THE

JOURNAL

OF

THE ROYAL INSTITUTION

OF

GREAT BRITAIN.

VOLUME THE FIRST.

OCTOBER, 1830; FEBRUARY, AND MAY, 1831.

LONDON:

JOHN MURRAY, ALBEMARLE-STREET.

MDCCCXXXI.

LONDON:
Printed by WILLIAM CLOWES,
Stamford-street.

CONTENTS.

Page

On certain Phenomena resulting from the Action of Mercury upon different Metals. By J. F. DANIELL, F.R.S. and M.R.I. . 1

On the means of giving a Fine Edge to Razors, Lancets, and other cutting Instruments. By THOMAS A. KNIGHT, Esq., F.R.S., Pres. of the Hor. Soc., &c. 13

On the Peculiar Habits of Cleanliness in some Animals, and particularly the Grub of the Glow-worm. By J. RENNIE, A.M., and F.L.S. 15

Description and Application of a Torsion Galvanometer. By WILLIAM RITCHIE, A.M., F.R.S., Ass. Mem. S.A. for Scotland, Rector of the Royal Acad. of Tain 29

Practical and Philosophical Observations on Natural Waters. By WILLIAM WEST, Esq. 38

General Remarks on the Weather in Madagascar, and chiefly at its Capital, Tananarivou; with a Meteorological Journal. By ROBERT LYALL, Esq., Brit. Res.-Agent, Member of many Foreign and Brit. Societies. [Communicated by J. F. DANIELL, Esq.] 47

On the Elucidation of some Portions of the Fabulous History of Greece, by the Application of the Analytical Principles of Philology. By WILLIAM SANKEY, A.M., &c. . . . 57

On the Limits of Vaporisation. By M. FARADAY, F.R.S., Director of the Laboratory of the Royal Institution, &c. . . 70

On the Effects of Electricity upon Minerals which are phosphorescent by Heat. By THOMAS J. PEARSALL, Ch. Assist. in the Royal Institution 77

On the Development of the several Organic Systems of Vegetables, with reference to their Functions; and especially on the Respiration of Plants, as distinguished from their Digestion. By GILBERT T. BURNETT, Esq. 83

Page

Contributions to the Physiology of Vision, No. I. . . . 101

Description of the Horns of the Prussian Elk; Difference between them and those of the American Moose-Deer. By William Wittich, Esq. 118

On Gunpowders and Detonating Matches. By Andrew Ure, M.D., F.R.S., &c. 121

Analysis of New Books.

Commentaries on the Mining Ordinances of Spain. By Don Francisco Xavier de Gamboa. Translated by Richard Heathfield, Esq., Barrister-at-Law . . . 142

Anatomical Investigation of the Structure of the Eyes in Insects and Crustacea, by Dr. J. Müller 152

On the Structure of the Eyes in the *Murex Tritonis*. By Dr. J. Müller 155

FOREIGN AND MISCELLANEOUS INTELLIGENCE.

§ I.—MECHANICAL SCIENCE.

1. Resistance opposed to Water moving in Pipes (D'Aubuisson) . 157
2. On the Resistance of Lead to Pressure, and on the Influence of a small Quantity of Oxide upon its Hardness . . *ib.*
3. On the Power of Horses (B. Bevan, Esq.) . . . 159
4. On the Change of Volume occurring when Bodies combine together 160
5. Apparent Hydrostatic Anomaly with Laurel-oil . . 161
6. On the Quantity of Light reflected by Metallic Specula at different Angles of Incidence (R. Potter, Esq.) . . . 162
7. On the apparent Projection of Stars upon the Moon's Disk . 163
8. On the Production of Coloured Bands by Plane Mirrors . 164
9. Size for Illuminators, Artists, &c. 165

§ II.—CHEMICAL SCIENCE.

1. Galvanic Currents during the Decomposition of Water . 166
2. Power of Metallic Rods or Wires to decompose Water, after their connexion with the Galvanic Pile is broken (Berzelius) . 167

Page
3. On Pyrophosphoric Acid and the Pyrophosphates . . 167
4. Production of Hydrocyanic (Prussic) Acid, under uncommon circumstances (A. A. Hayes) 169
5. Action of Chlorine on Carburetted Hydrogen, Alcohol, and Ether, (Morin) . . . . . . ib.
6. Bromide of Carbon . . . . . ib.
7. Preparation of Phosphuret of Lime (Dr. Coxe) . . 173
8. Iodide of Potassium a good Test for Arsenic—curious Compound produced . . . . . . ib.
9. Ammonia in Native Oxide of Iron (Boussingault) . 174
10. Atomic Weight of Titanium (Rose) . . . 175
11. On the Crystallization of Gold (Professor Henslow, of Cambridge) 176
12. Salicine—its power as a Febrifuge (Leroux) . . 177
13. Preparation and Composition of Malic Acid . . 178
14. Ulmin, or Ulmic Acid, and Azulmic Acid . . 179
15. On Caseum and Milk (Braconnot) 181
16. Manufacture of Charcoal 184
17. Potash obtained commercially from Felspar . . . ib.
18. Sale of Selenium . . . . . ib.

§ III.—NATURAL HISTORY, &c.

1. Mechanism of the Human Voice in Singing . . . 185
2. Globules in the Humours of the Eye . . . ib.
3. Use of Nitrogen in Respiration—Cyanogen in the Blood . . 186
4. Action of the Pile on Living Animal Substances . . ib.
5. On the Disorders arising from the long-continued Use of Iodine (Dr. Jahn) 187
6. Chlorine an Antidote to Hydrocyanic Acid . . . 188
7. On the Cure of Animal Poisons, and probably Hydrophobia, by the local Application of Common Salt (Rev. J. Fischer) . 189
8. On Restoration from Drowning by Insufflation of the Lungs . 190
9. Surgical Recovery of an Eye 191
10. On the Means of improving both the Quality and Quantity of Wool (M. Petri) 192
11. Vision of Birds of Prey (Dr. J. Johnson) . . . ib.
12. New Species of British Snake 193
13. On the Existence of Animalcula in Snow (Dr. Mure) . . ib.
14. Antipathy of the Chameleon to Black . . . 194
15. Phosphorescence of the Sea in the Gulf of St. Lawrence . . ib.
16. Rending of Timber by Lightning . . . 195

	Page
17. Protraction of Vegetable Life in a dry State	196
18. Market State of Hyosciamus	*ib.*
19. Snow of the Winters 1829-1830	*ib.*
20. Peculiar Fall of Snow (Mr. Sherriff)	198
21. Electricity of the Winds	*ib.*
22. Irised Aurora Borealis	*ib.*
23. Influence of the Age of Parents on the Sex of Children	199
24. Precautions in the Planting of Potatoes	*ib.*
25. Preservation of frozen Potatoes	200
26. Cure of Wounds in Elm Trees	*ib.*
27. Preservation of Fruit Trees from Hares	*ib.*
28. Waterspout on the Lake of Neufchatel	*ib.*
29. Mirage of Central India	201
30. Village lighted by Natural Gas	203
31. Singular Natural Sound	204

CONTENTS.

Page

On a peculiar class of Optical Deceptions. By M. FARADAY, F.R.S., Director of the Laboratory of the Royal Institution, &c. . 205

Description of a Mode of erecting light Vaults over Churches and similar Spaces. By M. DE LASSAUX. [Communicated by Professor WHEWELL, of Cambridge] 224

Account of a New Comet observed by M. DABADIE . . 241

On the Permanence of the Magnetism in Steel Bars. By S. H. CHRISTIE, Esq., M.A., F.R.S., &c. 243

On the Electro-Chemical Decomposition of the Vegeto-Alkaline Salts. By W. T. BRANDE, F.R.S., Prof. Chem. R. I. . . 250

Some Observations on Dr. Arnott's Explanation of the Nature of Stammering. By MARSHALL HALL, M.D., F.R.S., &c. . 253

Observations on Mr. Rennie's Paper on the peculiar Habits of Cleanliness in some Animals. By WILLIAM AINSWORTH, Esq. 261

On the Aurora Borealis of the 7th of January, 1831. By S. H. CHRISTIE, Esq., M.A., F.R.S., &c. 262

On the Mechanism of the Act of Vomiting. By MARSHALL HALL, M.D., F.R.S.E., &c. 265

Further Experiments on the Communication of Phosphorescence and Colour to Bodies by Electricity. By THOMAS J. PEARSALL, Chem. Assist. in the Laboratory of the Royal Institution . 267

On the Darkness between the Primary and Secondary Rainbows. By Mr. AINGER. [In a Letter to M. FARADAY, Esq., F.R.S., &c.] 281

On the Mode of Ascertaining the Commercial Value of Ores of Manganese. By EDWARD TURNER, M.D., F.R.S. L. and E., Sec. G.S., Prof. Chem. in the University of London. . . 293

Phenomena observed at the last Eruption of Mount Vesuvius in 1828. By Dr. E. DONATI 296

Page

A Mode of Regulating the Supply of Water between Intersecting Rivulets and Canals. Devised by the late Robert Almond, Esq., of Nottingham. [Communicated by Marshall Hall, M.D., F.R.S.E., &c.] 307

On the Geometric Properties of the Magnetic Curve, with an Account of an Instrument for its Mechanical Description. By P. M. Roget, M.D., Sec. R.S. 311

On the First Invention of Telescopes, collected from the Notes and Papers of the late Professor Van Swinden. By Dr. G. Moll. 319

Proceedings of the Royal Institution 333

Analysis of Books.

Philosophical Transactions of the Royal Society of London, for the year 1830. Part II. 337

Life of Sir Humphry Davy, Bart., LL.D., late President of the Royal Society, &c. By John Ayrton Paris, M.D., F.R.S. . . 347

Plantæ Asiaticæ Rariores; or Descriptions and Figures of a select Number of unpublished East India Plants. By N. Wallich, M.D. 360

A Numerical List of Dried Specimens of Plants in the East India Company's Museum, collected under the Superintendence of N. Wallich, M.D. . . . . *ibid.*

FOREIGN AND MISCELLANEOUS INTELLIGENCE.

§ I.—MECHANICAL SCIENCE.

1. On the Discharge of a Jet of Water under Water (R. W. Fox, Esq.) 368
2. On preventing the Discharge of a Bullet from a Gun by the Finger *ibid.*
3. Clement's Experiment—Easy Mode of repeating it 369
4. Browne's moving Molecules . . . . . *ibid.*
5. Exact Measure of a Degree 370
6. Svanberg on the Temperature of the Planetary Space . . *ibid.*
7. On the Relation between the general Direction of the Stratification of the Earth and the Lines of equal Magnetic Intensity in the Northern Hemisphere (M. L. A. Necker) . . . 372
8. Force of Terrestrial Magnetism 374
9. An Acoustic Rainbow 375

Page
10. Compression of Fluids (Professor Oersted) . . . 375
11. Peculiar Appearance of Saturn's Ring . . . 376

§ II.—CHEMICAL SCIENCE.

1. Decomposition of Water by atmospheric Electricty . . 376
2. Decomposition by ordinary Electricity . . . *ibid.*
3. On the Decomposition of Metallic Salts by the Voltaic Pile, and on the state of Chlorides, Iodides, &c. in solution . . 377
4. Voltaic Test of the State of Metals 378
5. Powerful Electro-magnet constructed by Professor Moll . 379
6. Laws of Electrical Accumulation 380
7. On the Emission of Light during the Compression of Gases . 381
8. On Oxamide, a Substance which approximates to some Animal Bodies (M. Dumas) 382
9. Preparation of Nitrogen (Professor Emmett) . . 384
10. Action of mixed Nitrate and Muriate of Ammonia on Glass . 385
11. Pulverization of Phosphorus . . . . *ibid.*
12. Inflammation of Phosphorus by Charcoal . . . *ibid.*
13. Preparation of Bi-Carbonate of Soda . . . *ibid.*
14. Rock Salt in Armenia 386
15. Preparation of Lithia (Quesneville, *fils*) . . . *ibid.*
16. On the Submuriates of Iron, and other Subsalts (Mr. Phillips) . 387
17. On the Reaction of Persalts of Iron and Neutral Carbonates . 388
18. On the Relative Action of diluted Sulphuric Acid and Zinc (M. A. de la Rive) . . . . . *ibid.*
19. Crystallization of Bismuth 393
20. On discoloured Chloride of Silver (M. Cavalier) . . *ibid.*
21. Composition of Fulminating Gold 394
22. Whewell's written Nomenclature for Chemical Compounds . *ibid.*
23. Para-Tartaric Acid and Para-compounds . . . 395
24. Preparation of Piperin, by Mr. Clemson . . . *ibid.*
25. On Salicene, by MM. Pelouze and Jules Gay Lussac . . . 396
26. Preparation of Salicene 397
27. New Kind of Indigo . . . . . *ibid.*
28. Charring of Wood at Low Temperatures . . . *ibid.*
29. Change of Colour in the Wood of certain Trees . . . 398
30. Preservation of Blood . . . . . *ibid.*
31. Presence of Manganese in the Blood (Professor Wurzer, of Marburg) 399
32. On two Ores of Tellurium, from the Altai Mountains (M. Rose) . *ibid.*
33. Berzelius' Method of preparing Urea . . . 401
34. On Distillation of Nitric Acid (by E. Mitscherlich) . . 402

Page

35. On a peculiar Property of Alloys (F. Rudberg) . . 404
36. On the Combination of Chloride of Gold with the Chloride of Potassium and Sodium (Berzelius) 409
37. On Magnesium (Justus Liebig) 411
38. On the Expansion of Bismuth and its Alloys during Congelation (Professor Marx, of Brunswick) . . . . . *ibid.*

§ III.—NATURAL HISTORY.

1. Formation of Hail 415
2. Georgia Meteor and Aërolite . . . . . . *ibid.*
3. On the Thermal Waters of Chaudes Aigues, in the Department du Cantal (M. Chevalier) 417
4. Humboldt's Account of the Gold and Platina District of Russia . 418
5. Parrot's Expedition to Ararat 419
6. Cuticular Pores of Plants . . . . . . *ibid.*
7. Smut in Corn 420
8. Structure of Leaves 421
9. Crystals of Oxalate of Lime in Plants 422
10. Growth of Vegetables . . . . . . *ibid.*
11. On Circulation in Vegetables 424
12. New Method of multiplying Dahlias . . . . *ibid.*
13. Seat of the Sense of Taste 425
14. Remarkable Case of the Reunion of a divided Part . . *ibid.*
15. Singular Effect of Opium 426
16. Mechanical Powers of a Spider . . . . *ibid.*
17. White Bait . . . . . . *ibid.*
18. To restore the Elasticity of a damaged Feather . . . 427
19. Ornithology . . . . . . *ibid.*
20. Ichthyology 429
21. Influence of the Aurora Borealis on the Magnetic Needle (A. T. Kupffer, of St. Petersburg) . . . . . . *ibid.*
22. Description of some Atmospheric Phenomena (Prof. Strehlke, of Danzig) 432
23. On the Produce of Gold and Silver in the Russian Empire (Alex. von Humboldt) 434
24. On the Change which the Air in Eggs undergoes during Incubation (Professor Dulk, of Königsberg) . . . 435

CONTENTS.

Page

On the Employment of Notation in Chemistry. By the Rev. W. Whewell, Professor of Mineralogy in the University of Cambridge 437

On the Plant intended by the Shamrock of Ireland. By I. E. Bicheno, Esq., F.R.S., Sec. L. S., &c. [Read at the Linnean Society.] 453

On the Earliest Epoch of Egyptian Chronology. [In a Letter from Professor Renwick to Captain Sabine] . . . 458

An Account of a Remarkable Instance of Anomalous Structure in the Trunk of an Exogenous Tree. By John Lindley, Esq., F.R.S., &c., Professor of Botany in the University of London . 476

On the First Invention of Telescopes, &c. By Dr. G. Moll, of Utrecht (concluded) 483

On the Contrivances of some Animals to secure Warmth. By J. Rennie, A.M., A.L.S., Professor of Natural History, King's College, London 496

On the Aurora Borealis of the 7th of January, 1831. By Dr. Moll, of Utrecht 519

Observations on the Aurora Borealis of the 7th of January, the 11th of January, and the 7th of March, 1831. By the Hon. Charles Harris 522

On the Height above the Surface of the Earth of a Luminous Arch of the Aurora Borealis, on the 7th of January, 1831. By S. H. Christie, Esq., M.A., F.R.S., &c. 525

On Elaterium; and a New Principle obtained from it by Analysis. By Henry Hennell, F.R.S., M.R.I., Chemical Operator, Apothecaries' Hall 532

Contributions to the Physiology of Vision. No. II. . . 534

On the Ripple-Marks and Tracks of certain Animals in the Forest Marble. By G. Poulett Scrope, Esq., F.R.S., F.G.S., &c. . 538

Page
Proceedings of the Royal Institution of Great Britain . . 547
Proceedings of the Academy of Sciences in Paris . . . 558

Analysis of Books, and Selections from the Transactions of Scientific Societies.

Life of Sir Humphry Davy, Bart., LL.D. 571
Acta Academiæ Cæs. Leop. Carol. Naturæ Curiosæ Bonnæ . 585
Memoirs of the Institute of France 595

FOREIGN AND MISCELLANEOUS INTELLIGENCE.

§ I.—MECHANICAL SCIENCE.

1. Stiffness and Strength of Timber 599
2. Proportion between the Metre and English Yard . . . *ibid.*
3. On the Velocity of an Elastic Fluid which flows from a Reservoir into a Gasometer . . . . . . *ibid.*
4. On the Discharge of a Jet of Water under Water (R. W. Fox, Esq.) . *ibid.*
5. Optical Deception upon the Liverpool and Manchester Rail Road 600
6. A Barometer of a new Construction (Proposed by M. Kupffer.) . 601
7. Occultation . . . . *ibid.*
8. Pendulum Observations 602
9. Dip of the Magnetic Needle at St. Petersburgh 604
10. On the Direction and Intensity of the Magnetic Force at St. Petersburgh (M. Erman) . . . . . . *ibid.*
11. Variation of the Needle 607
12. On the Figure of the Magnetic Equator . . . . *ibid.*
13. New Dipping Needle 608
14. Powerful Electro-Magnets 609
15. On the Intensity of the Earth's Magnetism (Kupffer) . . 610

§ II.—CHEMICAL SCIENCE.

1. Matteuci on the Origin of the Action of the Voltaic Pile . . 612
2. Conducting Powers of Liquified Gases (K. T. Kemp) . . 613
3. Generation of Steam by Heated Metal . . . . *ibid.*
4. On the Preparation of Iodic Acid (Serullas) . . . 614
5. On the Precipitation of the Vegeto-Alkalies by Iodic Acid (Serullas) 615
6. On the Action of Bromic and Chloric Acids on Alcohol (Serullas) *ibid.*
7. On Perchloric Acid and its Facil Formation (Serullas) . . 616
8. On the Spontaneous Inflammation of Pulverized Charcoal (Aubert) . 617

		Page
9.	Power of Carbon to destroy the Bitterness of certain Bodies	619
10.	Method of preparing Selenium from the Sulphuret (M. Magnus)	619
11.	On the Compounds of Ammonia with Anhydrous Salts (H. Rose)	620
12.	Test of the Protoxide and Peroxide of Iron (Berzelius)	624
13.	A new Metal Vanadium, associated with Iron (Sefström)	625
14.	Combustion of an Alloy of Tin and Lead (R. W. Fox)	626
15.	Vauquelin's Process for obtaining Metallic Chromium	*ibid.*
16.	On the Absorption of Oxygen at High Temperatures, by Silver (Gay-Lussac)	627
17.	Robiquet on a new Metallic Dye	628
18.	Purple Precipitate of Silver, Gold, &c. &c.	*ibid.*
19.	Œnometer or Alcohometer (M. Emile Tabarie)	629
20.	On the Manufacture of Sulphuric Ether (C. Wittstock)	*ibid.*
21.	On Columbine; a New Vegetable Principle (M. Wittstock)	630
22.	On the Composition of Camphor and Camphoric Acid (J. Liebig)	631
23.	Use of Mica in minute Chemical Analyses	633
24.	On Perforating and Cutting Glass, Earthenware, &c. (Mr. Marsh)	*ibid.*

§ III.—NATURAL HISTORY, &c.

1.	Circulation of Fluids in Vegetables	635
2.	Structure of Leaves	636
3.	Germination of Seeds at the Surface of Mercury	637
4.	Fertilization of Plants	*ibid.*
5.	Structure of the Radish Root	638
6.	Russet in Apples	*ibid.*
7.	Medicinal Use and Effect of the Ava Root	639
8.	Mexican Domestic Bees. (Melipona Beechei)	640
9.	Mean Meteorological Results	641
10.	Climate of England	642
11.	On the Earthquake at Odessa on the 26th of November, 1829 (M. Hauy)	643
12.	Geography of Siberia	644

ERRATA IN NO. III.

Page	Line	
320	4	from bottom, *for* Chartres *read* Castres.
325	5	from bottom, *for* vitro-crystal lines *read* vitrocristallines.
329	16	from top, *for* Bussi *read* Russii.
ib.	22	from top, *for* Teannin *read* Jeannin.
ib.	23	from top, *for* Bussi *read* Russi.
330	25	from top, *for* Tanz *read* Jansz.
ib.	2	from bottom, *for* Tanz *read* Jansz.
331	6	from top, *for* Borel *read* Boreel.
332	5	from top, *for* Borel *read* Boreel.
ib.	7	from bottom, *for* that *read* thus.
ib.	1	from bottom, *for* produced *read* procured.

ROYAL INSTITUTION OF GREAT BRITAIN.

4th April, 1831.

THE WEEKLY EVENING MEETINGS of the Members of the Royal Institution will be resumed on Friday the 15th instant, at half-past Eight o'Clock, and will be continued on each succeeding Friday evening till the end of the Season.

The following are the Arrangements of the Lectures which are to be delivered on each day at Three o'Clock in the Afternoon :—

CHEMICAL AND NATURAL PHILOSOPHY. By Michael Faraday, Esq., F.R.S., F.G.S., Corr. Memb. Royal Acad. Sciences Paris, Director of the Laboratory of the Royal Institution, &c. &c. To commence on Thursday, the 14th instant, and to be continued on each succeeding Thursday till the 5th of May. The following are the Subjects of the Course :—April 14th, Optical Deceptions—April 21st, Lithography—April 28th, Flowing of Sand—and May 5th, Caoutchouc.

GEOLOGY. On some of the most important points in Geology. By Thomas Webster, Esq., F.G.S. To commence on Saturday, the 16th of April, and to be continued on each succeeding Saturday till the 21st of May.

POETRY AND THE POETS. By James Montgomery, Esq., Author of 'The World before the Flood,' 'Pelican Island,' &c. To commence on Tuesday, the 26th of April, and to be continued on each succeeding Tuesday till the completion of the Course, on the 31st of May.

ACOUSTICS. By Robert Willis, M.A., F.R.S., Fellow of Caius College, Cambridge. To commence on Thursday, the 12th of May, and to be continued on each succeeding Thursday till the completion of the Course, on the 16th of June.

BOTANY. On Vegetable Physiology and Botany. By John Lindley, Esq., F.R.S. and F.L.S., Prof. of Botany in the University of Lond., and Assist. Sec. Hort. Soc. To commence on Saturday, the 28th of May, and to be continued on each succeeding Saturday till the completion of the Course, on the 18th of June.

The Sons and Daughters of the Members of the Royal Institution, under Fifteen Years of Age, may be admitted on payment of half the sum for each Course.

Syllabuses of the Lectures may be obtained at the Royal Institution.

JOURNAL

OF

THE ROYAL INSTITUTION

OF

GREAT BRITAIN.

ON CERTAIN PHENOMENA RESULTING FROM THE ACTION OF MERCURY UPON DIFFERENT METALS.

By J. F. DANIELL, F.R.S., and M.R.I.

THE results of the following experiments on the action of mercury upon different metals may probably be considered interesting; not only on account of the novelty of the facts, which have been hitherto, I believe, unnoticed, but from the relation in which some of them may be found to stand to the laws of molecular attraction.

EXPERIMENT I.

A piece of flexible metallic tube, which is composed of an alloy of tin and lead, was partly immersed in mercury contained in a wine-glass. In the course of a few days it was examined, and found studded with brilliant metallic crystals, in a line coincident with the level of the fluid. After this examination, it was replaced and left undisturbed for six weeks: at the expiration of which period it was carefully lifted out of the mercury; and a considerable groupe of well-defined crystals were found loosely adherent to its upper part, and many similar ones floating upon the surface of the mercury. Their form was that of hexahedral plates variously modified; some of them were above one-tenth of an inch diameter, and their lustre was white and silvery. By placing them in a small inverted cone of paper, perforated at its apex, the fluid mercury drained from

them, and they were left in nearly a dry state. The tube was dissolved away at its lower end to a thin edge, and the action of the mercury had evidently decreased as it ascended: the upper part to which the crystals were attached was but little acted upon, so that, in its whole length, it gradually tapered downwards. The substance of the metal, even above the part immersed, was saturated with mercury, and had become very brittle.

Hence it appears that the action of the mercury upon the alloy was, first to saturate its pores and disintegrate its substance, forming a brittle, uncrystallized compound which it must have subsequently dissolved. The amalgam thus produced, being of less specific gravity than the fluid metal, floated to its surface, where the attraction of cohesion between the particles of the compound, being greater than the attraction which held them in solution in the fluid, caused them to crystallize. I have formerly* remarked, that if a mass of any soluble salt be carefully suspended in water, it will be more acted upon at its upper than its lower end, and will assume, more or less, the form of a cone, with the apex at the surface of the liquid. The particles of water which are in immediate contact with the salt, combine with a portion of it, and thus becoming specifically heavier than the remainder, sink to the bottom of the vessel; others succeed and follow the same course. A layer of saturated solution is thus deposited, which increases in depth as the process advances, protecting in its rise that part of the mass which is covered with it from further action. In the present instance the process is directly the reverse: the solvent, by union with the solid, becomes specifically lighter, and the saturated solution is first formed upon the surface; and the action continuing longest at the bottom of the mass, a cone is produced with its apex downwards.

EXPERIMENT II.

A piece of pure tin, in the usual form of closely-aggregated imperfect prisms, in which it is found in commerce, was partly immersed in mercury, and left undisturbed for a month. Upon

* *Journal of the Royal Institution*, vol. i., p. 24. 1*st Series.*

examination, a large cluster of crystals, similar to the preceding, was found adhering to its upper part, and others floating upon the liquid. They were not quite so large as the first; but bore very distinctly the form of six-sided plates. The whole mass was thoroughly saturated with mercury, but had been more acted upon at the bottom than the top of the portion immersed. At the lower end, the prisms had the appearance of being more detached from one another than in their original state, from cracks which had taken place in the metal; and which conferred upon their extremities the semblance of imperfect pyramids. Several deep clefts also had been formed along the more prominent edges of the mass.

EXPERIMENT III.

A small bar of lead was plunged, for about half its length, into some mercury contained in a test-tube. Having been left undisturbed for ten days, it was carefully lifted out and examined. A bundle of very delicate, silver-white, feathery crystals was found loosely adhering to it, on a line with the surface of the fluid. Their form could not be accurately determined, but they resembled a heap of frosty particles swept together on a pane of glass; and their minute prisms appeared to be attached together at angles of sixty degrees. The bar had been most acted upon at its lowest extremity: it was thoroughly impregnated with mercury throughout its substance, but had not totally lost its ductility. After the operation, the tin crumbled to pieces under a slight blow of the hammer, but the lead could be flattened into a plate.

EXPERIMENT IV.

A bar of zinc was treated in the same way, and for a like period. It was found, upon examination, studded throughout the whole length which had been immersed with very bold crystals, of the form of hexahedral plates, which increased in quantity and size from below upwards. The bar tapered downwards to a point, and was more unequally acted upon than the former metals, its surface being rough, and corroded into cavities. Some of the crystals adhered very strongly to the surface, and

some of them had the appearance of being partly imbedded in the bar, or dissected from its substance. They were of a darker hue, and more brilliant than the crystals from lead and tin.

EXPERIMENT V.

A bar of fine silver was partly immersed in mercury, as in the preceding cases: at the expiration of a fortnight no crystals had been formed. The mercury had entered into its substance, but upon trial it had not lost its malleability. It was replaced, and at the end of six weeks had not apparently changed its characters. The test-tube, with its contents, was now heated till the mercury began to boil, and was set by to cool gradually. In twenty-four hours' time the bar was again examined, and a bundle of very fine needle-crystals was found clustered round the part which was just intersected by the surface of the liquid.

In this case, the affinity of the mercury for the silver enabled it to penetrate its pores, and thoroughly to saturate it, but its attraction for the resulting compound was not sufficiently strong to allow it to overcome the remaining attraction of aggregation, and dissolve the solid at the ordinary temperature of the air. When assisted, however, by heat, the solution was effected, and the compound, as in the former instances, being specifically lighter than the pure fluid, floated to the top, and crystallized.

EXPERIMENT VI.

A small portion of a bar of fine gold, about an inch and a half in length, was put into mercury, in which, of course, it sank, from its greater specific gravity. The fluid very quickly penetrated it, and completely destroyed its yellow colour. In a month's time it retained its malleability, and a part of it was flattened under the hammer into a very thin plate. Its surface was studded with very minute crystals, whose dimensions were too small to be determined. The gold was then heated in the mercury to the boiling point of the latter, when it was completely dissolved, and a pasty amalgam formed.

There can be no doubt that in all these instances the mer-

cury formed definite solid compounds with the several metals, which were capable of being held in solution by an excess of the fluid; but were also capable, in favourable circumstances, of separating from it, and crystallizing in peculiar forms. Whether, at the same time, any other compound may have been formed of an essentially liquid nature, I have not examined; but I may here remark, that the manufacturers of looking-glasses have made the observation, that the mercury which is pressed out of the tin amalgam, which they apply to the backs of their plates, is in as pure a state as that which they originally make use of.

