figure of the rhinoceros magnified, that the position of the 2 horns might appear distinct and plain.

Of the Rana Piscatrix. By James Parsons, M. D., F. R. S. N° 492, p. 126.

In some parts of Italy this fish is called rospo; in others bora; and by the Lombards zatto.

Lophius* ore cirroso. Petrus Artedus.

ὁ ἁλίευς βάτον, βάτραχος ἁλίευς. Aristot.

Rana piscatrix, by the following authors, viz. Bellonius, Rondelletius, Salvianus, Gesnerus, Charleton, Willoughby, Ray.

Piscatrix vel marina, by Schonveld; piscatrix vulgaris, by Aldrovand.

Though this fish is already described by most of the natural historians, yet several of its properties appear to have been overlooked. Dr. P. refers the curious to the general history of this animal, as collected by Gesner; and to Sir George Ent's account and dissection of him, as delivered by Dr. Charleton, in his Exercitationes de Differentiis et Nominibus Animalium; whose figure of him is copied by Willoughby, with most of the dissertation, and which was probably taken from Salvianus by Dr. Charleton, for the better illustration of Sir George's dissertation.

This animal is 4 feet 3 inches long, and about 19 inches from side to side in the widest part. His mouth is very wide, and his teeth are set in clusters in both the upper and under jaws, and not in regular rows, as was the common opinion; they are long and small like spikes, moveable, and directed inward, in order to secure his prey from escaping, after he has once laid hold on it. His lower jaw is far longer than his upper, having a large capacity in the skin of the former, to yield according to the bulk of the creature he seizes; for with this jaw, and the external clusters of the teeth of the upper jaw, he holds it fast, while with another inner cartilaginous jaw, the teeth of which correspond with an inner cluster of teeth in the upper, he chews and tears his prey, swallowing it by degrees as he minces it; neither the under jaw, nor external row of the upper, having any share in the mastication at all.

Though he is said to be of the cartilaginous kind, his head is as bony as that of any fish; having rough spiny ridges, serving as eye-brows. Between these arise 3 black limber twigs; the anterior is longest, the second shorter, and the next shortest; each having at its extremity a white flat piece, with which, it is said, he allures other fish to approach near enough to seize on them. There are two others less considerable on his back, between those fins or webs, which, in him, must be called humeral webs.

* *Lophius piscatorius.* Linn. It is well figured in Pennant's British Zoology, and in Bloch's Ichthyology.

These webs are cartilaginous and fleshy, and are supported by strong bones, analogous to the humeral bones of some other animals. Under each of these is a sacculus or marsupium, which runs up the side of his head, 28 inches deep, and 6 inches wide; these have not been duly taken notice of, except perhaps by Bellonius, who mentions two holes, without adding any thing else about them. But they are of so singular a nature, that there is some encouragement to make the following conjecture.

The branchial holes are 3 on each side, which are situated deep in the mouth, and open into these marsupia, the sides of which are the branchiostegæ, having several long slender cartilaginous bones running longitudinally for their support, analogous to the branchiostegal bones of other fishes; so that probably these sacks may answer two ends; first, to form the membranæ branchiostegæ; and, secondly, to make a convenient receptacle for the young, till they are able to shift for themselves. Perhaps the following conjectures may serve to strengthen this opinion; for if this end was not to be answered, the branchiæ might have been terminated near their origin in the mouth, as it is in other fishes.

Authors have ranked this fish among the cartilaginous tribe, who are said to be viviparous; but of this there are disputes among them as yet undetermined. Now if this fish does not bring forth its young perfect, there can be no use assigned to these sacks; for eggs are deposited by the oviparous tribe in sand, weeds, or any other proper nidus; nor could the creatures by any means place eggs in them, because they open in a wrong direction for such a purpose. But if they are viviparous, then the young may probably be harboured in them, being capable of crawling into them, as we may see by the pectoral webs on the under side.

And to strengthen this conjecture, we may draw another consideration from the manner of their feeding; for these are creatures of no swift motion, but crawl on the bottoms of shoal places, watching and alluring their prey; now their young cannot be supposed to have power or sagacity enough for this work, till they are grown large and strong, and have these twigs in perfection; therefore they must of necessity be protected by the parent, till they are able to provide for themselves; which probably may be when they grow too large to enter into these marsupia.

There are 7 small finny webs, like little indented leaves, on each side the under jaw, and others of the same kind all round the sides to the tail. He has a dorsal fin near the tail on the spine, and a ventral fleshy fin nearer the tail than the former. The five-fingered webs under the thorax are rough and fleshy, showing their business is to assist in slowly crawling from place to place; and there appears the vestige of the spine from the place of the vent to the tail on the most posterior part of the belly.

As to the sex of the fish, it could not be judged of, nor of any internal part, as the viscera had been taken out before he saw it, and all appearances destroyed that might inform us; and therefore we must refer the reader to that curious dissection of it made by Sir George Ent, as it is quoted by Charleton, in his Mantissa Anatomica.

Fig. 4, pl. 12, represented a back view of the rana piscatrix; aa, the bony ridges and asperities between the eyes; from the central sulcus of which arise bbb, the three virgæ piscatoriæ, or fishing rods; cc, the cirruli, or little webs, all round the borders of the fish; dd, the large humeral fins, under which are the openings into the marsupia and branchiæ; ee, the two posterior rods; f, the posterior and superior spinal fin; g, the tail, which in this fish is vertical.

Fig. 5, is a view of the under surface or belly of this fish; aa, the angles of the lower jaw, seen and felt through the integuments; b, the skin or floor of the mouth, capable of stretching into a sack, according to the bulk of the prey he holds; cc, the fleshy five-fingered webs, by which they crawl on the bottoms of shoals; dd, the openings into the marsupia and branchiæ; e, the vent or anus; f, the posterior and inferior spinal fin; g, the cartilaginous branchiostegal bones.

Fig. 6 is a view of the mouth, opened to show; a, the skin of the floor of the mouth, as at b in fig. 5; b, the tongue; cc, the external teeth in the upper and under jaws, for holding the prey; dd, the corresponding clusters of teeth in the inner cartilaginous jaw, for mastication, and tearing the prey; ee, the rictus oris; f, the upper jaw; g, the entrance into the gula and branchial holes.

Fig. 7 is a full view of the opening into the marsupium, lying under the fin d.

Observations on the Height to which Rockets ascend. By Mr. Benj. Robins, F.R.S. N° 492, p. 131.

The use of rockets may be so considerable in determining the position of distant places to each other, and in giving signals for naval or military purposes, that it is worth while to examine what height they usually rise to, the better to determine the extent of the country, through which they can be seen. Therefore, at the exhibition of the late fire-works, Mr. R. desired a friend to observe the angle of elevation to which the greatest part of them rose, and likewise the angle made by the rocket or rockets, which should rise the highest of all.

That friend was provided with an instrument, whose radius was 38 inches; and, to avoid all uncertainty in its motion, it was fixed in an invariable position; and its field, which took in 10° of altitude, was divided by horizontal threads. The station he chose was on the top of a house in King-street, Cheapside, where he had a fair view of the upper part of the building erected in the Green Park. There he observed that the single rockets which rose the most erect, were usually elevated at their greatest height about 6½° above his level; and that among these there were 3 which rose to $7\frac{1}{4}°$; and that in the last great flight of rockets, said to be of 6000, the crest of the arch, formed by their general figure, was elevated about $8\frac{3}{4}°$.

The distance of this station from the building in the Green Park, is 4000 yards, according to the last great map of London; and hence it appears, that the customary height, to which the single, or honorary rockets, as they are styled, ascended, was near 440 yards; that 3 of these rose 526 yards; and that the greatest height of any of those fired in the grand girandole, was about 615 yards, all reckoned above the level of the place of observation, which is near 25 yards higher than the Green Park, and little less than 15 yards below the chests whence the great flight of rockets was discharged.

It seems then there are rockets which rise 600 yards from the place whence they are discharged; and this being more than a third part of a mile, it follows, that if their light be sufficiently strong, and the air be not hazy, they may be seen in a level country at above 50 miles distance.

The observations on the single rockets are sufficiently consonant to some experiments Mr. R. made about a fortnight before; for then he found that several single pound rockets went to various heights between 450 and 500 yards, the altitude of the highest being extremely near this last number, and the time of their ascent usually short of 7″.

But though from all these trials it should seem as if good rockets of all sizes had their heights limited between 400 and 600 yards; yet he believes that they may be made to reach much greater distances. For he lately saw a dozen of 4 lb. rockets fired; the greatest part of which took up near 14″ in their ascent, and were totally obscured in a cloud near 9 or 10″ of the time; so that the moment of their bursting was only observable by a sudden glimmering through the clouds; and as these rockets, during the time they were visible, were far from moving with a languid motion, the extraordinary time of their ascent must have been attended by a very unusual rise.

Extract of so much of Don Antonio de Ulloa's Account of his Voyage to South America, as relates to the Distemper called there Vomito Prieto, or Black Vomit. Transl. from the Spanish by W. Watson, F.R.S. N° 492, p. 134.

The city of Carthagena in America is situate in 10° 25′ $48\frac{1}{4}$″ of north latitude. The weather there is always sultry hot. A thermometer constructed by Mons. de Reaumur gave, on the 19th of November 1735, one of their winter months, the degree of the warmth of the air $1025\frac{1}{2}$ divisions; and this with little variation, both night and day. The greatest height to which the spirit ascended at Paris the same year, by a thermometer graduated in the same manner, was $1025\frac{1}{4}$;

so that the heat of the cool nights at Carthagena was nearly equal to that of the hottest days at Paris.

As the heats in this climate are so great, without receiving any sensible mitigation from the nights, it is no wonder that the perspiration of the inhabitants is very great. Hence all those who remain there any time, appear pale and weakly, as if newly recovering from a fit of illness. There appears in all their actions, even so far as in speaking, a certain indolence and inactivity. Yet they are in good health, though their aspect indicates the contrary. The people who arrive there from Europe, hold the appearance of strength and colour in their countenances during 3 or 4 months; but after that time, they lose both one and the other, from the quantity of sweat, till they become like the former inhabitants. These effects are most observable in younger people; on the contrary, those who are farther advanced in life, when they go thither, preserve their former appearance better, and enjoy so good a state of health, that they live commonly to more than 80 years of age.

As the temperament of this country is particular, so are some of its distempers. These may be considered of 2 kinds, viz. those distempers to which the Europeans newly arrived there are liable, and they only; and those which are common to all persons, as well Criollos as Chapetones.

The distempers of the first class are many, as the resort of the Europeans there is very great. They are very dangerous, and often mortal. They frequently destroy a great part of the people, both sailors and others, who arrive there from Europe. The continuance of these distempers is very short; they last but 3 or 4 days, in which time the sick either die, or are out of danger. The particular distemper, to which they are most liable, is very little known; though it takes its rise in some from taking cold, in others from indigestion; but from whatever cause it takes its rise, it becomes in the short time before-mentioned the vomito prieto or black vomit, which is what kills them; it being very rare that those, who have it, escape. It is observed in some, that their delirium is so violent, that they are obliged to be tied down in their beds, that they may not tear themselves in pieces, and they often die raving with the greatest degree of agony.

Those only are subject to this distemper, who are lately arrived from Europe: the inhabitants of the country, as well as those who have been there any time, are by no means liable to it, and enjoy perfect health during its greatest violence. As the crews of ships are very liable to this distemper, and more so than the officers and passengers, who have greater variety of food and liquor, it has been conceived, that the great exercise and labour of these people, and their feeding on salt provisions, prepares their constitutions to be liable in this climate to a corruption of the blood and humours, from whence is supposed to proceed the vomito prieto. Yet passengers and others, who go the voyage under the greatest advantages, with regard to the conveniences of life, are not free from being exposed to it. It is remarked also, that those persons who, after having been used to this climate, go from thence, and are absent even 3 or 4 years, are not liable to it at their return, but retain their health like the other inhabitants; though in their way of living they have not observed the most exact regimen.

The desire of knowing the cause of this terrible calamity has occupied from time to time the minds of the surgeons who make this voyage in the galeons, as well as those of the physicians of the country; the physicians are of opinion, that it chiefly takes its rise from the labour to which the ships crews are constantly exposed, and their manner of living. Doubtless these may greatly contribute to it; but then it will be difficult to conceive, why persons who are better circumstanced are likewise liable to it: and it is somewhat extraordinary, that, notwithstanding many endeavours have been made towards finding out remedies against this disease, none have been discovered, either as specifics, or preservatives; for the inconstancy of the symptoms is so great, that they are not in the beginning to be distinguished from those which are in common to this with slighter distempers; but the principal complaints are at first a weariness, and great disorder in the head.

This distemper does not always attack the ships of Europe at their arrival in the bay of Carthagena; nor is it very ancient in that country; for what heretofore was called Chapetonada, so denominated, as those from Europe were only liable to it, were indigestions; and though they were in that climate always attended with danger, the women of the country, as they do now, cured them with ease, especially when they are taken in time. The ships afterwards going from Carthagena to Porto Bello, it was there succeeded with the great mortality, which was always attributed to the unseasonableness of the climate, and to the fatigue of the ships crews in unloading their ships, and in the business of the fair there.

The black vomit was not known at Carthagena, nor in its neighbourhood, until the years 1729 and 1730, when first it carried off a great part of the crews of the ships of war, which Don Domingo Justiniani then commanded, and were then there as guarda costas. These ships were first attacked at Santa Martha, where the severity of this distemper, and its great slaughter, had cast a great terror on their crews. The second attack of this distemper, was on board the galeons commanded by Don Manuel Lopez Pintado, when its mortality was highly formidable, and death followed the attack so quick, that persons who were one day seen walking at large, were next day met carrying to their graves.

Our author is of opinion, that this, as well as some other distempers to which Europeans are liable at first, or soon after their arrival at Carthagena, and other places under the same circumstances, should be considered as arising from the great alteration that happens in their constitutions there: and this change, which from the climate is soon brought about, makes them suffer this and other distempers, which either destroy them, or generate in them a disposition to bear the heats; after which, being as it were naturalized, they enjoy the same share of health with the natives.

Our author remarks, that at Carthagena, when the ships from Spain fail in their arrival, the European productions are sometimes quite expended: these more particularly are wine, oil, and raisins. When this is the case with regard to wine, the people there suffer much in their health; as every body, except the negroes, and those who use brandy, accustom themselves to drink it with their food. From the want of this, their stomachs fail, they grow sick, and this sickness becomes general. This want of wine happened when our author arrived at Carthagena, and the sickness in consequence was so general in that city, that mass was celebrated only in one of their churches.

The Declination of some Southern Stars of the 1st and 2nd Magnitude, in June 1738; and the Method of Finding the Hour of the Night at Sea, by Observing the Southern Cross. By Mons. de la Condamine, F.R.S., and of the Royal Acad. of Sciences of Paris.* N° 492, p. 139. *From the Latin.*

In the ship Argo α Canopus	52°	34′	16″
In the west arm of the cross, γ	57	17	32
ζ, in the foot of the cross	61	38	57
ε, in the top of the cross	58	15	5
γ, the western foot of the Centaur	59	5	35
α, the following foot of the Centaur	59	44	56

The southern cross, when in the meridian appears upright, or perpendicular to the horizon; and therefore may serve mariners to discover the hour without sensible error, as follows: from the table of mediation of the first point of Aries, in the Connoissances des Temps, the true hour of the night will be easily obtained,

* Charles Marie de la Condamine was born at Paris, Jan. 1701, and was admitted an academician in 1730. In 1731, he travelled into the Levant; an account of which tour was published by his valet, without his consent, in 1734. It was he who first proposed terminating the disputes about the figure of the earth by measuring a degree near the equator, and he was sent, in 1736, as one of the principal agents in that expedition, in which he suffered extreme hardships. In 1760, he was received in the French academy, and he contributed much to the last edition of their dictionary. Besides several separate publications, as the travels in South America, and the measure of the first three degrees of the meridian, &c. he communicated very numerous memoirs to the academy of sciences, and to the French academy, &c; and he died in December 1773, near 73 years of age.

by viewing the southern cross, and observing at what hour it shall appear perpendicular to the horizon, or rather, when the time will permit, by observing with the plumb line held in the hand the very moment when the stars ζ in the foot, and α in the head of the southern cross appear equally distant from the perpendicular, the latter on the east side, the former on the west: for at the point of time when this happens, there will hardly be an error of 1 minute of the truth, if 15 minutes be added to the hour of mediation of the first point of Aries; which will be determined by the above-mentioned table, the difference of the meridians of the calculator and observer being amended.

Of two Beautiful Echinites. By Mr. E. da Costa, F.R.S. N° 492, p. 143.

These 2 echinites are undoubtedly moulded in shells, of a genus of which we at present find some species now living in the seas; mostly in the West Indies. The echinometra of Aristotle, Aldrovand, and of Dr. Grew, is of this genus. Dr. Breyn calls the whole genus echinanthus; and Mr. Klein, scutum. Woodward, in his distribution of fossil echini, calls them the pentaphylloides, from the rays on the upper part forming a beautiful cinquefoil figure; but erroneously fixes their characteristics in having only one aperture, and that at the basis; in which he not only contradicts nature, but also the very specimens he quotes in his own collection, which have all two foramens or apertures, and are elegantly figured so by Agostino Scilla, who was the person that sent them to the doctor; and our late president Sir Hans Sloane has also figured and described 2 species of this genus, one species of which is an inhabitant of our English seas.

No author has ever described echinites, or stones moulded in the fossil echini of this genus; nor even have the fossil echini or shells themselves been ever exhibited by any lithologist, except by the above-quoted A. Scilla, who found them in Malta, sent them to Dr. Woodward, and to which the doctor in his catalogue recounts 2 other specimens, which were dug up in Maryland; so rare are the instances of the fossils of this whole genus! The 2 echinites here described were all found in the midst of some rocks, which were blown up at Port Mahon some years before, and whence they were all brought.