EXPERIMENT VII.

A square bar of tin, about five inches long, and whose sides were a quarter of an inch wide, was laid horizontally in a card-tray, and just covered with mercury. To render the action as equal as possible, it was frequently turned upon its different sides, and examined. At the expiration of twenty-four hours, minute fissures began to appear along all its lateral and terminal edges. The process was continued, and the cracks widened, until, on the third day, they opened to such a degree as to shew that the bar was resolved into four equal trihedral, rectangular prisms, with two equal angles. They were readily separated from each other by the point of a penknife, and two similar pyramids, whose angles at their bases were 45°, were at the same time detached. This groupe is accurately represented in their relative positions, a little separated, at Fig. 1, Plate I. *a*, *a*, *a*, *a* are the small triangular prisms, which, when in contact, made up the original square bar; and *b* represents one of the terminal pyramids. All the angles were as sharp and perfect, and the faces as neat, as if they had been carved with tools; and when brought into contact with one another, they adhered together with some force, from the cohesive attraction of a little mercury which hung about them. This experiment I immediately repeated, and obtained the same very remarkable results.

I was at first induced to consider this singular phenomenon as dependent upon the original structure of the bar, from the consideration of the following facts, which are well known to

most workers in the metals, and which I have myself verified by experiment.

No metal can be hammered *round* upon an anvil, either hot or cold. Blacksmiths very well know that they cannot forge a round bar of iron; and I have myself seen a rod of the best iron which, properly heated, could be extended indefinitely, when hammered square or flat, split into fibres, and become perfectly disintegrated after a few blows given equally round. When it is desired to give a round form to any part of a square bar of iron, it is effected by forcing it, while hot, into a kind of form, or mould, of the required dimensions; or, as is well known, it may be extended in a cylindrical form to almost any degree, by the equal pressure applied in the process of wire-drawing. If square bars of gold, silver, or copper, the most malleable of all the metals, be hammered upon the edges, and the blows repeated round, so as to give them a cylindrical shape, they soon become what is technically termed rotten, and break into fibres, while the bars may be extended under the hammer to any degree, by blows directed parallel to their original faces, or may be beat into leaves of almost inconceivable thinness, if the force be directed upon one surface only. The less malleable metals, lead, brass, and tin, become even sooner disintegrated when hammered round; and, although they are capable of considerable extension, when hammered square, they ultimately split along the edges in a manner very similar to the disintegration which I have just described as resulting from the action of mercury upon the tin bar.

It is also worthy of observation, that the metallic bars, when hammered square, generally assume a rhomboidal, rather than a perfectly rectangular form, and that the fissures take place indifferently upon all the angles; but if the hammering be continued, they sometimes split into two, in the direction of one of their diagonals, before the separation takes place in the direction of the other. I have not been able to satisfy myself whether this tendency to the rhomboidal form results from any inequality in the blow of the hammer, producing an inclination of the planes of compression to one another; or whether it may be referred to the forms of the ultimate particles of the metals; but I have ascertained that it takes place even when the greatest

pains are taken to keep the face of the hammer parallel to the surface of the anvil; and that it can only be counteracted, when required, by directing a blow from time to time upon the acute angle. To determine, if possible, whether any connexion subsists between these results of the direct application of mechanical force to the metals, and the structure of the bars of tin developed by the action of mercury, as just described, I instituted the following experiments.

EXPERIMENT VIII.

A bar of tin, of about the same dimensions as the last, which had assumed the rhomboidal form during the process of hammering, from the original cylindrical shape in which it had been cast, was treated with mercury in the manner described above: it was resolved, as before, into four rectangular trihedral prisms, but with two unequal angles, corresponding to the bisected angles of the rhomboid.

EXPERIMENT IX.

The tin bars upon which the previous experiments were made had been shaped by the hammer, and I was desirous of ascertaining whether the forces which had been applied had in any way disposed their particles to assume the structure which had thus been developed. For this purpose, a bar was cast, in a mould, of nearly the dimensions of that employed in Exp. VII. and was treated with mercury in the same manner. The four trihedral prisms, with their two pyramids, were formed as before; but the clefts and the planes of junction were not as neat as in the foregoing instances. This seemed to be owing to the angles of the original bar not having been so sharp as when formed by the hammer, but having necessarily come rounder from the mould, and presenting a surface to the action of the mercury.

EXPERIMENT X.

A cast cylinder of tin, five inches long, and a quarter of an inch in diameter, was substituted for the square bar in the

experiment: at the end of three days, during which it was frequently turned, the terminal edges were cleft all round, and irregular cracks appeared upon various parts of its surface. Two solid pieces, approaching the hemispherical form, but much flatter, were extracted from the ends by the point of a knife, and two cup-like cavities were formed in the bar. By introducing the edge of the knife into the cracks upon the surface, its substance was broken away in parts, and a concentric arrangement of the amalgam disclosed round a central nucleus; the appearance of which is represented at Fig. 2. The outside coating, *b*, *b*, was perfectly brittle, but the centre rod, *a*, *a*, still partially retained its malleability, and could be bent two or three times backwards and forwards, before it broke.

EXPERIMENT XI.

Another bar of tin was cast, of the form and dimensions of half the preceding cylinder, divided longitudinally. Its appearance, after being treated with mercury as described, is exhibited at Fig. 3. Its two lateral edges were sharply cleft asunder, as at *a*, *a*, and some irregular cracks appeared upon its round surface. Part of the substance of the amalgam was broken away, as shewn at *b*, when a centre cylindrical rod appeared, and the concentric arrangement was apparent, as in the last experiment.

EXPERIMENT XII.

Having cast a cylinder of tin, similar to that employed in Exp. x., one half of it was made square by the file, and the whole was then submitted to the action of mercury as before. The cleavage down the lateral edges, which were very sharp, was perfect, and a most beautiful pyramid was formed at the square end. The cylindrical portion of the bar was irregularly cracked, and there seemed to be a tendency of the clefts in the square edges to continue their course into this part. These results are represented at Fig. 4; *a* is the terminal pyramid, *b*, *b* the cleft upon one of the edges of the square bar; *c*, *c* the cylinder.

EXPERIMENT XIII.

I cast a square bar of tin, of similar dimensions to that which I employed in Exp. IX. One half of its length was hammered upon the edges till four new planes were formed in their places, and the square reversed from its original position. Thus both ends of the bar were still square, but the edges of one half were in the direction of the planes of the other half, and a small intermediate portion was irregularly octangular. The whole was soaked in the shallow bath of mercury. The cleavage upon the edges of the hammered half was perfect, and the trihedral prisms and terminal pyramid very distinct. The edges of the cast portion were not cleft, but the sharp divisions of the hammered edges were continued down its faces, in ragged, irregular cracks, which gaped particularly near the point of junction. This end, therefore, had a tendency to separate into four tetrahedral prisms, and the force was so great, that they broke off near the point of junction of the two parts of the bar, and ultimately assumed the appearance represented at Fig. 5. The sharp and even cleft upon one of the edges of the hammered portion is exhibited at *a*, *a*, and the ragged crack upon the corresponding face of the cast part at *b b*, gaping at the point of fracture, *c*, *c*, as if rent asunder with great violence.

I attempted in vain to produce analogous results with bars of lead, brass, gold, silver, and zinc, for in none of these instances could I obtain evidence of the action of any mechanical force acting upon the particles of the metals ; although their union with the mercury was, to all appearance, as intimate as that of tin. No cracks or disruptions appeared in any of them. The surfaces of the four first remained perfectly smooth and continuous, but that of the last was corroded into cavities. There can be little doubt, I think, that the disruptive force which effected the disintegration of the tin bars, in the manner above described, was the powerful contraction of the integrant particles of the metal, in the act of combining with the mercury. It has, indeed, been proved that the amalgam hence resulting is of considerably greater density than the mean of its component parts, and that such approximation of molecules must,

therefore, take place; the balance of force which determines its particular direction in the instances pointed out, forms an interesting subject of investigation, which, together with the cleavage and dissection of crystals, and the manner in which they are affected by light and heat, may ultimately contribute to the explanation of the laws of molecular attraction.

I shall conclude this paper with the result of some experiments upon the mutual action of mercury and platinum.

EXPERIMENT XIV.

There is no apparent action whatever between mercury and a bar of platinum, at the common temperature of the atmosphere; but when exposed together for a short time to the boiling point of the former, the latter becomes superficially coated with the fluid. The combination is so slight, that the mercury may easily be wiped off mechanically, as water from wet glass. Platinum, which has been kept constantly wetted with mercury for six years, has not become disintegrated, or in any way changed its properties.

EXPERIMENT XV.

A few grains of spongy platinum, formed from the ammonio-muriate, were violently agitated with mercury and a few drops of water in a test-tube: a kind of thick scum, or semifluid amalgam, speedily collected upon the surface, from which the still fluid metal could easily be poured off.

EXPERIMENT XVI.

The foregoing experiment was repeated; but the water was acidified with acetic acid. The test-tube was five inches long, and about half an inch diameter. The mercury occupied about an inch, and the weak solution of the acid about half an inch of its depth. The platinum was thrown in, and the whole shaken together for a short time; when the tube became filled with an amalgam, of the consistence of soft butter. When the tube was upset, a very few drops of fluid mercury ran out of it; and when the amalgam was shaken out into a saucer, it retained its consistence for many weeks. It possessed a dullish metallic hue, like that of lead which has become tarnished; and very

much resembled the amalgam formed by the electrization of mercury in contact with ammonia.

The experiment was frequently repeated, sometimes with the substitution of some neutral salt for the acid, and always with similar results.

When the amalgam was laid upon filtering paper, the moisture was gradually absorbed and evaporated, and the mercury returned to the fluid state.

EXPERIMENT XVII.

The experiment was varied by filling a tube, which was some inches longer, with the weak acid solution; and after the formation of the amalgam by agitation, inverting it in a cup of mercury. Minute bubbles of gas were immediately seen rising from the amalgam through the fluid, and collecting in the upper part of the tube. Upon close examination, particles of the spongy platinum could be discovered between the sides of the glass and the mercurial paste, round which bubbles of gas gradually accumulated, which gave the whole a honey-combed appearance. These, as they increased in size, slowly crept up the sides of the tube, till, reaching the fluid, they rapidly ascended to the top. In twelve hours' time, nearly the whole of the liquid had been expelled from the tube, and when a light was applied to the gas it exploded.

Some of the acetic solution, which had been frequently employed in repetitions of the experiment, was slowly evaporated, and afforded crystals of prot-acetate of mercury.

EXPERIMENT XVIII.

I endeavoured, in vain, to produce analogous results, by agitating amalgam of gold and other amalgams with diluted acetic acid and solutions of neutral salts. No action was apparent, and in no instance was anything like the frothy amalgam produced.

Hence it appears that, when minutely divided platinum is agitated with mercury, and moisture is present, an electrical action takes place, which, when heightened by the addition of a diluted acid, or the solution of a neutral salt, is sufficiently energetic to decompose water and evolve hydrogen: the oxygen

at the same time combines with the mercury, and a solution is effected by the acetic acid, which its unassisted affinity could not have produced. This action appears to be of the same nature as that described by Mr. Faraday *, in his account of the Alloys of Steel; during his experiments upon which, he found that steel, alloyed with an hundredth part of platinum, was acted upon by dilute sulphuric acid, with infinitely greater rapidity than the unalloyed steel, and that an acid, which scarcely touched the pure steel, dissolved the alloy with energetic effervescence.

It also appears that this electrical action communicates an adhesive attraction to the particles of the metal, by which the particles of liquid and aëriform bodies are entangled and retained, a kind of frothy compound formed, and the fluidity of the mercury destroyed. The appearance of this amalgam is so very like that of the ammoniacal amalgam formed by exposing a solution of ammonia in contact with mercury to the influence of the Voltaic pile, or when an amalgam of potassium and mercury is placed upon moistened muriate of ammonia, that it is impossible not to be struck with the resemblance. I am inclined, indeed, to believe, that the production of the latter may be explained upon the same principles as that of the former. When the effect is produced by the direct application of the electrical current, by means of the battery, it ceases the moment the connexion between the poles is broken; and when brought about by the agency of the amalgam of potassium, the electrical action is doubtless excited by the contact of the two dissimilar metals, and the frothy compound lasts no longer than the existence of the potassium in the metallic state. In the action which I have just described, between mercury and finely-divided platinum, the permanence of the metals produces a much more lasting effect, and the soft amalgam may be preserved for a great length of time without altering its appearance. At all events, these results cannot but increase the strong doubts which previously existed concerning the hypothesis of the *metallization of ammonia*, and the supposed compound of mercury and *ammonium*.

* *Philosophical Transactions*, 1822. Part II., p. 262.

ON THE MEANS OF GIVING A FINE EDGE TO RAZORS, LANCETS, AND OTHER CUTTING INSTRUMENTS.

By THOMAS ANDREW KNIGHT, Esq., F.R.S.,
President of the Horticultural Society, &c.

IN the preparation of steel, and in the art of subsequently forming it into cutting instruments, the British manufacturers are, I believe, unrivalled; and they have probably approximated, if they have not attained, perfection: but in the art of giving the finest possible edge to their instruments, when formed, I think that they have generally still something to learn; for I hear surgeons often complaining, that they rarely find themselves in possession of a perfectly well set instrument; and I have never yet, in any instance, seen a razor come from a cutler so set that I could use it with any degree of comfort, though I have obtained razors from many of the most eminent manufacturers of the metropolis. The machinery which they employ has long appeared to me to be imperfect and uncertain in its mode of operating, and in many respects inferior to that which I have been some years in the habit of using, and which I shall proceed to describe.

This consists of a cylindrical bar of cast steel, three inches long without its handle, and about one-third of an inch in diameter. It is rendered as smooth as it can readily be made with sand, or, more properly, glass-paper, applied longitudinally; and it is then made perfectly hard. Before it is used, it must be well cleaned, but not brightly polished, and its surface must be smeared over with a mixture of oil and the charcoal of wheat straw, which necessarily contains much siliceous earth in a very finely reduced state. I have sometimes used the charcoal of the leaves of the *Elymus arenarius* and other marsh grasses; and some of these may probably afford a more active and (for some purposes) a better material; but upon this point I do not feel myself prepared to speak with decision.

In setting a razor, it is my practice to bring its edge (which must not have been previously rounded by the operation of a strop) into contact with the surface of the bar at a greater or

less, but always at a very acute angle, by raising the back of the razor more or less, proportionate to the strength which I wish to give to the edge; and I move the razor in a succession of small circles from heel to point, and back again, without any more pressure than the weight of the blade gives, till my object is attained. If the razor have been properly ground and prepared, a very fine edge will be given in a few seconds; and it may be renewed again, during a very long period, wholly by the same means. I have had the same razor, by way of experiment, in constant use during more than two years and a half; and no visible portion of its metal has, within that period, been worn away, though the edge has remained as fine as I conceive possible; and I have never, at any one time, spent a quarter of a minute in setting it. The excessive smoothness of the edge of razors thus set led me to fear that it would be indolent, comparatively with the serrated edge given by the strop; but this has not in any degree occurred; and therefore I conceive it to be of a kind admirably adapted for surgical purposes, particularly as any requisite degree of strength may be given with great precision. Before using a razor after it has been set, I simply clean it on the palm of my hand, and warm it by dipping it into warm water; but I think the instrument recommended operates best when the temperature of the blade has been previously raised by the aid of warm water.

A steel bar, of the cylindrical form above described, is, I think, much superior to that of a plane surface for giving a fine edge to a razor or penknife; but it is ill calculated to give a fine point to a lancet; and I therefore cause a plane surface to be made, a quarter of an inch wide, on one side of the bar, by cutting away a part of its substance; and I have found this form to be most extensively useful.

The edge of some razors, whether formed of wootz, of mixed metals, or of pure steel, but particularly of mixed metals, has generally appeared to me to be more keen and active when used a few seconds after it had been applied to the bar, than on the following day; and I have often seen the utmost activity restored to the edge of such instruments, so instantaneously, and by so apparently inadequate means, that I have been

sometimes led to suspect the operation of the bar to have been something more than that of having worn away a minute portion of the metal: but I am not disposed to offer any conjectures respecting other effects which I may have conceived it to produce.

I have in many instances been able to give a very fine edge to razors in possession of my friends, which I could not set tolerably well by any of the ordinary means; and I have found that those composed of different materials could be set with equal facility, though the sensations they excited, when used, appeared to me to be in many instances dissimilar. The instruments upon which I have chiefly made experiments have come from the manufactories of Mr. Pepys, Mr. Stoddart, and Mr. Kingsbury. The material which appeared to me to receive that which I shall call the most eager edge (and it was very durable) was wootz, from the manufactory of Mr. Pepys; and that which received the smoothest edge, and which I thought best calculated for surgical purposes, was the mixture of rhodium and steel; the powers of the pure steel of Mr. Kingsbury appeared to be intermediate: and my experience leads me to believe that, under different circumstances, each of these materials might be used with some exclusive advantages.

ON THE PECULIAR HABITS OF CLEANLINESS IN SOME ANIMALS, AND PARTICULARLY THE GRUB OF THE GLOW-WORM.

By J. RENNIE, A.M., A.L.S.

In an excursion, for the purposes of natural history, to the woods in the vicinity of Dartford, in Kent, the 14th of last March, I found an insect, which I had not hitherto met with, creeping upon the mossy trunk of an oak, which, besides, was entwined with honeysuckle; and, near the bottom, a fern plant was rooted amongst the decaying bark. This insect much resembled the female glow-worm in external appearance, but it was considerably longer, and the colours different. Its head, though small, was formed like those of the grubs of pre-

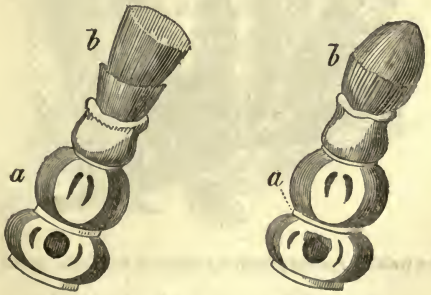
daceous beetles, whence I conjectured it might belong to some of their numerous families; but lest I might be deceived in this, and that after all it might be a vegetable feeder, I put some of the oak bark, moss, fern, and honeysuckle, along with it into a collecting-box. Into the same box I afterwards put several specimens of small snails, with pellucid shells, which I found in the same locality—a circumstance which led me to the discovery of one of those facts that, after eluding direct research, are often the result of accident.

It was not till next day that I looked into the box, when I perceived that none of the vegetable substances had been touched, for the snails had glued themselves to the lid, according to their usual custom when put into a dry place; and though the little stranger was sufficiently lively, and walked about in all directions, nothing within reach appeared to suit its taste. After watching it for some time, my attention was drawn to some very singular movements which it made with its tail, and which the reader will understand better if he has observed how the common earwig, or the insect popularly called the devil's coach-horse, (*Goërius olens*, Stephens,) bends up its tail over its back, somewhat in the manner of a spaniel when it trips along well pleased before its master. The forked tail of the earwig, however, as well as that of the goërius, is said to be used in assisting to unfold its long and closely-folded wings, an operation which I have never myself witnessed; but as the strange insect had evidently no wings, this could not be the design of the movements to which I have alluded. I have more than once seen a female moth strip the down from her body to furnish her eggs with a warm covering, for which purpose she bent in the required directions an instrument like a pair of tweezers, situated at the extremity of the tail; but in the instance in question this could not be the case, as there was no down on the body: and yet, upon closer inspection, it seemed to be pulling off something very assiduously from the parts upon which the extremity of the tail was turned back.

There appeared to be something so uncommon in these movements, that my curiosity was excited to observe them more minutely; and as the creature was not at all timid, I could easily observe it through a glass of some power. The

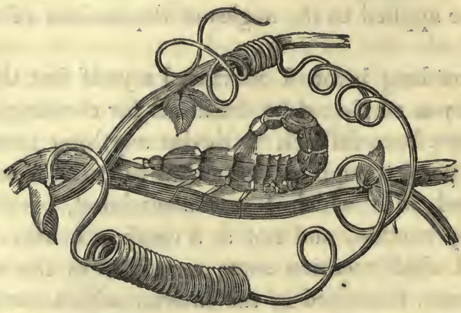
caudal instrument I discovered, by this means, to consist of a double row of white cartilaginous rays, disposed in a circle, one row within the other; and, what was most singular, these were retractile, in a similar manner to the horns of the snail. The rays were united by a soft, moist, gelatinous membrane, but so as to be individually extensile; one or two being frequently stretched beyond the line of the others. The rays were also capable of being bent as well as extended, and they could therefore be applied to the angles or depressions of an uneven surface.

It was not long before I convinced myself that this singular instrument was employed by the insect for cleaning itself; and it would have been difficult to devise anything more effectual for the purpose, though its action was different from all others of this kind with which I was acquainted, inasmuch as it operated by suction, and not as a comb, a brush, or a wiper, of which I shall mention some examples in the sequel. It was, moreover, furnished in the interior with a sort of pocket, of a funnel shape, formed by the converging rays, into which was collected whatever dust or other impurities were detached from the body, till it could hold no more, when, by a vermicular movement of the rays, the accumulated pellet was extruded, and placed with great care in some place where it might be out of the way of again soiling the glossy skin of the insect. This skin, if I may call it so, was of a soft, leathery appearance; exhibiting, when magnified, a minute delicate dotting, similar to shagreen—but to the naked eye this was not apparent.



Magnified views of the cleaning instrument, *open* and *closed*. *a*, the under side of the body; *b*, the cleaning instrument.

The instrument just described, accordingly, when expanded over a portion of this shagreened surface, was subsequently drawn out, with an evident effort, (repeated, if necessary,) in the same way as boys draw their moist leather suckers, when they amuse themselves in dragging stones after them. Every particle of dust or other extraneous matter is thus detached from the skin, and, by a peculiar movement of the retractile rays, is lodged in the funnel-shaped pocket.



Larv of the glow-worm on a tendrilled branch, using its cleaning instrument.

This singular instrument is also used for the very different purpose of assisting the animal to walk, and particularly to maintain a position against gravity, which its feet are ill calculated to effect; though its habits, as we shall presently see, render it in some measure indispensable.

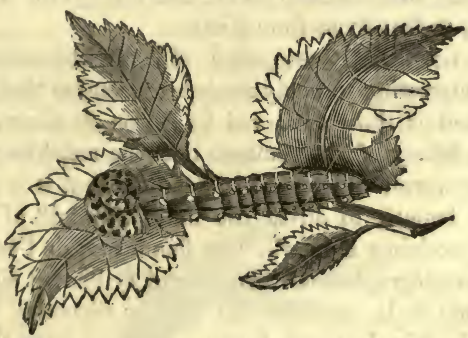
Larva walking against gravity by means of the funnel at the anus.

The interest which I began to take in the insect induced me to endeavour to ascertain its species; and on turning over the

voluminous work of Baron de Geer, I found it was accurately described and figured by him as the grub (*larva*) of the female glow-worm, (*Lampyris noctiluca*;) but though he had bred several of these, he does not seem to have observed their singular mode of cleaning themselves, which I have just described. He was also unsuccessful in discovering their peculiar food. 'I know not,' says he, 'what it eats; but the form of its teeth would make me suppose it to be carnivorous. It lived with me on moist earth, where I strewed grass and the leaves of various plants; having remarked that it became feeble and languishing when I failed to supply it with moisture*.'

Two of the most celebrated French naturalists of the present day make a similar statement respecting its food. 'It is believed,' says Dumeril, 'that the glow-worms are carnivorous in the perfect state, but that their grubs (*larvæ*) feed on vegetables—what, is unknown†.' 'This grub,' says Latreille, 'though furnished with strong jaws, (which would indicate that it is carnivorous,) feeds upon grass, and leaves of various plants‡;' but I doubt whether this is not a hasty and unwarrantable inference from De Geer.

The actual food of the grub in question shews, in a very striking point of view, the design of Providence in furnishing it with the instrument which I have described. I was not a little surprised one day to observe the creature moving about with one of the little snail-shells on its head, and could not

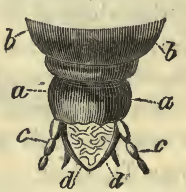


Larva feeding on a small snail.

* De Geer, Mem. Insectes, vol. iv., p. 48.
† Dict. des Sciences Naturelles, vol. xxv., p. 21
‡ Nouveau Dict. d'Histoire Naturelle, vol. xvii., 284.

conjecture what had made it take a fancy to so singular a helmet; but I soon perceived that it was in fact making prey of the poor snail—having, for that purpose, thrust its narrow extensile head half to the bottom of the shell, which it did not quit till it had devoured the inhabitant.

It was thus proved to me that it was not a vegetable feeder, but carnivorous; and I subsequently found, upon trial, that it would touch no animal except snails. Its head, from being extensile, is well adapted for pursuing its prey to the inmost recesses of their shells; and its mandibles, which are curved in form of a pair of calliper compasses, appear, as in the instance of the grub of the ant-lion (*Myrmeleon formicarius*), to be employed rather for sucking than for eating, though I was unsuccessful in satisfactorily ascertaining this point.



Head of the glow-worm grub. *a*, the head; *b*, the neck; *c*, the antennæ; *d*, the jaws.

It is more to the present subject to mention, that the grub cannot well devour one of its victims without being soiled with slime; and accordingly, after every repast, I observed that it went carefully over its head, neck, and sides, with its cleaning instrument, to free them from slime.

Though not directly connected with my immediate subject, it may be interesting to many readers to mention that the above grub, as well as those observed by Baron de Geer, distinctly proved the fallacy of the common doctrine respecting the light of the glow-worm, which goes to maintain that it is a lamp, lit up by the female, to direct the darkling flight of the male. 'Ce sont,' exclaims Dumeril, 'les flambeaux de l'amour—des phares—des télégraphes nocturnes—qui brillent et signalent au loin le besoin de la reproduction dans le silence et l'obscurité des nuits *.' Mr. Leonard Knapp, refining upon this notion, conjectures that the peculiar conformation of the head of the

* Dictionnaire des Sciences Naturelles, xxv. 216.

male glow-worm is intended as a converging reflector of the light of the female, ' always beneath him on the earth.' ' As we commonly,' he adds, ' and with advantage, place our hand over the brow, to obstruct the rays of light falling from above, which enables us to see clearer an object on the ground, so must the projecting hood of this creature converge the visual rays to a point beneath *.'

Unfortunately for this theory, the grubs—which, being in a state of infancy, are therefore incapable of propagating—exhibit a no less brilliant light than the perfect insect. De Geer says the light of the grub was paler, but in the one which I had it was not so. He also remarked the same light in the nymph state, which he describes as ' very lively and brilliant;' and, in this stage of existence, it is still less capable of propagating than in that of larva. ' Of what use then,' he asks, ' is the light displayed by the glow-worm ? It must serve some purpose yet unknown. The authors who have spoken of the male glow-worms say positively that they shine in the dark as well as the females †.' These plain facts appear completely to extinguish the poetical theory. But to return to our immediate subject.

A very remarkable instrument, which recent observations seem to prove to be intended for a similar purpose to that of the caudal apparatus of the glow-worm, just described, occurs in the fern-owl, or night-jar (*Caprimulgus Europæus*), popularly called the goat-sucker, from an erroneous notion that it sucks goats—a thing, which the structure of its bill renders impossible as that of cats sucking the breath of infants, as is also popularly believed. The bird alluded to has the middle claw cut into serratures, like a saw or a short-toothed comb; the use of which structure seems to have been misunderstood by White of Selborne.

Foot of the European night-jar, shewing the pectinated claw.

* Journal of a Naturalist, p. 292, first edition.

† De Geer, Mem. iv. 44.

'If it takes,' says he, 'any part of its prey with its foot, as I have the greatest reason to believe it does chafers, (*Zantheumia solstitialis*, Leach, ms.,) I no longer wonder at the use of its middle toe, which is curiously furnished with a serrated claw*.' Mr. Dillon has recently controverted this opinion; his observations leading him to suppose that the serratures are employed by the bird to comb its whiskers (*vibrissæ*)†. Mr. Swainson, again, a high authority on such a subject, thinks that the fact of an American group of the same birds (*Caprimulgidæ*), which have no whiskers to comb, and an Australian group, which have whiskers, but no serratures on the claws, are discordant with Mr. Dillon's opinion ‡. It frequently happens, however, that the most ingenious and apparently incontrovertible reasoning in natural history, is overturned or confirmed by facts accidentally observed. I was, I confess, disposed to think Mr. Dillon's opinion more plausible than true, and to agree with White, and the learned arguments of Mr. Swainson, till I met with some observations of the distinguished American ornithologist, Wilson, upon some of the transatlantic species. In his description of the whip-poor-will (*Caprimulgus vociferus*), he says, 'the inner edge of the middle claw is pectinated, and, from the circumstance of its being frequently found with small portions of down adhering to the teeth, is probably employed as a comb, to rid the plumage of its head of vermin, this being the principal and almost the only part so infested in all birds §.'

Of another species, called chuck-will's-widow (*C. Carolinensis*), he says, 'their mouths are capable of prodigious expansion, to seize with more certainty, and furnished with long hairs or bristles, serving as palisades to secure what comes between them. Reposing much during the heats of the day, they are much infested with vermin, particularly about the head, and are provided with a comb on the inner edge of the middle claw, with which they are often employed in ridding themselves of these pests, at least when in a state of captivity ||.' Considering the utility of such an instrument, we may wonder,

* Nat. Hist. of Selborne, i. 160. Ed. Lond. 1825.

† Loudon's Mag. of Nat. Hist. ii. 31. ‡ Ibid. iii. 188.

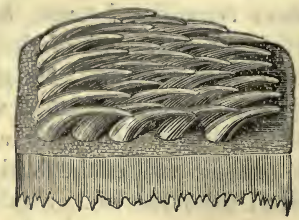
§ Wilson's American Ornithology, v. 77. || Ibid. vi. 97.

perhaps, that, besides the herons (*Ardeæ*), no other birds are similarly provided for attacking those troublesome insects (*Homaloptera*, MACLEAY, *Nirmidæ*, LEACH, &c.), which often seriously injure the vigour and health of the animal infested, and sometimes even occasion death. On going to visit the ruins of Brougham Castle, in Cumberland, I was struck by the unusual tameness of a swallow (*Hirundo rustica*), which I found sitting on the parapet wall of the bridge which crosses the Emont, on the road from Penrith. Swallows are, indeed, far from being generally shy, trusting, perhaps, to their rapidity of flight should danger threaten; but this poor swallow allowed itself to be approached, without offering to escape. It seemed, in fact, instinctively courting human aid, at least I was inclined so to interpret its pitiful looks. On taking hold of it, I found the feathers swarming with an insect (*Craterina Hirundinis*, OLFERS) somewhat larger in size than the common house-bug (*Cimex lectularius*). I took the poor bird immediately to the river; and, on being freed from its tormentors, it flew off joyfully to join its companions. Had it been furnished with a comb, like the night-jars, it would not probably have needed my assistance.

It may not fall in the way of many of the readers of this paper to make personal observations on the foot-comb of the night-jar; but similar instruments, of still more ingenious construction, may be inspected, by whoever will take the trouble, in two of our most common animals—the cat and the house-fly (*Musca domestica*), both of which may very frequently be seen cleaning themselves with the utmost care. The chief instrument employed by the cat is her tongue; but when she wishes to trim the parts of her fur which she cannot reach with this, she moistens, with saliva, the soft spongy cushions of her feet, and therewith brushes her head, ears, and face, occasionally extending one or more of her claws to comb straight any matted hair that the foot-cushion cannot bring smooth, in the same way as she uses her long tusks in the parts within their reach.

The chief and most efficient cleaning instrument of the cat, however, is her tongue, which is constructed somewhat after the manner of a currycomb, or rather of a wool-card, being beset with numerous horny points, bent downwards and back-

wards, and which serve several important purposes, such as lapping milk, and filing minute portions of meat from bones.



Magnified view of a portion of the upper surface of the Cat's Tongue.

But what falls chiefly to be noticed here, is its important use in keeping the fur smooth and clean; and cats are by no means sparing in their labour to effect this. The female cat is still more particular with her kittens than herself, and always employs a considerable portion of her time in licking their fur smooth. The little things themselves, also, begin, when only a few days old, to perform the office for themselves; and I have observed the half-fledged nestlings of the black cap (*Sylvia atricapilla*), and a few other birds, preening their feathers as dexterously almost as their dam herself could have done.

It requires the employment of a microscope of considerable power, to observe the very beautiful structure of the foot of the two-winged flies (*Muscidæ*), which still more closely resembles a currycomb, than the tongue of the cat does. This structure was first minutely investigated by Sir Everard Home and Mr. Bauer, in order to explain how these insects can walk upon a perpendicular glass, and can even support themselves against gravity. Of the structure of the foot of flies, considered as an instrument for cleaning, I have not hitherto met with any description in books of natural history, though most people may have remarked flies to be ever and anon brushing their feet upon one another, to rub off the dust, and equally assiduous in cleaning their eyes, head, and corslet with their fore-legs, while they brush their wings with their hind legs. In the common blow-fly (*Musca carnaria*), there are two rounded combs, the inner surface of which is covered with down, to serve the double purpose of a fine brush, and to assist in forming a vacuum when the creature walks on a glass, or on the ceiling of a room. In some species of another family (*Tipulidæ*), there are

A, side view of the last joint of the leg of the blue-bottle fly (*musca vomitoria.*) B, do. of the fever fly (*bibio febrilis.*) Both figures magnified 100 times.

three such combs on each foot. It may be remarked, that the insects in question are pretty thickly covered with hair, and the serratures of the combs are employed to free these from entanglement and from dust. Even the hairs on the legs themselves are used in a similar way; for it may be remarked, that flies not only brush with the extremities of their feet, where the curious currycombs are situated, but frequently employ a great portion of their legs in the same way, particularly for brushing one another.

Birds are peculiarly distinguished for their cleanliness, which appears to be instinctive; that is, it becomes apparent very soon after they are hatched, at least in those nestlings which are at first blind; the others (*Gallinæ*, &c.) do not so much requireit, from their running off immediately out of the nest after their dam. The parents of blind nestlings are particularly careful in watching, after feeding, till they moot, carrying it off in their beaks, an office which they even perform for the female while she is hatching. I have particularly remarked this in the common starling (*Sturnus vulgaris*), a thing the more necessary, from the bird nestling in the holes

of trees; and Colonel Montague observed it in the gold-crested wren (*Regulus cristatus*, Ray,) in the instance of a nest of young which were fed by the parents after being carried into a room*. 'In birds,' says White, 'there seems to be a particular provision, that the moot of the nestlings is enveloped in a tough kind of jelly, and therefore is the easier conveyed off without soiling or daubing. Yet, as Nature is cleanly in all her ways, the young perform this office for themselves in a little time, by thrusting their tails out at the aperture of their nests †.' Another delightful writer says, 'birds are unceasingly attentive to neatness and lustration of their plumage. Some birds roll themselves in dust, and occasionally particular beasts cover themselves with mire; but this is not from any liking or inclination for such things, but to free themselves from annoyances, or to prevent the bites of insects ‡.'

I may be permitted to illustrate one of these remarks of Mr. Knapp, by mentioning the fact, that in some parts of Africa the elephant and the rhinoceros, in order to protect themselves from flies, roll themselves in mud, for the purpose of forming an impenetrable crust upon their skin when it becomes dry. Their most formidable insect pest, according to Bruce, is a fly called *Isaltaya*, belonging, it would appear from the description, to Clairville's *Haustellata*. It is said not to be larger than a common bee, but is more terrible to those two animals than the lion himself. It has no sting, but insinuates its sucker (*haustellum*) through the thickest skin, in the same manner as our cleg (*Hæmatopota pluvialis*, Meigen) does. The effects of this sucking are such, that the part not only blisters, but frequently mortifies, and in the end destroys the animal; but the coating of dried mud over the skin affords them effectual protection, and therefore cannot be justly quoted as an instance of their dirty habits. It is highly probable, as it appears to me, that the proverbially unclean habits of swine may be referred to a similar origin, particularly as no animal is more careful to have its bed clean and dry.

There is another family of animals no less repulsive to the feelings of many people, though not proverbially dirty as the

* *Ornith. Dict.* Introd. † *Nat. Hist. of Selborne*, i. 269.

‡ Knapp, *Journ. of a Naturalist*, 311.

swine, which I have discovered to be peculiarly cleanly; I refer to the several species of spiders. During the course of a series of observations and experiments on the process by which they can shoot lines of their gossamer silk across a brook, or other intervening obstacle, it was indispensable that I should pry with minute attention to their every movement; and I was soon struck with one which interested me not a little, in the instance of one of the long bodied species, (*Tetragnatha extensa*, Latreille.) It appeared to be mumbling, if I may use the term, its legs between its mandibles, drawing each leisurely along, as a dog may be seen to gnaw a bone when not very much in earnest, but more by way of pastime than of making a dinner. I could not at first account for this; the ancient naturalists, who drew largely on their imagination when facts failed them, would at once, I have no doubt, have leapt to the conclusion, that the spider, in default of prey, actually devoured its own legs, as it has been asserted to do its web*.

A little attention convinced me, that the movements alluded to were precisely of the same kind as the preening of birds. Spiders have their legs more or less covered with sparse hair, which, being rather long and bristly, is apt to catch up bits of their own web and other extraneous matters, and these, from the delicacy of their semi-transparent skin, must produce uncomfortable irritation. To free themselves from this is one of their daily occupations; and when a spider appears to the less minute observer to be quite at rest, it will often be seen, on close inspection, to be assiduously and slowly combing its legs in the manner I have above mentioned. The term *combing* is very appropriate in the instance of the common garden-spider (*Epeira diadema*), which is furnished with a set of teeth somewhat in form of a comb; but it has another instrument in addition to this, peculiarly useful in the process, consisting of a smooth and somewhat curved horny needle, which bends over the teeth of the comb, and holds the limb which it is dressing more firmly down, as if, after entering it in the hair, we were to apply a finger over the edge of one of our artificial combs. In some other spiders (*Dysdera erythrina*, &c.), there is, in the situation of the comb just described, a closely set brush of

* Bloomfield's *Remains*, vol. ii.

thick hairs, which is employed in the same way. Any person who will take the trouble may readily verify these observations by confining a spider in a wine-glass, placed in a saucer filled with water, from which it cannot escape, so long as there is no current of air to carry off a silken line for a bridge.

Those who have paid attention to ants, may have remarked that a pair of them may be often seen touching one another with their antennæ, and even passing their tongues over part of each other's bodies, in the same way as they are seen to do with their eggs, larvæ, and pupæ, erroneously imagined by the ancients to be hoarded grain. The necessity which they are under of moving these to various parts of the colony, in consequence of variations in the weather, must often expose them (polished though they be) to soiling; but the careful nurses instantly remove every thing of this sort with their mandibles, or tongue —movements which have been misinterpreted, as licking the pupæ into shape; as the bear is no less erroneously asserted to do by her cubs. In all such cases, cleanliness seems to be the chief, if not the sole, motive; as those mutual caresses of the working ants are, I think, for the same purpose. These, indeed, remind me strongly of the common practice of horses and cows of cleaning each other's necks and heads, which the individual cannot itself reach with its tongue; and, in the same way, caged birds will sometimes do the friendly office to a fellow-prisoner, of pecking off anything extraneous adhering to the head or the bill, where preening is impossible, and the foot is seldom well adapted to the purpose.

Such are a few of the illustrations which have suggested themselves to me upon this subject: should they be found interesting, I may probably add a few more at a future opportunity.

Lee, Kent, 1*st July,* 1830.

DESCRIPTION AND APPLICATION OF A TORSION GALVANOMETER.

By WILLIAM RITCHIE, A.M., F.R.S.

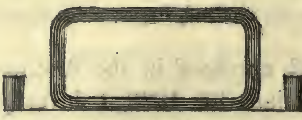
Assoc. Mem. S. A. for Scotland, Rector of the Royal Academy of Tain.

In a paper which appeared in the first part of the Philosophical Transactions for 1830, I investigated the elasticity of threads of glass, and applied that property to the construction of a delicate and accurate galvanometer. The instrument then described, though sufficient for most purposes, requires some modification to adapt it to researches of extreme delicacy. The description of the instrument, in this more perfect state, with a few of its numerous applications, will form the subject of this communication.

For experimental researches in electro-magnetism, it is extremely useful to have constantly at hand a quantity of copper wire, of different degrees of fineness, coated with sealing-wax. The most convenient mode of giving the wire this coating, is the following:—Stretch the wire between two supports, heat it gradually, from one end to the other, with an iron bar, or spirit-lamp, and continue rubbing the heated part with a stick of sealing-wax; the wire will receive a fine coating, sufficient to prevent metallic contact when portions of it are pressed together in the construction of any piece of electro-magnetic apparatus.

Take the wire thus coated, heat it slightly to prevent the wax cracking, and form it into a rectangular shape, consisting of six, eight, or ten repetitions of the wire, according to the delicacy of the instrument required. The upper side of the rectangle must then have the wires separated into two equal portions, bent round a small cylinder, and then continued straight, so as to leave a circular opening in the middle, about one-third of an inch in diameter. The use of the circular opening, in the upper side, is to allow a slender axis, carrying the magnetic needles, to pass through it, in order to increase the power of the instrument, and render the compound needle

astatic. Portions of a brass tube, about an inch long, are to be soldered to the ends of the wires forming the rectangle, for the purpose of holding a small quantity of mercury, to render the metallic contact complete. The annexed cuts exhibit a vertical section of the rectangle, and a horizontal one of its upper side.



The wires, forming the rectangle, are pressed close together, and secured by a waxed sewing-thread, rolled tightly round them. The rectangle is then fixed in a rectangular box, having the upper side formed of two sliding panes of window glass, for the purpose of shutting up the needle from the agitation of the air. Each pane has a small semicircle cut out of the middle of the edge, by means of a round file, so as to leave a circular opening directly above that in the rectangle. Various contrivances for suspending the magnetic needle might be adopted. The following is perhaps the most convenient:—Into a strong wooden sole, or base, fix two upright supports about three feet long. A small stage at the top, having a divided circle on its upper side, and which may be elevated or depressed at pleasure, completes the frame of the instrument. The stage has two holes of the same size as the supports, and at the same distance, with two small screws passing through its sides, opposite the centres of the openings, for the purpose of fixing the stage securely at the proper height. A small cylindrical wooden key or peg, having a small bore in the axis for the purpose of receiving the end of the glass thread, passes through the centre of the divided circle, and is made to turn easily, without much friction.

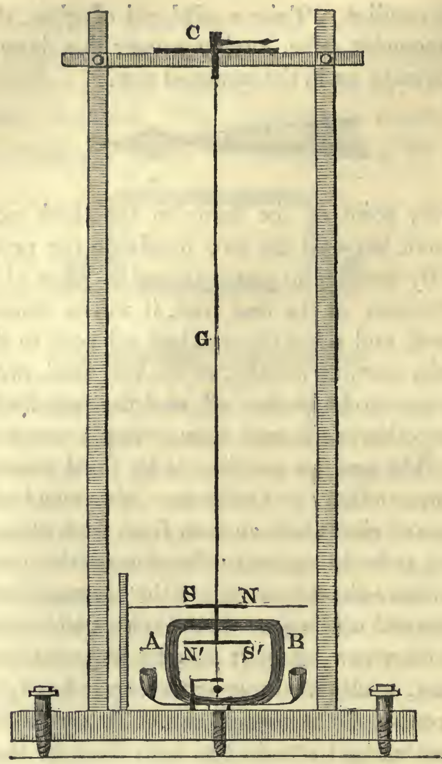
After numerous trials, the following appears to me the best mode of preparing the threads of glass, so as to have their extremities somewhat thick and tapering, for the purpose of securing them in the torsion key, and in the axis which carries

the magnetic needles. Take a solid rod of glass, or a piece of a clean thermometer tube, having a very fine bore, and draw out one of its ends, as in the annexed cut.

A

Direct the very point of the flame on the thick portion at A, and pull it out, between the two hands, to the proper length. As it is hardly possible to get a thread of glass of the proper length and fineness, at the first trial, it will be found necessary to draw several, and select the one best adapted to the purpose.

Two slender darning needles, of the best steel, are then to be selected, the eyes to be broken off, and the ends filed to a point similar to the other ends, and then strongly magnetised in the usual way. The needles are then to be fixed transversely in a piece of straw, or other light substance, about an inch long, and at the distance of about half an inch from each other, with their corresponding poles in opposite directions—the one needle intended to be above the upper side of the rectangle and the other below it. One end of the glass thread is then to be securely fixed in the end of the straw, or light axis, by means of strong cement or sealing-wax, whilst the other extremity is fixed, in like manner, in the centre of the torsion key. A single fibre of silk, having a small weight attached to it, is fixed to the lower end of the axis, and made to pass through a small hole near the lower side of the rectangle, for the purpose of keeping the axis carrying the needles, in the centre of the circular opening in the coil. The upper needle has two pieces of fine straw, several inches long, fixed on its ends, so that the slightest deflection may be readily observed. The extremity of one of the straws is made to oscillate between two upright pieces of glass, to prevent the needle moving over an extensive arc, and thus lengthen the time necessary to complete an observation. The whole will be obvious from the simple inspection of the annexed vertical section of the instrument, in which A B is the rectangular coil of wire, NS, S′ N′, the magnetic needles; C, the stage with the divided circle and torsion key, and G the glass thread. If, instead of the glass thread, the needle be



suspended by a single fibre of silk, the instrument becomes a galvanoscope of extreme delicacy. The following experiment affords a striking illustration of the extreme sensibility of the instrument with this modification.

EXPERIMENT I.

File off a few grains from a piece of zinc and copper by means of a coarse file; place two of these near each other in the bottom of a clean watch glass; bring the clean ends of two fine copper wires, connected with the cups of the galvanometer, in contact with them, and then drop over them a small quantity of dilute acid, and the compound needle will be deflected several degrees.

The instrument by which I ascertained the existence of a Voltaic current from this elementary battery, consisted of a greater number of coils in the rectangle, and the needles were light and strongly magnetised.

Having thus minutely described the torsion galvanometer, I will now shew some of its applications; but before doing this it may be thought necessary to establish its accuracy, not by reasoning (which is already done in the *Philos. Trans.*), but by direct experiment. The following experiments will shew in a striking manner the perfection of this instrument above those formerly employed.

EXPERIMENT II.

Take two equal rectangular slips of copper and zinc, an inch broad and eight or ten inches long, and divide them into square inches by narrow bands of wax or cement. Solder copper wires to their extremities, and fix them in a small frame, so that they may always be placed at the same distance from each other. Immerse them in a vessel of water, containing a small quantity of sulphuric acid, to the first horizontal division; turn round the torsion key till the untwisting force of the glass thread balances the deflecting power of the electric current, and note the number of degrees of torsion. Immerse them to the second division, turn round the torsion key as before, and the degrees of torsion necessary to balance the deflecting force of the current, from two square inches, will be found double of those for one square inch. Repeat the experiment with three, four, &c., square inches, and the degrees of torsion will be found to be proportional to the surface of the plates immersed.

Having thus shewn experimentally the accuracy of the instrument, I shall now apply it to determine the power gained by Dr. Wollaston's contrivance of a Galvanic battery above those formerly in use.

EXPERIMENT III.

Having provided a clean slip of copper, two inches broad and about four inches long, I formed it into a rectangle, open

at the top, and then covered the inner surface of the bottom with cement. A plate of zinc, of the same size with the rectangle of copper, was placed exactly in the middle, having a face of clean copper opposite each of the sides of zinc. Copper wires being soldered to the rectangle of copper and to the plate of zinc, and their ends dipped into the small metallic cups of the galvanometer, the elementary battery was then immersed in very dilute acid, and the torsion key turned till the deflecting force of the battery was vanquished, the number of degrees being about a thousand. Having removed the battery, I covered one side of the plate of zinc and the opposite surface of copper with cement, and repeated the experiment as before; when, as might naturally be expected, the number of degrees of torsion were found to be very nearly five hundred. We may, therefore, safely conclude that the double plate of copper doubles the *quantity* of electricity without, of course, altering its *tension*.

Immediately after Œrsted's beautiful discovery of the mutual action of magnets and Voltaic conductors, it was known that an immense increase of electro-magnetic power is gained by diminishing the distance between the copper and zinc plates; but, for want of a proper galvanometer, the law does not seem to have been determined with that rigorous accuracy which places its truth beyond the possibility of doubt. To accomplish this was the object of the following experiment.

EXPERIMENT IV.

In order to avoid every source of inaccuracy, I procured a rectangular wooden box, about a foot long, two inches broad, and two and a half inches deep, into which plates of zinc and copper two inches square might be fixed at any distance from each other. Having filled the box with dilute acid, I placed the copper plate at one extremity and the zinc plate at the distance of *nine* inches, and observed the degree of torsion, as in the preceding experiments. I then untwisted the thread, placed the zinc at the distance of *one* inch from the copper, and observed the degrees of torsion, which were now nearly three times as great as before. This was next repeated with the

plates at the distance of nine and four inches, and gave the deflecting forces in the ratio of 2 to 3, which are the square roots of 9 and 4. After trying the effects of the plates at different distances, the following law was established, which had formerly been obtained by a different process: viz.—that the *quantity* of Voltaic electricity circulating along the metallic conductor connecting two plates of dissimilar metals, is inversely as the square roots of the distances between the two plates. This law was originally deduced by Professor Cumming, by observing the deflection of a compass needle, and then taking the deflecting forces as the tangents of the angles of deviation from the original direction of the needle and straight conductor. When I undertook this investigation, it had escaped my memory that any law had been discovered which connected the deflecting force with the distance of the plates. This circumstance, as well as the different process by which it was deduced, affords the most complete proof of its truth.

This law is certainly very different from what we might at first have expected. We might, without experiment, have argued thus: If one inch of fluid between the plates offer a certain resistance to the electric current, two inches will present twice the resistance, three inches three times the resistance, &c. &c. With regard to the cause of this curious law, we can at present scarcely offer a conjecture. Does the electric fluid, after passing through a certain length of an imperfect conductor, acquire some power which enables it to pass more easily through an equal portion? There are phenomena in nature in which imponderable agents do acquire such properties. Light may be so far modified as to pass entirely through glass, which, without such a modification, would have been partly reflected. De Laroche discovered that invisible radiant heat, after passing through a thin plate of glass, passes with less resistance or loss through a second, &c. But, instead of being led away by analogies, which by some may be regarded as fanciful, I shall mention one practical lesson to be deduced from the law in question. In constructing a battery for electro-magnetic purposes, there is not so much power gained as might be supposed by putting the plates very near each other. For example, if the plates are at the distance of a quarter of an

inch, and then at the distance of one-eighth of an inch, the power gained will only be as the square root of .25 to the square root of .125, or nearly as 50 to 35; and the hydrogen constantly escaping, and partially occupying the place of the liquid in the narrow cell, considerably diminishes this apparent increase of power. This circumstance ought not to escape the attention of philosophical instrument-makers in the construction of batteries for electro-magnetic purposes.

Considerable uncertainty still prevails with regard to the law which connects the conducting powers of metallic wires with their lengths. According to Professors Barlow and Cumming, the law is the same as that established for fluid conductors.

According to the experiments of M. Becquerel, the conducting powers of metallic wires are simply as their lengths. The following experiment will set the question at rest.

EXPERIMENT V.

The galvanometer I have hitherto used requires the following modification for this investigation. Form the rectangle of a single copper wire, and suspend the magnetic needle directly over it, and in the same direction. Take a certain length of the same copper wire, and connect it with a small elementary battery, turn the key, and observe the degree of torsion. Take nine times the length of the same wire, and repeat the experiment with the same battery and acid, and the number and degrees of torsion will only be one-third of those obtained in the first experiment. This experiment I repeated with different lengths of bell-wire, and always found that the intensity of the current was inversely as the square roots of the lengths —the same as the law for liquid imperfect conductors.

M. Becquerel seems to have fallen into the mistake we have now pointed out, by using a galvanometer made of a long wire formed into a coil, and neglecting the resistance the electric current must have experienced in passing through the instrument itself.

The conducting powers of metallic wires, or their ribands, for common electricity, depends almost entirely on their surface, without any reference to their thickness. The fact would seem to be, that common electricity glides along the surface of the

metal, being prevented from escaping by the pressure of the ambient air, whereas Voltaic electricity requires a certain thickness of metal for its transmission*. Voltaic electricity, from a single pair of plates, seems to be conducted from molecule to molecule, in some measure resembling the conduction of caloric. Hence, if the diameter of the wire be too fine to allow of this depth of metal, a considerable portion of the electric fluid will be stopped. But, provided the wires be sufficiently thick to allow of this necessary depth of the electric film, then the conducting power ought to be nearly as the circumference of the wire, or as its diameter. If one of the wires be very fine, and the other of a large diameter, this law could not exist. This fact was clearly proved by the following experiment.

EXPERIMENT VI.

Having taken equal lengths of very fine copper wire and of common bell wire, I used them successively as conductors from the same elementary battery, and ascertained the degrees of torsion as in the former experiments, and found that the large wire conducted better than in the mere ratio of the diameters. For example, the diameter of the one wire was scarcely three times that of the smaller, yet the ratio of their conducting powers was nearly as one to four. I then passed the thick wire through rollers, till it was reduced to a very thin riband, having its external surface nearly twice that of the original wire, but instead of conducting double the quantity of the original wire, it conducted only three-fourths of that quantity †.

From the law established in the fourth Experiment, we need scarcely despair of seeing the Electro-Magnetic Telegraph established for regular communication from one town to another, at a great distance. With a small battery, consisting of two plates an inch square, we can deflect finely-suspended needles

* Hence if a metallic rod be raised to a red heat, its power of conducting common electricity is *increased*, whilst its conducting power for Voltaic electricity is considerably *diminished*.

† The fact here established bears a striking analogy to a curious fact discovered by Mr. Barlow. He found that it requires a certain thickness of iron or steel to receive the magnetic influence—Is there any relation between the thickness of the iron or steel necessary to receive the magnetic influence and the thickness of the conductor necessary to convey that kind of electricity which acts most powerfully on the needle?

at the distance of several hundred feet, and consequently a battery of moderate power would act on needles at the distance of a mile, and a battery of *ten* times the power would deflect needles with the same force, at the distance of a *hundred* miles, and one of twenty times the force, at the distance of four hundred miles, provided the law we have established for distances of seventy or eighty feet hold equally with all distances whatever.

PRACTICAL AND PHILOSOPHICAL OBSERVATIONS ON NATURAL WATERS.

By WILLIAM WEST, Esq.

§ 1. *On the Water from Peat Lands, and its application to domestic purposes.*

I HAD an opportunity, some time since, of closely examining many specimens of water from this part of the country, (Leeds,) which were soft and nearly pure, containing from half a grain to two grains of solid matter in the gallon, one part in 50 or 60,000, but tinged by colouring matter from peat. With most of the re-agents no action took place, or it was so slight as to be difficult of detection; but when evaporated until a gallon was reduced to a few spoonfuls, the composition of this small portion was easily shewn to be sufficiently complicated: and it varied greatly in different specimens which, passing in their original state under the action of the tests without any alteration being produced, might have been supposed exactly similar. The fact is, the water precipitated from the rain and mountain mists had taken up small portions of the soluble substances which came in its way; but its course had been too short, and its action too much confined to the earth's surface, to acquire much from any of these. These streams were on high moor land; either running in the ravines, or springing from natural or artificial openings in mill-stone grit.

One practical difficulty of considerable importance arises when water from brooks in such situations is employed, or

when a large quantity of such water has to be collected for the supply of a town. The upland streams, deriving their supply from high and barren land, barren of all but moss and heather, are more or less deeply coloured by vegetable matter derived from peat. There has occasionally been much controversy respecting this peaty water; and among those who have entered keenly into this, both parties have been, I think, somewhat in the wrong. While those are mistaken who condemn, in the gross and for every purpose, water tinged in any degree with peat, or who maintain that it cannot be deprived of this colour, they are equally so who treat such an impregnation as not injurious to any of the useful qualities of the water. What may be the exact effect on the human constitution of the small quantity of soluble vegetable matter, of whatever nature, from which the hill streams derive their colour, I do not pretend to say; but the water is unsightly, not only from the brown tinge, but from the coloured froth formed by the bubbles of air which escape on standing. These, in water in general, break as they reach the surface; but, from the viscidity produced by the peat, they collect and remain, giving an unpleasant and repulsive appearance.

Now, I hold an opinion, which has been confirmed by some experienced medical men, that the salubrity of water, as a beverage, depends less upon its absolute purity than upon its being brisk and palatable. We know how palling to the stomach is water which has been boiled and cooled, or has stood long in open vessels; yet, so far as the term 'pure water' means water free from the presence of other substances than water, such is frequently more pure than when originally drawn. I apprehend, that though much in diet which is agreeable to the palate is at the same time unwholesome, yet that will not commonly perform its part well which is itself positively disagreeable.

Again, in experimenting upon this peaty colour, either as strong as it could be obtained, or in its common and more dilute state, I found it closely to follow the habits of those vegetable infusions which are prepared expressly for the colour they impart. Thus it is found dissolved, not merely suspended, passing any number of times through filtering-paper without

diminution, and not much impairing the transparency or refractive power of the water. It is quickly and completely separated by aluminous earth in a state of minute division; the alumine, at first snow-white, becoming brown, and forming a true lake. It is separated by muriate of tin, in flakes composed of colouring matter and oxide of tin. Many other of its habitudes agree—all, indeed, which I have compared. Thus it more readily leaves the water, and fixes itself on the material boiled or washed in it, when that is of silk or woollen, animal productions—than when linen or cotton, vegetable fabrics. Printed cotton, however, of bright colours, is at once stained and altered, from the mordant of the print combining with or fixing the additional colour. White linen or calico, on the other hand, once washed in pale yellow water, is not perceptibly stained; with deep brown water it is discoloured by the first operation; and the same result takes place from the repeated use of that which, in a single trial, produces no sensible effect.

Though this substance obstinately resists mere filtering, such as would separate suspended impurities, yet sand, containing, as I apprehend, some alumine, is effectual in separating it: but the kind of sand which will filter at once most speedily and effectually; the degree of mixture with clay which will produce the greatest chemical effect without lessening materially the permeability of the sand; the depth of sand required; the fall or pressure which best unites speed and effect;—all these are points calling for experiment, and which, if not well ascertained before attempting to filter on the great scale, may cause much useless delay and expenditure. Long exposure in reservoirs to light and air, assisted, as I believe, by the action of the clay with which they are lined, destroys the colour. The water with which one large town is supplied enters the reservoirs more deeply stained than any of the streams which formed the subject of my experiments; and left them, at the time it was brought to me, less coloured than the water of the river Aire. But I am told that, in winter time, when a flood happens, the water from the surface, leaving each reservoir soon after it enters, is delivered from the pipes to the houses

very much discoloured. Where such water, then, must be used, (and from its softness it has many advantages,) a well-arranged and well-managed filter is highly desirable.

It may serve to shew how cautious we should be in attributing mistakes on scientific points to any writer, from minute criticism of the terms used, to notice that, in an official report from one of the most eminent chemists of the present day, it is stated that the yellow or light-brown peaty water is not, in his opinion, objectionable for 'any domestic purpose.' He undoubtedly used the term in a limited sense, confining it to the preparation of food, and to its power as a simple detergent, without taking notice of the probability of its leaving a colour of its own. Again, the Commissioners of 1825, on the Supply of Water to the Metropolis, in their able report, say, 'It must, however, be recollected, that insects and suspended impurities only are removed by filtration; and that, whatever substances may be employed in the construction of filtering-beds, the purity of the water, as dependent upon matter held in a state of solution, cannot be improved by any practicable modification of the process,' &c. &c. Now this, as I have proved by experiment, is not applicable to some dissolved animal and vegetable matters: it can only be strictly true as applied to the salts contained in water; and though undoubtedly correct in the general as to these, yet exceptions are still possible.