The first or largest is composed of a hard or stony arenaceous greyish substance, and is of an escutcheon or heart-like shape: it measures about 14¼ inches in circumference, or quite round the limb or edge, about 2 inches high from the flat or basis, to the tip of the apex, 5 inches in length at the basis, and 4¼ in breadth. On the upper part it rises nearly gradually from the edge quite to the apex. A central point, with a slight declining space, tops the said apex; from which space the body regularly divides into 5 parts figured like leaves to the edge. These leaves are narrow at the apex, greatly widen towards the bottom, and narrow a little again at their end. Each division or leaf is bounded on each side by a row of parallel ridges, which are accompanied also on each side of every said row, with 2 other ranges of points or knobs; all which rows do not meet or close together at the lower end of the division, but leave a void unwrought space: a row of larger irregular knobs runs through the midst of each leaf. From the divisions between each leaf runs a rugged knobb'd pillar, joined to the edge: the other parts between the leaves and the edge, are hollows, or void spaces. The edge or limb is of a thick cylindric make, runs quite round the whole body, and only has some signs of being disjoined at the one extreme of the length, or where the aperture was; the stone answering which is here extended a little cylindrically outward like an appendage, and was so formed by the stony matter being too much in quantity for the shell, and so was protruded through the said foramen. On the outer edge of the limb, are some few irregular stony concretions. The basis is flat, and is divided into 5 parts from the centre, which is one of the foramens; the other foramen being placed at one of the extremes of the length. This foramen or centre is about the size of a shilling. The 5 divisions extend to the utmost edge of the body, or quite over the limb, contrary to the divisions on the upper part, which extend only to it. Each division is formed by a stony line edged on each side with stony cylindrical bodies of the thickness of a pin, but of different lengths, so as to appear like the teeth of a comb, or the gills of a fish; the interstices between all which is a rugged stony work, and hollows pervading quite through the body to the upper part.

The other echinite is of a different species, though of the same genus, of a heart-like shape, and about one third the size of the above described. This is greatly copped, the apex lying very high, and the 5 divisions running nearly perpendicular down to the edge. The upper part of this is elegantly perfect; the work is nearly the same as on the other; only that, by the perfection this preserved is in, we observe that the rows of parallel ridges, which adorn each side of each leaf or division, rise into a kind of arched work or bridge, made up of arched cylindrical bodies, through which the middle row runs, joined or connected in a long straight cylindrical stem, in a most curious and elegant manner. The basis or under part of this specimen is very imperfect, and only seems to differ in the centre being greatly excavated or concave, answering to the great copping or height of the apex or upper part. This fossil also consists of a hard stony arenaceous substance like the other.

From the inspection of the several hollows of these echinites, it is evident that they were not immediately moulded in the shells, but were formed in cavities which those shells formerly filled in the rocks they were lodged in. The rocks were apparently of a loosened arenaceous texture, and the water, &c. continually pervading them, rotted and destroyed the inclosed shells, and bore away their whole substance. In the same manner, and by the same means, were the stony particles replaced into those very cavities which the shells formerly filled; consequently these bodies were moulded exactly to the said cavities.

This remark carries a conclusion with it, that the hollows and solid parts of these stones exactly answer to the hollows and solid parts of the very shells themselves; which, had they been moulded in the very shells, must have happened directly contrary; the solid parts of the shells forming hollows in the stone, and vice versa. In all sandy or lax earthy matter, fossil shells are very seldom found, but only the moulded stones; the loose texture of those substances giving free access to water, vapours, and mineral exhalations, &c. which entirely corrode and destroy the shells buried in it.

*The State of the Tides in Orkney. By Mr. M. Mackenzie.** N° 492, p. 149.

There is little or nothing uncommon in the manner of these tides.

Ordinary spring tides rise 8 feet perpendicular, ordinary neap tides $3\frac{1}{2}$; extraordinary high spring tides rise 14 feet; extraordinary low, only 5; extraordinary high neap tides rise above 6 feet; extraordinary small neap tides not above 2. Low water neap tide, at a mean, is about 3 feet above low water spring tide, and high water spring tide about 3 feet above high water neap tide.

On the coast of Orkney, and fair isle of Shetland, the body of the flood comes from the north-west; on the east and west coasts of Lewis, one of the western isles of Scotland, it comes from the south. A league or two off the coast, the strength of the stream is scarcely sensible, except when it is confined by land, or near rocks or shoals. When the tide begins to rise or fall on the shore, about that same time the stream near the shore begins to turn or reverse its direction, a few irregularities excepted.

The stream of tide changes its direction sooner near land than at a distance from it; insomuch that, in a place 2 or 3 miles from land, the turning of the tide is 2 hours, or more, later than on the adjacent shore: at intermediate distances the streams turn at intermediate times. Hence a vessel may find a favourable tide near land, while it would be against her a mile or two from it; and the contrary.

During the continuance of flood, the stream varies its direction gradually from the east towards the south, and the stream of ebb from the west towards the north: that is, if the stream, when it becomes first sensible, runs east, at the latter end of the tide it will run south, if the proximity of land or shoals does not hinder this change of direction.

* Author of a treatise on Maritime Surveying, printed 1774, in 4to.

The greatest velocity of spring tide in Orkney, in the channels where it runs quickest, is about 9 miles an hour: the greatest velocity of neap tide is about one third or fourth of spring tide. The tides are most rapid commonly between the third and fourth hour. Spring tides acquire a considerable degree of strength in less than one hour after their quiescent state begins; neap tides are hardly sensible in 2 hours after.

Some Account of the Remains of John Tradescant's Garden at Lambeth. By Mr. W. Watson, F.R.S.* N° 492, p. 160.

Upon a visit made to Mr. John Tradescant's garden at South Lambeth, May 21, 1749, by Dr. Mitchell and Mr. Watson, were observed the under-mentioned exotic plants.

* "John Tradescant (says Dr. Pulteney, in his work entitled "*Historical and Biographical Sketches of the Progress of Botany in England,*") was by birth a Dutchman, as we are informed by A Wood. On what occasion, and at what period he came into England, is not precisely ascertained. He is said to have been for a considerable time in the service of Lord Treasurer Salisbury, and Lord Wotton. He travelled several years, and into various parts of Europe; as far eastward as into Russia. He was in a fleet that was sent against the Algerines in 1620, and mention is made of his collecting plants in Barbary, and in the isles of the Mediterranean. He is said to have brought the trifolium stellatum Lin. from the island of Fermentera; and his name frequently occurs in the second edition of Gerard, by Johnson; in Parkinson's Theatre of Plants, and in his Garden of Flowers, printed in 1656. But I conjecture, that Tradescant was not resident in England in the time of Gerard himself, or known to him. He appears however to have been established in England, and his garden founded at Lambeth; about the year 1629, he obtained the title of gardener to Charles the First. Tradescant was a man of extraordinary curiosity, and the first in this country who made any considerable collection of the subjects of Natural History. He had a son of the same name, who took a voyage to Virginia, from whence he returned with many new plants. They were the means of introducing a variety of curious species into this kingdom; several of which bore their name. Tradescant's spiderwort, Tradescant's aster, are well known to this day; and Linnæus has immortalized them among the Botanists, by making a new genus, under their name, of the spiderwort, which had before been called ephemeron. His museum, called Tradescant's ark, attracted the curiosity of the age, and was much frequented by the great, by whose means it was also much enlarged, as appears by the list of his benefactors, printed at the end of his *Museum Tradescantianum*; among whom, after the names of the king and queen, are found many of the first nobility. This small volume the author entitled "*Museum Tradescantianum, or a Collection of Rarities preserved at South Lambeth, near London,* 1656," 12mo. It contains lists of his birds, quadrupeds, fish, shells, insects, minerals, fruits, artificial and miscellaneous curiosities, war instruments, habits, utensils, coins, and medals. These are followed by a catalogue, in English and Latin, of the plants of his garden, and a list of his benefactors. Prefixed to this book were the prints of both father and son, which, from the circumstance of being engraved by Hollar, has rendered the book well known to the collectors of prints, by whom most of the copies have been plundered of the impressions. In what year Tradescant died is not certain, but his print represents him as a man advanced in age. The son inherited the museum, and bequeathed it by a deed of gift to Mr. Ashmole, who lodged in Tradescant's house. It afterwards became part of the Ashmolean Museum, and the name of Tradescant was unjustly sunk in that of Ashmole. John, the son, died in 1662. His widow erected a a curious monument, in memory of the family, in Lambeth church yard."

This garden was planted by the above-mentioned gentleman about 120 years before, and was, except that of Mr. John Gerard, the author of the Herbal, probably the first botanical garden in England. The founder, after many years spent in the service of the Lord Treasurer Salisbury, Lord Wotton, &c. travelled several years, and procured a great variety of plants and seeds before not known in England; to several of which at this time the gardeners give his name, as a mark of distinction; as Tradescant's spiderwort, Tradescant's aster, Tradescant's daffodil. He first planted here the Cupressus Americanus Acaciæ foliis deciduis, which has been since so much esteemed, and is now one of the great ornaments of the Duke of Argyle's garden at Witton.

Mr. Tradescant's garden has now been many years totally neglected, and the house belonging to it empty and ruined; and though the garden is quite covered with weeds, there remain among them manifest traces of its founder. We found there the Borrago latifolia sempervirens of C. B. Polygonatum vulgare latifolium C. B. Aristolochia clematitis recta C. B. and Dracontium Dod. There are yet remaining two trees of the arbutus, the largest he has seen; which, from their being so long used to our winters, did not suffer by the severe colds of 1729 and 1740, when most of their kind were killed throughout England. In the orchard there is a tree of the rhamnus catharticus, about 20 feet high, and near a foot in diameter, by much the greatest he ever saw.

*On the Acceleration of the Moon. By the Rev. Richard Dunthorne.**
N° 492, p. 162.

After comparing a good number of modern observations, made in different situations of the moon and of her orbit, in respect of the sun, with the New-

" The Museum Tradescantianum, says Mr. Pennant, in his History of London, is a proof of the industry of the Tradescants. It is a catalogue of their vast collection, not only of the subjects of the 3 kingdoms of nature, but of artificial rarities from a great variety of countries. The collection of medals, coins, and other antiquities, appears to have been very valuable. Zoology was in their time but in a low state, and credulity far from being extinguished: among the eggs is one supposed to have been the egg of the dragon, and another of the griffin. You might have found here two feathers of the tail of the phenix, and the claw of a ruck, a bird able to truss an elephant. After his death, which happened about the year 1652, his collection came into the possession of the famous Mr. Elias Ashmole, by virtue of a deed of gift which Mr. Tradescant jun. had made to him of all his varieties, in true astrological form, being dated December 16, 1657, 5 hours 30 minutes post merid."

* Rd. Dunthorne was born in the year 1711, at Ramsey, in Huntingdonshire. While very young he contracted a thirst for learning by reading some old magazines, which his father, who was a gardener, used for the purposes of wrapping up seeds, &c. Being sent to the free grammar-school of his native place, he so distinguished himself, as to gain the notice of his superiors, among whom was Dr. Long, master of Pembroke Hall, Cambridge, who afforded him great encouragement; and at length removed him to Cambridge as his foot-boy. In the short time he remained in

tonian theory, as in the Philos. Trans. N° 482, Mr. D. proceeded to examine the mean motion of the moon, of her apogee, and nodes, to see whether they were well represented by the tables for any considerable number of years, and whether he should be able to make out that acceleration of the moon's motion which Dr. Halley suspected in the Phil. Trans. N° 218.

To this end Mr. D. compared several eclipses of the moon observed by Tycho Brahe, as they are set down in his Progymnasmata, p. 114, with Mr. Dunthorne's tables, and found them agree full as well as could be expected; considering the imperfection of his clocks, and the difficulty there must commonly have been in determining the middle of the eclipse from the facts observed, as published in his Historia Cœlestis. Indeed the small distance of time between Tycho Brahe and Flamsteed rendered Tycho's observations but of little use in this inquiry.

The next observations that occurred, were those of Walther and Regiomontanus, which being at double the distance of time from Flamsteed that Tycho's were, seemed to promise some assistance in this matter: on comparing such of

this capacity he pursued his studies, which had been already extended to mathematics, with increasing activity, and considerable success. Having arrived at maturity, and stored his mind with various kinds of knowledge, he undertook the management of an academy at Coggeshall, in Essex, for preparing young gentlemen for the university.

From this situation, however, he was called by his kind master, who procured him the butlership of his college, and engaged his assistance in his various scientific and mechanical pursuits, particularly in the construction of the amazing hollow sphere of 18 feet diameter, which it is well known Dr. Long erected at Pembroke Hall. During this time Mr. D. published his treatise on the moon's motions, and his several papers in the Philos. Trans. Vols. 44, 46, 47, 52. It was also said that Dr. Long kept a printing press in his house, with which Mr. D. actually printed all the first volume, and part of the second, of the doctor's astronomy.

On the death of his patron, in 1770, by whom he was always treated with the greatest kindness, and with whom he always lived in the strictest intimacy and friendship, he found himself named in the doctor's will, as one of his executors, an office which he discharged with every possible attention to the honour of his benefactor. Mr. Dunthorne retained his office in the college till his death, which happened March 3, 1775. He had lived in habits of intimacy with many scientific men in the university; and, when he died, left behind him a character of integrity in all his conduct; of kindness to his poorer relations, many of the younger branches of which he settled in the world; and of sincere respect among those who obtained his acquaintance. It seems Mr. D. left behind him a great number of valuable manuscripts and drawings, most of which were inconsiderately burnt soon after his death, as waste paper. Their value is conjectured from some portions which remain, and which, by lying in another place, escaped the fate of the former. The writer of this has seen a manuscript volume in Mr. D.'s own hand writing, of observations, taken for the purpose of forming a map of Cambridgeshire, at all the principal places in that and the neighbouring counties. The manuscript is written with the greatest neatness and apparent accuracy; but the map, if it ever was finished, has doubtless been destroyed. Mr. D. was also at different times consulted and engaged on the drainage of the fens; and the locks on the river Cam, near Chesterton, were built under his direction.

their eclipses of the moon, whose circumstances are best related with the tables, he found the computed places of the moon were mostly 5′ too forward, and in some considerably more, which he could ascribe to the errors of observation; but concluded, that the moon's mean motion since that time, must have been something swifter than the tables represent it; though the disagreement of the observations between themselves is too great to infer any thing from them with certainty in so nice an affair.

He then compared the 4 well known eclipses observed by Albategnius with the tables, and found the computed places of the moon in 3 of them considerably too forward: this, if he could have depended on the longitude of Aracta, would very much have confirmed the opinion that the moon's mean motion must have been swifter in some of the last centuries than the tables make it; though the differences between these observations and the tables, are not uniform enough to be taken for a certain proof of it.

Mr. D. could meet with no observations of eclipses to be at all depended on, between those of Regiomontanus and Albategnius, except 2 of the sun and 1 of the moon, made at Cairo in Egypt, related in the prolegomena to Tycho Brahe's Historia Cœlestis, p. 34; nor any between those of Albategnius and Ptolemy, besides the eclipse of the sun observed by Theon at Alexandria. These eclipses of the sun are the more valuable, because they were observed in places the longitudes and latitudes of which are determined by Mons. Chazelles of the Royal Academy of Sciences, who was sent by the French king in the year 1693, with proper instruments for that purpose. Du Hamel Hist. Acad. p. 309, 395.

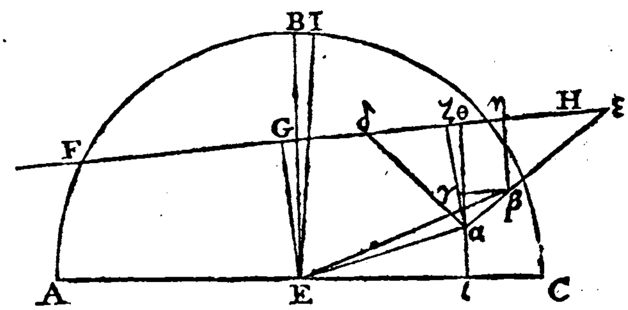
The solar eclipse observed by Theon, was in the 1112th year of Nabonassar the day of Thoth, according to the Egyptians, but the 22d day of Pauni, according to the Alexandrians: he carefully observed the beginning of 2 temporal hours and 50^m afternoon, and the end at $4\frac{1}{2}$ hours nearly afternoon at Alexandria. Theonis Comment. in Ptol. mag. Construct. p. 332. This eclipse was June 16, in the year of Christ 364. And the temporal hour at Alexandria being at that time to the equinoctial hour as 7 to 6, makes the beginning at 3 equinoctial hours and 18^m afternoon, and the end at 5 equinoctial hours 15^m nearly.

The eclipses observed at Grand Cairo were as follow.

The solar eclipse Decemb. 13, in the year of Christ 977, the beginning at $8^h\ 25^m$, and the end at $10^h\ 45^m$, apparent time in the morning.—2. The solar eclipse June 8, 978; the beginning at $2^h\ 31^m$, and the end at $4^h\ 50^m$, apparent time afternoon.—3. The lunar eclipse May 14, 979; but as the middle cannot be known from what was observed of it, he made no use of it in this inquiry.

That the before-mentioned solar eclipses might be applied to the examination of the lunar motions, he contrived the following method; which he thinks renders eclipses of the sun as useful at least as those of the moon are in that business.

Let ABC, in the annexed figure, represent half the earth's enlightened disk, AEC a portion of the ecliptic projected on FGH, the path of the moon's shadow over the disk, EI, the universal meridian; α the situation of the place at the beginning of the eclipse, β its situation at the end, δ the centre of the shade at the beginning, and ε its centre at the end of the eclipse. Draw EG, $\alpha\zeta$, and $\beta\eta$ perpendicular to the path of the shadow, $\beta\gamma$ parallel to it; join $\alpha\delta$ and $\beta\varepsilon$, and through α draw $\theta\iota\alpha$ perpendicular to AC.



Then (computing the true places of the sun and moon at the observed times of the beginning and end of the eclipse) we shall have given $\delta\varepsilon$ the motion of the moon from the sun in her orbit during the time of the eclipse, and $\alpha\delta = \beta\varepsilon$ the semidiameter of the penumbra; which are to be reduced into such parts as the semidiameter of the disk contains 10000: the angles BEI and BEG, being found by methods commonly known, GEI their sum or difference will be given. Also Eα and Eβ will be sines of the sun's altitude at the beginning and end of the eclipse respectively; IEα and IEβ are the angles at the sun between the vertex of the place and the pole of those times; which being found, the angle αEβ their difference, will be known, whence the line $\alpha\beta$ and the angle E$\alpha\beta$ may be computed.

The angle GEα is the sum or difference of the known angles GEI and IEα: in the figure the complement of this to a semicircle is E$\alpha\gamma$; which being subtracted from E$\alpha\beta$, leaves the angle $\gamma\alpha\beta$, from which, and the line $\alpha\beta$, $\alpha\gamma$ and $\gamma\beta, = \zeta\eta$, may be found.

Let $a = \delta\varepsilon - \zeta\eta$, $b = \alpha\delta = \beta\varepsilon$, $c = \alpha\gamma$, and $x = \beta\eta = \gamma\zeta$. Then $\sqrt{bb - xx} = \eta\varepsilon$, and $\sqrt{bb - cc - 2cx - xx} = \delta\zeta$, by Eucl. 1. 47. Consequently $a - \sqrt{bb - xx} = \sqrt{bb - cc - 2cx = xx}$; which being reduced, gives the quadratic equation $xx + cx = \frac{4a^2b^2 - a^4 - 2a^2c^2}{4aa + 4cc}$. This equation solved, gives the value of x, from which $\delta\zeta$ and $\eta\varepsilon$ will be also had. In the triangle $\alpha\zeta\theta$, we have $\alpha\zeta$ and the angle $\zeta\alpha\theta$ = GEB given, whence $\alpha\theta$ and $\zeta\theta$ may be found: consequently $\delta\theta$ will be known; and from the observed time of the beginning of the eclipse, and hourly motion of the moon from the sun, the time when the centre of the shade is at θ will be had. Lastly, in the triangle E$\iota\alpha$, we have given the side Eα, and the angle Eαι, = BEα (the sum or difference of the angles BEI and IEα): therefore the sides Eι and αι may be found. But Eι is the distance of the moon from the sun in the ecliptic, and αι ± αθ the moon's latitude at the time when the centre of the shade is at θ; which may be compared with the computation from the tables for that time.

By this means Mr. D. compared the aforesaid solar eclipses with the tables, and found the difference in longitude and latitude as follows:

A.D.	Apparent Time at Greenwich.	Dist. ☽ a ☉ from Eι.	Lat. ☽ from θι.	☽ a ☉ by Tab.	Lat. ☽ by Tab.	Diff. from Obser. in Long.	Diff. from Obser. in Lat.	Diff. in Lat from Digits observed.
364	June 16, 2^h 4^m 20^s	39′ 41″ in conseq.	34′ 37″ N.	35′ 25″	37′ 26″ N.	−4′ 16″	+2′ 49″	−2′ 36″
977	Dec. 12, 19 12 30	43′ 39 in antec.	30 23 N.	36 3	31 50 N.	+7 36	+1 27	+3 38
978	June 8, 1 16 10	29′ 3 in conseq.	8 24 S.	37 48	3 21 S.	+8 45	−5 3	

The agreement between the last 2 of these differences in longitude, shows that the tables represent the mean motion of the moon's apogee very well for above 700 years, the moon being very near her perigee at the time of one of those eclipses, and near her apogee at the time of the other.

By the same method he also compared the sun's eclipse, July 29, 1478, (which appears, from what is related of it, to have been carefully observed by Walther at Nuremberg,) with the tables, and found the difference in longitude to be + 10′ 29″, and in latitude + 9′ 12″. This wide difference in latitude, from the tables, that agree so well with the former ancient observations, confirmed the opinion, that the Nuremberg observations are too inaccurate to determine any thing from them in this affair.

The eclipses recorded by Ptolemy in his Almagest, are most of them so loosely described, that if they show us the moon's mean motion has been accelerated in the long interval of time since they happened, they are wholly incapable of showing us how much that acceleration has been. There are indeed 2 or 3 of them attended with such lucky circumstances, as not only plainly prove that there has been such an acceleration, but also help us to guess at its quantity. One of these is the eclipse said by Hipparchus to have been observed at Babylon, in the 366th year of Nabonassar, the night between the 26th and 27th days of Thoth, when a small part of the moon's disk was eclipsed on the north (east, half an hour before the end of the night, and the moon set eclipsed. This was in the year before Christ 313, Decemb. 22. The middle of this eclipse at Babylon (supposing with Ptolemy) the meridian of that place to be 50^m in time east of the meridian of Alexandria,) by Mr. D.'s tables, was Dec. 22^d 4^h 4^m apparent time; the duration was 1^h 37^m, Ptolemy makes it 1^h 30^m nearly; hence the beginning should have been about 8^h 15^m after midnight: according to

Ptolemy, the night at Babylon was at that time $14^h\ 24^m$ long, and therefore sun rise at $7^h\ 12^m$ after midnight; and as the moon had then south latitude, and was not quite come to the sun's opposition, her apparent setting must have been something sooner, i. e. more than an hour before the beginning of the eclipse, according to the tables; whereas the moon was seen eclipsed some time before her setting; which demonstrates, that the moon's place must have been forwarder, and consequently her motion since that time less than the tables make it by about 40′ or 50′. But the computed place of the moon, in each of the before-mentioned solar eclipses, observed at Cairo, being about 8′ before her place from observation, shows us that the mean motion of this luminary has been something greater in the last 700 years than the tables suppose it, and therefore must have been accelerated.

This acceleration is further confirmed by the eclipse which Hipparchus says was observed at Alexandria, in the 54th year of the second Calippic period, the 16th day of Messori, when (he says) the moon began to be eclipsed half an hour before her rising, and was wholly clear again in the middle of the third hour of the night. This was in the year before Christ 201, Sept. 22. The middle of this eclipse at Alexandria by the tables, was Sept. 22, $7^h\ 44^m$ apparent time; and the duration $3^h\ 4^m$, which makes the beginning at $6^h\ 12^m$ apparent time, that is about 10^m after the rising of the moon at Alexandria, or 40^m later than the beginning from observation. This difference in time makes a difference of near 20′ in the moon's place.

The most ancient eclipse of which we have any account remaining, namely that related by Ptolemy, as observed at Babylon the first year of Mardokempad, in the night between the 29th and 30th days of Thoth, in which the moon began to be eclipsed when one hour after her rising was fully past; if by reason of the latitude of the expression, it be not a direct proof of the acceleration, it may nevertheless help to limit its quantity. This eclipse was in the year before Christ 721, March 19. The middle at Babylon, by the tables, was March $19^d\ 10^h\ 26^m$ apparent time; and the beginning at $8^h\ 32^m$, the apparent rising of the moon at that place was about $5^h\ 46^m$ afternoon; so that the observed beginning of the eclipse was at least $6^h\ 46^m$ afternoon, i. e. not above $1\frac{3}{4}^h$ before the beginning, by the tables: therefore the moon's true place could precede her place by computation, but little more than 50′ at that time.

If we take this acceleration to be uniform, as the observations on which it is grounded are not sufficient to prove the contrary, the aggregate of it will be as the square of the time: and if we suppose it to be 10′ in 100 years, and that the tables truly represent the moon's place about A. D. 700, it will best agree with the before-mentioned observations; and the difference between the moon's place by the tables, and her place in the heavens, will be as follows.

Years A.C.	Error of Tables.	Years A.D.	Error of Tables.	Years A.D.	Error of Tables.	Years A.D.	Error of Tables.	Years A.D.	Error of Tables.
700	− 56′ 6″	200	− 28′ 30″	300	− 9′ 20″	800	+ 1′ 30″	1300	+ 4′ 0″
600	− 49 50	100	− 24 0	400	− 6 30	900	+ 2 40	1400	+ 3 30
500	− 44 0	A.D.0	− 19 50	500	− 4 0	1000	+ 3 30	1500	+ 2 40
400	− 38 30	100	− 16 0	600	− 1 50	1100	+ 4 0	1600	+ 1 30
300	− 33 20	200	− 12 30	700	0 0	1200	+ 4 10	1700	0 0

Some Histories of Morbid Structure observed in Dead Bodies. By Albert Haller, Archiater, Professor of Physic at Gottingen, and F.R.S.* N° 492, p. 172.

On the Lacrymæ Batavicæ, or Glass Drops, the Tempering of Steel, and Effervescence, accounted for by the same Principle. By Claud. Nic. le Cat, M.D., F.R.S. N° 492, p. 175.

The glass-tear, or drop, commonly called lacryma Batavica, or lacryma Borussica, because it was first made in these countries, is much celebrated among natural philosophers, on account of the singular phenomena which it exhibits, and which have for a long time exercised their sagacity. The make of this drop is as simple as its explanation is difficult. It is the work of the meanest workman in a glass-house. On the top of an iron rod they take up a small quantity of the matter of glass in fusion: they let it drop into a pail of water: the drop makes that part of the water which it touches to boil with a hissing noise, as a red-hot iron does, which it resembles in that instant; and when it does not break in this operation, as it most frequently does, it forms the little pyramidal mass, which is known by the name of a glass drop.

This drop is of such hardness and resistance, that it bears smart blows of a hammer, without breaking. Yet if you grind the surface of this drop which resisted the hammer, or if you only break the tip of the small end or tail, the whole shatters into powder. This shattering of the drops is attended with a loud report: and the dust or powder to which it is reduced, darts out, and scatters all around. If the drop be ground with powder of emery, imbibed with oil, it often escapes breaking. If this experiment be made in the air-pump, the drop bursts with greater impetuosity, so as sometimes to break the receiver; and its dust is finer than when done in the open air; and if it be made in the dark, the drop in bursting produces a small light. If this drop be annealed in the fire, it loses all these singularities; and being reduced to the state of common glass, it easily breaks under the hammer,; and does not burst on breaking the small end. The drops that are made by letting them cool in the air, produce no other effects than those which have been annealed.

* By some oversight, this paper is a republication of that inserted in the Phil. Trans. N° 483, vol. xliv. and at p. 348, vol. ix. of these Abridgements.

The first natural philosophers who endeavoured to investigate the cause of these phenomena, imagined that they found it in the air. Some of them supposed, that this air was shut up in the drop by the crust which the cold water forms on its surface while it is yet red-hot; and attributed its rupture to the violence with which this air issued through the too narrow passage made for it, in breaking the small end of the drop. Others maintained, on the contrary, that the drop, in this state, contained no air at all, nor any thing but particles of fire, or subtile matter; in short a mere vacuum as to air; and that the sudden bursting of the drop was occasioned by the impetuous entry of the air into this kind of vacuum. In fine, the Cartesians have substituted their subtile matter in the room of this exterior air, and say, that the drop is bursten by the less subtile particles of this matter; which entering with force into the drop by the opening made in it, and finding large pores on the inside, and small ones on the outside, burst the sides of the drop, by rushing from the centre to the circumference, with which its passage is obstructed.

Messrs. Mariotte and Homberg came afterwards: being provided with an air pump, they caused one of these drops to be broken in vacuo; and Homberg having observed, that it there broke better, and with a louder report, than in the open air; they both inferred, that neither the impetuous entry of the outward air, nor that of a fluid somewhat less gross, could be the cause of this shock; because the receiver of the air-pump is void of these fluids; and even if a little should remain there, it is too much rarefied, and too thin to be capable of such an effect.

Mr. Mariotte, through some remains of attachment to an opinion, which he had held till that time, did not entirely exclude the exterior air from the cause of the phenomena of the drop; but thought proper to add another to it; which he makes use of as a substitute in cases like those of the preceding experiment, where the insufficiency of the air, or of a fluid nearly similar to it, plainly appears.

Mr. Homberg shows no indulgence to the exterior fluid; and ascribes the whole to the new cause, which is, the quality of tempered glass, which the drop acquires, like steel, by being thrown red-hot into cold water. This tempering, according to these great academicians, confers at the same time more springiness to the parts, and less connection with each other. When a steel sword-blade is bent forcibly, it breaks more easily than one of iron; and the jarring which is occasioned by its spring, is capable of breaking the other parts of the blade; and thus we see, that it generally breaks into several pieces. This blade is the image of the glass drop.

This is the point to which Mons. le Cat found things brought, when he began to study the phenomena of the glass drop. The air was partly banished from the inside of this mass of glass: there is none in the liquid red-hot matter of a glass furnace. It was purely out of complaisance for a generally received opinion, that Mr. Mariotte allowed the exterior fluid any share in the phenomenon; and Mr. Homberg put the finishing hand to its exclusion. But the kind of temper given to the drop by plunging it red-hot into cold water, and its comparison with tempered steel, is not so much a cause as a comparison: and is this comparison very just? can there be any, between a long thin sword-blade, which breaks into 2 or 3 pieces, and a thick inflexible mass of glass, which flies into powder? The tail alone of the drop might seem to favour this parallel; but an experiment which he made entirely destroys this opinion, and proves that it is not the spring, or the vibrations of the parts of the drop, that occasion its bursting.

He put about half the tail of a glass drop into a vice, between two bits of deal-board of about a finger's breadth. He screwed the vice, till he saw this small cylinder or thread of glass make impressions in the wood on each side, for its lodgment, in order to be sure that it could not be susceptible of vibrations. In this condition he broke the end of the tail, supporting it on his nail, to prevent forcing any part but the end which he intended to break; and in order to be the more certain of giving no shock to the part that was squeezed in the vice. The drop flew into powder as usual; and the portion secured between the two bits of wood, perfectly retained its figure in the impressions wherein it was lodged. But when he touched this little cylinder, it was reduced to powder, much in the same manner as is said to have happened to some men who had been struck with lightning. Now it was not possible for this glass to receive, or convey to the body of the drop, any vibrations; or if any, they must be infinitely small; and yet the effect was precisely the same as usual. Therefore the system of vibrations is not happier than those invented before it.

It is among the glass-workers, and in their art, that the secret of the glass drop is to be sought; and there it is that he discovered it. Those who have seen glass-houses know, that when a piece fails in the hands of a workman, he throws it aside; and this piece is not long exposed to the air, before it breaks in pieces; and when the same workman has succeeded in making a piece, and is willing to preserve it, he takes great care not to let it cool in the air; but carries it hot into another oven of a moderate heat, where he leaves it for a certain time. And this last operation is called annealing the glass.

A natural philosopher, who is witness to this management, ought to inquire into the reasons and necessity of it. How comes it that the glass which cools in the air breaks, and when it has been nealed, it does not break? this is the reason. A bit of melted glass, red-hot and liquid at the same time, is in that state, purely because its particles are divided by so great a quantity of particles of fire, or subtile matter so violently agitated, that these component parts of the glass hardly touch each other; they swim, as it were, in a flood of this matter of fire; and for this reason it is, that melted glass affects the colours of flame.

When this substance is exposed to the air, the coolness of this fluid, which touches the surface of the glass, cools that surface first; that is, brings the particles nearer together, braces their pores, and thus imprisons the particles of fire, which still fill the inside of this substance. While these fiery particles find pores enough on the surface to move freely, the glass continues whole; but when the glass grows colder, that is, when the pores of its surface begin to confine these fiery particles; then their whole action is exerted against the parts of the glass, which they break into a thousand pieces. In order to avoid this bursting, nothing more is requisite than to keep the pores on the surface of the glass wide enough, that the fiery particles may pass through, and fly off insensibly. Now this is what is done by putting the hot piece of glass into an oven, the moderate heat of which keeps these pores open to a certain pitch, and yet allows the glass to acquire its due consistence in this state of middling porosity, in which consists the annealing of glass and other fused substances.

Hence it appears, that all unnealed glass carries within itself its principle of destruction, which is the matter of fire imprisoned. But the glass drop is in this respect, in a worse case still than unnealed glass; for besides that it has not been exposed to this secondary heat, which keeps its pores open, till the glass has acquired its due consistence, for fear that the coolness of the air alone should not close its pores soon enough and imprison a sufficient quantity of the igneous matter, it is suddenly thrown into cold water, which by its coldness and weight is fitter than the air to produce such an effect speedily and effectually. Hence the only surprising circumstances in these glass drops is, that any of them remain without breaking, by the great quantity of igneous matter suddenly shut up in them by the cold water. And indeed this accident befalls more than the half of them; and those that escape doubtless owe their preservation to the spherical or cylindrical figure of the compact shell, which the coldness of the water forms on their surface; for it is well known that this figure produces an equality of resistance on all sides, which considerably increases the resisting force; and this is the first reason why, as soon as this equilibrium is broken, either by rubbing away one side of this surface, or by making a hole in it, or, in fine, by breaking the small end of the drop; the resistance is instantly overcome, and the igneous matter, imprisoned within the glass, and constantly on the strain against it, bursts it into powder.

This destroyed equilibrium is but one disposition that favours the effect of the imprisoned igneous matter; but the communication which is opened for it with the subtile exterior fluids, rouses this matter which is in a state of inactivity, developes its spring, kindles it somewhat in the manner of the phosphorus, which produces no effect while close shut up, but takes fire, as soon as a free communication with the outward air is given it.

On the union of these causes depends the phenomena of the glass drop. It is of a hardness that resists the strokes of a hammer, because the violent condensation, given to its surface by the cold water, into which it was thrown when in a soft state, rendered its texture very close, compact, and consequently hard. It bursts with great noise; and in so doing it retains the character of all the effects produced by the explosion of the igneous matter. Its dust flies 2 or 3 feet all around, because it is pushed forward by the action of a fluid contained in its centre: which would not happen, if it had been the effect of an exterior fluid. This same dust of the glass drop darts forward with greater force in the air-pump than in the air, because the air is an obstacle, from which it is freed in the receiver of the air-pump; hence it sometimes breaks the receiver; and for the same reason its dust is finer, that is, more minutely broken, as being done by a stronger power, and less counterbalanced.