Besides the superiority of filtering over mere subsidence, for the mechanical separation of impurities, I think enough attention has not been paid to the power of alumine to separate both animal and vegetable matter, however perfectly dissolved. I evaporated deep-coloured peaty water, previously filtered and very bright, and obtained at the rate of 1.6 grains from one pint. On calcination, these 1.6 grains were reduced to 0.6. About 0.8 grains of the quantity thus dissipated was vegetable matter; 0.2 or upwards was carbonic acid, from carbonate of lime. I then separated the colouring matter by well-washed alumine; the water was left perfectly limpid: on evaporation it left one grain, which, on calcination, became 0.6, as before: allowing for the carbonate of lime decomposed by the heat, not less than four-fifths of the vegetable matter had separated in combination with alumine.

I exposed a weak solution of gelatine to similar treatment, with correspondent results: the greater part was separated in the same manner; the water evaporated left little but the salts contained in the jelly. I ascertained that common clay produced, by allowing longer time, the same changes in appearance, and that the weight of the dissolved matter was less than before; but I could not easily free the clay so entirely from salts as to bring the proportion separated to the same degree of certainty by weighing.

§ 2. *On the deposition of Sulphate of Lime from Hard Waters, and on the Solvent Power of Hard Waters.*

Sulphate of lime, being held in waters by its own solubility, cannot be wholly separated by mere boiling: on applying heat to its solution, one of two effects takes place: if the evaporation is slow, the solution is left more concentrated and stronger; if it be boiled briskly, the solution may remain of the same strength though not saturated, while a portion of the sulphate separates in a solid form. The manner in which this proceeds is curious; the property is common in a greater or less degree to all substances difficultly soluble, though lime itself is the most striking instance. When a bubble of steam arises, the salt which was dissolved in that portion now vaporized, separates in the solid state; and as such bodies require not only a large portion of water, but a long time to effect solution, before this is again dissolved, many other particles are separated, and thus the quantity deposited goes on increasing, the strength of the solution itself remaining all the time nearly the same, though any difference which may take place is of course in the way of increase.

Thus in the production of 'fur,' in the vessels in which it is boiled, the sulphate of lime acts about as speedily as the carbonate, and probably more injuriously. In many operations, therefore, it becomes a very serious evil. I was assured at Manchester that it was necessary frequently to empty the engine boilers, and chip out the crust formed, in some cases as often as once in six weeks; the labour of effecting this, and the hin-

derance to work, and loss of fuel consequent on letting out the fire, are not the only disadvantages attendant upon the circumstance. The boilers must be more quickly destroyed, from the great heat of the outside being very slowly conducted through the earthy crust. In fact, they told me that for a short time before the usual periods for cleaning, it was difficult to get the steam up, whatever firing was used. Nor is the employment of the Sabbath for this purpose to be left out of the question. The adhesion of the earthy matter to the iron is lessened, and the interval between the cleanings consequently protracted, by the use of potatoes, the pulp of which enveloping the crystals, lessens their tendency to cohere, and preserves them for a time suspended in the water of the boiler.

I have taken much pains with a set of experiments to settle this point, among others, viz., 'On the comparative solvent powers of waters holding in solution various salts in different proportions.' I have come to the conclusion, that the earthy salts exert a great influence in *preventing* the solvent action of water on vegetable substances; the proportion dissolved by pure or soft water being considerably greater than that by hard water. Portions of tea of the same weight, viz., thirty-six grains after drying, with equal quantities of boiling water of different kinds, standing in similar vessels for the same time, yielded, the hard water, after deducting the weight of the earthy matter, about four grains of extract; that is, the infusion left, besides the earths, four grains, on evaporation to dryness; the leaves again dried weighed thirty-two grains: the extract from the soft, or distilled water was pretty exactly eight grains; the leaves, after drying, twenty-eight grains. Thus, the soft water had extracted from the tea just twice as much as the hard. I made numerous experiments of the same description, but found it difficult, from circumstances connected with the absorbent nature of the leaves, to obtain exactly the same quantities of extract and of spent leaves, in repetitions of the same experiment, and cannot, therefore, depend on this mode of comparing waters differing little from each other; and the effect of pure water is certainly very near to that of any natural water containing carbonate of soda. I think, however, that the soda does a little increase the quantity

taken up, and that it is probable the celebrity of these waters for such purposes depends on three circumstances,—the real increase of solvent power; the darker colour, giving the appearance of greater strength; and the sensibility of the palate being increased by the soda.

§ 3. *On the Gaseous Contents of Waters.*

The gases usually found in such waters as are commonly employed for other than medicinal purposes are, carbonic acid gas, azote or nitrogen and oxygen.

In the waters containing soda, there are commonly small portions of sulphuretted hydrogen and of carburetted hydrogen, but these soon escape on exposure to the atmosphere, leaving the water free from its original unpleasant smell. Oxygen gas is less frequent and less abundant than might be supposed. In many cases I have proved its absence, by introducing into the water substances which readily absorb oxygen, by exposing to such substances the gas separated by boiling, and by exploding a mixture of the gas with a known quantity of oxygen, and more than its equivalent of hydrogen. This absence of oxygen is easily accounted for, where substances exist which would at once combine with it, as oxide of iron, or sulphuretted alkalies; but I have had the same results in cases where it might have existed without interfering with the constituents of the water.

From its absence under these circumstances, as well as upon other grounds, we may infer that the gases disengaged from spring water are not absorbed from the atmosphere, but are formed and taken up by the water while in the earth. Standing water and streams, however, undoubtedly absorb air. Dr Ure states, that he obtained from such waters about 1-35th of their bulk of gases, of which from 1-20th to 1-10th was carbonic acid, and the remainder common air. He does not, however, say whether he tried any experiments to ascertain this last point, or only assumed it to be so. I have invariably found less oxygen, in proportion to the nitrogen, than in air, and from the principles which determine the absorption of gases by water, it should be so. I have also always obtained

a greater portion of carbonic acid, as well as a greater volume of the mixed gases.

From hard, brisk pump water I obtained, by boiling, quantities which, in round numbers, and for the specific gravity, varied but little, in repeated trials, from sixteen cubic inches of carbonic acid, and the same quantity of a mixture of azote with a small quantity of oxygen.

The water of the Aire, taken from the cut which supplies the waterworks, gave two inches and three-quarters of carbonic acid, and eleven inches and three-quarters of azote and oxygen, the total quantity of gases being less than half that from pump water. From that of a large fish-pond I obtained more gas than from the river, but less carbonic acid, viz., two cubic inches and a quarter of carbonic acid, fourteen inches azote, and two oxygen.

It is only within these very few years that carburetted hydrogen has been recognised in water. Its presence was first noticed, I believe, by Dr. Scudamore and Mr. Garden, in Harrogate sulphur-water. It is found accompanying sulphuretted hydrogen in every water which I have tried in which that gas occurs, and is disengaged from many springs in much greater quantity than the water can absorb, so as to form large bubbles. This phenomenon has been observed in many parts of the world, and the inflammability of the gas disengaged in such situations had been often noticed, but its exact nature has been in most cases rather inferred than proved.

This circumstance of an inflammable gas, great part of which is carburetted hydrogen, issuing spontaneously from water, may be seen in several places in our neighbourhood. At Harrogate large bubbles occasionally rise through the water. At Stanley there is a continual flow of small bubbles; the difference depends upon the figure of the well or boring, and that of the passages through which it is supplied. At Slaithwaite the disengagement of gas is still more abundant, so that there is a succession of large bubbles, and the gas may easily be collected in considerable quantities, or set fire to at the surface of the water.

The nature and amount of gaseous impregnation, though often of moment in medicinal waters, is almost immaterial for

domestic purposes, with the single exception of water used unmixed as a beverage. The gases do not appear to interfere with the solvent properties of water, at least while cold, and when heated they are quickly disengaged.

The changes which take place as to the gases, when brisk pump water is exposed in open vessels, are rather curious. I found that water yielding twenty-six cubic inches, viz., ten carbonic acid, and sixteen azote, &c., when fresh drawn, gave, after standing five hours, twenty-five inches; the diminution was in the azote, the carbonic acid remaining the same. At the end of nine hours, the total gases, twenty-two inches, one-fourth of the azote had escaped, but very little, not two per cent., of the carbonic acid. After three days, however, the case was different; no further escape of azote had taken place, the water yielded about fifteen inches per gallon, and of this only from one and a half to two consisted of carbonic acid. The quantity of this gas was smaller than in river or pond water. If these experiments, which I have not had time to repeat, are tolerably correct, they would shew that, on exposure to the atmosphere, the azote and oxygen contained in water very soon begin to separate from it; that after a time the carbonic acid partially escapes, the other gases remaining; and that this continues until very little carbonic acid is left. The power of water to retain gases in solution depends, the temperature and pressure remaining the same, on the affinity of water for the gas, and upon the proportion of that gas in the superincumbent atmosphere. Those having a great affinity for water fly off in some degree when the gases above the water are wholly different, and those least readily absorbed are retained under an atmosphere of the same gas. Now, these two are antagonist principles in the case before us, azote having little affinity for water, but constituting four-fifths of the surrounding common air; carbonic acid being much more abundantly absorbed, but having no atmosphere of its own desscip-tion to press on the water containing it. No calculation could enable us, I think, to ascertain beforehand the order and the degree in which these effects would take place; that is, to predict the result of these experiments.

GENERAL REMARKS ON THE WEATHER IN MADAGASCAR, AND CHIEFLY AT ITS CAPITAL, TANANARIVOU,

From the 27th of June, 1828, till the 1st of January, 1829; with a Meteorological Journal from the 1st of January to the 25th of March, 1829.

By ROBERT LYALL, Esq.
British Resident-Agent, Member of many Foreign and British Societies, &c., &c.

[Communicated by Mr. J. F. Daniell.]

We arrived at Tamatave on the 27th June, 1828. During our residence there till the 11th of July, and of four days at Ivondrou, the weather was very warm, and much resembled that we had experienced at Mauritius before our departure. On the journey to Tananarivou, it continued very warm, even during the passage of the great forest, and until we crossed the river Mangoor: it then became gradually cooler; and, as it was cloudy and windy on traversing the mountain called Augave, (the height of which, above the level of the sea, may be five thousand feet,) it was even cold. Indeed, the change of climate was very remarkable; and the weather continued cold, not only on the road to the capital, but after our arrival in it.

I entered Tananarivou on the 31st of August, on a beautiful morning, with a splendid sun. The weather continued very fine, and in the middle part of each day it was warm for a considerable period; but, as there was no rain, the mountains had a very barren and bleak appearance. East and south-east winds blew hard, almost every evening, and rendered it so cold, that, in slender houses, we were necessitated to have recourse to woollen clothes, to a small fire both morning and evening, and to blankets in the night. Excepting a few days, on which it was warm, (as the 12th of August, when his late majesty, Radama, was interred, and a few hours before and after mid-day,) the thermometer ranged from 50° to 60° of Fahrenheit for a considerable time. On the 17th of August it was windy, and so cold, that we put on our cloaks to go to church. The sympiesometer and the barometer were very little affected, and the medium altitude of the latter may be reckoned

25.32 inches. We had neither rain, nor storms, nor even any high winds.

The weather, about the end of August, became considerably warmer, and continued fine; the evenings and the mornings were beautiful and highly salubrious, being clear, dry, and cool. Afterwards the thermometrical range became higher, the temperature being generally between 60° and 70° Fahrenheit: now and then, however, for a few hours after the middle of the day, it rose as high as 75° and 80°. Again, when the heat had been less intense in the day, it descended in the evening to 65° and 60° of Fahrenheit. The sympiesometer and the barometer seemed nearly stationary during September and October; and the wind was regular, at least every evening, and blew from the east and south-east. From about the beginning to the 22nd of November we had, now and then, a heavy shower, but no great quantity of rain fell; so that the Malagash government and people began to fear the loss of their rice crops, in consequence of long-continued drought. They had recourse to their idols, or gods, for assistance, as recorded in my journal of this period. On the afternoon of the 22nd, however, 'a day sooner than the gods predicted,' rain fell very copiously, and continued to do so all night, and even during a part of the 23rd. This date (though afterwards we had some fine days) may be reckoned the commencement of the rainy season in 1828. This remarkable epoch has therefore been late; but I have been told that it has occurred, though very rarely, that the rainy season has not commenced till January, which must always be a serious misfortune. It has been remarked, that the periodical rain has not followed the usual course—*of falling in heavy showers, some time between the hours of two and six o'clock,* P. M. On the contrary, it has frequently commenced earlier, and, more frequently, at a later hour; indeed, it has sometimes rained the whole night, and even in the morning and forenoon we have had heavy showers. Again, after heavy rain-falls, there have been periods of one day, of two days, and even of three days, very fine weather, and without a drop of rain; but with such heavy dews during the night, as, in the morning, led me to suppose that it had rained. Thunder-showers, which have been fre-

quent, have generally fallen in the afternoon. The lightning was vivid, and the thunder loud and near, so that a number of lives were lost by the former, in the capital and in its vicinity. Very often in the evening, and especially after thunder-storms, as in Russia, a great part, and even nearly the whole of the hemisphere was illuminated by that kind of lightning (called *zara* by the Russians) which flashes from cloud to cloud, but never approaches the earth, and by which lives, I believe, are never lost.

About the end of November, or the beginning of December, at four o'clock, P. M., a very heavy shower, mixed with large hail, fell, to the astonishment of Mr. Chenard, the tutor of my children, who had often heard of, but had never seen such a 'phenomenon.'

Ever since the rainy season has set in, with the exception of a few hours before and especially after noon, the heat has been very moderate. The barometer, comparatively speaking, has varied little; nor has the sympiesometer been greatly affected by the changes of weather. The wind, since the 22nd of November, has been more variable, and frequently from the north, north-west, and west.

The quantity of rain which fell previous to the 22nd of November may be estimated at two inches, and that since the 22nd of November at about twelve inches—total, fourteen inches—till the commencement of the report for the month of January, 1829, which accompanies these observations.

JOURNAL

Moon.	January.	Barometer.	Thermometer. Max.	Thermometer. Min.	Hygrometer.	Hygrometer. Dew Pt.	Rain.	WIND. Direction.	WIND. Force.
	1	25·25	75	66	75	68	·35	E	Gentle
	2	25·25	78	67	74	67	. .	E & NW	Do.
	3	25·30	77	70	74	66	. .	NW	Do.
	4	25·28	79	69	76	65	·65	NW & W	Strong
●	5	25·28	74	66	74	68	. .	E	Moderate
	6	25·27	74	62	70	68	·65	ENE N & NW	Strong
	7	25·32	74	64	70	68	·28	E & NE	Gentle
	8	25·28	74	66	68	68	·1	NW	Do.
	9	25·24	75	68	69	66	. .	NW & W	Do.
	10	25·25	76	67	68	65	1·5	Ibid.	Do.
	11	25·28	74	66	69	64	·58	NW & W	Gentle with breezes
☾	12	25·28	75	66	69	67	·80	NW W & E	Calm with breezes
	13	25·22	79	65	68	65	·45	E and calm	Do.
	14	25·24	74	65	68	68	·48	Calm, E and calm	Gentle
	15	25·30	73	64	66	64	·10	Calm, E ESE & E	Gentle with breezes
	16	25·25	73	65	66	64	. .	Calm, & E & SE	Do.
	17	25·34	73	65	66	63	. .	Calm & E	Do.
	18	25·33	73·5	65	66	63	1·07	Calm, E NNE & E	Calm and gentle with breezes
	19	25·32	73·5	66	67·5	64	·27	ENE & calm	Gentle
○	20	25·31	72·5	65	67	66	·60	E calm & SW	Do.
	21	25·28	73	62	63	64	3·20	ENE E & SW & calm	Gentle with breezes
	22	25·30	73	62	65	65	1·0	E & SW	Breezes and squally
	23	25·31	73	62	67	65	·76	NNE & E & SW	Squally
	24	25·29	74	63	69	66	·56	NE & E	Calm and breezes
	25	25·24	75	62	67	65	·1	NE W & NW	Do.
	26	25·22	74	64	66	64	1·45	NE NNE & calm	Gentle with breezes
	27	25·26	75	65	67	64	·8	SE & E	Do.
☽	28	25·30	72	65	66	64	·15	SE & E	Do.
	29	25·28	73·6	64	66	64	. .	E & SE	Gentle
	30	25·26	72·4	64	66	65	·11	NE E & SE	Do. with breezes
	31	25·28	73	64·5	66	67	·1	Do.	Do.

GENERAL OBSERVATIONS ON THE WEATHER, &c.

Weather cloudy.

Weather clearer.

Weather cloudy.

Wind in the evening, west and strong.

Heavy showers, with thunder and lightning.

Weather cloudy; during the last 18 hours, the wind went nearly round the compass; heavy showers.

Weather clear. Sunshine with gentle showers. Unsettled appearance.

Weather in the morning clear, and till noon, with strong breezes. Trifling showers. Heavy dew in the night. Fog this morning.

Weather clear. Heavy dew in the night.

Weather clear till 3, then cloudy. Excessively heavy rain in the evening, when the bar. sunk to 25° 18′ and there was a corresponding fall of the symp. Much thunder and lightning.

Weather clear till about 2 P.M., then overcast. In the afternoon rain fell amidst a fog. A good deal of thunder and lightning.

Weather cloudy, especially after 2. Thunder and lightning, P.M. . { Sympiesometer, 27·72. Temperature, 74·6. Barometer, 25·23.

After 2 weather became cloudy. About 5 P.M., partial rainbow in the south-east. Moon obscure in the evening. Constant gentle rain, with heavy showers in the night.

Weather cloudy, with clear intervals. Fine day. Rainbow as yesterday. Rain began at 2 P.M. Heavy showers. Moonlight and starlight evening, with clonds here and there. No rain in the night. Cloudy and calm this morning.

Clear intervals and showers. Moon very hazy. Evening darkish. Some thunder and lightning with showers, and fair intervals.

Symp. and bar. kept rising all day. In the evening, symp. 27·98. Temp. 65. Aurora borealis in the evening. Moonlight and many constellations visible till 10 P.M., when the whole heavens became obscure; yet, as was to be expected, no rain fell; but there was a strong squall. Morning cloudy.

Beautiful day. Symp. and bar. fell a little after mid-day. Clear moonlight and starlight evening, and many planets and constellations beautifully seen; but at half past 9, not a single star was visible.

Morning fine. Mid-day sultry and cloudy. At 1 P.M., symp. and bar. falling; at 2 weather overcast; at 4, symp. 27·83; temp. 71; bar. 25·83, when there was a heavy shower with thunder. Weather now fair.

Weather cloudy in the morning. Fine from 9 in the morning till 2 P.M.; then overcast. Symp. fell to 27·94, and bar. to 25·8; heavy shower at 3, then fair weather, alternately with thunder and showers. Sheet lightning in the evening.

Weather cloudy and sultry; at mid-day dew point 66 (black ball,) temp. 73. Fall of symp. and bar. trifling; though there were heavy showers. Morning foggy.

Weather cloudy; then clear with sunshine and sultry. At 1 P.M., symp. 27·78, temp. 72·6, bar. 25·26, dew point 62. Heavy showers at 1, 3, and 5 P.M. Rained all night. Now fair with sunshine.

Weather cloudy, then fine, and again cloudy. After half past 1 rain began. Very heavy showers in the afternoon, evening, and night, with fog. Now foggy.

Weather cloudy, then fine, afterwards overcast, and rain commenced before noon. Thunder in the afternoon with squalls and heavy showers. One remarkable heavy shower. Fall of symp. and bar. trifling. When the wind was at S.W. there were squalls.

Weather cloudy, then fine till near noon, when rain commenced. Heavy showers with thunder. Very warm, sultry, and oppressive at 2, and afterwards.

Weather very fine from 9 A.M. till 2 P.M.; then cloudy, and a trifling shower fell. Symp. and bar. began to sink at 1, and though the day was still very fine, they sunk till 2, when a trifling shower fell. Sultry, breezy, and even squally in the afternoon. Much thunder. Appearance of a surrounding storm. Conjecture that rain fell at a distance.

Though neither symp. nor bar. rose, weather very fine from 6 A.M. till 1 P.M., then cloudy and sultry. Much thunder in all directions, and continued heavy rain with breezes.

Fine weather, though cloudy and sultry. Symp. and bar. nearly stationary all day; a little rain. Symp. and bar. ascended in the evening. Aurora borealis in the evening.

Weather cloudy, sultry and fine. No rain during day. Symp. and bar. rose in the evening. A heavy shower between 5 and 6 this morning. Now cloudy.

Fine weather toward 4 P.M., symp. fell to 27·76, and bar. to 25·26, but they soon rose again. No rain. Magnificent starlight evening.

Fine weather. Symp. and bar. fell a little in the evening. Heavy shower at 11 last night. Now cloudy, but with indication of fair weather.

Beautiful weather. Last twenty-four hours, symp. and bar. varied very little. At 5 this morning a trifling shower.

Moon.	February.	Barometer.	Thermometer.		Hygrometer.		Rain.	WIND.	
			Max.	Min.		Dew Pt.		Direction.	Force.
	1	25·31	72	62·3	65	64	·9	E & SE	Gentle with breezes
	2	25·29	72	63·2	65	64	. .	Do.	Do.
	3	25·29	73	64	67	67	. .	ENE & E	Do.
	4	25.29	73·4	61·2	61	57	. .	E	Gentle
	5	25·28	76·2	66·3	68·5	66	. .	Do.	Do.
●	6	25·30	73·5	67·5	67	66	. .	E & SE	Do.
	7	25·25	76·6	68·3	69	67	·3	Do.	Do.
	8	25·22	75·3	64	64·5	61	. .	E	Gentle with breezes
	9	25·20	74·5	65·2	65	63	. .	E & E by S	Do.
☾	10	25·18	77·2	60·3	61	54	. .	E	Gentle during day, strong in the night.
	11	25·12	76·4	66·8	67	64	. .	ESE & S	Gentle with breezes
	12	25·6	76·2	68·5	68	67	·22	SE & NNW	Gentle and then strong
	13	25·21	75·8	67·4	68·6	67	·36	NNW & NW	Strong with squalls
	14	25·28	79·3	67·3	67	65	·4	NW N & NE	Gentle
	15	25·34	73·2	66	67	65	·20	E & calm	Do.
	16	25·35	72·7	65·5	65·5	64·50	·1	SE E & NE	Very gentle
	17	25·34	73·8	62·5	64·65	62	·1	E. E by S & E	Moderate
○	18	25·33	73·6	62·8	64	61	. .	E	Strong with breezes
	19	25·36	73·7	62·7	63·5	61	. .	E	Moderate with breezes
	20	25·37	73·2	61·8	64	57	. .	ESE	Do.
	21	25·35	71·4	60·2	64	61	. .	Do.	Do.
	22	25·37	72·6	60·2	64	63·5	·1	Do.	Strong with breezes
	23	25·34	74·1	58·4	63	61	. .	Do.	Do.
	24	25·34	73·5	61·3	65	60	·1	SE	Do.
	25	25·34	70·6	61·4	64	60	. .	ESE	Do.
	26	25·36	69·8	60·2	63	59	·7	ESE SE & ENE	Do.
☽	27	25·34	68·5	57·3	63	60	. .	E & ENE	Do.
	28	25·32	69·4	57	62	58	·1	ENE	Do.

GENERAL OBSERVATIONS ON THE WEATHER, &c.

Weather cloudy. Sympiesometer and barometer nearly stationary. Gentle shower one P.M. Evening cloudy. Morning cloudy.

Weather cloudy. Symp. and bar. fell a little in the forenoon, but ascended again in the afternoon.

Weather fine, but sultry. Symp. and bar. nearly stationary. Sheet lightning in the evening.

Weather very fine. Symp. and bar. still nearly stationary. Star-light evening, with much sheet lightning. Beautiful fresh morning.

Beautiful weather; yet after two o'clock P.M. the bar. fell to 25.25, and the symp. also sunk. Evening fine. Much sheet lightning in the evening. A great deal of distant thunder. Conjecture that rain fell at a distance.

Fine weather. Beautiful evening. At half-past ten o'clock, when taking an observation of Castor, the whole heavens became covered by clouds. Much sheet lightning in the evening. Fine morning.

Fine weather: toward 3 P.M. symp. and bar. fell, and still they remain low. A trifling shower at half past 9 in the evening. Some sheet lightning in the evening. Cloudy morning.

Fine weather. Symp. and bar. rose a little after seven P.M.; but fell again in the night. Sheet lightning in the evening. Fog between five and six o'clock this morning. Heavy dew.

Soon after mid-day, symp. and bar. fell to their present state, though the weather was, and still is beautiful. Splendid evening, with some sheet lightning.

Weather continues beautiful, though the symp. and bar. remain low. Sheet lightning in the evening.

Weather as yesterday. Symp. fell to 27.54; temp. 76.4; and bar. was depressed to 25.12 by four o'clock P.M.; dew point 70, black ball; and 76 covered ball. No rain. Wind south and gentle till four o'clock, P.M., then was NN.W., strong and squally; and the weather became overcast. Thunder and appearance of storm in the N. and N.W, Star-light evening and night.

The quantity of rain which has fallen during the last fifteen days amounts only to $\frac{46}{100}$. The weather has generally been beautiful, and such (as it is said) is rarely experienced at this season of the year. The ground has become arid, and dust is flying about as in the dry season: rain much wanted. Though for the last week, and especially during the last three days, the instruments and appearances lead again and again to the expectation of rain, yet till yesterday evening at half past 9 P.M. we had none. Distant thunder; though little rain has fallen here, I believe much has fallen in the vicinity. The symp. stood at 27.52, temp. 75.20; weather sultry. Bar. 25.60. Wind went to the N.W., and was strong and squally at 4 P.M. Dirty appearances. Sheet lightning in the evening.

Fine clear weather, with scattered clouds, till four o'clock, P.M., when it rained. Symp. and bar. rising: indeed the latter rose to 25.18, though there were showers last night. Some thunder. Sheet lightning in the evening.

Forenoon fine, warm, and sultry. In the afternoon, a good deal of thunder. Weather cloudy. Appearance of rain all round the capital. Much sheet lightning in the evening.

Weather cloudy all day. Thermom. at eleven o'clock, A.M. 72·20; at noon, only 69·30; at one o'clock, P.M., 71·50, in consequence of the fall of trifling showers. Sheet lightning in the evening. Although showers fell in the afternoon, evening, and night, yet symp. and bar. kept on the ascent. The statement of the weather for last ten days merits particular attention.

Weather cloudy the whole day: at ten o'clock A.M., drizzling rain, which also took place at different times, but in all was very trifling; yet the symp. ascended to 27.98, and the bar. to 25.38; afterwards they slowly fell to their present state. Sheet lightning in the evening.

Cloudy weather, but fine; trifling showers, especially about three o'clock, P.M. Fine moonlight and starlight evening. Bar. rose to 25.38, but fell again in the night.

Weather fine, and generally clear, with now and then scattered clouds. Morning fresh. Climate, upon the whole, delightful and healthy. Evening moonlight and clear, except at intervals, in consequence of rapidly passing clouds. Some sheet lightning.

Weather fine, fresh morning; most agreeable at noon, in consequence of the sun's influence, became very warm in the afternoon; beautiful evening. Fine morning.

Beautiful weather; splendid evening.

Fine weather. Temp. moderate. Sometimes cloudy in the evening. Clear in night. Morning foggy in the east.

Fine day; but as the wind was nearly constant, and the temp. never high, at times, rather fresh. Afternoon cloudy. Rain showed itself in the last twenty-four hours.

Fresh morning. Shower of rain. Fine agreeable healthy weather. Evening pleasant.

Weather continues good; though part of the last twenty-four hours it was sometimes cloudy, especially in the N.W.: a little rain fell. Wind strong in the night. Cloudy morning, especially towards the N.E. Threatens rain.

Weather cloudy, often threatened rain; but the quantity that fell during the last twenty-four, and the preceding forty-eight hours, did not amount to more than $\frac{1}{10}$ of an inch. Still cloudy.

Weather cloudy all day, with trifling showers and strong breezes. Fresh. Still cloudy this morning.

Good healthy weather, though sometimes cloudy.

Ditto, the wind has varied little for some time past; the breezes have generally taken place in the night, or with showers. The quantity of rain that has fallen this month forms a great contrast to what fell in December, 1828, viz. 12 inches; and what fell in January, 1829, viz. 14·02 inches. Aurora borealis in the evening.

Moon.	March.	Barometer.	Thermometer.		Hygrometer.		Rain,	WIND.	
			Max.	Min.		Dew Pt.		Direction.	Force.
	1	25·32	70·7	59	63	59	. .	ENE	Moderate with breezes
	2	25·30	70.2	61	65	64	. .	ENE E & calm	Do.
	3	25·28	74	59	64	60	. .	E & calm	Do.
	4	25·25	76·1	62	68	66	2·73	E	Do.
●	5	25·26	75·4	55	67	65	·60	E & calm	Gentle with breezes
	6	25·25	72	62	69	66	2·26	Do.	Do.
	7	25·29	72·4	64	66·7	66	·48	ENE & calm	Do.
	8	25·30	71·2	62	67	66	1·33	E NE & E	Do.
	9	25.28	67·6	59·4	63	66	·68	E calm & E	Gentle
	10	25·28	71·5	61·4	67	65	1·75	E & calm	Do.
	11	25.29	71·3	60·4	65	63	.45	E NE & E	Do.
☾	12	25·32	70	62	67	65	1·43	E & NE	Moderate
	13	25·30	71·3	61	68	62	·73	Do.	Do.
	14	25·26	73·8	58	67	67	·01	Do. & NW	Do.
	15	25·28	73·2	62·4	69	67	. .	NW	Do.
	16	25·22	72·8	63	68	66	. .	Do.	Do.
	17	25·28	75·6	63·2	69	67	·01	NW & E	Gentle
	18	25·30	71	60	66	62	·85	Do.	Moderate
	19	25·30	72·2	61·2	67	65	. .	NE & E	Do.
○	20	25·37	72·6	62	67	64	. .	E	Gentle
	21	25·37	72·3	60·5	64·5	61	. .	E	Gentle in the day, strong all night
	22	25·35	72·4	59	64	62	. .	E & calm	Moderate
	23	25·32	71·8	59	65	63	. .	Do.	Do. with strong breezes
	24	25·31	72	60	66	64	. .	Do.	Gentle with breezes
	25	25·29	72·4	60·2	65·5	64	. .	Do.	Do.