This violent explosion produces light, because the property of shining lightning is always the effect of such an explosion of the matter of fire; hence this fact affords another proof, that this matter is the principle of the phenomenon of the drop. If the surface of the drop be ground with fine powder of emery, imbibed with oil, it frequently happens that it does not burst; because the kind of oily mastic that results from this mixture, stops the pores of the drop, and prevents the sudden communication of the exterior fluids with the imprisoned igneous matter; and as glass cannot be ground with very fine emery and oil, but by long rubbing; such rubbing heats the drop, and gradually opens the pores so as to grant an insensible passage to the igneous matter, by which the drop becomes at last in the same case with nealed glass; and in the case in which itself is, when it is put into the oven to be nealed. When a glass drop is made, by suspending it in the air only, it does not break sooner than nealed glass; because as this small mass of glass retains its heat a long while in the air, the heat serves as a nealing oven, and keeps its pores dilated long enough for the igneous particles to find a free passage.

The principles, by which he has accounted for the effects of the glass drop, are not confined to this phenomenon alone: they are more general than is commonly imagined. Thus steel, like the glass drop, acquires its hardness by being plunged into water; and if Messrs. Mariotte and Homberg had compared them together in this circumstance alone, they had been in the right.

The most celebrated natural philosophers, in order to account for the tempering of steel, have had recourse to different arrangements of its parts produced by the fire, and fixed, by the cold of the water, in the new state, in which the violent heat had put them.

The mechanism of the tempering of glass drops, applied to that of steel, is the most simple of all the hypotheses, and answers all its properties, which are these: 1. Tempered steel has a coarser grain. 2. It is increased in bulk. 3. It is harder and brittler. 4. By annealing it becomes less brittle.

Explanation: Steel made red-hot is filled and swelled, and its pores dilated, by the igneous matter. In this state, the cold water, into which it is thrown, compresses and closes the parts of the surface, while the imprisoned igneous matter dilates the pores within; thus the texture of steel becomes more compact by these two causes, while its pores are dilated. These large pores constitute the coarse grain of tempered steel. Its dilatation by the igneous matter, which could not be thoroughly condensed by the cold of the water, causes its augmented bulk; the close texture of the substance that surrounds the pores, and the imprisoned igneous matter, occasion its hardness and brittleness. Its recoction or annealing deprives it of this brittleness, and of a part of its hardness; because it opens this texture, which it relaxes at the expence of the neighbouring pores, and drives the igneous matter out of it.

Also the fermentation of acids and alkalis seem to be another corollary of the same principle.

First, It is pretty universally allowed, that the acid particles have the figure of small needles; and that alkalis are spheroidal or polyhedrous bodies, with a vast number of pores proper to admit the acid needles. Secondly, Experience shows, that salts are alkalised by fire, and that our juices are alkalised by heat, &c. What can the repeated action of the fire produce on salts, in order to alkalise them? it calcines them, blunts their points, and hollows them with a vast number of pores; and we see with the naked eye, that calcination has this effect on all bodies. In short, it converts an angular very solid body into a very porous and light spheroid; and this body is an alkali by the first supposition. Thirdly, calcination introduces, and generally leaves in the pores of the calcined body, after the operation, a great quantity of igneous matter. This matter is perceptible to the senses in the lapis bononiensis, which becomes a phosphorus by calcination; in lime-stone, which by calcination is furnished with so great a quantity of igneous matter, that in the effervescence, which is raised in it by throwing a little water on this stone, you may kindle sulphur or a match by it. The alkaline, or alkalised salts also, that is, those that are calcined, have their pores full of the igneous matter. Fourthly, such is the nature of the igneous matter, that it tears asunder whatever opposes its passage, and makes it fly off with a report. This principle is universally allowed: the effects of gunpowder, of volcanos and earthquakes, prove it; and to come nearer our subject, unnealed glass breaks in the air, and the lacryma Batavica does as much on breaking its small end.

Whereas an alkali is a spongious body filled with the igneous matter, and an acid are points proportioned to these pores; these ought to be regarded as so many pegs or pins, which enter into the holes on the surface of the alkali, and fill them up exactly: by which the igneous matter is imprisoned; and by the preceding principle it bursts the alkaline globule with noise, and scatters around the acid pegs, in the same manner as it bursts the glass drop.

A mixture of an alkaline and acid liquor being composed of an infinite number of such particles that burst and broke to pieces, the liquor must take up more room, or swell. The particles of contained air being tossed about by all those little explosions, together with the neutral liquors, which are a vehicle to the salts, form the scum or froth; and the igneous matter, which gets out of the alkalis, and is agitated by the shocks of all these explosions, produces heat, drags with it the aqueous and other volatile particles, which form the steam.

Yet there are cold fermentations, because then, either the motion of the particles of fire, and their burstings are inconsiderable; or because these particles fly off easily by a direct motion. Further, at this day that we have it in our power to be convinced, that the brush or stream of electric matter is very cold, nobody will be surprised that a stream of the matter of fire may produce cold.

If all the alkalious corpuscles bursted at once, the fermentation would last but an instant: but as the acid liquor requires a certain space of time, to penetrate the whole alkaline liquor, and fill the pores of the alkalious corpuscles, the fermentation is performed successively in a certain number of corpuscles at a time, till they are all broken: and this succession constitutes the duration of the fermentation; which ceases when there are none of the alkalis left entire.

These principles not only serve to explain the fermentation which results from the mixture of acids and alkalis, but also almost all the motions of this kind, which are occasioned by the mixture or penetration of 2 or more substances. For example; lime, which we have mentioned above as a body filled with the matter of fire, and which produces an effervescence capable of lighting sulphur, if water be thrown on it; lime then produces this effect, only because the particles of water, which enter into its pores, have a tendency to shut up the igneous particles more closely. It is by a mechanism entirely similar, that Homberg's phosphorus kindles into flame, on being exposed to the air: it is on this principle also that a mixture of spirit of wine and water acquires a new degree of heat; and so of other phenomena of this nature.

On the Electricity of Glass, that has been Exposed to Strong Fires. By Mr. Prof. Geo. Matthias Bose of Wittemberg. N° 492, p. 189.

It seems that a glass ball, which has often been employed for violent distillations, and other chemical operations, sends forth the electricity incomparably more strong than any other glass, which never since its making had been exposed to a violent fire. As I am the first, says Mr. B. that has mentioned this notable circumstance, be pleased to let me have the honour of this improvement in the Phil. Trans.

Of an Extraordinary Rainbow, observed July 18, 1748. *By Peter Daval, Esq. Sec. R.S.* N° 493, p. 193.

On Monday July 18, 1748, about a quarter before 7 in the evening, the weather being temperate, and the wind about N.N.W., walking in the fields beyond Islington, Mr. D. saw a distant rainbow, which appeared to take in a large portion of the heavens; but had nothing remarkable, and vanished by degrees. Continuing his walk, about 20 minutes after the disappearing of the first rainbow, a rainy cloud crossed him, moving gently with the wind, which exhibited a more perfect and distinct rainbow, than he had ever before seen; in which he could plainly distinguish all the secondary orders of colours noticed by the late Dr. Langwith, in his letters published in the Philos. Trans. N° 375, that is, within the purple of the common rainbow, there were arches of the following colours: 1. Yellowish green, darker green, purple. 2. Green, purple. 3. Green, purple.

Of the Present Condition of the Roman Camp at Castor in Norfolk, with a Plan of it; also a Representation of a Halo or Mock-Sun observed July 11, 1749. *By Mr. Wm. Arderon, F.R.S.* N° 493, p. 196.

The town of Castor is at present in a very low condition, containing no more than between 20 and 30 small cottages. It stands about 4 miles south-west of Norwich, and by tradition, and some learned authors, is supposed to have been a considerable city, out of whose ruins Norwich took its rise. However, at this day, excepting the camp, not the least trace or footstep of any thing remarkable is left remaining.

The camp itself lies near a furlong south-west from the town of Castor, and leads you by a gentle descent down to the little river Wentsum, which swiftly glides close to the end of it, and doubtless at the first forming of the camp was designed to be part of the fortification on that side, as well as to supply the army with water, and to bring up such things as they wanted from the sea, if their communication by land should at any time be impeded. This river is by some called Taus, or Tese: but probably it did not formerly take that name till it approached the Roman camp at Teseburgh, 3 or 4 miles higher.

We are told by tradition, as well as by some learned authors, that the sea came up to this camp; and indeed every intelligent observer must confess, that the marine bodies found in every part of Norfolk, on the highest hills, as well as in the lowest pits and valleys, are indubitable proofs, that at some time or other the sea must have covered this whole country: but then we may be assured by the present condition of this camp, that the sea has not exceeded the level of it since it has been in being, which if we credit several of our ancient historians, it was upwards of 1700 years ago. It may therefore serve to prove, that the sea since that time has not exceeded these bounds, and that the fossils dug up above this level are more ancient than it, though we have no proper data to discover how long before the sea had passed this height.

The figure of the camp is not a square, but a parallelogram, whose 2 longest sides are each 440 yards, and its ends or 2 shorter sides 360 yards each. These are its dimensions withoutside the rampart and ditch; but within the length is 392 yards, and the breadth 264. The breadth of the fosse and rampart, in some places where it remains most perfect, was 48 yards, though in others not above 30. And the whole ground taken up, including the ditch and rampart, is 32 acres, 2 roods, and 36 poles; or the area within the ditch and rampart 21 acres, 1 rood, 21 poles.

Three sides only of this camp have been fortified with a rampart, whose upper part was faced with a thick and strong wall, made of lime and flints; of which wall there are still remains in several places of the rampart, besides a very deep ditch, that seems to have been most considerable on the east and south sides. The wall on the north side appears to have been built at 2 different times; that is, it seems to have been raised higher than it was built at first, at some distance of time afterwards; for a parting may be observed at a certain height running from end to end.

The ruins of 2 old towers still remain, one of which stood on the north side, and the other at the west end; the last of which is at present the most considerable of the two. They were both built in a manner perhaps peculiar to the Romans at that time, and which it may not be improper to describe. They began first with a layer of bricks laid flat as in pavements; on that they placed a layer of clay and marl mixed together, and of the same thickness as the bricks; then a layer of bricks, afterwards of clay and marl, then of bricks again, making in the whole 3 layers of bricks and 2 of clay: over this were placed bricks and lime 29 inches, the outside being faced with bricks cut in squares, like the modern way of building in some parts of Norfolk, then bricks and clay, again stratum super stratum, as high as the old ruins now remain standing.

The mortar is found extremely hard at this day: it is a composition of lime, sand, and ashes, and so compact, that he could by no means break a piece of it, of an inch diameter, from the base of one of the towers at the east gate, but on striking it with a sharp flint it flew off in dust.

The Roman bricks which he examined, were made of 2 different sorts of clay mixed; when burnt, one appears red and the other white: at the time of viewing them, they were exceedingly hard and solid, and far superior to any thing of the kind now made with us. Perhaps they are little worse than when they were first laid down. These bricks were made without the assistance or addition of sand, as is too much the practice at present here in Norfolk: for when sand enters the composition in any considerable proportion, it renders the bricks friable, soft, and rotten, subject to be broken or ground to pieces with the least motion or pressure. The length of these bricks is $17\frac{4}{10}$ inches, or a Roman foot and half; and their breadth $11\frac{6}{10}$ inches, or precisely a Roman foot: which may serve as some proof that the Roman measures, handed down to us by several authors, are right, and may likewise inform us of the proportionable stature of man at that time. The thickness of these bricks is $1\frac{3}{10}$ inch.

The great number of Roman medals that have been, and still are found in and about this camp, are a matter of great wonder. One lady who lives near the place, has it seems picked up at least 100; and several are daily gathered up by boys, and sold to strangers who come to visit the place. That these pieces have been used as money seems exceedingly clear, from their different degrees of perfection; some being worn almost quite smooth, others having imperfect busts without letters, and others again having both the busts and inscriptions fair and legible, which could only happen from their different wear as money.

A particular kind of halo was observed at Norwich, on the 11th of July 1749, at 5 o'clock in the evening: the colours were exceedingly vivid, and the centre of it, contrary to what he ever yet saw, was not in the sun, but in the zenith. The sun's rays shone through the clouds at the same time, as they frequently do when the sun is near the horizon.

Part of a Letter from Leonard Euler, Prof. Math. at Berlin, and F.R.S. To the Rev. Mr. Caspar Wetstein, concerning the Gradual Approach of the Earth to the Sun. Dated Berlin, June 28, 1749. *Translated from the French, by S. T., M.D., F.R.S.* N° 493, p. 203.

M. le Monnier writes to me, that there is at Leyden an Arabic manuscript of Ibn jounis (if I am not mistaken in the name, for it is not distinctly written in the letter), which contains a history of astronomical observations. M. le Monnier says, that he insisted strongly on publishing a good translation of that book. And as such a work would contribute much to the improvement of astronomy, I should be glad to see it published. I am very impatient to see such a work which contains observations, that are not so old as those recorded by Ptolemy. For having carefully examined the modern observations of the sun with those of some centuries past, though I have not gone farther back than the 15th century, in which I have found Walther's observations made at Nuremberg; yet I have observed that the motion of the sun, or of the earth, is sensibly accelerated since that time; so that the years are shorter at present than formerly: the reason of which is very natural; for if the earth, in its motion, suffers some little resistance, which cannot be doubted, since the space through which the planets move, is necessarily full of some subtile matter, were it no other than that of light, the effect of this resistance will gradually bring the planets nearer and nearer the sun; and as their orbits thus become less, their periodical times will also be diminished. Thus in time the earth ought to come within the region of Venus, and in fine into that of Mercury, where it would necessarily be burnt. Hence it is manifest that the system of the planets cannot last for ever in its present state. It also incontestably follows, that this system must have had a beginning: for whoever denies it, must grant that there was a time, when the earth was at the distance of Saturn, and even farther; and consequently that no living creature could subsist there. Nay there must have been a time, when the planets were nearer to some fixed stars than to the sun; and in this case they could never come into the solar system. This then is a proof, purely physical, that the world, in its present state, must have had a beginning and must have an end. In order to improve this notion, and to find with exactitude, how much the years become shorter in each century; I am in hopes that a great number of older observations will afford me the necessary succours.

On the Effects of the Mixture of the Farina of Apple-trees; and of the Mayze or Indian Corn: and of a Child Born with the Jaundice on it, received from its Father; and of the Mother taking the same Distemper from her Husband, the next Time of being with Child. By Mr. Benj. Cooke, F.R.S. N° 493, p. 205.

When the farina of one apple impregnates another's blossom of differing species, we see the change * in the fruit; but whether any lasting impression is left on the bough which bore it, as seems to be in tulips and some other flowers, is not so easy to determine, experiments of this sort being not to be made at all, but catched at distant opportunities; and till this point is settled, the distemper of my good friend's tree must rest unexplained.

Artificial helps of sight have added to former discoveries the explosive manner of the farina's action; but what may be the effect of the inconceivably fine subtile matter emitted from its globules, and continually wafted about in great plenty and variety in the summer air, not only on vegetable productions (where on different subjects it may not improbably have opposite effects) but other matters not yet suspected to be so much under its influence, remains a field of inquiry for future ages. However, to what Mr. Loggan has very justly observed (Trans.

* See these Transactions, N° 490.—Orig.

440) on the manner of impregnation of the seeds in mayze, Mr. C. adds, that if the seed and whole species of mayze be planted about 2 yards distance from each other, there will be a mixture of red and white grains in the ears of each plant, and you may with pleasure observe the filament in the white plant, which has been struck with the red farina, discovering its alien commerce by a conscious blush, and by counting the threads they stained, foretell how many corresponding seeds will appear red, at the opening of the ear, when ripe.

A man of about 22 years married a healthy young woman, much of the same age. Soon after he went to America, and at the end of 7 years returned cachectic, anasarcous, and deeply tinged with the jaundice, endemical in hot latitudes. In a few months after his return his wife became pregnant with her first child, of which she was delivered in due time. The child was born with a jaundice on it, and died about 6 months after, under ascitical and icterical symptoms, of which the mother had not the least impression. Soon after this, (and before the husband, though much better, was quite cured) she became again pregnant, and after about 3 months turned yellow, and was the whole time of her going with child, and some months after her delivery, deeply affected with the jaundice. But the child was born quite fair, white, and healthy, without any thing of that distemper on it; and is still living, and the last born.

Concerning a very Cold Day, and another a very Hot Day, in June and July 1749; *and of the Near Agreement of Thermometers in London and at Tooting. By the Rev. Henry Miles, D.D., F.R.S.* N° 493, p. 208.

June the 10th, suspecting a frost that night, Dr. M. set a China saucer full of water on the grass plot in the garden; and the next morning, a little before sun-rising, the water was frozen over, of such a consistence, that he forced a hole through the centre of it with his finger, without breaking it elsewhere, and carried the cake of ice into the house, where it remained a good while not dissolved. Wind was N.W. On some following days there were several considerable frosts, the wind continuing the same way; the fatal effects of which are sufficiently known throughout the kingdom.

July 2, at $12^h\ 20^m$, his thermometer of Farenheit's scale, in the shaded air, stood at 88, and at 2^h p. m. at 87. At which last number, 2 others of the same sort stood exactly, at that hour, in London.

Having agreed with Mr. John Canton of Spital-square, to make observations of the temperature of the air here, at Tooting, and in London, at a stated hour. They procured thermometers, made exactly alike, by that accurate workman Mr. Bird; and having found, by hanging them first together a sufficient time, that they perfectly agreed, they began their observations in April, and continued them ever since.

It appears by a comparison which they made, that the difference in the temperature of the air, as to heat and cold, is very little between this place and Spital-square. Sometimes the one thermometer has been higher; and sometimes the other. So that on the whole, it may reasonably enough be concluded, that the difference between the temperature of the air in the two places, is imperceptible to sense.

Of a Bas-relief of Mithras found at York, explained by the Rev. Dr. Stukely, F.R.S. Communicated by Mr. Francis Drake of York, Antiquary and F.R.S. N° 493, p. 214.

As York was doubtless the Roman imperial city of Britain, so is it still casually throwing up remains of its ancient grandeur and magnificence, even down to our time. About 2 years ago, in digging the foundation of a large house, since built, in our Trans Tyberim Street, called Micklegate, quasi Muckle, or Great Street, the workmen went much below any former foundation that could be observed on this spot. And at the depth of 10 feet, came to a stone, which on taking up appeared to have figures on it, but miserably defaced. Mr. D. sent as just a drawing of it, as could be taken to Dr. Stukely, who according to his deep knowledge in the learning of the ancients, soon after returned the following short, but curious explanation of this uncommon piece of sculpture.

The drawing you sent of the bas-relief, dug up in a cellar in Micklegate, anno 1747, is a great curiosity. It is a sculpture of Mithras, as usual, sacrificing a bull. He has on, the Persian mantle, called candys, and the Phrygian bonnet, called tyara. He represents the archimagus, performing the great annual sacrifice, at the spring equinox: according to the patriarchal usage. These ceremonies to Mithras, were generally celebrated in a cave of a rock; therefore, this sculpture was found so deep in the earth.

There is commonly a figure on each side of him, habited in the same manner, standing cross legged: the one holds a torch up, the other down: here is only the latter, in your sculpture; the other is imperfect.

Underneath is the figure of a horse, denoting the sun's course: for in time, when the old patriarchal customs became profaned, and desecrated into idolatry, they made Mithras to be the Apollo, or the sun. Whence these sculptures had a number of symbols, relating to the solar circuit of the year, through the 12 zodiacal constellations.

The two figures attending on the archimagus, are inferior officers to him. There is a mystery in their standing cross-legged, like our effigies of croisaders in churches, and it means the same thing: for the cross was one part of the mithriac ceremonies. These two, by the different attitude of their torches, represent day and night, as Mithras represents the sun. The figure imperfectly drawn, at the tail of the horse, is probably a genius, twisted round with a snake; which means the vitality, imparted to all things, by the solar power and circle. The other figures are officiating priests, and drest in such a symbolic manner, as intimates the sun's influence, and annual motion.

Part of a Letter from James Mounsey, M. D. Physician to the Czarina's Army, concerning the Russia Castor, the Baths at Carlsbad, the Salt Mines near Cracow, and various other Notices. Dated Riga, July 1, 1749.

Concerning Russia castor, Dr. M. observes that it is not all from the same animal, some of it being the prostatæ, testes, and kidneys of the beaver, gathered in the spring; but the true sort comes from a quite different creature, which resembles a wild goat, just by whose navel the castor is found, like 2 glands. This he was assured by people who had seen it on the spot; but as they were quite unacquainted with natural history and anatomy, he would not trust to them too much: he hoped soon however to procure an account that might be depended on.*

He then proceeds to give some account of his travels in Bohemia, which he says is a fine fertile country, rich in metals and minerals of all sorts. The frontiers all round are very high mountains; the inner parts of the country are hilly, with plains and rising grounds intermixt, that have the appearance of being the remaining bases and ruins of former mountains, the soil being a composition of decayed rocks mixed with some vegetable earth. The rocks on the highest mountains are an aggregate stone of lapides calcarii, spati, quartzi, micæ, &c. The plains are covered with the least dissolvable parts of such rocks. Their finest crystals, and precious stones, are gathered behind the plough; many still retaining the same figures they had received at their formation in the veins and hollows of the rocks. He found on the tops of mountains decaying rocks, which, when mixed with a little vegetable earth, made exactly the same soil with that in the rising grounds and plains below.

There are several places in this kingdom where the mountains are wholly of lapis scissilis, which breaks into rhomboids; and he observed for many miles the shelves of this stone running through different mountains in the same direction, facing the south-east, with an inclination of the shelves of about 35°. The soil here in the plains is clayey.

Not far from the frontiers of Saxony, in the mountains, are the famous hot springs of Carlsbad, the tin mines of Schlachtenwald, and mines of Pyrites,

* The animal from which the true castor is obtained, is the castor fiber Linn. The account given to Dr. M. was a very erroneous one; for this animal bears no resemblance to "a wild goat." The odorous substance called castor is secreted in follicles situated near the anus.

where they prepare sulphur and vitriol. Carlsbad is a small town, situated in a hollow between 2 high mountains: a small river called Toeple runs through it from S. E. to N. W. The principal fountain rises on the north-east side, about 20 paces from the river, and about 5 or 6 feet higher than the surface of the water. This spring rises through a square tube of wood, of about 7 inches diameter, with a considerable degree of violence: whence it is called the sproudle, or furious fountain. It comes from the mountain on the other side, and passes underneath the river, where the petrifying quality of its own water has formed for itself an aqueduct of tophus, through which it is conducted to this place. Sometimes this aqueduct is so filled and choaked up with the tophus, that it bursts into the river, and puts the inhabitants to a considerable expence for repairing it. But to prevent this, they bore and clean it every year near the fountain. It forms rocks of tophus along the river side, composed of strata of several colours, according as the water has been impregnated with different matter, or perhaps from the difference of heat or cold, or the impressions of the air at the times of forming the lamellæ. This tophus is hard, and receives a good polish, and of it they make snuff boxes, heads of canes, and other toys.

On the other side of the river, at the foot of the mountain, are a good many houses, and a broad street; cross under which the stream runs, and in the winter no snow lies on the place where it passes. Some rooms in a house built here are always warm like a bagnio, and in one of the cellars may be heard the noise of the water running under ground. Along this side of the river are several hot springs, which differ in quality from one another, as well as from the water of the sproudle. The principal of these is called the mill-fountain, which is much used, and reckoned milder than the sproudle. It is not near so saturated with the limy matter, and forms scarcely any tophus. The sproudle is so full of the stony matter, that any thing laid into it is covered over with a thick tophus in a few days. When the water is taken up, and let stand a little in the air, it incrusts the vessels that contain it, and its surface is covered with a scale, like lime water, which is made use of as a dentifrice.

Most of the rocks about Carlsbad are an aggregate of spatum, mica, quartzum, rubrica, cum matrice lapidis calcarei, and cleave into rhomboids. The soil on the side of the mountain is made by the dissolution of such rocks intermixed with some vegetable earth; and the whole surface is covered with the least dissolvable parts, often adhering together in masses by the intervention of a limy matter like incrusted spatum, and he found higher up the mountain some rocks mouldering into such soil. The Carlsbad waters give a good deal of neutral salt by boiling and crystallizing. From 1080 lb. of water 22 oz. of pure salt. His thermometer being broken, he procured one of a friend: but not knowing of what construction it was, he tried it in the following manner: in melting ice the mercury fell to $28\frac{1}{2}$ of its equal parts, and by the heat of his body it rose to 66 of those parts. This thermometer held into the sproudle fountain rose by its head to 96, and in the mill-fountain to 67.

About 20 miles from Carlsbad to the south-west near the town of Egra, is a cold spring of mineral waters, much in use in these countries. This gives also a salt much of the same kind. To the south from Carlsbad about 25 English miles are likewise several cold springs: one of which is much richer in this same kind of salt than the former. It belongs to the monastery of Toeple. In the winter, when they boil this water, from 10 lb. of water they get sometimes above 1 oz. of salt. They prepare here a neutral salt by adding a mineral acid, or perhaps some other neutral salt, (but the preparation they keep a secret) which makes it shoot into beautiful crystals. It is called sal medium toeplicense, and is sold in many places of Germany. On exposing these salts some time to the air, they fall into a magnesia, but dissolving and crystallizing them again recovers them; though the oftener they are dissolved, the crystals shoot the smaller.

About 7 miles south-west from Carlsbad, at Altsettle, are mines of black schistus, and formerly they made a great deal of alum and vitriol from it; but it is now neglected, as they find in the same mines plenty of gleba pyriticosa, from which thev distil sulphur. Six hundred weight of this pyrites give one of sulphur: and the oven (furnace) makes from 1 to 2 cwt. per week. The residuum being thrown in great heaps in the open air, takes fire, and constantly smokes. This matter they throw into large reservoirs of water, which afterwards they let run off into the boiling-house, and so make copperas.

About 9 English miles to the south from Carlsbad, are the tin mines of Schlachtenwald. They reckon this mine has been wrought near 500 years. There are 5 entries, 4 of which are provided with machines for hoisting the barrels with the tin stone: the 5th is for drawing the water out of the mine. The number of miners who work below ground, are 90: each man delivers 25 barrels of this stone per week, and receives something less than half a crown wages. They have different inventions in the mine for splitting the rock, but the most effectual one is bursting it with gunpowder. The whole people employed in these mines are about 300. The main body of the mine is nearly 700 feet in diameter, and from this go several east and west; for so the mineral runs. The broadest of these ways is about 2 feet, and the mineral in these veins is richer than what is found in the main body of the work, whose greatest depth is 650 feet. The tin-stone is first burnt in kilns, which they say betters the tin considerably, and makes it much more easy to stamp. After this preparation it is brought to the stamp mills, where by stamping it becomes like grey river sand, which they wash, and separate the tin from it in the following manner. They throw it by shovels-full into basins where there passes a current of water, and by keeping it stirring, it runs over by a broad conduit descending by steps, which are covered with coarse linen cloth; and by this operation the sand is washed away, and the tin remains on the cloth in form of a black scaly powder, and dried is fit for melting. One hundred weight of the stone gives only 3 oz. of tin; and 150 lb. of the clean washed tin mineral give 140 lb of tin. There are 10 melting ovens, (furnaces,) each of which can melt 9 or 10 cwt. in 24 hours; the breadth of these ovens (furnaces) within side is 8 or 9 inches, and from 10 to 12 feet long, blown by 2 pair of bellows. The proportion of charcoal to the metal is nearly an equal weight. They are thrown into the oven by degrees alternately: the residuum they melt 3 times over, which always yields new metal. They make here about 800 centers per annum, which is sold from 53 to 56 Imperial gouldens per center. They find sometimes the black and sometimes the white crystal mineral in nests, or clusters: the stannum polyhedron nigrum is a very pure and rich tin ore: they say the white is rich also, but so hard and difficult to melt, that the tin is burnt to an ash before it can be brought to fusion.

Near Geffries, in Bareith, they boil vitriol. The mineral from which they make it, is a black schistus, but some of it is brown. It has several small veins of pyrites in it. When first taken out of the pits it has no taste; but after it has been exposed some time to the weather, and begins to moulder, it acquires a very sharp taste. It is laid in great heaps, under which there are cisterns for receiving the water that runs from it after rain, or that they pump on it when the weather is dry. This water is conveyed by conduits into the boiling house, where there are 2 leaden kettles, in which it is boiled to a strong lie, and then let off into receivers where it shoots. These 2 kettles make from 8 to 9 cwt. per week, which is all wrought by 2 servants: it not having been found necessary to add any new mineral to the heaps these 15 years past. But as the quantity of the mineral consumed in that time, is not known, it is impossible to determine how much of this salt has been supplied by the air. They only add to the quantity $\frac{1}{4}$ cwt. of iron, which is consumed in the kettles every week, and makes it shoot into copperas: but instead of this, if they add copper, it makes blue vitriol. Formerly they made alum here likewise from the same lie, only instead of iron or copper, they added potash and urine: but the expence of the first, and the difficulty of getting the other in sufficient quantity, has made them leave off making alum here for some years past. [Then follows an account of the salt mines near Cracow; but as descriptions of these celebrated mines are given in numerous books of travels and geography, it was deemed unnecessary to reprint it in this collection.]

The Case of a Lady, who was delivered of a Child, on whom the Small Pox appeared in a Day or 2 after its Birth; drawn up by Cromwell Mortimer, M.D. Secr. R.S. N° 493, p. 233.

This gentlewoman had never in her own knowledge or that of her relations, had the small pox. In Feb. 1700-1, she was within about a fortnight or 3 weeks of her full reckoning, when the following accident happened. A poor widow woman in the neighbourhood was seized with the small pox, and had nobody to assist or nurse her; the country people, as much afraid of this distemper as of the plague, would neither send her necessaries, nor suffer her to come to their shops to buy: in this extremity she made shift to get to this lady's house, who was noted for her goodness to the poor, especially for giving them medicines when sick: her business was to entreat the lady to desire her husband to use his authority with the overseers of the poor to appoint a proper nurse to attend her; for that otherwise she must certainly perish for want of necessaries; for even the parish officers would not go near her. She expressed a very earnest desire to speak to the lady herself, who consented to go to a window, and spoke to her across a court-yard at 30 or 40 feet distance, thinking herself safe from infection in that situation. She looked upon her without any surprize, but thought the sight very disagreeable, the woman having her face and arms full of a large distinct sort, in the state of maturation. About a fortnight after, viz. Feb. 25, 1700-1, the lady was brought to bed of a fine boy: in a day or 2 there appeared an eruption all over his skin, which was at first taken by the nurse for the red gum, though the appearance was earlier than that disorder usually attacks children; but in a day or 2 more it showed itself to be the confluent small pox. The child was immediately removed from his mother; but the distemper proved to be of the very worst sort, so that the child died before the turn: the mother took no infection, and lived to the year 1736, without ever having the small pox.

The above account Dr. M. took down in writing from a daughter of the gentlewoman. Indeed many years before he had heard the lady herself mention the accident; but he did not commit it to writing; but he thought it was with this difference that she was surprized, and that the child was born with the small pox upon it, in the eruptive state.

Some Accounts of the Fœtus in Utero being Differently Affected by the Small Pox. By W. Watson, F.R.S. N° 493, p. 235.

That the human species should only once in their lives be liable to the small pox, has long been observed with surprize, both by physicians and philosophers: nor is it less extraordinary, that the child before birth, which in every circumstance is equally supported by its receiving and circulating its mother's fluids, should be so differently affected by that distemper.

From the dissections of those who have died of the small pox, we find that the viscera are subject to the variolous abscesses as well as the skin; but that the fœtus does not always partake of the infection from its mother, or the mother from the fœtus, is the subject of this paper.

About 4 years before Mr. W. attended a young man, a servant to a carpenter, who had a very putrid and offensive kind of small pox; of which however he recovered. His mistress, during his illness, came frequently into his room, and sometimes continued there a considerable time. She was then about 7 months gone with child, but had had the small pox herself many years before. At the usual time she was delivered of a girl, whom Mr. W. saw very soon after its birth; and there appeared very plainly the marks of about 40 pustules, in different parts of her body. From this appearance he then informed the parents, that he apprehended the child would hereafter be very secure from the infection: but as about a month ago the parents thought proper to have a little boy of theirs inoculated, he requested that they would permit the before-mentioned girl to be inoculated also. As he desired, they were both inoculated, from a child of his own, who had, from inoculation, had a favourable kind. On the 10th day after the operation the boy sickened, and had the small pox, very favourably: about the same time the girl grew pale, and lost her appetite. This indisposition continued for 2 or 3 days, and then she recovered.

In both these children, the incisions, which were made only in one of their arms, were extremely superficial, and inflamed in both as usual: that in the boy produced the variolous fever and its attendants, as is before-mentioned; but in the girl occasioned only a paleness and loss of appetite without a fever, and one variolous abscess in one part of the incision, such as is sometimes seen in nurses, and in those who have attended persons in the small pox, who have had it themselves before. This one pustule was a sufficient argument of the variolous matter taking place, and endeavouring to excite the usual symptoms.

Dr. Mead, in his learned Treatise concerning the Small Pox, takes notice of a woman's attending her husband, who, a short time before she expected her delivery, was ill of the small pox. As she had undergone the distemper herself a considerable time before, she felt no inconvenience from it; but on her delivery the child was found dead, and its body covered with the small pox.

These 2 histories evince, that the child before birth, though closely defended from the external air, and enveloped by fluids and membranes of its own, is not secure from the variolous infection, though its mother has had the distemper before. They demonstrate the very great subtilty of the variolous effluvia; as we find them capable either from their floating in the air, and by their being taken in by the inspiration of the mother, or by penetrating the absorbent vessels on her skin, and thus mixing with her blood, of exerting their effects on the child: and it may be observed further from the first of these cases, that it is possible for the child to live through the small pox before its birth; as well as that after that period under the before-mentioned circumstances it is not liable to the infection again.

The following history is equally remarkable with the preceding. A lady of high birth and quality, well known to several members of the R. S. had the small pox to a great degree when 7 months gone with child; notwithstanding which, she went her time and was delivered of a son, who did not appear to have on his body any marks of the distemper. As this lady had been severely handled by the small pox, it was judged that her child would never after be liable to it; but when about 4 or 5 years old, he was attacked with the distemper, but got very well through it, and was then alive.

A case in some respects resembling this last, is taken notice of by Mauriceau, (Maladies des Femmes Grosses, Case 576,) who delivered a woman of a healthy child at her full time, who, during the 5th month of her gestation had had the small pox to a great degree; though the child, from any marks of its body, did not appear to have been affected with the distemper.

These cases are the very reverse of the former; where though from inoculation the most minute portion of lint, moistened with the variolous matter and applied to the slightly wounded skin, is generally sufficient to propagate this distemper; yet here we see that the whole mass of the mother's blood, circulating during the distemper through the child, was not sufficient to produce it.

It generally happens, as we are informed by medical writers, and as Mr. W. had himself seen in practice, that if women are delivered during the course of the small pox, the distemper of the child does not keep pace with that of its mother, but is subsequent to it. Thus the child of the lady, mentioned by Dr. Mead, (Tract. de Variolis, pag. 66,) who was brought to bed on the 11th day, when labouring under a very malignant small pox, was born without any appearance of the pustules; but on the 4th day after its birth, the child was seized with convulsions, and died at the eruption of the small pox. And in a woman, whom Mr. W. attended, and who was delivered of an apparently healthy child on the 9th day of a distinct small pox, the child was not seized till the 8th day after its birth, which is about the time that the infection would have taken place, if it had been received from any other quarter, independent of its mother's having the distemper before its birth.

From these histories it appears, that the child before its birth ought to be considered as a separate, a distinct organization; and that, though wholly nourished by its mother's fluids, with regard to the small pox, it is liable to be affected in a very different manner, and at a very different time, from its mother.

The Case of Nicolas Reeks, who was born with his Feet Turned Inwards, which came to Rights after being some Time used to sit Cross-legged. By Wm. Milner, Esq. at Poole. N° 493, p. 239.

Nicolas Reeks was born in the town of Poole, 1724, with both his feet turned inwards. His mother carried him to a surgeon, who on examination gave it as his opinion that he was incurable. The boy, as he grew up, was with great difficulty able to walk, but always on the outward edge of his feet and heels, so that he frequently fell down in walking, one foot striking against the other.