GENERAL OBSERVATIONS ON THE WEATHER, &c.

Fine weather, though sometimes cloudy. A Scotch mist for a few minutes. Partial rainbow in the east at 5, P.M.

Delightful day, but at times cloudy. Symp. and bar. fell, however, yesterday afternoon, and remain low. Dull morning.

Weather as yesterday. Bar. fell $\frac{2}{100}$ and remains at 25·28. Symp. same as yesterday. Aurora borealis in the evening.

Symp. and bar. fell after noon, about 2 P.M., heavens became overcast all around, and at 3 a heavy shower fell. Rainbow in the east at 5 P.M. In the evening much thunder and lightning, and rain, alternating with fair intervals, during which many beautiful coruscations, and much sheet lightning illuminated parts of the hemisphere. Much rain in the night. Foggy morning.

Forenoon fine. Sultry. Thunder and lightning. Dark wet evening, except when illuminated by sheet lightning. Raining heavily at present.

Gentle rain during greatest part of the day with heavy showers; much rain in the night. Foggy morning.

Weather cloudy, with now and then heavy showers. Atmosphere sultry and oppressive. No thunder. Some lightning in the evening. Foggy mild morning.

Fine forenoon. Showers and gusts of wind from the N.E. Rained all night, but not heavily. Still raining gently. The low lands are completely inundated.

Fresh and rather cold day. Long continued scattered rain, both in the day and the night. Symp. and bar. not much affected yesterday, but have descended a little in the night. Cloudy morning.

Cloudy, mild, but disagreeable weather. Surrounding country much inundated.

Bad weather. Inundation of the country in some parts very complete. Alarm was sounded at 5 this morning, that the river Kioupa had burst through its banks to the south. The quantity of stagnated water is therefore likely to be much augmented. Symp. and bar. have operated in contrary direction. Foggy morning.

Fine weather and showers alternately during day. Good deal of distant thunder and lightning. Heavy showers in the evening and night. Foggy morning.

Showers and sunshine. Pretty good weather in the intervals. Some thunder and lightning. Sheet lightning in the evening. Foggy morning.

Some distant thunder after 4, just before which there was a smart breeze from the W., accompanied by a trifling shower. Fog this morning.

Weather fine, but the plain surrounding Tananarivou is so inundated as in many places to resemble ponds and lakes.

Fine weather. A good deal of sheet lightning in the evening. Notwithstanding the fall of the symp. and the bar. Morning beautiful.

Some distant thunder was heard, but only 0·1 of rain has fallen the last 24 hours. Fog early this morning which has changed to a Scotch mist.

Fine weather till 1 P.M., when there was a shower. Symp. and bar. fell. A good deal of thunder and much evening lightning. Heavy rain in the night. Appearance of fair weather this morning.

Weather fine and healthy. As the country still is, and is likely to be for some time to come, inundated, the air is moist. Distant thunder. Sheet lightning in the evening. Fine morning.

Fine weather. Beautiful moon-light evening with some sheet lightning. Hazy in the S.E. this morning.

Fine weather continues. Aurora borealis in the evening. Cloudy in the E. and S., but with other favourable indications.

Weather very fine, yet when two black clouds passed there was for two or three minutes a Scotch mist. Sheet lightning in the evening.

Beautiful day. Sky clear before 6. Cloudy morning.

Weather continues charming. No rain last twenty-four hours, notwithstanding fall of symp. and bar.

Fine weather. Sheet lightning in the evening. Mr. Lyall had scarcely registered the above observations, when he was made a prisoner at the instance of the gods of Madagascar, torn in a moment from his family and removed to Ambouhipaina, seven miles east of Tananarivou.

GENERAL OBSERVATIONS.

1. TANANARIVOU, the capital of Madagascar, is situated in 18° 56′ 20″ S. L., and, I conjecture, in about 47° E. L. From barometric observations, I reckon its elevation to be nearly five thousand feet above the level of the sea ; and its highest pinnacle, called Ambouin Simboun, about seven hundred and fifty feet above the level of the greatest part of the surrounding plain. I am about to make extensive and more accurate observations respecting some of these points, which I shall not fail, in due time, to make public.

2. In consequence of the peculiar situation of Tananarivou, and especially of its great elevation, a series of well-conducted and well-recorded meteorological observations must be of the highest interest. By the acquisition of additional instruments, and greater practice, I trust to render every month's report more detailed and more interesting than another, until the climate here is sufficiently known.

3. The observations have been made every morning at six o'clock, because this is the only hour on which I could count for regularity: therefore, the day commences at six o'clock, A. M., and ends at six, A. M., of the succeeding day.

4. The sympiesometer used is Adie's, No. 497.

5. In consequence of an accident having happened to one of Newman's mountain barometers (an excellent instrument), I have been compelled to make the foregoing observations with Jones's mountain barometer, which is constantly suspended. In order to have the means of making comparative observations, however, I myself filled the tube of Newman's barometer, which, though the starting point be somewhat different, acts upon the same general principles and in the same manner as Jones's. The two instruments work together.

6. Rutherford's register thermometer is used for the maximum and minimum.

7. In all my observations with the sympiesometer, after carefully adjusting the *fleur de lis*, I find it necessary to attribute 10, 12, 14, 16, or even more degrees to the mere effect of temperature, between the hours of one or two o'clock and four o'clock, P. M.; otherwise I should be constantly predicting rain.

8. Daniell's hygrometer is used.

9. I have given a statement of facts, without attempting to draw conclusions. Time does not permit such inquiries ; besides, professed meteorologists will do this much better than I could : therefore, copies of this table, and of that of all future tables, shall be forwarded to my friends, Mr. Dalton, of Manchester, and Mr. Daniell, of London.

ROBERT LYALL.

ON THE ELUCIDATION OF SOME PORTIONS OF THE FABULOUS HISTORY OF GREECE,

BY THE APPLICATION OF THE ANALYTICAL PRINCIPLES OF PHILOLOGY.

By WILLIAM SANKEY,
A.M. of the University of Dublin, and *ad eundem* of Cambridge, &c.

IN a former essay I directed my attention to the legitimate principles which should guide us in the analysis of languages, and applied the same to the investigation of the origin of some of the distinguishing characteristics, as well as apparent anomalies, of the Greek tongue. I would now bring the principles to bear upon points still more interesting, as shewing us that this is a subject which does not confine its views to the mere mechanism of language, but that it may be advantageously employed in enabling us to arrive at the accurate meanings of words on the one hand, or, on the other, in throwing light upon the darker ages of history, while as yet dawning through the thick mists of fable.

With respect to the assistance we thus derive in ascertaining the appropriate meanings of words, we may exemplify this in the word λαος, *a people*, which however, analytically, signifies more accurately *a multitude*, being obviously resolvable into the radix λα and ος. Now, λα is clearly the same as the particle λα, *valde*, presenting, therefore, at once, in the compound λα-ος, the idea of *largeness*. We are also enabled thus immediately to detect the error of the older etymologists, who, being unacquainted with the just principles of analytic philology, deduced λαος, *a people*, from λαας, *a stone*.

Again, to take the particle δε: this word generally ranked as an adversative, but we shall probably be led to question the justness of this classification when we consider that δε is closely allied in sensible character to δε-ω, ligo, from which it is at once obtained by a direct analysis. The idea, therefore, conveyed by this particle δε, must be connected with that of *binding*. This will further appear from its affinity to δει, *oportet*, which is, indeed, the third person singular of the former verb δεω, the notion of a *physical restraint*, which is primarily conveyed by this latter being metaphorically transferred in what is called the impersonal δει, to a *moral obligation*. Hence then

it follows that δε should be ranked amongst the conjunctions copulative, and not among the disjunctives, as it has been generally classed by grammarians and lexicographers. Indeed, when we consider the very forced and inelegant construction which, following the present rendering of this particle, is commonly given to sentences wherein it occurs, we might be apt *à priori* to doubt whether its proper meaning had been yet assigned. In truth, I believe there will be found but few passages in which the sense would not be much improved by taking δε as a *connective*, instead of an *adversative*. I do not, however, deny, but that in some instances it may be used with somewhat of a disjunctive signification. For example, where it is put, as it were, in opposition to μεν. Even in these instances, however, the meaning would not be much obscured by rendering δε as a copulative. Perhaps, in such cases, the force of δε might be very well given by the English *yet*, which is itself derived from the Latin copulative *et*, and that from the Greek ετι, *moreover*. This view of δε, as a connective, may receive still further support from the consideration that this particle is closely allied in sensible character to the copulative τε, the difference lying solely in the interchangeable letters δ and τ. Now τε unquestionably signifies *and*, the same as και, with which also it is frequently used, και being put in the former member of a connected sentence, whilst τε occupies the latter. But δε itself is also sometimes used in the same manner after και. Indeed, in most of those instances in which τε is used, it will be found that δε, according to the laws of enunciation, would necessarily be pronounced τε, the δ being changed into τ, as occurring after ν or σ. It is true indeed that δε is sometimes found following after σ, but then in such cases the σ must be pronounced like our *z*.

In close connection with this part of our subject, we shall take an analytical view of verbals in τεον, which will afford us a still further confirmation of the correctness of the opinion which we have advanced respecting the particles δε and τε. Now the force of this termination τεον is evidently that of *necessity, what must be:* τεον is, therefore, clearly δεον, the δ being, on account of the euphony, changed into τ. The same is further manifest from the Latin gerund in *dum*, which

answers to the Greek verbal in τεον; for the Latin termination *dum* is here, in fact, the Greek δεον, changed according to the analogy of the Latin language, the *u* being substituted for εο, and *m* for ν. Hence I may observe, that the Greek verbal has just as good a claim to be classed as a part of the verb as the Latin gerund. θε is also connected with δε and τε. Considering therefore θε as the radix of both τιθημι, *pono*, and θεω, *curro*, it follows that, analytically, the former means to dispose or arrange in a *connected* order, and that the latter retains the idea of *connection* in the rapidity of *consecutive* motion.

We come now to consider the application of the analytic principles to the elucidation of the fabulous history, which cannot fail to be highly interesting, as enabling us to detect the fallacious grounds on which some of the most remarkable fables in the Greek mythology have been raised; and first with respect to that of Δημητηρ or Ceres, and Κορη or Proserpine. Now, Κορη, analytically investigated, is evidently connected with χειρω, *to shear*. The ει, however, of the radix having been changed into ο, shews us that, as I observed in my former essay, χορη denotes the *result* of the action expressed by the verb χειρω. Hence, therefore, χορη clearly signifies *the harvest*, whilst Δημητηρ, being a contraction of Δε-αμητηρ (derived from δεω, *ligo*, *I bind*, and αμαω, meto), *I reap*, represents under a combined form both the *binder*, and *shearer*, or *reaper;* consequently these names taken together in relation to one another express the physical fact that *the harvest* or *sheaf*, χορη, is the production of the labours of the shearer and binder Δημητηρ. Χορη, however, signifying also a girl, and Δημητηρ having an apparent connection with μητηρ, *a mother*, and by the force of a false etymology being supposed to be *quasi* γη μητηρ, the Greeks have thereupon raised a fabulous allegorical structure.

A further confirmation of this view is to be found in the Latin term Ceres, which corresponds to the Greek Δημητηρ. For Cer-es, as we have remarked before of Κορη, is *also* manifestly derived from χειρ-ω, *tondeo*. The ε, however, of the radix being retained in *Cer-es* is a proof that its signification is connected with the action of the verb in a present and active energy, whilst the plural termination *es* shews that this word

was not originally limited merely to *a single* individual. *Ceres,* therefore, analytically and primarily, meant *the shearers* collectively. So that the terms Ceres, Κερ-ης and Core, Κορη, in this respect answer to one another both etymologically and physically, as cause and effect. From this instance we may be led to perceive that much of the Greek mythology, which was almost altogether physical, had its foundation in the radical meaning of the appellative terms therein used.

Thus the origin of the numerous fables spread about Proteus is at once explained on attending to the real import of the Greek name Πρωτειος, which being derived from πρωτος, meant the first element; this, many amongst the Greeks considered to be simple. Out of it, therefore, every thing material being imagined to be produced by variety of combinations, the fable accordingly took its rise, that Proteus, Πρωτειος, was capable of assuming every shape.

Again, the Curetes, Κουρητες, originally signified the winds, being derived from χορεω, *verro, to sweep along.*

In a double meaning of words, and the ambiguity thence arising, originated the fable of Cadmus and the offspring of the dragon's teeth. The history, as deduced from the fable, seems to have been simply this: Cadmus brought with him into Greece many of those improvements which Asia, as earlier inhabited, had already made in agriculture and the arts of life. Amongst others he introduced the culture of the *σπαρτον, genista, broom,* whose twigs were manufactured into a species of cordage. This plant is of a deep *copper* or *serpent* colour. Now, it is remarkable, that the same word נחש in the Hebrew and Syriac or Phœnician languages, signifies both a *serpent* and *brass* or *copper,* owing, no doubt, to the similarity in the colour of these objects. Hence, therefore, it is likely, originated the mistake which gave rise to the fable. For this word נחש, having probably been used by Cadmus and his Phœnician followers, in reference to the colour of the broom, it was erroneously interpreted, according to its ambiguous meaning, as denoting a serpent. Hence, the seeds of the broom were called *serpents' teeth,* which they might themselves also be fancied somewhat to resemble in size, &c.; and so they were fabled to be particularly the teeth of one of

the larger of those noxious reptiles which, as much infesting the adjacent parts, Cadmus, in clearing the country for his new settlement, had but a short time before destroyed. The ambiguity of the word σπαρτον, also, which etymologically may signify *anything sown*, contributed to spread the error. The *genista* having been sown in drills, as in an oziery, with its upright form and spear-like branches very naturally presented an appearance somewhat like that of a battalion of armed men, to which the imagination would find a still further resemblance in the helmet-shaped *carina* of its papilionaceous flower. In accordance therefore with this view, the cutting down of the plant for the purposes of manufacture would be represented as a mutual combat amongst the offspring of the dragon's teeth, ignorance, credulity, and fear, coupled with a lively imagination, easily converting the strenuous labours of the workmen into a mutual assault of combatants. The survivor or survivors, as the fable has it, were clearly the labourers employed by Cadmus, who having been at first concealed by the standing broom, and afterwards becoming visible on its being cut down, were imagined to be the remains of the crop of armed men; and these same workmen, as being his ordinary attendants, probably themselves also Phœnicians, further assisted Cadmus in building the walls of Thebes. I am aware that this fable has been otherwise explained, as resting altogether upon the ambiguity above remarked, of the Phœnician word נחש, which signifies both a *serpent* and *brass*, as though the dragon referred to a king armed in brass who was overcome and killed in battle by Cadmus, and the seed of the dragon's teeth to the scattered troops of the slain monarch's subjects, that rose up in arms of the same brazen materials upon his death. That, however, the explanation I have given is more correct, will be evident from the similar achievement of Jason, where we have, besides the account given of the very preparation of the ground for the seed by the labours of the oxen, a circumstance completely confirmatory of its being an agricultural rather than a military exploit. What gives greater weight to the argument derived from this source is the close connection in every point of view between this feat of Jason and that of Cadmus, notwithstanding the interval of time that

had elapsed between them, and the distance at which Colchis lay from Thebes: for Phryxus, himself a native of Thebes, and born during the lifetime of Cadmus, had probably brought along with him, on his flight from Bœotia, the seeds of the *σπαρτον*, the culture of which, as we have seen, had been already introduced into that country by Cadmus. Hence, therefore, the merit of Jason in this particular will consist in the readiness with which he learned the sowing, rearing, and management of this plant in all its stages, as also the skill with which he guided the plough drawn by brazen-shod oxen. For such is the true history, when divested of fable, of the brazen-hoofed bulls; whilst the panting breathing of the smoking animals, blown with their exertions in ploughing a heavy soil, gave rise to the poetic exaggeration that they vomited forth flames of fire.

From this view of the history of one part of the Argonautic expedition, I am naturally led to notice another instance, connected with another part, where the ambiguity of a name gave rise to a very remarkable mistake. The Argonauts, cut off in their retreat from Colchis through the Euxine Sea by the outlet of the Hellespont, were necessarily driven northwards to the Palus Mæotis, whence, the country about the mouths of the Tanais and the Vistula being probably at that time under water, they were enabled, by transporting their light vessel but a short way across the land, again to launch into the deep. Sailing therefore through the Baltic, and crossing the German Ocean, they passed along the shores of the British Isles. Here it was that, as they discerned the coasts of Ireland, the melancholy coincidence of a name raised in their superstitious bosoms the most gloomy apprehensions. Informed, no doubt, that this island was called Eirionn or Erin, as it is still denominated in the Erse or Gaelic, the original language of the country, those adventurers connected this appellation with the Greek word Eριννυς, a name which, derived from ερις, *strife*, a sinful idolatry had given to the imaginary inflictors of avenging torments. Believing, therefore, that this island belonged to these fancied tormentors, they were appalled at the circumstance; and, struck with horror and remorse at the recollection of their conduct towards the Colchians, and the disastrous

fate of Absyrtus, they were naturally filled with the most alarming terrors for their situation.

Another instance in which the application of true principles of etymology will be found to afford us much assistance in elucidating history, where it has been obscured by fable, is that which relates to one of the most remarkable of the adventures attributed to Perseus. I allude to that which refers to his obtaining, as it was fabled, the Gorgon's head; an adventure, as generally narrated, altogether incredible, but which, when properly understood, is highly interesting, as giving us an account of perhaps the earliest introduction, as it would appear, of the coral into Greece,—at least of that species which is called the Medusa's head. It is probable, indeed, that the Tyrians, in their commerce with the Greeks, had brought them specimens of the madrepore, and other coralline productions, at the same time describing the Medusa's head as being of a rarer kind and more difficult to be obtained. A desire, therefore, of obtaining this species, as well as of satisfying an excited curiosity in visiting and exploring strange countries, combined, it is likely, with general objects of a commercial character, impelled Perseus to undertake what may be truly called a voyage of discovery. That he indeed was considered by the Greeks as having first made known to them the coral, and that as one of the fruits of this expedition, is evidently to be inferred from the epithet given to this natural production, the *coral*, in that line of the poet, where the author expressly denominates it, clearly in reference to this very fact, *Persean*,—

—Περσηϊδαο μενος μεγα κουραλιοιο.
The great strength of the *Persean coral.*

It is remarkable, also, that this author, after describing the coral as being originally a vegetable production growing in the depths of the sea, the saltness of whose waters withered its leaves, and so left it, with its branches denuded and bare, to float the sport of every wave, till, thrown at length upon the shore, it indurated on being exposed to the air; it is remarkable, I say, that he connects the coral with the Gorgon's head, fabulously ascribing the deep colour of the red coral to the effect of its being tinged with Medusa's blood.

Now, this explanation, though obviously false, marks the local, or, so to speak, geographical affinity, which was at that time admitted to hold between the coral and the Gorgon's head, and that both were considered to have been obtained from the same place. The truth is, that the error has arisen here from the ambiguous etymology of the name κοραλλιον, or κουραλιον. This word, analysed, is evidently a compound of two distinct words, κουρη or κορη, and αλς, *mare*. Now we have already seen, that the first part, κορη, *etymologically* signifies *the harvest*. Hence κουραλλιον or κοραλλιον analytically means *the harvest of the sea*—an expression which, as very happily designating those resemblances of vegetable life which grow, as it were, beneath the bosom of the deep, was a most appropriate appellation for those crops of coral which Perseus brought home with him. The Greeks appear, however, to have been misled here also, as in the fable of Ceres and Proserpine, by the more common acceptation of the word κορη, and so interpreted the compound κουραλιον, as though it signified *the girl of the sea*. This interpretation may also have received some further support from Perseus having possibly encountered and slain, in this expedition, one of the native queens of the country which was the more immediate scene of his exploits. We need not, therefore, be surprised that, in conformity with this view, it was imagined that the Medusa coral, with which Perseus, it would appear, adorned the boss of his shield, was the head of a female, as that species of coral bears some resemblance to the human face, especially that of a woman encircled with heavy ringlets of thick curling hair. Even though we suppose the name to have been originally derived from κορη, *puella*, and from this fanciful resemblance given primarily to this species, but afterwards extended to corals in general, this will still nowise militate against the view I have taken of the origin of the fable, as grounded on the introduction of the coral into Greece. Further, from their likeness to vegetable and animal life, corals were considered as petrifactions; and hence arose the idle tale, the offspring of superstition and credulity, of the petrifying qualities of the Gorgon's head, and of whole hosts of armies turned into stone immediately on its being presented to their view by Perseus.

If we examine a little also into the origin of the other fabulous circumstances recorded as connected with this adventure, we shall find them also tending still further to confirm the view I have been endeavouring to establish. Thus the wings and talaria with which the supposed Hermes or Mercury is said to have furnished Perseus, clearly signified nothing else than an oared ship or ships with which he had been furnished probably by the Tyrians; the wings evidently denoting the sails, and the talaria the oars. Even in the etymology of the name Hermes, Ερμης, we may find, perhaps, a stronger support for this conjecture than at first view might be imagined. The verb ερεσσω signifies *to row* in particular, and to *navigate* in general; but verbs in σσω are generally derived from, or rather are but other forms of verbs in ω or εω. Hence we are led to an obsolete radix, ερεω, of the same signification, from which, according to the general analogy of the language, comes Ερμ-ης, *the rower, navigator*, or, more properly, being originally Ερμ-εες, of the plural form, the *rowers, navigators*. The Greeks, however, naturally enough considered this word to have been derived from ερω, *dico*, or perhaps ειρω, *necto*; and from this, and the circumstance that the communication between the different parts of Greece and the chief seat of government (which, it would appear, was at that time in Crete or Asia) was carried on by means of sailing vessels, arose many of the fabulous stories related of their Hermes or Mercury. Another etymology might, perhaps, be assigned for this name Hermes, which would no less agree with the main facts of the fable. The radical part of Ερμ-ης, when analysed, is evidently Ερμ. Now this, written in Hebrew characters, is הרם, which, both in the form of the radical letters and the pronunciation, is not very dissimilar to חירם or חורם, the name of the king of Tyre contemporary with David and Solomon. Under this view, then, Hermes, Ερμ-ης, being of the plural form, would denote the seamen of king Hiram, and so point to the Tyrians as the people who, from their maritime situation and habits, furnished Perseus in particular with the ships necessary for his voyage, as they had all along been the medium of intercourse between the chief seat of government and the provinces of Greece. The chronology, I may also remark, would herein agree with that

set forth by Sir Isaac Newton, who makes Acrisius, the grandfather of Perseus, contemporary with David. Even, according to the more received chronology, the name Hermes, Ερμ-ης, may have been derived from a king of Tyre, as the appellation of Hiram, which seems to have been not uncommon among the Tyrians, may have been borne by others of their monarchs, prior to him who was the friend of David and Solomon. However this may be, it is remarkable that the Latin name *Mercurius*, as well as the Greek, Ἑρμης, seems originally to have been of a plural form, *Mercuri*,—the terminal *us* being afterwards added, in conformity with the erroneous notions of a false theology. Hence, whilst the Greeks gave the appellation Ερμης, either from the mode in which these foreigners visited their shores as *sailors*, or from the name of the *monarch* whose subjects they were, the Latins, on the other hand, designated them, from their occupation as *traders*, *Mercuri*, which word, indeed, considered as a translation of the ambiguous Hebrew word כנעני, will at the same time point out both the pursuits in which they were engaged, and their original parentage and country, viz., *merchants*, *Canaanites*. This view will be found to agree very well with the various characters ascribed to Hermes or Mercury, so celebrated for eloquence and craft, for his skill in mercantile transactions, and readiness in embassage, &c. It is supported also by the fabulous history of his birth, as reported to be the son of Maia. The personal existence of such a female may indeed be well considered doubtful. The name, however, Maia, as derived from μαιω, *cupio*, was obviously directly given, as a very appropriate appellation, to the fifth month of the year, our May—a month *so desirable* to mortals, after the gloom and nakedness of winter, for the serenity of its skies, the fresh verdure of its foliage, and the richness of its flowery blooms. In the same season, also, navigation, which had been impeded by the raging storms and the roaring seas of winter, was again resumed. As, therefore, these Tyrian or Canaanitish merchant mariners, כנעני, *Mercuri*, Ερμεες, generally revisited the shores of Greece and Italy in the month of May, they were allegorically said to be the offspring of Maia; and this, literally taken, was afterwards transferred, as the real origin of his birth, to the

fictitious being *Mercurius*, who had been made, as it were, their personal representative.

To return, however, to the fable of Perseus. I may just remark, in further confirmation of the Medusa's head having been a real coralline production, that Perseus is represented as cutting down the coral with the αρπη, or *sickle*—the very instrument which he is said to have been furnished with for the express purpose of cutting off the Gorgon's head; but which, in truth, it is clear he had really provided himself with, with a view to this coralline expedition.

Many other instances might be adduced, in which an analytical investigation of the names of persons and things throws light upon the history where it has been obscured beneath a mass of fable. Such a view, indeed, of fabulous history, is highly important. No doubt that which is called the fabulous age of Grecian story is deeply involved in thick clouds of obscurity; yet here and there, through the breaks in the gloom, we can dimly discern forms cast in the same mould with ourselves. It will scarcely be denied, that most of the personages which are spoken of as flourishing at that early period did really then exist. The main body of the events in connexion with which their names have been handed down to us, must have had some foundation, in fact, more solid than the mere imagination of the poet, or the fanciful story-telling of the dealers in the marvellous. The early history of every people, except that of the Jews, we find mingled with fable; and this has arisen, not merely from that love of the marvellous which characterises a rude, untutored race, but also from the want of those faithful records which, by presenting an accurate delineation of events, would, in a great measure, have corrected those popular errors and delusions which have interwoven themselves with the facts. Indeed, there exists at all times a class of persons whose minds are prone to see everything under an exaggerated form, and no less so to communicate their own impressions with additional circumstances of exaggeration unto others. Any person who is at all acquainted with the peasantry, may have observed how distorted a form any more than ordinary event will come to assume among them, as it is conveyed from mouth to mouth, even in the immediate neighbour-

hood of the transaction. Unquestionably, these appendages to the fact will vary much, both in tone and extent, according to the peculiar character of the age and people. Still, we may be certain that facts, orally transmitted, must, through a lapse of time, receive a considerable degree of colouring from the prevailing hues of the various media through which they are transmitted. Many of the leading circumstances may be altered, and not a few may be omitted, whilst some additional ones may be grafted upon the original. The general outline, however, still will have been drawn from fact. Amid all the windings, therefore, and intricacies of the labyrinth, we need not despair of being able to discover the thread which shall serve to extricate us from the maze. It will be found, indeed, I believe, that most of these fabulous legends may in general be traced to—1. Exaggerated descriptions. 2. Mistakes in the reasons and explanations assigned for any particular line of acting, where that was such as might, perhaps, appear extraordinary to persons unacquainted with the circumstances and motives that influenced. 3. Allegorical representations of persons and events. 4. Metaphorical language; and 5, as above, ambiguities of words. This last requires no further confirmation, after the many instances we have already been considering, in which the foundation of the legend obviously rests upon such ambiguities. In like manner, too, each of the other heads might also be illustrated by appropriate examples, were it not that this would lead us beyond the proper limits of this Essay. I cannot, however, help adducing one which falls under the second head, inasmuch as, though altogether absurd, as at present narrated, it is capable of receiving the simplest explanation; I allude to the singular tale of the punishment of the daughters of Danaus. The solution is clearly this: The perforated vessels which the daughters of Danaus filled with water were evidently clepsydræ, the use of which they had brought with them from Egypt. The Greeks, however, in their then state of ignorance, could naturally enough perceive no benefit to be derived from pouring water into vessels merely for the purpose that it might run out again through holes in the sides and at the bottom; the more so as this operation, being constantly repeated, seemed as endless as,

no doubt, it appeared to them unmeaning. Hence, therefore, they imagined it was a retributive punishment inflicted upon these females on account of their cruel murder of their husbands. What may, perhaps, add confirmation to this view of the fable, is the fact recorded by Diodorus Siculus respecting the priests of the temple of the false god Osiris in Egypt; namely, that they filled three hundred and sixty milk bowls every day. Sir Isaac Newton, in his *Chronology*, imagines that the historian here means that the priests filled each day one bowl out of three hundred and sixty bowls, counting thereby the days of the Egyptian calendar year. Now it is probable that these bowls were clepsydræ, each running for twenty-four hours, thus noting also the time of the day, by being adjusted with something of a graduated scale, according to the descent of the fluid. They would answer, therefore, the double purpose of a diurnal time-piece and of an annual calendar. Taking, however, the historian according to the more obvious meaning of his words, namely, that the priests filled the whole number of the three hundred and sixty bowls every day; then, if each bowl ran exactly four minutes, and they were filled by these numerous attendants accurately in succession, the entire cycle would be completed just in the twenty-four hours; so that these four-minute chronometers would give precisely the time of the day. Commencing also every day one bowl lower down, if I may so say, in the order, then the days of a year of three hundred and sixty days would be likewise kept by the *number* of the bowl with which they began each day. Indeed, were we even unable to assign any probable reason for this custom, still it would serve so far to explain the fable of the Danaïdes; inasmuch as, Egyptians as they were by birth, there can be but little doubt but that they carried with them into Greece their Egyptian predilections and Egyptian rites. Hence, therefore, we might naturally expect to find some notice, though tinged as it is with fable, of their having adopted this Egyptian custom of pouring a fluid into perforated vessels, and that, no doubt, with the same view, whatever that might be, with which it was originally practised in their native land.