His parents being poor, in 1735 the parish put him apprentice to Mr. Richard Mocket, of the same town, taylor, apprehending it the only trade he could be fit for as a cripple. His shoes were made in a peculiar manner to lace on to his legs, the muscles of which were much smaller than those of boys of his age. He lived with his master, and worked at the trade, till about 1741, when they began to perceive a manifest alteration and turn in both his feet; which was brought on without the assistance of any manner of art, or application of plasters, oils, or bandages, till both feet were turned to their right and natural situation. He was able to wear his master's shoes, the muscles of his legs grew larger, his feet and legs like other peoples of his age; if any difference, they turned outwards more than others do. In March 1742-3, he ran away from his master, entered on board a ship of war as a marine, and was living at Portsmouth, in Nov. 1749.

The Description and Uses of an Equatorial Telescope. By Mr. James Short, F.R.S. N° 493, p. 241.

This instrument, fig. 8, pl. 12, consists of two circular planes or plates, marked AA, supported on 4 pillars: and these again supported on a cross-foot, or pedestal, moveable at each end by the 4 screws BBBB: the 2 circular plates AA are moveable, the one above the other, and are called the horizontal plates, as representing the horizon of the place; and on the upper one are placed two spirit levels, to render them at all times horizontal; these levels are fixed at right angles to each other; this upper plate is moved by a handle C, called the horizontal handle, divided into 360°, and has a nonius index divided into every 3 minutes.

Above this horizontal plate is a semicircle DD, divided into twice 90°, called the meridian semicircle, as representing the meridian of the place, and is moved by a handle E, called the meridian handle, and has a nonius index divided into every 3 minutes.

Above this meridian semicircle is fastened a circular plate, on which are affixed 2 other circular plates FF, moveable on each other, and called the equatorial plates; one of them, representing the plane of the equator, is divided into twice 12 hours, and these are subdivided into every 10 minutes of time. This plate is moved by a handle G, called the equatorial handle, and has a nonius index for showing every minute.

Above this equatorial plate is a semicircle HH, called the declination semicircle, as representing the half of a circle of declination, or horary circle, and is divided into twice 90°, being moved by the handle K, called the declination handle. It has also a nonius index for subdividing into every 3 minutes.

Above this declination semicircle is fastened a reflecting telescope LL, of the Gregorian construction, the focal length of its great speculum being 18 inches.

To adjust the instrument for observation, the first thing is to make the horizontal plates level or horizontal, by means of the 2 spirit levels, and the 4 screws in the cross pedestal. This done, move the meridian semicircle, by means of the meridian handle, so as to raise the equatorial plates to the elevation of the equator of the place; which is equal to the complement of the latitude, and which, if not known, may likewise be found by this instrument, as shall be afterwards shown. And thus the instrument is ready for observation.

To find the Hour of the Day, and Meridian of the Place.—First find, from astronomical tables, the sun's declination for the day, and for that particular time of the day; then set the declination semicircle to the declination of the sun, taking particular notice whether it is north or south, and set the declination semicircle accordingly. Then turn about the horizontal handle, and the equatorial handle, both at the same time, till the sun be precisely concentrical with the field of the telescope. If there is a clock or watch at hand, mark that instant of time; and by looking on the equatorial plate, and nonius index, you will find the hour and minute of the day, which comparing with the time shown by the clock or watch, shows how much either of them differs from the sun. In this manner you find the hour of the day.

Now, in order to find the meridian of the place, and consequently to have a mark, by which you may always know the meridian again, first move the equatorial plate, by means of the equatorial handle, till the meridian of the plate, or hour line of 12, be in the middle of the nonius index; and then, by turning about the declination handle till the telescope comes down to the horizon, you observe the place or point which is then in the middle of the field of the telescope; and a supposed line drawn from the centre of this field to that point in the horizon, is the meridian line. The best time of the day for making this observation for finding the meridian, is about 3 hours before noon, or as much after noon. The meridian of the place may be found by this method so exact, that it will not differ at any time from the true meridian above 10^s of time; and if a proper allowance be made for the refraction at the time of observation, it may be found much more exact. This line thus found will be of use to save trouble afterwards; and is indeed the foundation of all astronomical observations.

To find a Star or Planet in the Day-time, even at Noon-day.—The instrument remaining as rectified in the last experiment, set the declination semicircle to the declination of the star or planet to be seen; and then set the equatorial plate to the right ascension of the star or planet at that time; and, looking through the telescope, the star or planet will be seen; and after having once got it into the field, you cannot lose it; for, as the diurnal motion of a star is parallel to the equator, by moving the equatorial handle so as to follow it, you will at any time, while it is above the horizon, recover it, if it be gone out of the field.

The easiest method for seeing a star or planet in the day-time is this: the instrument being adjusted as before-directed, bring the telescope down so as to look directly at the meridian mark; and then set it to the declination, and right ascension, as before-mentioned.

By this instrument, most of the stars of the first and second magnitude have been seen even at mid-day, and the sun shining bright; as also Mercury, Venus, and Jupiter; Saturn and Mars are not so easy to be seen, on account of the faintness of their light, except when the sun is but a few hours above the horizon.

And in the same manner in the night-time, when you can see a star, planet, or any new phenomenon, such as a comet, you may find its declination and right ascension immediately, by turning about the equatorial handle, and declination handle, till you see the star, planet, or phenomenon: and then, looking on the equatorial plate, you find its right ascension in time; and you find, on the declination semicircle, its declination in degrees and minutes.

In order to have the other uses of this instrument, make the equatorial plates become parallel to the horizontal plates; and then this instrument becomes an equal altitude instrument, a transit instrument, a theodolite, a quadrant, an azimuth instrument, and a level. The manner of applying it to these different purposes is too obvious to need any explanation.

As there is also a box with a magnetic needle fastened in the lower plate of this instrument, by it you may adjust the instrument nearly in the meridian; and by it also you may find the variation of the needle; if you set the horizontal meridian, and the equatorial meridian, in the middle of their nonius indexes, and direct the telescope to the meridian mark, observe how many degrees from the meridian of the box the needle points at; and this distance or difference is the variation of the needle.

An Eclipse of the Moon, observed at Mr. Graham's in Fleet-street. By John Bevis, M.D. and Mr. James Short, F.R.S. N° 493, p. 247.

1749.	By the clock.			App. time.			
Dec. 11,	23^h	56^m	$15\frac{1}{4}^s$				The sun passed the meridian.
12,	6	43	0	6^h	46^m	36^s	A sensible penumbra.
	6	47	20	6	50	56.	. Eclipse begins.
	9	9	5	9	12	38.	. Eclipse ends.
	9	13	30	9	17	3.	. Penumbra gone.
	12	5	$53\frac{1}{2}$				.. Moon's centre passed the meridian.
	12	20	2				.. Sirius passed, his mean right ascension being 98° 31′ 38″.
13,	23	56	46				.. The sun passed the meridian.

A computation by Dr. Halley's tables gave the beginning 6^h 52^m 0^s app. time.
And the end.................................... 9 14 58

Of an Extraordinary Meteor seen in the County of Rutland, which resembled a Water Spout. By Tho. Barker, Esq. N° 493, p. 248.

Sept. 15, 1749, a remarkable meteor was seen in Rutland, being much like the account of two spouts seen at Hatfield in Yorkshire; Phil. Trans. N° 281, and N° 284. It was a calm, warm, and cloudy day, with some gleams and showers; the barometer low and falling, and the wind south, and small. The spout came between 5 and 6 in the evening; at 8 came a thunder shower, and storm of wind, which did mischief in some places; and then it cleared up with a brisk N. W. wind.

A great smoke rose over or near Gretton, in Northamptonshire, with the likeness of fire, either one single flash, or several bright arrows darting down to the ground, and repeated for some time. Yet some who saw it did not think there was really any fire in it, but that the bright breaks in a black cloud looked like it. Coming down the hill, it took up water from the river Welland, and passing over Seaton field, carried away several shocks of stubble, and crossing Glaiston, and Morcot lordships, at Pilton town's end, tore off two branches and carried one of them a good way. In a hedge-row in the meadow, at right angles to the spout's course, stood an oak and an ash 15 yards asunder; the oak a young sound one, 16 inches thick, it split 2 yards down, and one half fell to the ground, but was not quite parted from the other; the ash, about 8 inches thick, was torn off in the middle, and carried 10 or 12 yards.

*An Inquiry into the Original State and Properties of Spar, and Sparry Productions, particularly the Spars, or Crystals found in the Cornish Mines, called Cornish Diamonds. By the Rev. Mr. Wm. Borlace.** N° 493, p. 250.

In the present advanced state of mineralogy, this paper is not deemed sufficiently interesting for republication.

Of the Great Black Wasp,† from Pensylvania. By Mr. John Bartram. N° 493, p. 278.

You will see by the specimen, fig. 9, pl. 12. the size of this great black wasp, it supplies itself with food, by roving about the meadows, catching grashoppers, and other insects, on these it feeds, and not on fruits, as other wasps do. But what is more remarkable, is the method of making their nests, and providing for their young. With great pains and industry they scratch a horizontal hole, near an inch diameter, and a foot long, in the steep side of a bank of loamy earth; then away the wasp flies, and catches a large green grashopper, and lodges it in the farther end of her nest; she then lays an egg, and goes and catches 2 more, and deposits them with the other, then plasters up the hole. The egg soon produces a maggot. These grashoppers, by marvellous instinct, are provided for its food, till it changes into its nymph state, in which it lies for a certain period, and then eats its way out, and flies away, seeking its mate.

But what may deserve further attention, is the wonderful sagacity of this creature, not only in catching these large grashoppers, which are very like ours, and are very strong and nimble, as most may have observed that take them up; but their peculiar skill is to be admired in disabling them, either by bite or sting, so as not to kill them; for then they would soon putrefy, and be unfit for nourishment. Life sufficient is left to preserve them for the time the maggot is to feed on them.

An Observation of an Eclipse of the Moon, Dec. 12, 1749, made at Earith, near St. Ives, in Huntingdonshire. By Mr. Wm. Elstobb, Jun. N° 493, p. 280.

At 7^h 0^m the shadow or eclipse began.
9 16.. the same ended.

A Catalogue of the Immersions and Emersions of the Satellites of Jupiter, for the Year 1751. Computed to the Meridian of London from the Flamsteedian Tables. Corrected by James Hodgson, F.R.S. &c. N° 493, p. 282.

Omitted as useless now for the same reason as formerly given.

* Rector of St. Just, F.R.S. and author of Antiquities and Natural History of Cornwall. He also wrote Observations on the Scilly Islands. He possessed a good collection of minerals, and died in 1772, aged 76.

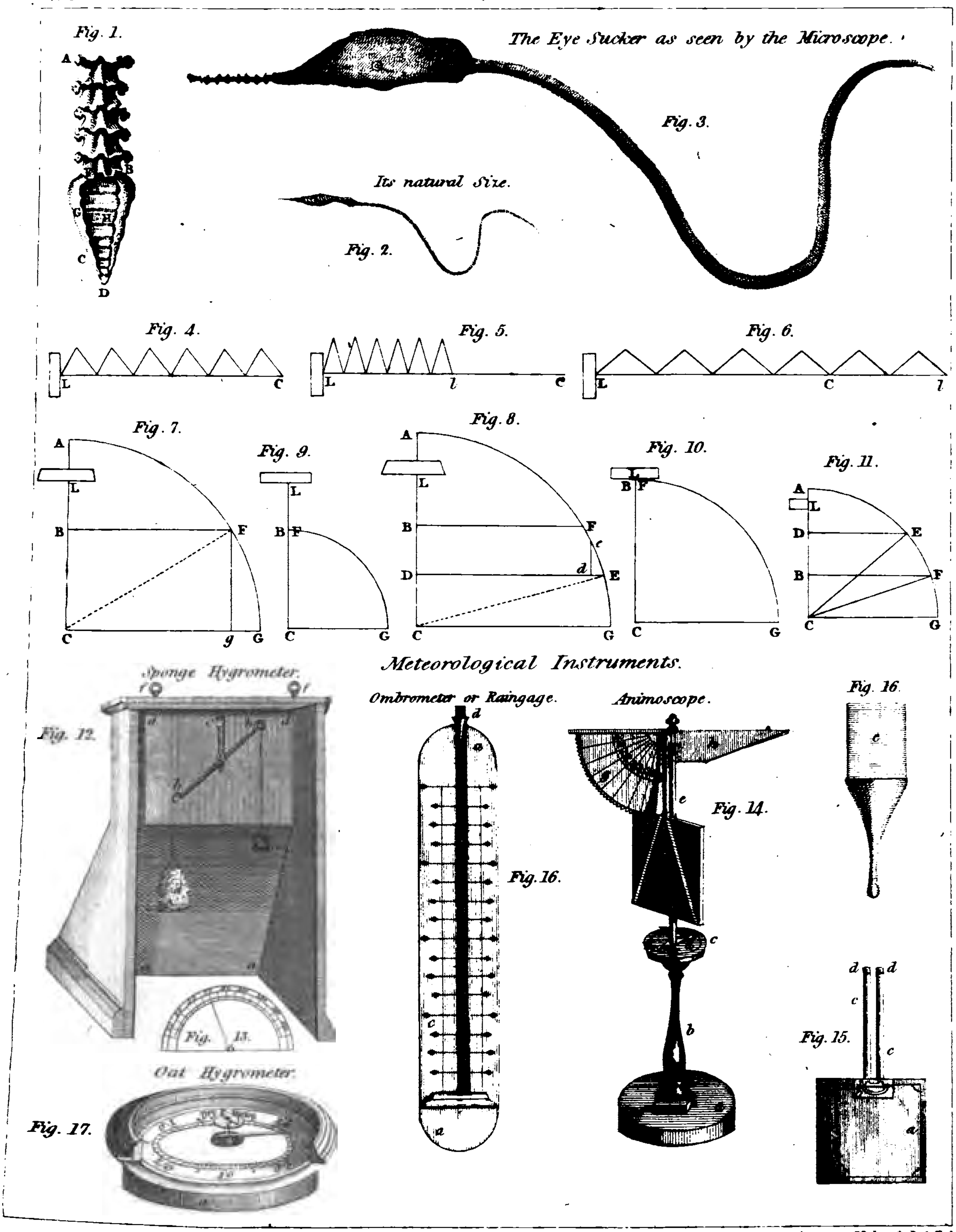
† This insect belongs to the Linnean genus sphex.

A Letter from the Widow of the late Mr. John Senex, F.R.S. to Martin Folkes, Esq. P.R.S. concerning the large Globes prepared by her late Husband, and now sold by herself, at her House in Fleet-street. N° 493, p. 290.

This is a letter merely to request the good offices of the president and Royal Society, in recommending and encouraging the sale of Senex's globes, in preference to those of foreigners.

END OF VOLUME NINTH.

C. and R. Baldwin, Printers,
New Bridge-street, London.

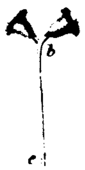


Mutlow Sc. Russell Ct.

Published by C & R. Baldwin, of New Bridge Street, London, 1806.

Fig. 1.

Fig. 2.



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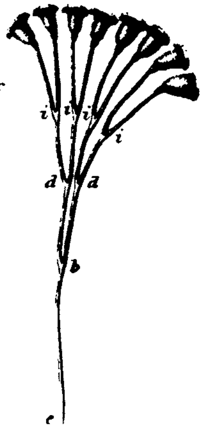

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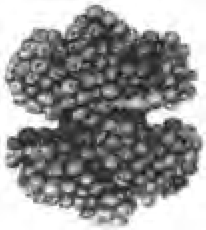

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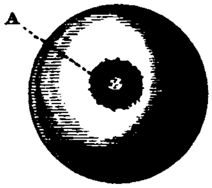

Fig. 7.

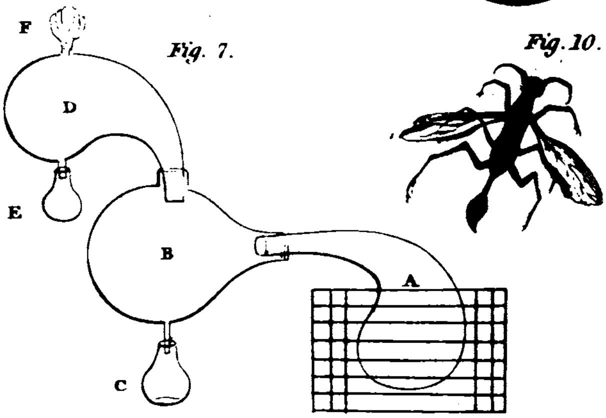

Fig. 10.

Fig. 8.

Fig. 12.

Fig. 9.

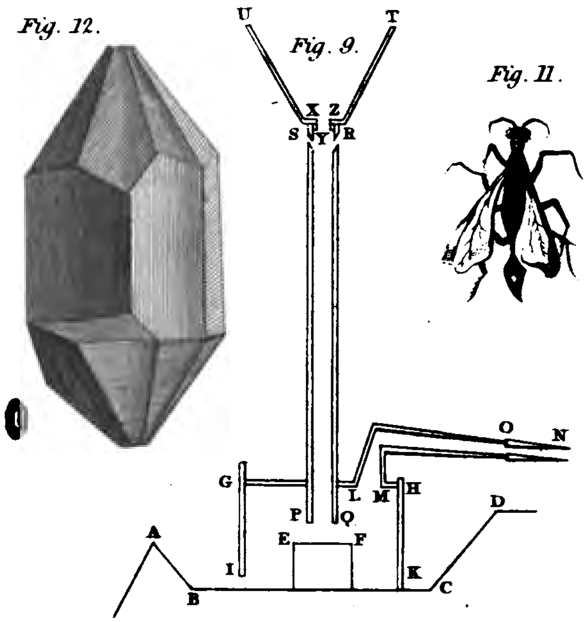

Fig. 11.

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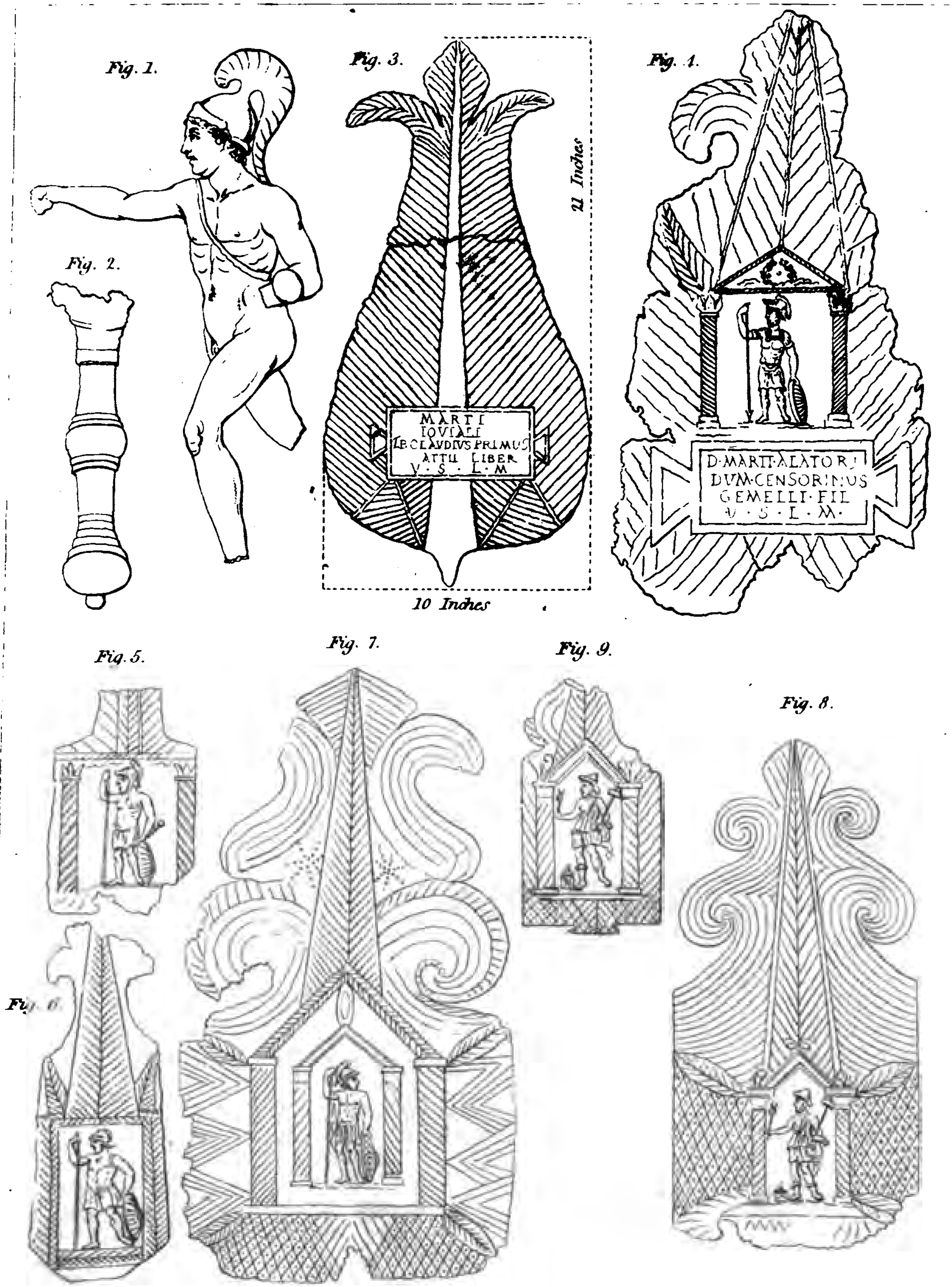

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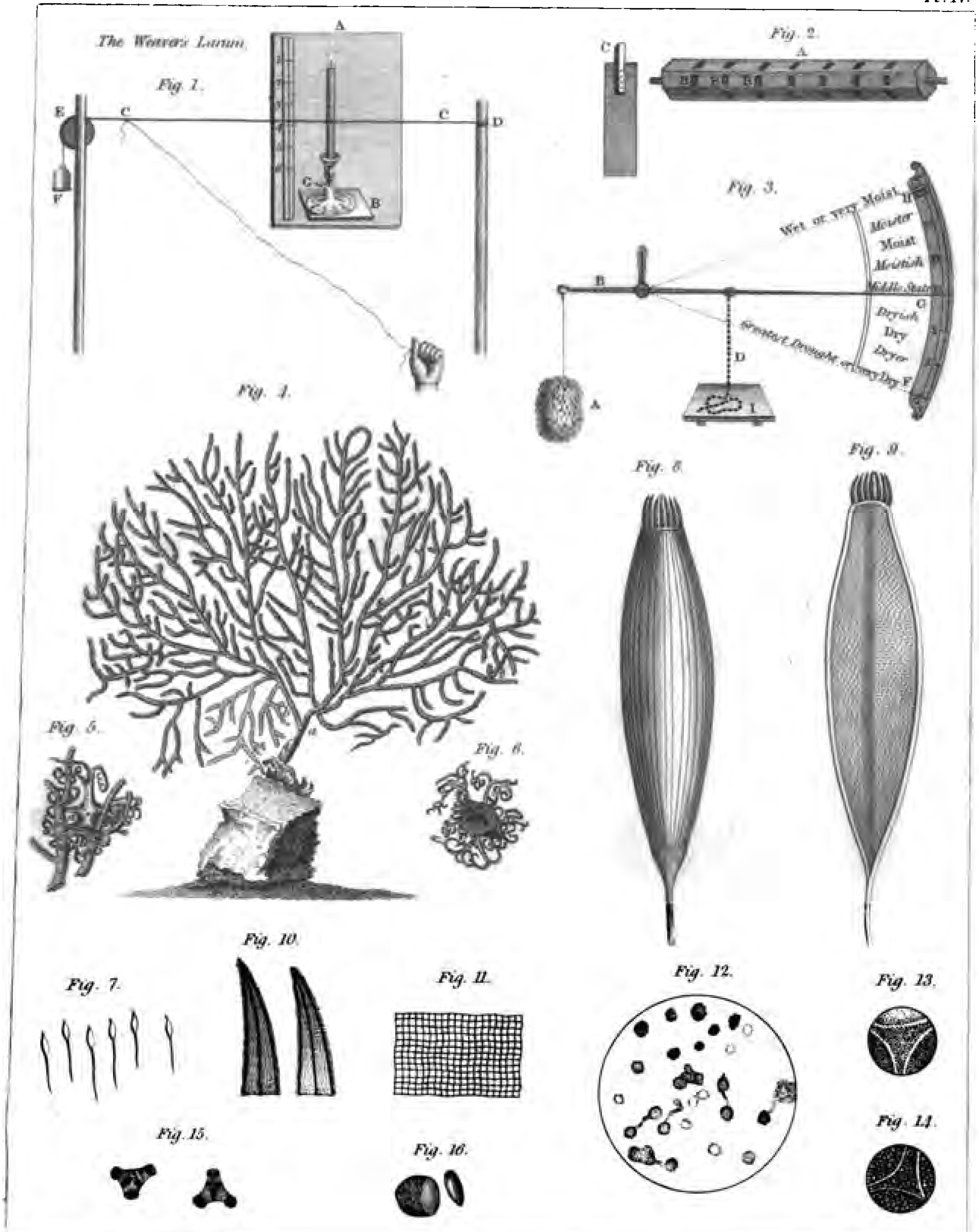

Mutlow Sc. Russell Ct.

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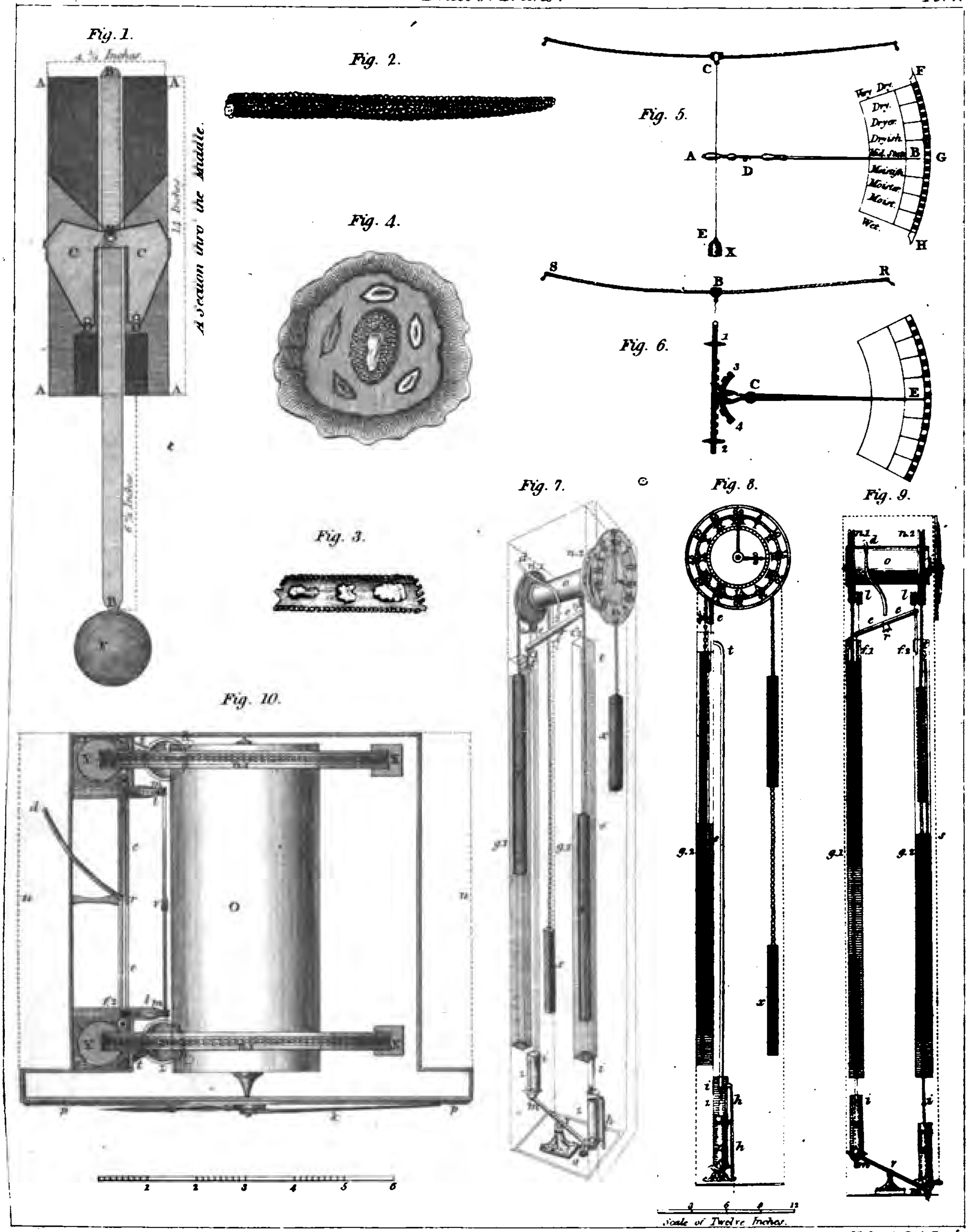


Mutlow sc. Russell del.

Published by C. & R. Baldwin, of New Bridge Street, London, 1806.

Fig. 2.

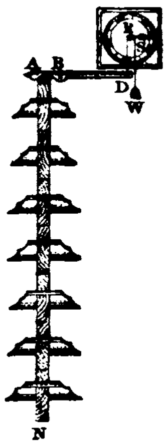


Fig. 1.

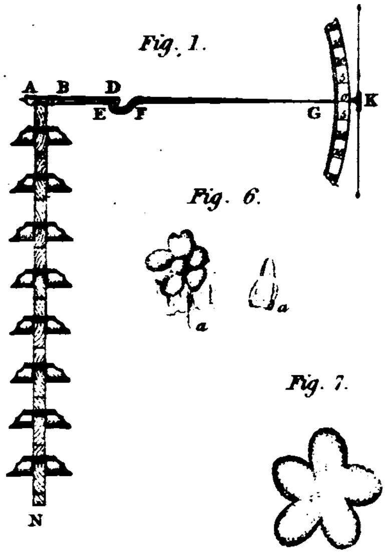


Fig. 3.

Fig. 4.

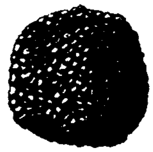
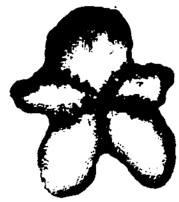


Fig. 5.

Fig. 6.

Fig. 7.

Fig. 8.



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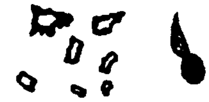

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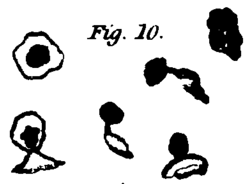


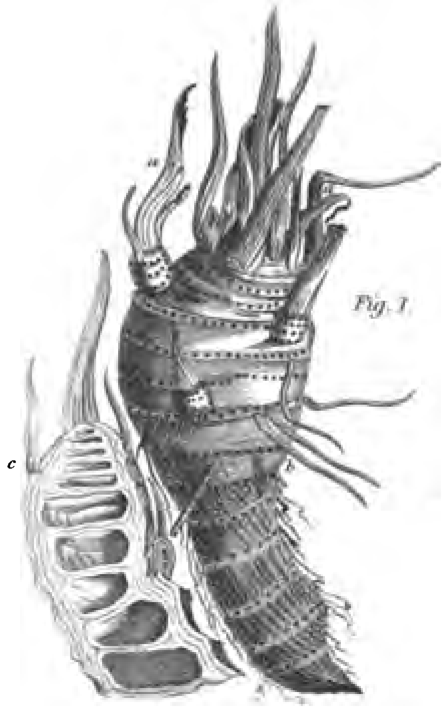
Oenanthe, Cicutæ facie, succo viroso crocante.

Fig. 11.

Cicuta Aquatica Wepf. Sium alterum Olusatri facie.

Fig. 12.

The Root of the Acuta Aquatica in Winter.



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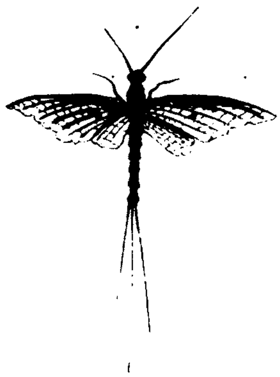

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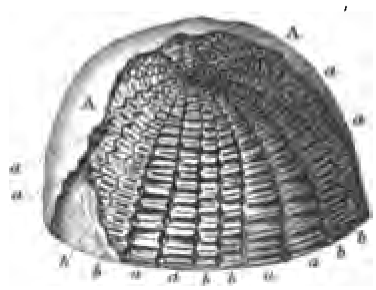
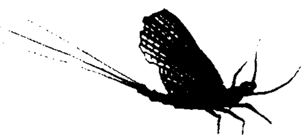


Fig. 4.

Fig. 3.



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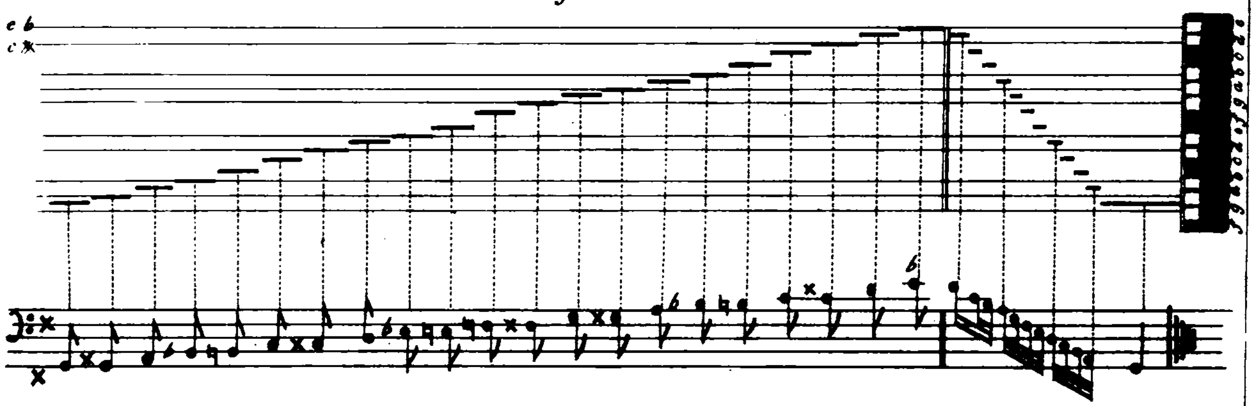

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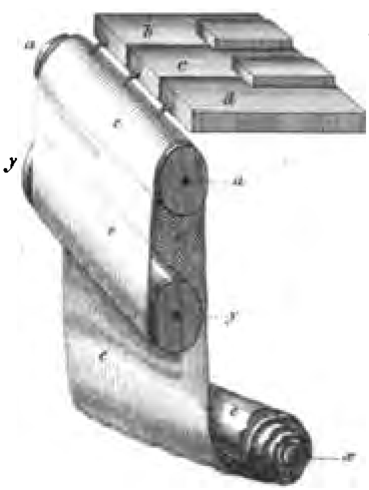

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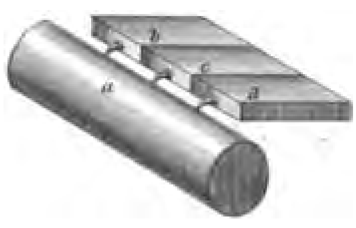