ON THE LIMITS OF VAPORISATION.

By M. FARADAY, F.R.S.,

Director of the Laboratory of the Royal Institution, &c. &c.

I WAS induced some time since to put together a few remarks and experiments on the existence of a limit to vaporisation, which were favoured with a place in the Philosophical Transactions for the year 1826. When the experiments there mentioned were published, I arranged some others bearing upon the same subject, but which required great length of time for the developement of their result. Four years have since elapsed, during which, the effects, if there had been any, have been accumulating, and it is the object of this brief paper to give an account of them.

The point under consideration originally was, whether there existed any definite limit to the force of vaporisation. Water at 220° sends off vapour so powerfully, and in such abundance as to impel the steam-engine; at 120° it sends off much less; at 40°, though cold, still vapour rises; below 32°, when the water becomes ice, yet the ice evaporates; and there is no cold, either natural or artificial, so intense as entirely to stop the evaporation of water, or in the open air prevent a wet thing from becoming dry.

The opinion of many, among whom were the eminent names of Sir H. Davy and Mr. Dalton, was, that though the power of evaporating became continually less with diminution of temperature, it never entirely ceased, and that therefore every solid or fluid substance had an atmosphere of its own nature about it and diffused in its neighbourhood; but which being less powerful as the body was more fixed, and the existing temperature lower, was, with innumerable substances, as the earths, metals, &c., so feeble as to be quite insensible to ordinary or even extraordinary examination, though in certain cases they might affect the transmission of electricity; or, rising into the atmosphere, produce there peculiar and strange results.

The object of my former paper was to shew that a real and distinct limit to the power of vaporisation existed, and that, at common temperature, we possess a great number of substances

which are perfectly fixed. The arguments adduced, were drawn first from the power of gravity, as applied by Dr. Wollaston, to shew that the atmosphere around our globe had an external limit, and then from the power of cohesion; either of these seemed to me alone sufficient to put a limit to vaporisation, and experiments upon the sufficiency of the latter force were detailed in the paper.

The conclusion was, that although such substances as ether, alcohol, water, iodine, &c., could not as such be entirely deprived of their vaporising force, by any means we could apply to them, but still, if in free space or in air, would send off a little vapour, yet there were other bodies, as iron, silver, copper, &c., most of the metals, and also the earths, which were absolutely fixed under common circumstances, the limit of their vaporisation being passed; and further, that there were a few bodies, the limits of whose vaporisation occurred at such temperatures as to be within our command, and therefore passable in either direction. Thus mercury is volatile at temperatures above 30°, but fixed at temperatures below 20°, and concentrated sulphuric acid, which boils at temperatures about 600°, is fixed at the ordinary temperature of the atmosphere.

It is well known in the practical laboratory that vaporisation may be very importantly assisted so as to make certain processes of distillation effectual, which otherwise would fail. Thus with the essential oils, many of them which would require a high temperature for their distillation if alone, and be seriously injured in consequence, will, when distilled with water, pass over in vapour with the vapour of the water at a much lower temperature, and, being condensed, may be obtained in their unaltered state.

It has been supposed that the vapour of the water, either by affinity for the vapour of the essential oil or in some other way, has increased the vaporising force of the latter at the temperature applied, and so enabled it to distil over; but there is no doubt that if air or any other similar elastic medium were made to come in contact with the mass of essential oil at 212° in equal quantity, and in a manner to represent the vapour of water, it would, according to well known laws, carry up the vapour of the essential oil perhaps to an equal extent,

and pass it forward; only the facility with which the carrying agent is condensed when it consists of steam, allows of the condensation of every particle of the essential oil vapour, whereas the permanency of the elastic state of the air would cause it to retain a large proportion of the vapour of the oil when cold, and consequently a diminished result would be obtained.

There are, nevertheless, some appearances which seem to favour the idea that occasionally water favours vaporisation beyond what air, equal to the bulk of the vapour of the water, would do in the manner referred to above; and it was to ascertain whether substances which, from a consideration of the general reasoning already referred to, and the high temperature at which they sensibly volatilized, might be considered as *fixed* at common temperatures, could have any sensible degree of volatility, in conjunction with water or its vapour, conferred upon them at ordinary temperature. It is well known that a theory of meteoric stones has been founded on the supposition that the earthy and metallic matter found in them had been raised in vapour from similar matter upon the earth's surface; which vapours, though extremely attenuated and dilute at first, gradually accumulated, and by some natural operation in the upper regions of the atmosphere became condensed, forming those extraordinary masses of matter which occasionally fall to us from above. The theory has in its favour the remarkable circumstance, that, notwithstanding many substances occur in meteoric stones and iron, yet there is none but what also occur on this our earth *; and it also has a right to the favouring action of water, if there be such an action; because vaporisation is one of the most important, continual, and extensive operations that goes on between the surface of the globe and the atmosphere around it.

In September, 1826, several stoppered bottles were made perfectly clean, and several wide tubes close at one extremity, so as to form smaller vessels capable of being placed within

* This very striking circumstance does not *prove* that aërolites in any way originate from our planet; but then, if we could by other arguments deduce that they were extraneous, it would lead to the conclusion that the substances which have been used in the construction of this our globe, are the same with those which have been used extensively elsewhere in the material creation.

the bottles, were prepared. Then selected substances were put into the tubes, and solutions of other selected substances into the bottles: the tubes were placed in the bottles so that nothing could pass from the one substance to the other, except by way of vaporisation. The stoppers were introduced, the bottles tied over carefully and put away in a dark safe cupboard, where, except for an occasional examination, they have been left for nearly four years, during which time such portion of the substances as could vaporise have been free to act and produce accumulation of their specific effects.

No. 1. The bottle contained a clear solution of sulphate of soda with a drop of nitric acid,—the tube, crystals of muriate of baryta. One half or more of the water has passed by evaporation into the tube, and formed a solution of muriate of baryta above crystals, but both that and the remaining solution of sulphate of soda is perfectly clear; there is not the slightest trace of sulphate of baryta in either the one or the other, so that neither muriate of baryta nor sulphate of soda appear to have volatilised with the water.

No. 2. Bottle, solution of nitrate of silver; tube, fused chloride of sodium. All the water has passed from the nitrate of silver to the salt; but there is no trace of chloride of silver either in one or the other. No nitrate of silver has sublimed with the water, nor has any chloride of sodium passed over to the nitrate.

No. 3. Bottle, solution of muriate of lime; tube, crystals of oxalic acid. The water here remained with the muriate of lime. In the tube, the oxalic acid when put in had formed a loose aggregation, with numerous vacancies, and with a very irregular upper surface about an inch below the upper edge of the tube. No particular appearances occur in the vacancies; but at the top there has evidently been a sublimation of the oxalic acid, for upon the crystals and glass new crystals in exceedingly thin plates and reflecting colour have been formed; these rise no higher in the tube than to the level of the most projecting part of the original portion of oxalic acid; no appearance of sublimation is evident above this, and it seems as if the most elevated parts of the salt have given off vapour, which has sunk and formed crystals on the neighbouring

lower surfaces, but that no vapour has risen to the upper part of the tube. On examining the solution by a drop or two of pure ammonia, it was however found that a slight precipitate of oxalate of ammonia occurred. The experiment shews, therefore, that oxalic acid is volatile at common temperatures, and had not only formed crystals in the tube, but has passed over to the solution of lime.

No. 4. Bottle, solution half sulphuric acid, half water; tube, crystallized common salt. No water has passed to the salt. On opening the bottle, the clear diluted sulphuric acid was examined for muriatic acid, but no trace could be found. Hence chloride of sodium has not been volatilised under these circumstances.

No. 5. Bottle, solution of muriate of lime; tube, crystals of oxalate of ammonia. The oxalate of ammonia appeared quite unchanged. The solution of muriate of lime was perfectly clear; but when a little pure ammonia was added to it, a very faint precipitate of oxalate of lime was produced.

No. 6. Bottle, little solution of potash; tube, white arsenic in pieces and powder. This bottle was opened because of the appearances, in October, 1829, and had then remained three years undisturbed. The arsenious acid was to all appearance unchanged. The solution of potash was turbid and foul. On chemical examination, it proved to have acted powerfully on the glass. It had dissolved so much silica as to become a soft solid, by the action of an acid, and it had also dissolved a considerable quantity of lead; but there was no trace of arsenious acid in it; so that this substance, although abundantly volatile at 600°, had not risen in vapour when aqueous vapour and air was present at common temperatures.

No. 7. Was some of the sulphuric acid used in these experiments, preserved for comparison.

No. 8. Bottle, solution half sulphuric acid, half water; tube, pieces of muriate of ammonia. When this bottle was opened, the pieces of muriate of ammonia presented no appearance of change; there was no moisture about them, nor any appearances of dissection that I could distinguish. The diluted sulphuric acid being examined by sulphate of silver, gave no appearances of muriatic acid; so that muriate of ammonia appears fixed under these circumstances.

No. 9. Bottle, a little solution of persulphate of iron; tube, crystals of the ferro-prussiate of potash. Both were unchanged; there was no appearance of Prussian blue about either the crystals or solution; neither of the salts had been volatilised.

No. 10. Bottle, a little solution of potash; tube, fragments of calomel. Here the potash had acted upon the glass, as in No. 6; but, with respect to the calomel, the volatility of which was in question, there was not the slightest trace of such an effect. No black oxide nor other substance existed in the potash solution which could allow the presumption that any calomel had passed.

No. 11. Bottle, solution of potash; tube, fragments of corrosive sublimate. Here the potash had acted on the glass as before; carbonic acid had also gained access by the stopper; so that no caustic potash was present; but there were distinct appearances of the sublimation of corrosive sublimate, and minute crystals of the substance were even attached to the under part of the stopper in the bottle. Hence corrosive sublimate is volatile at common temperatures.

No. 12 and 13. Bottles, solution of chromate of potassa; tubes, in one, chloride of lead in powder, in the other nitrate of lead in crystals. In both these experiments the chromate of potash had acted upon the lead of the glass, and rendered it yellow and dim; so that no indication could be gathered relating to the non-volatility of the compounds of lead.

No. 14. Bottle, solution of iodide of potassa; tube, chloride of lead. Both remained unaltered; the solution of iodide was perfectly clear and colourless; no trace of the chloride of lead had passed over in vapour.

No. 15. Bottle, solution of muriate of lime; tube crystals of carbonate of soda. A part of the water has passed to the carbonate of soda; but both it and the remaining solution of muriate of lime are perfectly clear. No portion of either salt has volatilised from one place to another.

No. 16. Bottle, dilute sulphuric acid; tube, nitrate of ammonia in fragments. The nitrate was slightly moist. The acid being examined was found to contain nitric acid, whilst the test acid, No. 7, was perfectly free from it. It would

therefore appear that nitrate of ammonia is a salt volatile at common temperatures, although it is just possible that slow decomposition may take place in it, and so nitric acid or its elements pass over.

No. 17. Bottle, solution of persulphate of copper; tube, crystals of ferro-prussiate of potash. The crystals had attracted most of the water from the cupreous salt; but the solution of ferro-prussiate and that of the copper had their proper colour; neither were rendered brown; no salts had been volatilised.

No. 18. Bottle, solution of acetate of lead; tube, iodide of potassium. The acetate of lead is now dry; the iodide of potassium has taken all the water and formed a brown solution, in which there is free iodine; probably a little acetic acid has passed over and caused the change in the iodide of potassium. There is no appearance of iodide of lead in the tube, but there is in the bottle, and most probably in consequence of the vaporisation of the free iodine from the solution in the tube.

From these experiments it would appear that there is no reason to believe, that water or its vapours confer volatility, even in the slightest degree, upon those substances which alone have their limits of vaporisation at temperatures above ordinary occurrence, and that consequently natural evaporation can produce no effects of this kind on the atmosphere.

It would also appear that nitrate of ammonia, corrosive sublimate, oxalic acid, and perhaps oxalate of ammonia, are substances which evolve vapour at common temperatures.

Royal Institution, Aug. 30, 1830.

ON THE EFFECTS OF ELECTRICITY UPON MINERALS WHICH ARE PHOSPHORESCENT BY HEAT.

By THOS. J. PEARSALL,
Chemical Assistant in the Laboratory of the Royal Institution.

DURING some experiments, made to observe the effects of an electrical discharge passed over the variety of fluor spar called chlorophane, which is peculiarly distinguished for its phosphorescence when heated, I remarked certain appearances, which are detailed in the following investigation.

When the electrical discharge is passed over fragments, or the coarse powder of a very fine specimen of chlorophane, a brilliant green light is produced. On repeating the experiment many times, I found the phosphorescence re-occurred with each repetition of the discharge, and was even sensibly strengthened by the operation.

This striking appearance induced me to suppose that even such minerals as had been deprived of the power of phosphorescing by calcination might have it restored by virtue of electric action, and led me to make the following experiments, which will shew how far this supposition was confirmed.

A specimen of chlorophane, possessing naturally the property of phosphorescence in a very high degree, was first subjected to the action of heat. The light emitted was coloured, first bluish-green, very bright; then pinkish, blending with pale-whiteness as it became red-hot, when it lost all peculiar light.

A portion of the same mineral, which had been calcined, and thus deprived of its power of phosphorescence, was then subjected to a single discharge from a small Leyden jar, of about a square foot of coated surface. The substance became luminous during the passage of the electricity, producing a *green* light.

On the application of heat to the portion thus electrized, it was found to be phosphorescent, and to emit a *green* light nearly as strong as a portion of the mineral in its natural state, with which it was compared. This experiment was repeated, and always with constant results.

An inferior specimen of chlorophane was then heated, when it gave out a strong light of a *faint purple colour;* but it decrepitated so violently during calcination, that a piece of sufficient size to be electrified alone could not be obtained.

The splinters were then placed in a glass tube, through which three electrical discharges were passed, producing a deep *purple* light after each discharge. They were then heated upon platinum, when they evolved phosphoric light of *different colours,* some fragments appearing *green,* others *yellow,* the whole finally assuming a *deep purple* light. These colours were obviously distinct from those of the natural mineral, a portion of which, heated at the same time, shewed only light tints of purple.

Part of the same calcined specimen, but not electrified, gave no light when heated*.

Chlorophane, whose phosphorescence had been destroyed by an intense heat, was exposed for two days to the sun's rays without effect; but a single discharge again restored its phosphorescence.

Repeated discharges were made upon the same substance, and it was found that the property was increased by the number and intensity of the discharges, the green light evolved by heat being deeper and of longer duration after three, six, or twelve discharges, than after a single discharge.

Chlorophane, which had been heated intensely, and had been since exposed, under ordinary circumstances, to daylight for eight months, had not acquired the least phosphorescence; but it gave a greenish light during the passage of the electricity, increasing with the strength of the discharge, and was afterwards luminous by heat †.

A crystal of purple fluor spar, calcined at the same time,

* The mode I adopted was to heat the portions of mineral upon a platinum capsule, covered by a watch-glass. The phosphorescence was thus rapidly produced, and easily governed by the regulated flame of a spirit-lamp. The identical fragments were also readily submitted to repeated examinations; and I conceive that by using platinum instead of iron, as usually recommended, I guarded against the introduction of matter which might have interfered in the experiments. The calcinations were performed in a crucible at a red heat.

† Dr. Brewster exposed specimens to the sun's rays concentrated in the focus of a lens, but without the slightest indication of returning phosphorescence.—Brewster *on the Phosphorescence of Minerals. Edin. Phil. Journal,* i. 387.

and similarly exposed to ordinary light, did not phosphoresce when heated, until it had been electrized, when it was faintly luminous, with a deep-purple light.

Apatite was then experimented with, and likewise deprived of its phosphorescent property by calcination; but, upon electrifying it, and applying heat, it was found to have resumed the power, and evolved a lemon-coloured light, which rendered the figure of the fragment distinctly visible.

With apatite, as well as with chlorophane, the light reproduced was in proportion to the discharges made. A fragment of apatite answers better than the mineral powder.

These experiments proved, that the phosphorescent property, when destroyed by heat, can be restored by electricity to minerals which had thus been deprived of it.

I was therefore led to investigate how far other mineral substances which phosphoresced by heat could have this property increased and restored to them; and also whether some substances, which did not possess this property naturally, could have it imparted to them by electric action. The following experiments were accordingly made:—

A colourless variety of fluor spar was tried, which gave not the least indication of light when heated; but after six discharges had been made from the Leyden jar, it was capable of evolving a beautiful flame-coloured or orange light. In this case, the property was conferred upon a substance which probably never possessed it previously.

The experimental results obtained with other specimens are given in the form of a table, under the respective heads.

Mineral.	Colour.	Effect by Heat.	Calcined.	Electrified.	Effect when Re-heated.
1. FLUOR SPAR.	White crystallized mass.	No light.	Decrepitated strongly.	Small fragments in tube.	Faint light.
2. "	Ditto, another specimen.	Ditto	Ditto.	Ditto, ditto.	Momentary light, but distinct.
3. "	Ditto, ditto.	Ditto.	Ditto.	Ditto, ditto.	Faint light.
4. "	White crystals.	Faint violet-coloured light.	Ditto.	Fragment.	Light faint violet-coloured, ending with deep purple.
"	Green crystals.	Light, faint purple.	Ditto.	Ditto.	Yellowish green, ending with intense purple, bright and beautiful.
6. "	Ditto, another specimen.	Violet light.	Ditto.	Six discharges through tube.	Green, violet, and deep purple,—distinct changes.
"	Amber-coloured crystals.	Strong light, violet and pink.	Ditto.	Six ditto, ditto.	Light orange colour—bright, short duration.
8. "	Purple crystallized mass, the palest portions selected.	Ditto, ditto.	Ditto.	Six ditto, ditto.	Light yellow, or flame-coloured.
9. "	Deepest purple parts, same specimen.	Ditto, ditto, stronger.	Ditto.	Six ditto, ditto.	Greenish yellow light.
10. "	Another specimen, entirely dark purple.	Ditto, ditto.	Ditto.	Six ditto, ditto.	Bright yellow light, very beautiful.
CARBONATE OF LIME.					
11. "	Crystals.	No light.	Heated to redness.	{ Solid—six discharges. Fragments in tube.	No light. Faint reddish light.
12. "	Crystals.	Ditto.	Ditto.	Small solid—six discharges.	Ditto.
13. "	Dogstooth spar.	Steady yellow light, remaining for a considerable time.	Ditto.	Solid crystal—six ditto.	Orange light,—required high temperature.

In these, as well as in the preceding experiments, portions of the same calcined mineral, but not electrified, were heated at the same time; but in no instance did the non-electrified substance evolve light.

In this table, it will be observed that Nos. 1, 2, and 3, did not possess light in their natural state, but light was *imparted* to them by electricity.

No. 4 possessed a light of a faint colour, which became *whiter* as it was heated, but its *conferred light* ended in *purple.*

Those numbered from 5 to 10 had light *restored* to them, which differed, however, in colour from their previous natural phosphorescence.

11 and 12 had *light given* to them.

No. 13 had *light restored* to it.

I now proceed to some remarks on *colour* given to fluor spar by electricity. In some experiments with the white fluors which had a yellowish tinge, it was observed that, after the powder was electrified, or when six or seven discharges had been made through a piece of the mineral, that a difference was perceptible between the electrified and the natural mineral, the electrified substance having a *bluish* tint, whilst the other was *white.* The phosphorescence was also stronger, where the tint thus given was most obvious.

As the colour had been most decidedly given by electricity to some portions of a crystallized mass of dark compact purple fluor, which had been rendered colourless by heat, some white pieces were selected and broken; one portion had twelve discharges passed over and through it, which produced a *light blue colour*, very decided upon the edges and angles of the laminæ, especially toward the exterior. Both fragments were then heated; that which had been electrified gave a *pale blue light* of short duration, and, when cold, had lost its blue tint; the other portion evolved no light.

The fact, also, was well shewn by confining the electrical effects to one extremity of a colourless portion; a perceptible tint was caused by a few discharges.

Some splinters and fragments were placed in a small heap, inside a glass tube, open at both ends, and between the two ends of the wires of the discharger, which were about an inch apart,

and likewise introduced into the tube; after several discharges had been made, most of the splinters had acquired a blue tint; when heated they evolved a strong *light of a pale yellow colour.*

Larger pieces electrified, assumed a *blue* tint, giving also a *blue light* when heated; but when these pieces were crushed into small fragments, electrified in the tube, and then heated, they evolved a *pale yellow light,* as in the preceding experiment.

In some instances, however, fragments gave a *light,* at first *blue,* afterwards changing to a *straw colour;* but in every repetition *the colour and intensity of the light differed according to the size of the specimen,* as in the above examples.

The blue tint caused by electricity seemed to be superficial, or nearly so; for when some coloured portions were broken, they were colourless in the interior, but tinted upon the external edges.

The colourless parts were not phosphorescent, while the coloured and exterior parts were. So that it is probable that the phosphorescent property is also conferred principally upon the superficies, which may be the cause of the differently-sized pieces evolving differently-coloured light.

To avoid any fallacy from the transfer of metal from the wires, and its oxidation by the electrical explosions, experiments were repeated, and the discharges were made from platinum points, with the same resulting blue colour as before.

Other substances were then examined, which, however, produced nothing immediately bearing upon the preceding experiments, excepting, however, that it was found that, by passing twelve discharges through a diamond, it afterwards evolved a pale blue light when heated; it had been made red-hot previous to electrization, but without effect.

Two other diamonds gave no light when heated, until from twelve to twenty discharges had been made over them, when they, also, gave a *pale blue light by heat.*

Diamonds probably vary in respect to this property; for a cut diamond gave no light, neither could any be imparted to it by electricity; whilst, on the contrary, another diamond was found slightly phosphorescent by heat, shewing feebly a pale bluish light; and this specimen, when electrified and again heated, gave a stronger blue light than any other diamond.

An amethyst, sapphires, rubies, and garnets, with many ordinary mineral substances, gave no indication either of natural or acquired phosphorescence.

In conclusion, I may be allowed to remark, that I am not aware that the phosphorescent property has ever been restored, or imparted, to this class of bodies, by any other means.

Note.—The consideration of other varieties of fluor, and the duration of the effects, as well as other circumstances bearing upon the preceding facts, may form the subject of a future communication.

ON THE DEVELOPMENT OF THE SEVERAL ORGANIC SYSTEMS OF VEGETABLES,

with reference to their Functions; and especially on the Respiration of Plants, as distinguished from their Digestion.

By GILBERT T. BURNETT, Esq.

In no subject has indeterminateness, arising from conflicting dogmata, prevailed in a more perplexing, in a more disheartening degree than in the general philosophy of life, and particularly in the physiology of plants. At one time even vitality was denied to exist in them: their curious structures and still more curious functions being all considered as merely mechanical and chemical phenomena. When subsequently their vitality was proved, the reaction perverted the very truth that it established, by attributing to simpler plants the organs of the more complex animal frame: thus we hear of the arteries, the veins, and the nerves of plants; of the uterus, the vagina, and the testes: the roots have been declared lacteals, and other parts placental vessels; the leaves have been considered lungs or gills, and in their functions sometimes they have been compared to kidneys: again, the wood has been esteemed the osseous compages of the plant, and the pith, its spinal marrow, or the centre of its nervous system; which last idea introduced the absurd doctrines of the instincts, sensation, and perceptivity of vegetables. Nay, when not even a semblance of parallelism could be either found or feigned, as was

the case with the stomach and the heart; then by a licence still more exceptionable, analogy and affinity were confounded with each other, and all deference both to structure and to function disregarded; for in this instance heat was regarded as the heart, and the earth as the stomach of plants. Similarity of function is often found, however, to be a far less erring guide than similitude of external form and structure; the one is more general and less modified than the other, for very diversified means may be adopted to achieve the self-same end: thus nutrition may be performed without a mouth to receive, teeth to chew, or even a stomach to digest the food; respiration may take place without either lungs or gills; prehension without either hands or claws; and progression without wings or feet.

Thus among plants, although the root may in general be the prime organ of nutrition and the seed of reproduction, many plants are efficiently nourished and propagated without either root or seed; at least without those modifications of the nutritive and reproductive systems being present, which are ordinarily so called; the functions remaining when the organs have disappeared, *i. e.*, the ends being still the same, though the means have been greatly varied. From these circumstances such plants have been called imperfect plants; yet this has been only done, because they have been imperfectly considered; and still more, because the abstract idea formed by many phytologists of a seed or of a root, has been rather the amplification of the idea of some particular seed or root, *e. g.*, of an acorn or an oak root, than an enlarged and liberal view of the various modifications of external configuration under which the nutritive and reproductive functions may be efficiently performed; and consequently without reference to the numerous forms under which these systems are developed: *i. e.*, under which the potential root and seed can, nay do, very frequently appear. Hence from these imperfect premises has arisen the deduction of characters as essential, which in truth are only accidental to each prime organism; for not only have the most highly developed organs of men, animals, and plants been comparatively referred to, to explicate the obscurities incident to each, but the organs of the one have too often been selected as rules or examples for the other;

and because a stomach and a heart are present there, a stomach and a heart are feigned to be present here; or, on the contrary, because such and such functions are performed without such and such organs here, they are not performed by the especial organs there: *e. g.* because in cellular plants the fluids move from part to part without the intervention of tubes and ducts, so where tubes and ducts are present, it has been denied that they are the channels through which the transit does take place; just as if it should be said that because the eye of the mole is destitute of optic nerve, therefore the optic nerve is not the nerve of vision.

Again, the dedication of especial organs to especial functions, in all alike, (a segregation only to be truly found in the more highly developed individuals,) has led to much physiological obscurity; for to this source may be traced many of those apparent parodoxes, and much of that experimental contradiction which so grievously embarrasses the vegetable physiologist: for among plants some highly developed instance is selected as a type, its reproductive organs are called seeds, its chief nutritive, *i. e.* its absorbent, organs are called roots, &c., &c., and all other nutritive and reproductive organs, notwithstanding they are as potentially roots and seeds to the individuals they nourish and reproduce, are no longer considered such, when their accidental figures no longer conform to that of the type which ignorance or prejudice has set up, and with which they are compared; although their essential functions are the same. To diminish, if not entirely to remove this dilemma, is an object that I have long kept in view, and to this end have been engaged in the prosecution of an extensive series of experiments on the subject of vegetable life, *i. e.* on the phenomena living vegetables exhibit, and the functions they perform; more especially on the nutrition and propagation of plants. Many of these have been of course but verifications of previous observances; some have led to directly opposite results, and others, suggested by the train of inquiry, seem to have thrown new and additional light on several of the more obscure departments of vegetable physiology. It is not my intention now to give even a cursory detail of the whole, which would necessarily involve either a very extended or a

very superficial essay; but rather to illustrate the functional distribution of the vegetable organism, on the principles just now adumbrated, according to the progressive stages of its development: and then to follow out one series or rather one section of one series, leaving other sections and other series of this very extensive inquiry to a future time, as fit topics for subsequent elucidation.

In the functional distribution of the vegetable organismus, it is found most convenient (it is also thought to be most philosophical, *i. e.* most consistent with natural phenomena) to distinguish the various organs under whatever external modifications they may appear, as nutrients and generants, *i. e.* into the nutritive and reproductive systems, *e. g.* to associate as nutrients all those organs which tend chiefly to the growth and preservation of the individual; and as generants all those which are more especially designed to favour the increase and preservation of the specie: but to these two opposite and essential spheres, a third, which is accessary, or intermediate, must be added, viz., the organ of extension, formed more or less of both extremes, and serving equally for their varied segregation and extension.

In many of the more highly developed plants the nutritive and reproductive systems are easily distinguishable from each other; and in many even some of the subordinate parts of each are separable likewise; thus of the nutritive system the root is the chief organ of absorption, the leaves of respiration, and so on: furthermore it would seem that occasionally one surface of the leaf is the more especial organ of respiration, while the other is that of aqueous transpiration; and again, with regard to transpiration itself, it would appear that absorption and exhalation are the peculiar functions of separate parts.

In the simpler forms of vegetable existence, however, these distinctions vanish; for here one organ performs many functions, and the several members elsewhese separate, or separable, become blended together in one uncertain state; and it is often difficult to say whether the semblance is most that of root, or stock, or stem, for each appears occasionally predominant, and all in turn are equally latent and obscure. What, for instance, is the growth of the tremellæ? what that of many of the con-

fervæ? what the vegetation of tuber? of the fuci? of many rushes, of the Cacti, *e. g.* C. opuntia, C. melocactus? &c., &c. It would be difficult to say from ordinary definitions whether in some of these examples either root or stem be present, and whether in others they are leaves entirely, or whether they have no leaves at all; though, from their increase both in size and number, it is manifest that both the nutritive and reproductive systems are present, and as energetic, as efficient in them as in more highly developed individuals.

Let us, on these principles, pursue the analysis of a plant. Let the three primary systems of the vegetable organism, separable in idea, though not separate in nature, be figuratively represented by three spheres or circles, more or less containing and contained in each other; these are the nutrient, the stock, and the generant, or the organs of nutrition, extension, and reproduction. In the simplest protophytes these three essential systems are equally present and equally undistinguishable from each other; in the lower confervæ, in the tremellæ, &c., every part absorbs and nourishes equally, and every part will equally reproduce an individual like that from which it is disjoined; the generative molecules not being produced in or confined to any especial part, as are seeds and buds to flowers and stems, but scattered indifferently throughout the substance of the plant. Again, in some of the higher algæ and the musci distinct and separate roots appear; and in the fuci and lichens the spores are chiefly situated in peculiar parts, *i. e.*, in certain discs, or thalami, at first being aggregated internally towards the surface, and then becoming external tubercles. Among the fungi also, at least among the more developed ones, a still further segregation may be noted, in the formation of lamellæ or pores for the reception of the sporules.