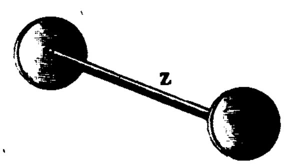

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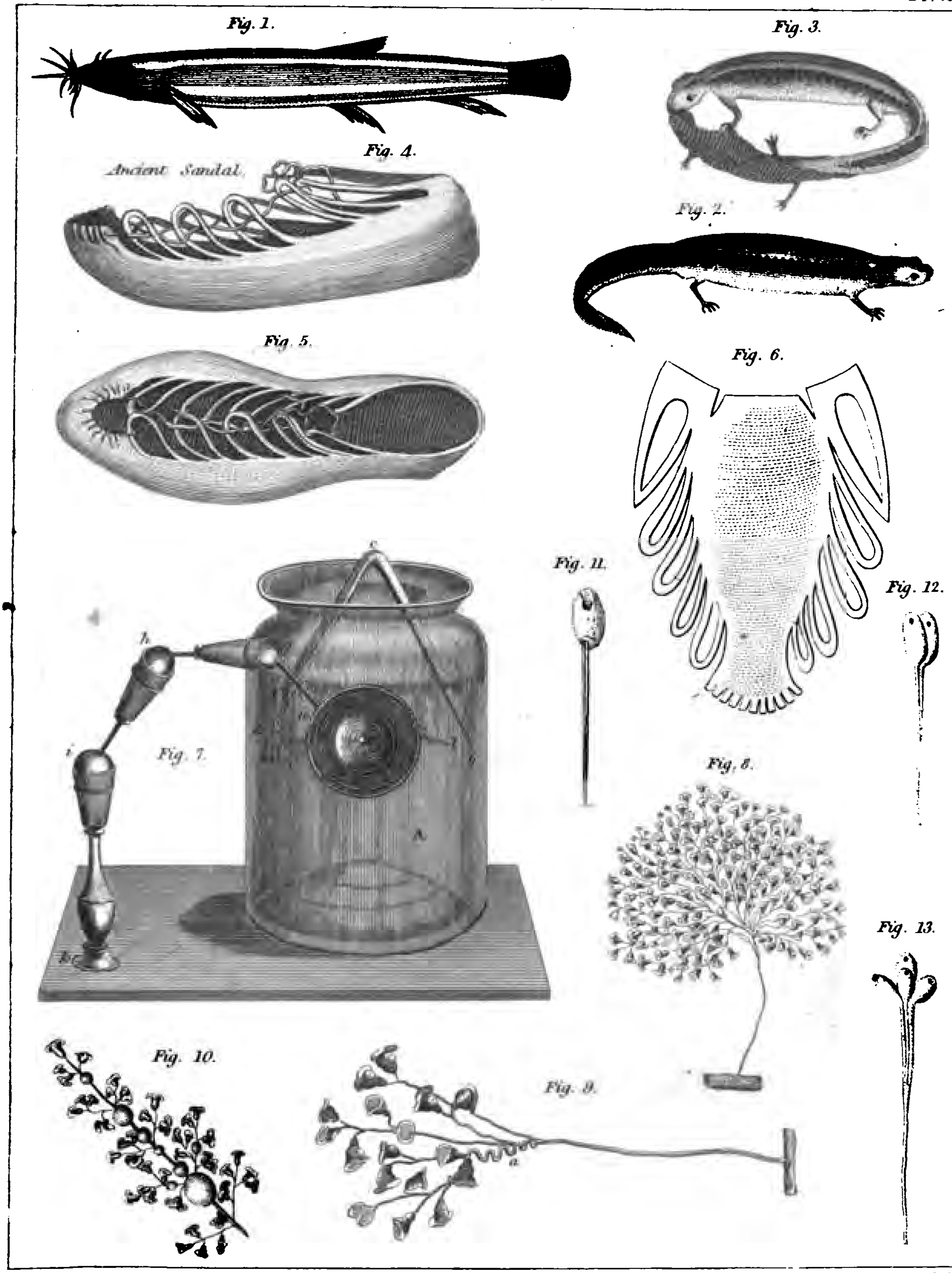

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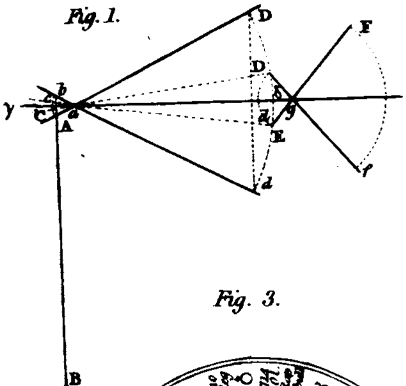
Fig. 1.



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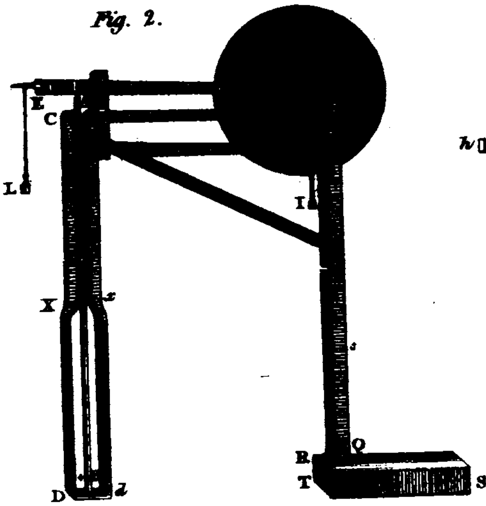

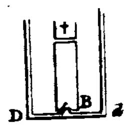

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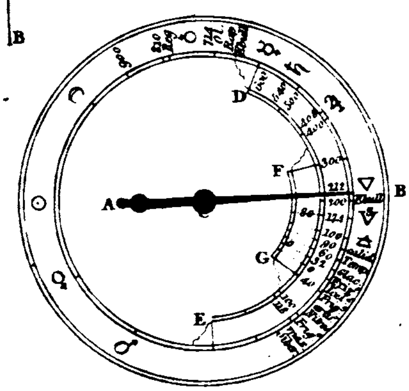

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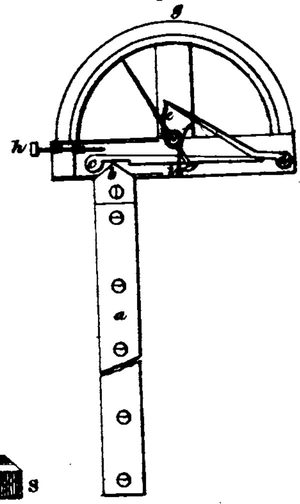

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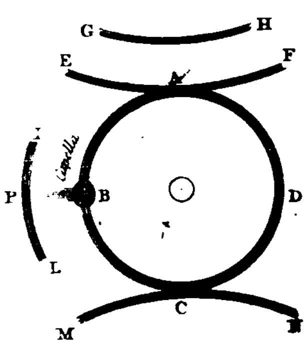
Fig. 6.



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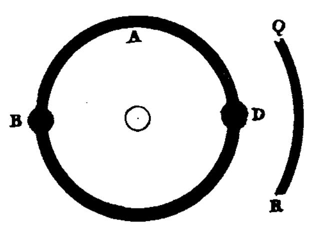

Fig. 8.

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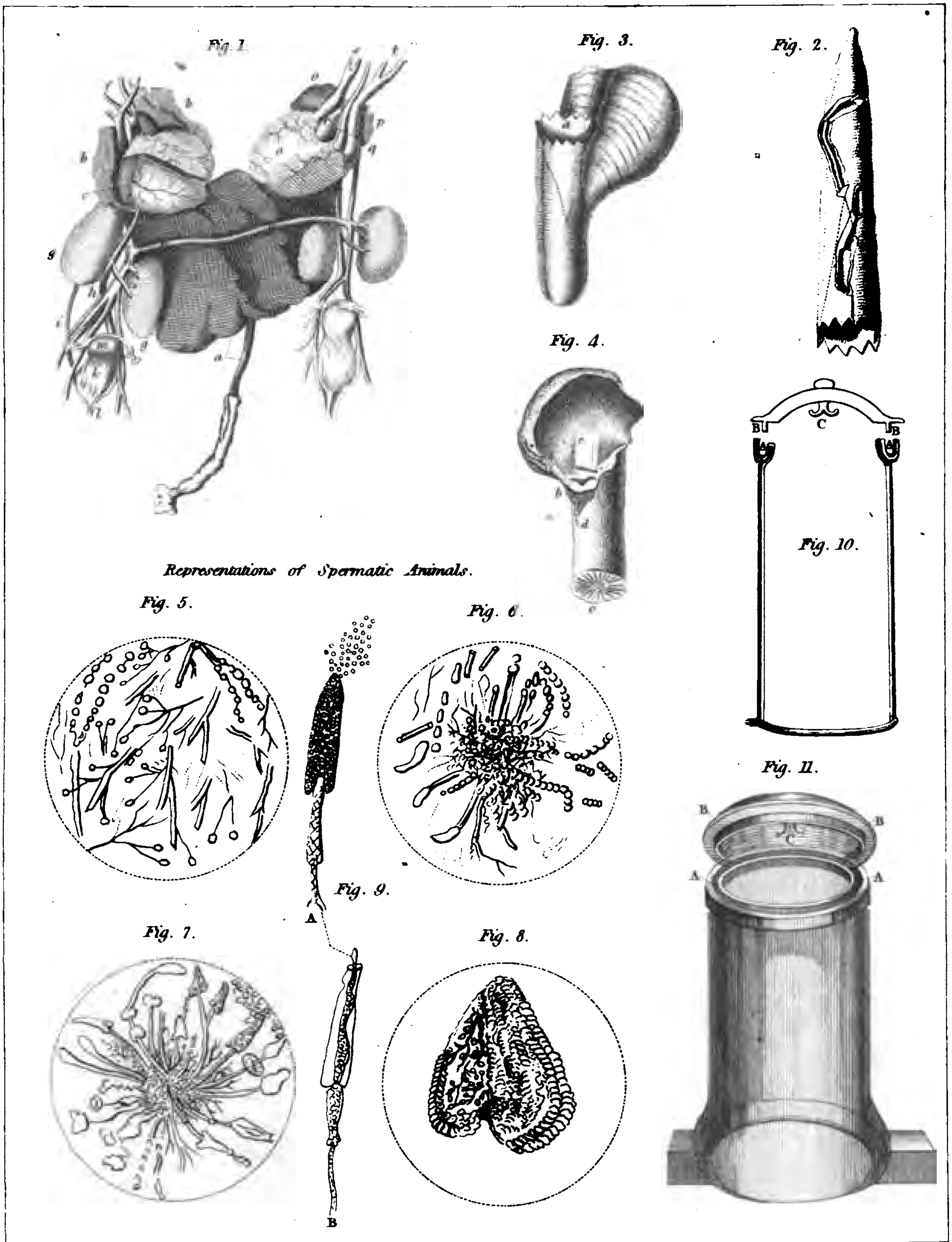

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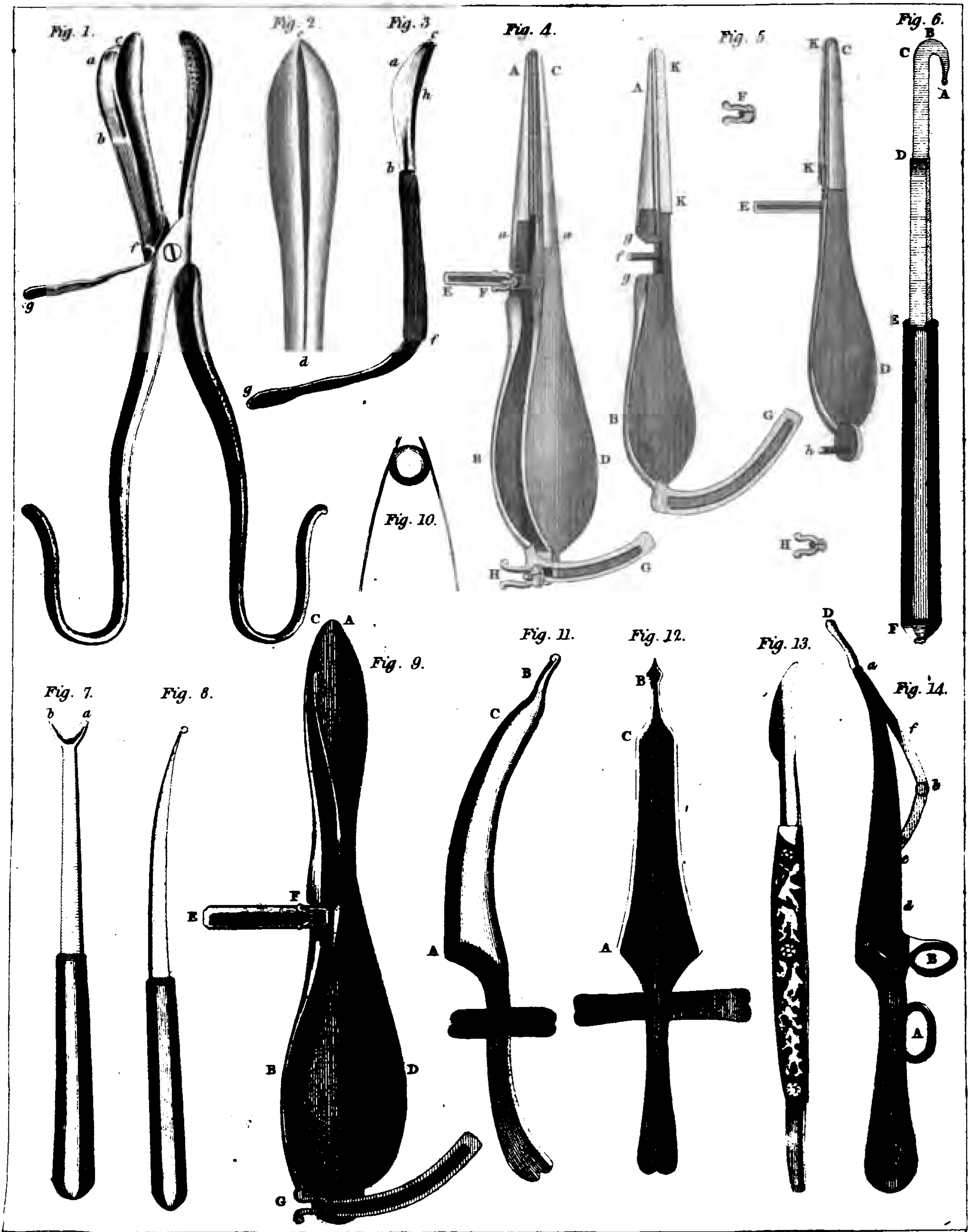

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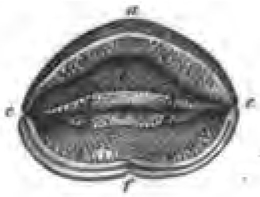
Fig. 3.

Fig. 1.

Fig. 2.

Fig. 6.



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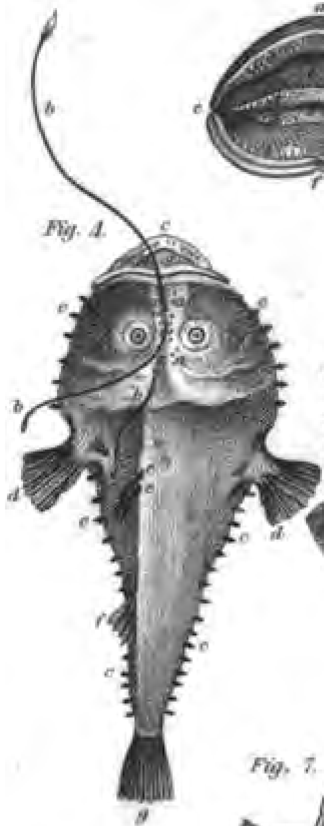

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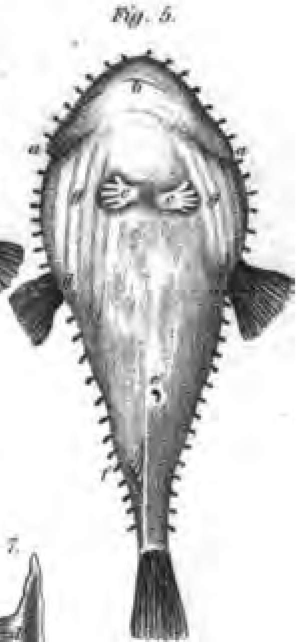

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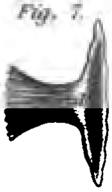

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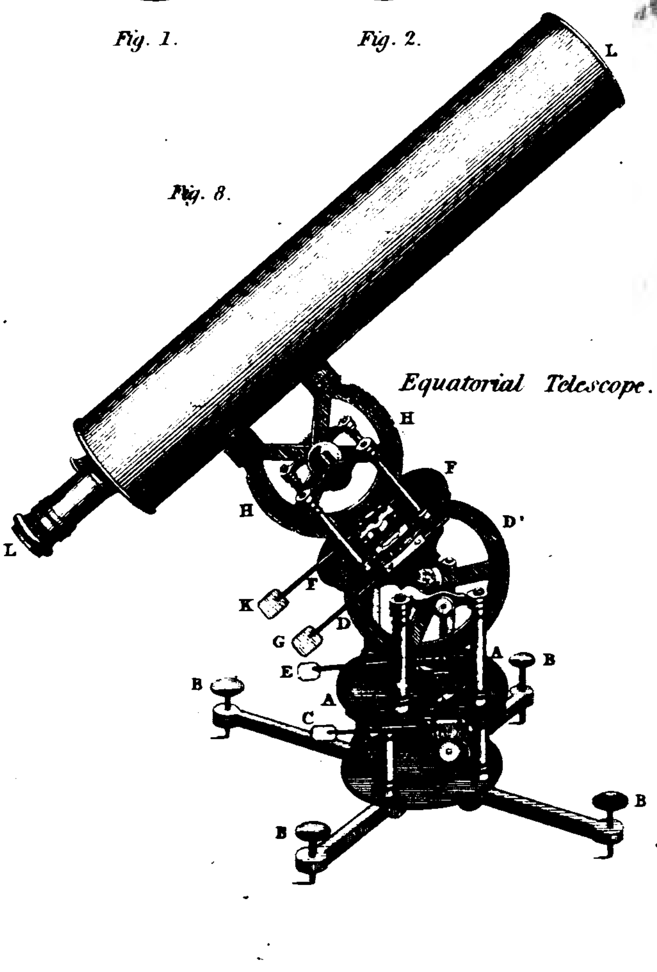

Equatorial Telescope.

Fig. 9.

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