These illustrations might be multiplied and the thread of organic evolution be pursued through the whole of the vegetable reign, but here the cumulative argument is needless. The fact might be allowed to rest on the bare enunciation that these three systems are present in every plant, though variously distinct or intimately blended with each other; and that no one is ever truly, however all may in turn apparently be, absent. All plants must have, as postulates of their exist-

ence, organs of nutrition and reproduction; and although the intermediate or accessary organ of extension is not so essential as these to mere existence, it will be found, that upon the relative development of this accessary part depends in a great measure the relative development and distinction of the other systems.

But to proceed:—Let the intermediate stock or caudex, *i. e.*, the organ of extension, be that from which the examination of the vegetable may commence. In its simplest forms, as in the protophytic algæ, and in some of the lower fungi, as reticularia, sphæria, &c., it remains latent, or but partially evolved, so that the nutrient and generant are equally present in every part, and no distinction of organs for different functions is perceived; gradually, however, in the rising scale of organization, the stock or caudex becomes extended, and as gradually the nutrient and generant become located in distinct and different parts, the nutrient chiefly in that which ordinarily pursues a downward course, hence called the descending caudex [caudex descendens] or caudescens; the generant, principally, [though, like the nutrient, not exclusively,] in that which quits the earth, and, rising upwards, seeks the air, hence called the ascending caudex [caudex ascendens] or caudascens. The whole of the organ of extension abstractedly considered, without reference to the nutrient and the generant, is the stirps or stock, *i. e.*, the caudex; the central portion is the stock-heart, and, according as it has been considered either the crown of the root, or the seat of the stem, or the boundary between them both, it has been called indifferently corona, sedes, vel limes, and, sometimes, from its shape, collum or coarctura: the caudescens is the stake of the root, and the caudascens the stalk of the herb; the rootstake, when in union with its absorbents or radicles, *i. e.*, with the chief organs of nutrition, becomes a true and proper root, as the stalk, when bearing the generant, *i. e.*, the reproductive organs, viz. gems and seeds, becomes the stem or herb.

Such being the physiology of segregational development, it cannot seem surprising that the functions of plants are but relatively distinct; that many instances occur in which they are intimately blended, and in which, without much care, both

organs and functions must be, nay have been, frequently confounded with each other; and the obscurity is increased by each external organ being too exclusively considered as the seat alone of its predominant function; as the root of absorption without respiration; the leaves of respiration without absorption; the flowers of reproduction, &c., whereas the ascending caudex will frequently evolve radicles, and the descending as frequently propagate by subterranean buds. But of this enough, to it we shall return at another time. It is curious to note into what errors a contrary doctrine has led its advocates, by some of whom it has been affirmed that leaves never absorb, and that roots can never develop buds: when, in fact, it is notorious that even in the most segregated examples, some traces, though faint, may frequently be found of that primitive community of function in which the original simple structure was all-sufficient for every purpose; when each and every part was nutrient and generant alike, before either root, or stock, or herbage, as distinct organs, were developed; and by reverting to this consideration, we shall find a clue to explicate some of those apparent paradoxes which seem to have bewildered the vegetable physiologist: *e. g.* gardeners have practically become convinced that seeds, and the roots of plants, when covered with too thick a stratum of soil, refuse to grow; and foresters have also found, that when large trees have been embanked they have languished and generally died. By many this was attributed to the accumulation of earth round the trunk, and hence when raised roads have been to be made through groves or plantations, cylinders of brick have frequently been built round trees at a very considerable expense, to prevent, if possible, their destruction. This was the case near the new bridge in Hyde Park and Kensington Gardens, but the trees, in spite of this precaution, as might have been foretold, still have died: for the shock that, under such circumstances, they receive, arises not from the inclosure of the trunk by earth, but from the suffocation of the roots, which the sudden embankments exclude from the access of air; and this before any fresh roots can be protruded nearer to the surface, which would take place during a more gradual deposition of extra soil; and thus in the instance alluded to, those

trees which have been entirely surrounded with the embankment have wholly died, whilst those which stand on the slope of the artificial mounds, so that the roots of one side only have been deeply buried, have suffered much less from the cause which has killed the others, though all were equally surrounded with cylinders of brick, which is at least a useless, if not an injurious plan. It is a common error to suppose that the roots of trees pierce the earth very deeply, whereas four or five feet will be often found more than the depth of the roots of trees of from sixty to eighty feet in height. Roots spread much further laterally, that they may gain easier access to the air, and even some send up shoots at intervals to the surface, apparently as respiratory organs; and it is well known how favourable the loosening of the soil is to the health of trees, as well as of all other plants.

The nutrient system of vegetables consists of several subordinate parts, which, like the prime organisms, are, in different instances, more or less distinct or blended with each other: these are the transpiratory, the assimilating, and the respiratory systems; the transpiratory consisting of the absorbent and exhalant organs, the assimilating of the secretory and excretory, and the respiratory perhaps of the inspiring and expiring ones, or at least performing functions equivalent thereto. Concerning all these much contrariety of opinion both has existed and does still exist: on the one hand, (to take two extreme examples,) it is contended that the root is the sole organ of absorption, and that on this point the leaves are wholly inefficient; while, on the other, it is as strenuously contended that the leaves are the chief, if not the sole absorbents, and that the root serves merely as a fulcrum to steady the plant and attach it to the soil in which it stands; the one maintaining the ascent, the other the descent of the sap.

Again, with regard to the channels through which the transit is performed, opinions as contrary exist: one doctrine being that it ascends by means of the spiral vessels, another that it passes through the non-spiral tubes, and a third declares that, although the plant may be composed of cells and tubes, the sap mounts not in them, but is transmitted from one part of the vegetable to another, by choosing for its pas-

sage the intercellular spaces, *i. e.*, the unfilled interstices which lie between the exterior parietes of contingent vessels. These doctrines cannot all be true; they must be more fully examined when we come to treat of the course of the sap and the assimilation of their food by plants: but even now, when the remaining object is chiefly to distinguish by experiments the respiration of plants from their digestion, and to follow out the phenomena of the first-named process alone, even this cannot be satisfactorily pursued without an enunciation of what experiments hereafter to be detailed will more fully shew, viz. that absorption takes place in most plants both by their roots and leaves; that the first course of the sap is upwards; and that its passage (at least its frequent passage) is through the non-spiral tubes. Let one or two experiments illustrate these points.

During the last spring I had several notches cut in the trunks of various trees, viz. lime, birch, horse-chestnut, &c., and at several heights in each tree, from one to six feet; the result of which was, that in every instance the sap was seen distinctly exuding from the lowest side of the lowest section first, and progressively rising to the others day by day: but I have found that the chief current of sap is axial, for it will traverse the whole extent of the trunk before it will enter any of the branches, how near soever to the root they may be situated; and also that when it does enter the branches its course is still axial with regard to them, *i. e.*, it will pass to the end of the main branch before it enters its lateral ramifications. This tendency will explain two interesting facts: viz., first, why plants which have lost their leaders raise one of the drooping lateral branches, as in the Norway fir, the larch, &c. &c., to the erect position; and secondly, why it is generally (perhaps invariably) found that terminal buds are the largest and finest, and are always the first developed; and also why the topmost branches are in leaf before the buds of the lower branches are expanded.

To obviate the objections which have frequently been raised against an experiment somewhat similar to those above detailed, but in which holes were bored in the trunks of trees with an augur, and to which observations it was urged, that

the sap might first have exuded from the upper side of the perforation and have collected on the lower, I preferred notches to holes in my experiments, and furthermore had a diaphragm made in the centre of the wound, thus separating the upper and under parts, so that this shelf would intercept the descent of any sap to the lower section ; and the moisture still exuding and covering the lowest part, while the upper continued dry or nearly so, very satisfactorily demonstrated the upward progress of the sap. Again, in other experiments, in which vines, rose-trees, &c., were cut off within a few inches of the ground, the sap exuded abundantly from the stumps, although there were in these experiments no stems or branches present from which it could possibly descend.

As a converse to these experiments, I repeated some of those of Bonnet, placing vine-leaves, &c., on the surface of water by their different surfaces, but always taking care, which he seems scarcely to have done, that the cut ends of the petioles were covered with soft wax, that no fluid might enter thereby, but all that was absorbed must have passed through the cuticular covering of the organs: and leaves thus treated will continue fresh and green for days, nay even for weeks, and some, if wholly immersed, for months together; while other experiments shew that, if left without water in a dry place, they wither and decay in a few hours. Again, that these observations might not be open to the same objections which have been raised to somewhat similar ones, viz. that although it is confessed leaves and plants continue fresh longer when placed on water or in damp situations, than when left without it and in dry ones, yet that this arises from the moisture with which the specimens are surrounded preventing the exhalation of the peculiar juices of the plant, rather than from any actual intus-susception of external fluid which the leaves effect, the following modification of these experiments was devised. To decide this point, I took several leaves of Potamogeton natans, which, when wiped quite dry, were weighed, and after remaining out of water for two hours they lost from three grains and a half to five grains and a quarter each; they were then put in water, and after the lapse of two hours more were again wiped quite dry and again weighed, by which it was proved that

they had severally gained from three to five grains each, which increase (which was evident from their succulent appearance also) could only have taken place by absorption through the cuticle, as their cut petioles were, as in the previous experiments, defended by soft cement.

That the exhalation of leaves is very great was formerly shewn by Hales, in his *Vegetable Statics*, but his mode of experimenting was extremely inconvenient: I have found a much simpler means to answer better. It is to put the leaves or cuttings, or plant to be experimented on, into a glass vessel, graduated so that the quantity of water which is put in may be accurately known, and the quantity that is lost ascertained by the variation in its height. The surface of the water should then be covered with a stratum of oil about half an inch thick, which will closely invest the stalks and prevent any evaporation from the surface of the water; so that all which is lost must have been perspired by or be contained in the plant. If a growing vegetable in a pot be the subject of experiment, it will merely require to be set in a larger vase, and to have the water above the edge of the pot so that the oil may inclose and cover the whole*. By experiments so conducted, I have found that a single leaf of the common sunflower (Helianthus annuus), weighing only thirty-one grains and a half, absorbed in four hours twenty-five grains of water; the leaf had increased in weight only four grains and a half, so that twenty grains and a half of water had disappeared, the greater part of which had been exhaled, as was proved by other similar experiments, in which the apparatus was placed under a receiver and the vapours condensed upon its sides. These details might be multiplied, but the above are sufficient for our present purpose.

The experiments, however, with the leaves of plants, which have excited the most attention, are those that have shewn the changes they effect in the constitution of confined portions of atmospheric air; which changes are of two directly opposite kinds, viz., its deterioration and its amelioration, *i. e.*, the increase and the diminution of the oxygen it contains. Both

* To prove the power of oil in preventing the evaporation of water, I have kept two ounces of water in an open graduated measure, covered only by a very thin stratum of oil, for upwards of two years, without any sensible diminution.

these effects have been too generally attributed to the same cause, viz., the respiration of plants, which has been supposed under certain circumstances to form, and under others to decompose, carbonic acid ; but they are, in truth, distinct, and performed by two separate systems—the one being the result of the digestive, the other of the respiratory function.

Since the researches of Priestley and Ingenhouz directed the attention of philosophers to the effects produced by plants on atmospheric air, very different opinions have been successively, and some that are incompatible, simultaneously inculcated; speculation being so far confused with facts, that the simplest phenomena have been misunderstood, and the clearest indications have led theorists astray. Thus, when it had been shewn that confined portions of atmospheric air—in which a lighted taper had been burned till it became extinguished, so that the oxygen was converted into carbonic acid, and the air rendered irrespirable—became again purified in the course of a few hours if growing plants were placed therein, so that the air would again be respirable, *i. e.*, again would support the combustion of a taper, it was by some precipitately assumed that this change resulted from the respiration of the plants: and the opinion became prevalent, that vegetables breathe carbonic acid, and convert it into oxygen by the retention of its carbon, so that, by their agency, the deterioration of the atmosphere caused by the respiration of animals, and by combustion, which convert large quantities of oxygen into carbonic acid, was counterbalanced, and the consumptive process of the one neutralized by the restorative respiration of the other. This idea was beautiful, and we can scarcely wonder that it was with difficulty relinquished, even when other experiments seemed to shew that it was untenable, viz., those which prove that, in the shade and in the dark, or during the night, vegetables, so far from improving the constitution of the atmosphere by converting the superabundant carbonic acid into oxygen, deteriorate it by consuming a part of that already there, and replacing it by carbonic acid, just as is found to take place with animals: but there are quite wonders and beauties enough in the truths of nature to excite our admiration, without wishing to increase their number from the pages of romance.

'The phenomena here adverted to, although produced in a great measure by the same organs, are nevertheless the results of different functions; the one being the effect of the respiration, the other of the digestion of the vegetable, as already stated. Hence it is not that plants at one time respire carbonic acid and convert it into oxygen, and at another respire oxygen and convert it into carbonic acid—thus breathing differently at different times, and undoing by night what they had done by day—but that the respiration of plants, always and at every time, by day as well as by night, in the sunshine as in the shade, convert oxygen into carbonic acid, which process seems essential for the maintenance of their vital irritability, for, if deprived of it, they die. This doctrine was first, I believe, adumbrated by Darwin, who guessed, although he did not give any experiments to substantiate or even to illustrate his speculation, that the oxygen restored by plants to deteriorated atmospheric air was derived from a source independent of their respiration; and this, he fancied, was the decomposition of water by the assimilating powers of the plant, by which the hydrogen was retained to form its peculiar proximate principles, as oil, resin, &c., while the oxygen was liberated. This, doubtless, in some measure is the case; but, as Ellis (some of whose experiments I have repeated) decidedly shews, the chief restoration is effected by the decomposition of the carbonic acid present, so that when that gas is wholly withdrawn, very little if any oxygen is produced. Ellis, likewise, as well as Darwin, very philosophically distinguishes between the processes by which these different results are produced, although the distinction seems since to have been too generally lost sight of. Thus Thompson observes, it is 'pretty clear that the leaves of plants absorb oxygen, and the whole series of chemical experiments on plants led to the supposition, that this absorption was confined to the night;' and, after a reference to the labours of Saussure and others, he continues:— 'During the night plants absorb oxygen, and form with it carbonic acid; a portion of this gas is sometimes emitted, together with a little azote, but the greatest part is retained and decomposed by the leaves during the day.' 'Plants,' he continues, 'will not live without this nightly inspiration, even

though supplied with carbonic acid, provided the oxygen formed by them during the day be constantly withdrawn at the approach of night;' and so on. Thus he treats of both changes as if the effects of the same function, although it is not clear whether that function is considered by him respiration or digestion, as the term absorption is used in one place, and inspiration in another. This statement is certainly much less perspicuous than that of Ellis, but who nevertheless seems to have erred in the doctrine he insisted on, viz., ' that this operation of affording oxygen is not properly a vegetative function, but only a subordinate office; for experiments have led me to conclude that it is more essentially a vegetative function as depending on the assimilation of the food, than even the respiration itself.

Such being the state of the inquiry, several problems appear to demand solution, and these we will shortly discuss in order. 1st, What are the changes produced by plants on atmospheric air? 2dly, Are these changes the effects of, and necessary to, healthy vegetation; or are they merely accidental to the vegetable structure? 3dly, Are these changes the varied results of the same, or the unvarying results of different functions?

1st. What are the effects produced by plants on ordinary atmospheric air?

Experiments: Into several large wide mouthed stoppered bottles, (the stoppers during all these experiments being further secured by cerate, and in most instances inverted and the necks placed under water,) leaves of sunflower, nasturtium and other plants were severally introduced, and some put in the sunshine, some in diffuse light, some in the shade, and some enveloped in opaque coverings and kept wholly in the dark. These bottles all contained, besides the leaves, ordinary atmospheric air and a little water. At the end of six hours they were tested with lighted tapers and with lime water. The tapers were instantly extinguished when introduced into those which had been kept in the dark; in those bottles which were standing in the shade, they burned but feebly; while in the two others they burned brightly, especially in those which were exposed to the direct rays of the sun. Yet when the untouched duplicates of these were tested by means of lime water, all of them without exception formed precipitates,

though most abundantly in the shaded ones; thus shewing the production of carbonic acid in every variety of situation. These experiments were repeated many times. Again, fresh healthy plants were put into bottles as before, but instead of plain water, lime water was introduced; and after being in the same situations for upwards of six hours, the lime was found on examination to be precipitated in all. Again, fresh plants were introduced into the bottles with a little plain water as in the first experiment, but closed phials of lime water were likewise put into the larger bottles, which being firmly stopped were put as before in the four different situations, and after six hours, some of them were tested by tapers, which were extinguished in those in the shade, and burned in those in the light as at first; and then the duplicates of each, by inversion, allowed the lime water to escape from the close phials, and immediately precipitates of carbonate of lime were formed in all. In the foregoing experiments healthy growing plants were invariably selected. In the following, healthy plants were put into some of the bottles, and unhealthy or decaying ones (not dead) into the duplicates: these, as in the previous trials, were placed in the four different situations with regard to light; at one time the bottles being filled with common air, and subsequently, in repeating the experiments, tapers burned out in each. After six hours they were examined: and first, of those which had healthy plants and which had stood in the sunshine; tapers when introduced into these burned brightly, both in the bottles which had had them burned out in them before (so that their oxygen had been converted into carbonic acid), as well as in the others; and thus the carbonic acid must have been reconverted into oxygen, for that this was the chief source of that gas was evident from the circumstance of the introduction of lime water into some duplicates, when the precipitate was much less than when the carbonic acid was thrown down at first after burning out the tapers, and without exposure to the sun. The air in the bottles which stood in diffuse daylight supported the combustion of a taper when reintroduced, but feebly; while those in the shade extinguished it at once. The bottles which contained unhealthy plants, with the oxygen of the air converted into carbonic acid by the combustion of a

taper, did not in any instance again support combustion, although exposed for the usual time to the sun's rays; and furthermore those which were placed in ordinary air depraved it and formed abundance of carbonic acid even in the sunshine. In subsequent experiments healthy plants exposed to brilliant gas light for four or five hours purified depraved air, though less decidedly than daylight.

2dly. Are these changes produced in atmospheric air by plants dependent on their vegetation, *i. e.* are they effected by the influence of vegetable life, or are they the effects of mere mechanical structure, such as their filamentary covering and the numerous points and angles that plants afford, by which the solar light may be assisted in decomposing and separating the constituents of the air or water? To ascertain these points, which have frequently been mooted, numerous experiments were instituted, in which water with sand, hanks of cotton, wool, &c. &c., were inclosed as in the former experiments, the oxygen being converted into carbonic acid by combustion, and in some the carbonic acid being thrown down by lime water, but in no instance, whether with or without the presence of this gas, was oxygen reproduced, so as to re-allow the combustion of a taper. Similar experiments were tried with dead plants and with similar results. Thus it is evident, that light, assisted only by the filamentary forms of lifeless matter, is unable to effect those changes which the living plant so quickly and so certainly induces; nay, further experiments shew that the decaying leaves of plants and newly turned up mould deprave the air in which they may be confined.

3dly. Are these changes, which living plants effect, the varied results of the same, or the unvarying results of different functions?

Experiments on unhealthy plants shew that they deprave the air, both in the sunshine and the shade; if the leaves of healthy plants be crushed so as to interfere with the due performance of their functions, they deteriorate the atmosphere likewise. Healthy plants inclosed in vessels of carbonic acid are speedily destroyed, whether kept in the light or not. Plants made to grow in the dark, although they increase in size, do not augment the absolute volume or weight of their

solid contents; for Saussure has proved, which other experiments have verified, that seeds, bulbs, slips, &c., made to grow under such circumstances, when incinerated, and the unassimilated water driven off, weigh less than similar plants did before their exclusion from the light; so that matter has been lost by them in the formation of carbonic acid, and none gained by assimilation of their food. In germinating seeds the same phænomena notoriously occur.

Again, I have found by other experiments, that if vigorously growing plants be inclosed in bottles containing a little lime water, and the oxygen be converted into carbonic acid by the combustion of a taper and then precipitated, that if kept in the sunlight for a few hours, a small quantity of oxygen is reproduced, which appears to be derived from the decomposition of the water; as it can scarcely be supposed that the chalk affords it: and this opinion receives further confirmation from an experiment of Sir Humphry Davy; in which a vine-leaf was inclosed in a vessel in which aqueous vapour was conveyed through mercury from water that had been a long while in a state of ebullition, and yet a little oxygen was liberated, which Sir Humphry Davy believed to be derived from the decomposition of the water—which decomposition is a vital action of the plant assisted by the stimulus of light. Furthermore, experiments shew that whenever carbonic acid is produced in excess, the solid substance of the plant is lessened; but on the contrary, when oxygen is evolved, that its solid materials are increased.

From these, which have been selected from numerous other experiments, are we not justified in concluding that the production of oxygen and its converse, the formation of carbonic acid, are the unvarying results of two different functions: *viz.*, this of respiration, that of digestion; and that both are vegetative actions, dependant on vitality?

To conclude, the formation of carbonic acid is constant both by day and by night, during the life of the vegetable; it is equally carried on whether in sickness, or in health; it is essential to its existence for the sustentation of its irritability; for, if deprived of oxygen and confined in carbonic acid gas, plants, like animals, quickly die. This function, which is per-

formed chiefly by the leaves and petals, though also in a less degree by the stems and roots, like the respiration of animals, is attended with and marked by the conversion of oxygen into carbonic acid: it is the respiration of plants.

Again, vegetables at certain times and under certain circumstances decompose carbonic acid, and renovate the atmosphere by the restoration of its oxygen; but this occasional restoration is dependent not on the respiratory, but the digestive system; it in part arises from the decomposition of water, but chiefly from the decomposition of carbonic acid, absorbed either in the form of gas or in combination with water, either by the roots or leaves, or both: and here, again, the analogy holds good between the functions of respiration and digestion in animals and plants, for to both is carbonic acid deleterious when breathed, and to both is it invigorating to the digestive system when absorbed as food.

The presence or absence of light seems to have little or no influence on the respiration of vegetables; but it produces very notable effects on their assimilating powers, by enabling the specific vitality of the plant to separate from air and water those principles which they hold in solution or combination, which are necessary for their support, and to liberate such others as may be too abundant in the crude aliment they introsuscept; for plants growing in the dark become etiolated, and assimilate but little solid matter, and scarcely ever form their appropriate and peculiar secretions: thus assimilation tends to increase, respiration to decrease the solid materials of the plant. Another agent of great power, though hitherto scarcely noticed, is the electricity generated by plants, and excited by their profuse perspiration. I have frequently found, by experiments such as have been related, the perspiration of leaves and plants to be a moiety of their own weight, and this for days and weeks together: and in some extreme cases, as was shewn by Hales, plants will perspire during twenty-four hours as much water as is equal in weight to two-thirds of their gross bulk. This abundant exhalation must be designed to answer some important end, and this use I believe to be (as at another time I shall attempt to shew) to keep the plant constantly in that electrical state which will favour the entrance of fluid into

the roots, and its passage upwards through the stem into the leaves: it also must have much influence in changing and overcoming the chemical attractions of the elements of vegetable food, and seems to be one great agent in the curious process of vegetable assimilation.

CONTRIBUTIONS TO THE PHYSIOLOGY OF VISION.

No. I.

Under the above title it is proposed to bring forward those stores of knowledge on this subject which have been hitherto locked up in the repositories of foreign scientific literature. The physiology of vision has a peculiar claim on the attention of philosophers, as presenting some of those links which connect physical with mental phenomena. Metaphysicians, physiologists, natural philosophers, and artists, have equally made it an object of their study; and the names of Baptista Porta, Leonardo da Vinci, Kepler, Descartes, Newton, Berkeley, Reid, Buffon, Darwin, Wells, Brown, Young, &c., are among those who have advanced the inquiry by their investigations and discoveries. That the subject is of such equal interest to so many different classes of inquirers, is perhaps the cause that, as a whole, it is so imperfectly known. Each person who occupies himself with its study, looking at it only from his own point of view, disregards those facts which he considers as belonging to the province of others, and thus is unable to arrive at those general conclusions which can only be obtained from a complete survey of all the various phenomena and their relations. To render some assistance towards forming a more complete theory of vision, we shall successively give an account of the discoveries of Purkinje, Goethe*, Mile, Müller, Plateau, &c. The number of these interesting memoirs on this interesting branch of science, which have been entirely unnoticed in this country,

* An account of the 'Farbenlehre,' or theory of colours, of this illustrious poet and philosopher, will form one of the subsequent papers of this series.

might surprise us, did we not know that the same neglect extends to many other important departments of knowledge.

'*Beiträge zur Kenntniss des Sehens in subjectiver Hinsicht, etc.*' (Essay on the Subjective Phenomena of Vision, by Dr. J. Purkinje, Professor of Physiology at the University of Breslau.) Prague. 1823.

This little volume has excited considerable interest in Germany; it relates to those appearances which, independently of external objects, are perceived in the organ of sensation itself. To distinguish these phenomena from those which arise on the presence of their appropriate external objects, the author employs the term subjective, which, as denoting this class of phenomena better than any other we are acquainted with, and, to avoid circumlocution, we have purposely retained: it will, however, on consideration, be perceived, that the term is not strictly proper, as, correctly speaking, all phenomena, *as such*, are subjective, *i. e.* in the mind; and were we, without qualification, to admit the classification of phenomena into objective and subjective, we should be unable to determine, with any degree of accuracy, where the objective ends or the subjective begins. Thus, the vessels of the eye and the retina itself are subjective, considered as parts of the visual organ; yet we shall see that in some of Dr. Purkinje's experiments they become real objects, and are perceived as such. But we shall not further discuss this question; what we have said will be sufficient to explain the term subjective as employed by Dr. Purkinje and by ourselves in the following extracts. We now proceed to an abridged description of the most interesting of Dr. Purkinje's experiments.

I. *Luminous Figures produced by rapid alternations of Light and Shade.*—These figures are most distinctly seen in the following manner: the eye-lids being closed and the eyes directed towards the sun, the observer quickly moves his hand with the fingers spread, from one side to the other, so that the luminous rays are alternately intercepted and admitted; at the beginning of the experiment a yellowish-red glare is perceived which is afterwards replaced by a beautiful and regular figure, which it is, however, impossible to fix or determine,

unless the experiment be continued for some length of time. Figs. 1, 2, 3 and 4 represent the phenomena as observed by Dr. Purkinje in his right eye.

Fig. 1 consists of small squares, chequered as in a chess board, and alternately bright and dark; this entire figure is bounded by zigzag lines, which are continually varying in direction, length, and brightness, and which appear rather more illuminated than the squares; at the centre of the square field is a dark point, with a luminous area, surrounded by rapidly moving semicircular lines, which nearly resemble rose-leaves in form; these are around and principally below the luminous area; below these semicircular lines is a field of hexagons, the circumferences of which are gray, and the centres white. This figure, Dr. Purkinje says, may be obtained very distinctly, and without any admixture of the other figures, if the experiment be modified so that the eye, being open, is directed towards an equally illuminated white wall, and the spread fingers are moved before it; if the experiment be made as before described, the secondary figures rather predominate. The appearances also take place under various other circumstances and modifications; for instance, the semicircular lines at the centre of the figure are particularly visible when the eyes are directed as near as possible to the flickering flame of a candle. The square field is also seen by looking at Newton's circle of colours, when it is in rapid motion; in this case it is not necessary that the colours be distributed in any particular manner, for the experiments will succeed, if the segments of the disc be merely alternately light and dark; the nearer the segments are to each other, the less rapid the motion of the circle is required to be, but bright sunshine is indispensable to the experiment. Lastly, the figure is seen when a wheel rapidly revolves between the eye and the sun, or a strong light; and it appears accordingly that the general condition of the phenomenon is a rapid alternation from light to shade.

The secondary figures, as Dr. Purkinje calls them, are indistinct when the experiment is made whilst the eyes are open; they appear under two modifications, the rectangular spiral, and the star with eight rays: at the commencement of the experiment, whilst the eyes are not over excited, both figures

appear, as it were, mingling with each other, the radiated figure evidently predominating (fig. 2); but as the experiment continues, the rectangular spiral (fig. 3) becomes more visible, and the star gradually disappears: the central line of the spiral is the smallest and darkest, as will be seen by the figure, and has an oblique direction to the right and below; the line itself consists of a darker axis and a bright margin, and is divided, as it were, into joints; towards the periphery of the figure the axis becomes enlarged, and fades to a greyish tint; the lateral margin also loses its brightness, and at the termination of the spiral line the illumination seems even to be inverted —that is, the centre appears light, and the circumference dark: it is, however, impossible to determine with great accuracy the external parts of the figure; the intervals between the coils of the spiral are occupied by a faint gleam of the squares of fig. 1.

In the two oblique lines of the star (fig. 4), the light axes are brighter than in the other two lines; in the latter, on the contrary, the dark borders are of a deeper black. The spiral and radiated figures are in continual motion and fluctuation: sometimes the rectangular spiral changes into a triangular one; at other times the centre of the star dissolves, and the rays intersect each other at various points, or become parallel, or form squares, triangles, &c. The four figures above described are, however, those which most frequently occur; and though, as Dr. Purkinje judiciously remarks, these subjective phenomena might appear to other eyes different from what he observed, yet the experiments made by others at his request seem to confirm his own observations: we may therefore, perhaps, be justified in concluding that the above phenomena do not depend on a morbid or individual condition, but physiologically result from the very organization of the human eye.

The figures in Dr. Purkinje's left eye, the sight of which was very weak, were very indistinct, but did not in any other respect appear different from those perceived by the right eye: the squares were more like network formed by curved lines; the secondary figures were apparently the same as before, but, as might be supposed, were in the opposite direction.

II. *Figures produced by pressure on the eyeball.*—If gentle

pressure be exerted on the middle of the eye, a large luminous ring is seen, which, on close attention, will be found to resemble fig. 5. It consists of numerous small oblong rectangles, obliquely arranged, and more or less bright and obscure; the sides of the figure have an oblique position, and its form is that of a rhombus with obtuse angles; the centre, as well as the space round the rhombus, is dark, but gradually becomes traversed by a luminous star (fig. 6). The rectangles become more intensely illuminated, and after some time one of the angles is filled by a yellowish-white spot with distinct edges (fig. 7), which progressively enlarges, and ultimately occupies the entire rhombus. In this luminous space, which is now of a bluish colour, very small circular lines are observed, which are either concentric or variously intersect each other, and seem to be in a continual glimmering fluctuation (fig. 8): at the circumference of the rhombus there is a very narrow orange-coloured ring, and round this is occasionally seen beyond a dark interstice a larger ring of the same colour. On discontinuing the pressure, the figure successively repasses through all the metamorphoses in an inverse manner to that in which they have taken place.

When a strong pressure is made on the eye, the figure 9 is seen. The serpentine rays seem to proceed from the centre, and are in continual fluctuation from brightness to obscurity; after some time the black intervals between them become filled with squares (fig. 10); the radiated figure then gradually disappears, and the square field itself terminates at last in the luminous rhombus. The pressure being still increased, the appearance represented in fig. 11 is perceived; the luminous spots alternately appear and disappear, and during their disappearance are replaced by black spots, which again give place to the luminous ones; the larger spots, which are of a bluish tint, appear and disappear more slowly than the smaller ones, which occur nearer the centre. On continuing the pressure, the small luminous spots gradually fade away, while the larger spots near the circumference remain much longer, but they ultimately also disappear in succession: in the mean while a vague and continually fluctuating gleam has been dawning, which now develops itself into various groups of spots, rings,

and squares, which after some time arrange themselves into small squares and larger hexagons (fig. 12). To see this figure distinctly the pressure must be equal, and the eye be kept steady, for on the least motion of the eyeball a general fluctuation prevails, and no defined figure can be distinguished: sometimes curved lines are seen, which rapidly move round a centre in alternate directions. If the pressure be discontinued, luminous ramifications, the fragments of a figure which will be hereafter described, appear (fig. 13): the appearance subsequently terminates in the luminous rhombus, &c.

When the luminous rhombus has been produced by gentle pressure, if the eye be opened and directed towards the unclouded sky, numerous parallel and converging grey semi-transparent lines will be perceived, which evidently correspond to the bright oblong rectangles, and on closing the eye they again become luminous. On opening the eye during the appearance of figs. 9 and 10, the daylight is at the first moment invisible: suddenly the figure bursts, as it were, at the centre, rapidly opens towards the circumference, and at last entirely disappears. If the eye be opened when fig. 12 has been produced by strong pressure, twenty seconds sometimes elapse before the daylight can be seen, and even then it is obscured for a considerable time by opaque lines and spots.

Similar figures to those produced from pressure, particularly the rectangles, are produced by impeded circulation through the brain, violent exertions, and the use of narcotics; they appear also during fainting fits, strong mental emotions, &c.

Dr. Purkinje institutes a comparison between the above and acoustic figures, and concludes that both phenomena are objectively identical: the primary rectangular, &c., figures he considers to be analogous to the small reticulated undulations communicated from a sounding plate to the surface of a liquid; the secondary ones to those which are caused by the intersection of the undulations. He does not, however, state where he conceives this undulatory motion to be: probably they arise in the humours of the eye, and are thence communicated to the retina; but he speaks only of a contraction of the eyeball as its immediate cause. The luminous rhombus he considers to be caused by the lens; the figure 13 by the central vessels of the eye, &c.

III. We come now to some most interesting experiments, viz., those concerning the *luminous figures produced by galvanism.* These experiments were made with a pile of twenty pairs of copper and zinc plates, and layers of cloth dipped in a solution of muriate of ammonia. The pile was constructed in the following manner: zinc, copper, moistened cloth, zinc, copper, &c.; zinc being the undermost. When the eyes were shut, whilst the positive conductor was placed in the mouth, and the negative wire made to touch the middle of the forehead, fig. 14 was perceived: it consisted of a dark arch, traversing the centre of the common field of vision, with its concavity upwards, and the extremities losing themselves imperceptibly in a lateral direction. Above the arch there was a bright violet gleam, the greatest intensity of which was towards the middle of the arch; laterally from this gleam there were two distinct dark spots, which apparently correspond with the insertions of the optic nerves: the space below the arch was also filled with a bright violet gleam, but so that the greatest intensity was seen externally, in the form of luminous roses. When, during the experiment, the right eye only was kept shut, one half only of the figure was seen, but with this difference, that the brightest point of the upper light was seen in the visual axis. When the galvanic poles were changed, the contours of the figure remained the same, but the violet light was changed into a faint yellow glare, the intensities of which were also distributed in an inverse manner, viz., the middle of the field of vision above the arch, and the lateral points below it, being darkest, and the dark points which correspond with the insertions of the optic nerves appearing as distinct bright violet-coloured spots. The direction of the transverse arch was further observed to change in a remarkable manner, according to the different places which the conductors were made to touch during the experiment. When the wire was transferred from the middle of the forehead to the bridge of the nose, the centre of the arch became depressed, and its extremities were raised; when it was carried along the lower eyelid from the inner towards the outer angle, the arch gradually became indistinct, and ultimately seemed to be divided. At the outer angle the appearance

was similar to fig. 15: the oblique and almost perpendicular direction of the arch was of course gradually changed into the former horizontal one, when the wire was carried back to its former place. During a quick repetition of shocks, there appeared, in the light places above and below the arch, parallel curved lines, alternately light and dark, which intersected each other and formed squares, but of much larger size than were observed in any of the former experiments: these squares were also, and still more distinctly, seen when the lower conductor was brought into contact with that near the eye. When, during the galvanic experiment, the eye was pressed, the luminous rhombus, &c., appeared, and nothing could be seen of the galvanic figure; when strong pressure was exerted, figs. 21 and 22 were perceived, and on every shock the ramifications proceeded from the dark centre with a most beautiful violet-coloured light.

IV. *Nebulous striæ.*—If the eyes are well protected against external light, and the observer fixes his attention to the darkness before them, nebulous figures and glares are soon seen to arise, which at first are extremely vague and almost formless, but gradually acquire a more distinct and perceptible shape; they consist of luminous streaks with dark intervals, and move in a centripetal, transverse, or circular direction (figs. 16, 17, 18). Their motion is rather slow, and, in Dr. Purkinje's eye, about eight seconds elapsed between the rising and disappearing of one of the transverse streaks. When the experiment was continued for a few minutes, Dr. Purkinje distinguished the following figures:—

1. A feeble glare in the middle, surrounded by dark concentric rings, and the intervals between them filled with a faint light, which gradually loses itself in the darkness of the rings; the whole figure is in continual centripetal motion, the gleam of light in the middle gradually fades and makes room for the shade of the next ring, which, having now become a dark spot, also disappears, &c.

2. At other times the light comes from above as a large horizontal luminous streak (fig. 17); it slowly moves down-

wards, and, on approaching towards the middle, its lateral ends bend until they unite and form a luminous ring, which is then dissolved into darkness, as in the preceding case.

3. The luminous streak comes from below and moves upwards. Sometimes the streaks move in rather an oblique direction.

4. The appearance is as in fig. 18, and the nebulous streak moves in a circular direction, like the sails of a windmill.

When the experiment has been continued some time, and the attention becomes exhausted, all regular appearances dissolve themselves into a fluctuation between light and obscurity, which ultimately terminates in a feeble gleam covered as it were by a veil.

V. The following is another instance of subjective vision. When the eyes are fixed on a large illuminated surface (a white wall, a regularly clouded sky, &c.), the observer sees, after a few seconds, bright points suddenly starting up in the midst of the field of vision; they rapidly disappear, making room for black spots, which also quickly dissolve. If, after the appearance of the bright points, the eyes are shut or directed to a dark surface, the phenomenon continues; but in a milder light the bright appearances are changed into a feeble glimmer. The bright points are also seen when the eyes are shut before they have appeared.

VI. *Place of insertion of the optic nerve.*—It was first shewn experimentally by Mariotte, and was afterwards mathematically ascertained by Euler and Bernoulli, that the image of an object disappears in that point of the field of vision which corresponds with the insertion of the optic nerve. Besides this, there are some other circumstances under which objects within the field of vision will disappear. If a number of black dots are made on an equally illuminated surface, with one of them in the middle, and the eye is fixed to the central dot, an indistinct nebulous floating begins, and some of the dots, sometimes all of them, alternately disappear and reappear, whilst the light ground on which they are marked remains unaltered.

That the place of insertion of the optic nerve is not entirely insensible to light, as has been sometimes stated, appears from the following simple experiment: If a small flame be placed in the projection of that part of the field of vision which corresponds to the insertion of the optic nerve, it will directly disappear, but in its stead a beautiful red nimbus is seen; if the flame is slightly moved in a lateral direction, upwards, or downwards, there appears in the opposite side of the nimbus a dark gap, which spreads parabolically downwards or upwards, and the margin of which is coloured with the light of the flame. If the flame is moved in a small circle, the shade also shews a circular motion, being always opposite to that of the flame.

VII. If the eye, being well covered, is quickly turned outwards, a large luminous ring (fig. 19) will be seen, the light of which is in a constant glimmering fluctuation: this phenomenon is particularly striking in the morning, immediately after awaking, and then, besides the luminous orb, the entire field of vision, but particularly the upper and lower parts of it, is filled with large equidistant sparks. The central area of the orb appears of a grey colour if, during the experiment, the eyes are open and directed towards a white surface; and of a deep-blue colour if they are shut and directed towards the sun. If, during the experiment, the eye is directed to any other colour, the inner surface of the ring is not of the complementary, but of the same colour, though rather deeper. Round the luminous orb, towards the centre of the field of vision, there are concentric bright streaks with dark intervals (fig. 20). It appears that the luminous orb is produced by the nerve being forcibly stretched by the rapid lateral motions of the eye.

VIII. Another very interesting experiment is the following. If a flame, at about two or three inches distance, is slowly moved before the right eye in various directions, figure 21 appears painted as it were in the luminous area round the flame. The vessels, for such they evidently appear to be, seem to proceed from the insertion of the optic nerve, and consist

of two upper and two lower principal branches, which are variously ramified towards the middle of the field of vision, where a dark point is seen, which sometimes appears concave. A similar, but inverted figure is perceived in the left eye; but to Dr. Purkinje, who is weak-sighted in this eye, it appeared rather irregular and incomplete (fig. 22). The origin of the vessels is a dark oval spot, with a light areola; the figure itself, or rather fragments of it, are seen under various other circumstances. As was observed above, there can be no doubt that the figure is formed by the central vessels of the retina *.

* This is an easy experiment to repeat, and is certainly a singularly beautiful one; the blood-vessels of the retina, with all their ramifications, are distinctly seen projected, as it were, on a plane without the eye, and greatly magnified. I have found the experiment to succeed more perfectly when, the eye being stedfastly directed forwards, the light is made to move right and left below the eye, or upwards and downwards at the side of the eye; for when the flame is in the field of view the image is indistinct: the eyelids of the unemployed eye should not be closed, but the light should be obstructed by the hand or any other covering. It is indispensable that the light be in motion, for directly it becomes stationary the image breaks into fragments and disappears: during the motion of the light the image also moves, and in a contrary direction to that of the light. No image arises when the light moves to and from the eye, nor when it is alternately shaded and uncovered; the effect, therefore, cannot be attributed to variations of intensity in the light. One of the most remarkable circumstances of this phenomenon is that at the point corresponding to the projection of the foramen centrale, a crescent-formed image is occasionally observed; its appearance depends on the position of the light with respect to the axis of the eye: for instance, when the light is placed below the eye, the image appears on looking downwards, and becomes obliterated on looking upwards, and in general it appears on looking towards the light, and disappears on looking from it: the mark always appears concave in the direction opposite to the light. That the variable mark just mentioned is in the centre of distinct vision I ascertained by the following experiment: I impressed on my eye the spectrum of a coloured wafer, by looking intently on a black dot at its centre; on causing then the vascular image to appear, I saw the centre of the spectrum coincide with that of the mark. Dr. Purkinje has given no explanation of this phenomenon; the following is an endeavour to supply the omission. Were the blood-vessels which are spread on the anterior surface of the retina entirely opaque, they would prevent the transmission of light to the nervous matter beneath them, and their distribution would be constantly visible; but they are transparent, and in ordinary cases the intensity of the light which passes through them does not materially differ from that which falls directly on the retina. When, however, the retina is fatigued by a strong light, the veins become visible, because the retina is rendered insusceptible to a portion of the light they transmit; but this effect is only momentary, for those parts which are thus shaded from the more intense light promptly recover their usual susceptibility, and the images vanish: but they may again be made perceptible by displacing them on the retina; and by making them constantly change their places the images may be rendered permanent. The momentary appearance of these images may be frequently observed on looking at a strong light immediately after waking in the morning, and may be reproduced several times by successively shutting and opening the eyes. The mark in the middle of the field of vision is most probably a shadow, occasioned by a slight convexity or concavity in the retina at that point.

The more minute vessels of the retina may be rendered visible in the following

VIII. Dr. Purkinje has made numerous experiments on what are generally called ocular spectra, *viz.*, the images which remain after objects have been regarded for some time; he gives a more detailed account of them than we believe is elsewhere to be found, and the following extract will, we trust, be read with interest.

1. Looking stedfastly for a very short time at the flame of a candle, then quickly covering the eye, the bright image of the flame remains, but instantly disappears from the circumference towards the centre, leaving a red shining flame, which becomes invisible in the same manner, and is replaced by a white image; this also, though rather slowly, fades away, and after having completely disappeared, leaves a dark coloured contour of the flame with a greyish nimbus, which ultimately enlarges towards the centre, and thus terminates the whole appearance. If, at the beginning of the experiment, the eye, instead of being covered, is directed towards a white surface,

manner: place as near to the eye as possible a plate of ground glass, and upon its external surface lay a card, in which a large pinhole has been made; adjust this aperture so that it shall be in a right line drawn from the eye to the flame of a candle. When the card is kept in motion so as to displace continually the image of the light in a small degree (*vide* Scheiner's experiments), the veins will be seen distributed as above, but they will now appear brighter than before, and the spaces between the ramifications will be seen filled with innumerable minute tortuous vessels, which were in the former experiment invisible: in the very centre of the field of vision there is a small circle, in which no traces of them appear, and in the centre of this circle is seen a darker point. The same appearance will be seen by moving a pinhole close to the eye when looking at a ground-glass window-pane, an illuminated white wall, or a sheet of paper. The absence of vessels at the centre of the retina will probably account for the greater distinctness with which small objects are there seen, and also for the difference of colour observed by anatomists in that part of the nervous expansion.

The following experiment, as well as the preceding, is original, both having been observed by myself in attempting to verify the discoveries of Purkinje. In the ordinary circumstances of vision, particles floating in the humours of the eye, or specks in the cornea or crystalline lens, are invisible, because their shadows are projected by different rays of light on every part of the retina, thus permitting no distinct image of them to be formed; but they may be rendered visible by allowing only a single ray of light to fall on the eye through a hole made in a card by a very fine needle, and placing the light and aperture so that the object within the eye may be in a right line with them and the centre of the retina: they may be projected on any part of the retina, but they will be most distinctly seen at this point of most perfect vision. I have thus observed, in my own eye, collections of transparent globules which, from their free motions, evidently exist in the humours; and one remarkable spot (in my left eye), which, from its permanence, must be either in the cornea or the lens: after winking, the secretion from the lacrymal ducts is also very obvious.

These experiments may probably afford to the oculist a means of ascertaining, from the direct observation of his patient, various morbid changes in the retina and lenticular apparatus of the eye.—C. W.

the first two images of the flame are the same, but the former white spectrum is now of a dark grey colour with a white margin. The same appearances are obtained when the flame has been regarded for a longer time, except that the metamorphoses take place more slowly, and the proportion between the time during which the flame has been looked at, and the duration of the spectrum remains always the same, *viz.*, about 1 : 20.

2. When the flame has been stedfastly looked at for a much longer time (from twelve seconds to a minute) the succession of the images is nearly the same as in the first experiment, except that the bright and coloured ones rather predominate. First the bright image of the flame is seen, then the yellow, red, blue, white, and black images follow, and the whole appearance is ultimately covered by the grey gleam, as in the above case. All the images disappear in a centripetal direction, the first much more rapidly than the others, the black remaining visible for the longest time. During the experiment, fragments of the vascular figure are frequently observed; they are of the same colour as the image in which they are perceived.

3. If the sun or the focus of a lens has been stedfastly regarded for a short time, a bright white image remains, and lasts for a considerable time; the coloured spectra then appear, and follow each other in rapid succession.

4. If the windows be regarded on a cloudy day for about twenty seconds, and the eyes be then quickly covered, at first the panes are seen white and the frame black, but the former rapidly change into black and the latter into white; and after a repetition of these changes four or five times, the whole appearance is dissolved in a grey gleam.

IX. Very different from the ocular spectra are what might perhaps be called the mental spectra, viz., the images of objects before the internal eye, (if the expression may be allowed,) after they are inaccessible to the external sense, as for instance, during winking, or whilst shutting the eyes during meditation. They seem to depend entirely on the observer's will and attention, and may even be recalled after having com-

pletely disappeared: this is, in fact, a memory peculiar to one particular sense, and thus far, perhaps, the purest instance of *subjective* vision. The ocular spectrum goes through its regular metamorphoses; and so far from its distinctness being proportionate to the observer's attention, it is most prominent if his look only is fixed to the object—his thoughts being otherwise engaged. The ocular spectrum further follows the rotation of the eye, whilst in the mental spectrum the objects maintain their real position, independent of the motions of the eye and the body. Narcotic and spirituous substances, an excited state of mind, some febrile diseases, congestions towards the brain, &c., appear to augment the permanence and clearness of the mental spectra; that of the ocular spectra is increased during a nervous asthenic state, &c.

X. Dr. Purkinje says, 'I am standing before a white surface, and direct my eyes as if I were looking at a very near object. I perceive in the midst of the field of vision a white transparent circle, with a brownish, semitransparent area, and an indistinct border. If I now discontinue the effort, the brownish area disappears, and the white surface is at its circumference brighter than anywhere else. If, whilst the effort continues, a slight lateral pressure is made on the eyes, the area becomes opaque, of a dark-brown colour, and lined at its outer side with a light violet semitransparent border: the white circle in the midst continues, but on increased pressure a brown central point is seen in it. If the eye is closed, and well secured against external light, the circle in the middle appears dark, and the brown colour of the area is changed into a feeble gleam.'

During this experiment the brownish area sometimes presents a peculiar phenomenon, which, according to Dr. Purkinje, is the circulation of the eye becoming visible, in the shape of a series of globules (fig. 23) on each side of the white circle, ascending on the left and descending on the right side. The 'mouches volantes,' Dr. Purkinje is inclined to consider as depending on the same cause: they are best seen if, after violent exertion, the eyes are steadily directed towards a white equally illuminated surface, as the clouded sky, or a snow-field; a

large quantity of bright points (fig. 24) are then seen, which, like shooting stars, suddenly arise and disappear after a rapid motion, in various straight and curved lines. On close attention, it will be found that every light point is accompanied by a shade at the opposite side of the field of vision, and that also between the small, larger but less bright points are slowly moving. These larger points are very distinctly seen after violent exertion, particularly after lifting a weight: they move from the extreme margin of the field of vision towards the middle, and are in a straight or bent direction, always accompanied by a shade at the opposite side; the nearer they come to the middle, the less distinct and shining do they appear, and the less dark are their shadows. As they are visible only as long as the eyes are held open, and as they require a strong and equal light, in order to be seen, they must be considered as differing from the bright points described above, as far as these evidently depend on the different state of the various points of the retina, whilst the phenomenon in question is caused by external bodies, with reference to the retina, viz., according to Dr. Purkinje's opinion, by free blood-globules in the aqueous humour; which, according to their different distances from the crystalline lens, are seen of different size and distinctness.

XI. *Luminous Rings.* This phenomenon, which is sufficiently known to be caused by lateral pressure on the eye, has been carefully examined by Dr. Purkinje: the following are the results of his experiments:—

1. If the observer makes an effort, as if to look at something very near, the slightest pressure produces the luminous ring; whilst, on looking at a distance, the pressure must be considerably increased.

2. The rings, as well as the places of insertion of the optic nerves, are most vivid in the morning, and the proximate cause of both phenomena appears to be identical, viz.; pressure on the retina.

3. If a piece of white paper is held in the inner angle, and whilst the eye is as much as possible directed towards it, the observer presses, with a small conical piece of wood, on the external side of the globe, near the orbit, a great number of

concentric white and black lines are seen (fig. 25), similar to the appearance of fig. 20; they extend over the spot in the middle of the visual field (fig. 20), and are always parallel wherever the pressure may be exerted. On the paper there appears, at the same time, a large black circular spot, the centre of which is of a dark bluish green, or deep violet, similar to the eye of a peacock's tail, sometimes with fragments of the vascular figure (fig. 13). That side of the spot which is directed towards the middle of the field of vision, touches at the above-mentioned parallel lines; the opposite side is bordered by a yellowish-white gleam, which, on increased pressure, reaches as far as the middle of the black spot.

4. If by placing the piece of wood between the orbit and eye-lid, the pressure is exerted on the posterior point of the globe, the parallel lines are seen extending towards the middle of the field of vision, and terminating in a white semilunar streak, which, in its concavity, contains a small bright circle, and at the convex portion of which there is a brownish semilunar spot; both spots follow all the motions of the coloured eye, and turn round the centre of the field of vision as on an axis. If the pressure is increased, the coloured eye advances towards the white semilunar streak, so as to cover it entirely with the exterior of its middle portion, which remains as a white circular spot in the middle; the brown semilunar spot also disappears.

5. If the pressure on the globe is suddenly discontinued, the white circular spot as suddenly moves outwards, and in its stead a light-brown violet cloud remains, which is divided by a white streak into two parts, the upper of which is larger and darker; this cloud, especially the middle portion of it, generally remains for a considerable time, and greatly impedes clear vision.

6. The experiment in question shews also the coincidence of the two fields of vision; for if each eye is pressed at the corresponding place, the luminous rings are always seen to coincide.

7. If the eye is well covered during the experiment, the colours in the middle of the circular spot, as well as the margin round the periphery, are luminous; the concentric lines are very

indistinct, and of a faint gleam, and the yellowish-white glare at the outer side of the circular spot is black. On suddenly discontinuing the pressure, a bright luminous streak flashes from the inner towards the outer side.

Dr. Purkinje has evidently bestowed much time upon these experiments. It appears from some passages in his work, that he began, even in his boyhood, to amuse himself with some of the luminous appearances therein described. The study of physiology afterwards led him to an accurate and scientific inquiry, which he even pursued at the risk of health; for, although he in one passage of his work states that his experiments had not been injurious to his sight, the circumstance of his right eye being myopic, and the left near-sighted (amblyopic,) seems almost to contradict this assertion; we ourselves cannot, after a great number of experiments which we made before and since our perusal of Dr. Purkinje's work, withhold our conviction, that their frequent repetition may be attended with dangerous effects on the eyes. On the other side, it is indispensable that the experiments should be frequently repeated and varied; for at the commencement of the inquiry the observer must be quite unaccustomed to this new field of experiment. The condition of Dr. Purkinje's sight might further raise some doubts whether some of his experiments be not the effects of a morbid state, rather than depending on the organization of the human eye.

We have not yet exhausted the experiments which this interesting pamphlet contains; some of those which are now omitted we shall have occasion to refer to in our succeeding papers*.

* Since the preceding pages were printed, we have ascertained that the interesting experiment of § VIII. was first described by Steinbuch, in his *Physiolie der Sinne*, 1811.

DESCRIPTION OF THE HORNS OF THE PRUSSIAN ELK; DIFFERENCE BETWEEN THEM AND THOSE OF THE AMERICAN MOOSE-DEER*.

By WILLIAM WITTICH, Esq.

THE European elk and the American moose-deer are still considered by naturalists as belonging to the same species. Pennant and even Cuvier seem to have no doubt respecting their identity. Blumenbach and some other naturalists, indeed, are less decisive in their opinions; but their doubts rest on conjecture. The reason of this uncertainty seems to be, that a sufficient number of data have not yet been accumulated, to enable the promoters of science to form a clear and decisive judgment. The small number of facts which till now have been well established, refer to one side of the question; they regard almost exclusively the American moose-deer. This animal has often been brought to France and England from the transatlantic shores, where it is found in numerous herds; and on our continent it has been subjected to a more minute and accurate investigation. But the *Scandinavian* elk has *perhaps* never found its way to London or Paris; and what I dare not affirm positively of the Scandinavian elk, I believe I may assert with certainty of the *Prussian*—it was never seen in London or Paris.

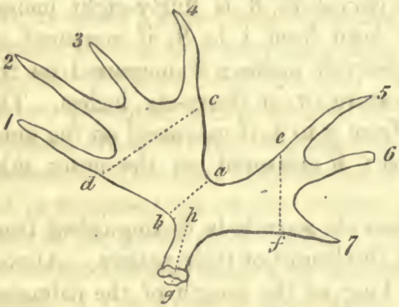
The same may be observed respecting the collections of elk-horns: they consist almost exclusively of horns of the moose-deer; and what is not known to belong distinctly to that species is somewhat doubtful. That is the case with the collection in the British Museum; and if I may judge according to the drawings given in Cuvier's great work (*Recherches sur les Ossemens Fossiles*, tom. iv., pl. iv. 24—29), the Parisian collection likewise does not contain the horns of the Prussian elk-deer. As they, however, seem to exhibit in some points a different formation, I shall give here the description of a pair

* Cervus Alces, Linn.

of Prussian elk-horns in the possession of Mr. Plaw, Modifort-court, Fenchurch-street.

This pair of Prussian elk-horns weighs twenty pounds and a half: the larger weighs ten pounds and a half; the smaller

Horn of the Prussian Elk.



Horn of the Moose-deer.

***The horn of the Moose-deer is copied from a sketch in the "Recherches sur les Ossémens Fossiles" of Cuvier,* tom. iv., pl. iv., p. 70.**

exactly ten pounds. The palmated part is divided by a deep and wide cut between the antlers 4 and 5. This cut terminates exactly over the stem.

The two palmated portions, formed by the above-mentioned cut, are not equal in extent; but the surface of the smaller contains more than one-third of the whole. The larger portion has four antlers, the smaller three. The neck, formed by the cut, and uniting both palmated partitions, is only *four* inches wide (from *a* to *b*). The breadth of the larger partition at the root of the antlers (from *c* to *d*) is ten, and that of the smaller (from *e* to *f*) is eight inches and a half. The length of the larger partition, from the root of the largest antler to the neck, is twelve, that of the smaller ten inches.

The largest antler of the larger partition is ten inches long; the others from seven to ten. The antlers of the smaller partition are from five to eight inches long. The circumference of the largest antler at the root is six inches and a half; at that place it is still somewhat palmated, but two inches further up it is round. The stem of the horn (from *g* to *h*) is six inches long; from the root of the stem (*g*) to the termination of the cut (*a*) are ten inches.

The two partitions do not lie in the same plain; but the

largest part of the smaller partition forms an angle of almost 90° with the prolonged plain of the larger partition; both are united together by a bending of the smaller partition towards the common neck. The width of the whole horn from 1 to 5, if measured on the inner side, is thirty-two inches; if measured on the outer side over the curvature, it is thirty-eight inches. The width of the whole horn from 1 to 8, if measured on the inner side, is twenty-seven inches; if measured on the outer side over the curvature, it is thirty-six inches. The whole length of the horn from *g* to 4, if measured on the inner side, is twenty-three inches; if measured on the outer side, twenty-seven inches.

The horns of the Prussian elk seem to be distinguished from those of the moose-deer by the length of their antlers. Almost all of them are nearly as long as the length of the palmated part to which they are united, whilst most of those of the moose-deer are short, as it were lopped, and in general do not arrive at one fourth of the length of the palmated part. The antlers of the Prussian elk are, therefore, more like those of the fossil elk than of the moose-deer.

Another difference is produced by the division of the palmated part. In the moose-deer it forms in general an extended plain, not separated by any cut. Sometimes, indeed, the lowest antler, and always a long one, is separated by a cut from the main body; but it is rather to be considered as an antler with a palmated root.

It may yet be worth observing, that the whole mass or weight of the horn is differently disposed in the Prussian elk respecting the stem. A straight line, drawn in the direction of the stem over the palmated part, divides the Prussian elk-horn into two parts almost equal; but such a line, applied to the horn of the moose-deer, divides it into two parts greatly different from one another. Whether such differences are sufficient to constitute a distinct species, or distinguish only a variety, if horns so different in the disposition of their parts can be supported by bones and muscles of the same strength and order, or whether they require both bones and muscles to be of different dimensions and structure, I leave to the decision of men better versed in the knowledge of the laws of nature than myself.

How far the Prussian elk agrees with, or differs from, the Scandinavian animal of that denomination, can probably not be made out in the present state of the science. It may even remain an undecided point for some length of time; for the Prussian elk falls not easily in the way of a scientific traveller: it is, as far as I know, only to be met with in one place, a low, swampy tract of land stretching along the eastern shores of the lake called *Curish Haff*, between the *Russ* and the *Gilgue*, the two principal outlets of that river, which is called by the Germans *Memel*, by the Polanders *Niemen*, by the Lithuanians *Niemona*. This tract is for the most part covered with wood, and in this wood the elk finds shelter and food; it goes by the name of the *Forest of Ibenhorst*. The thriving population in the neighbourhood would long ago have destroyed this valuable animal, if the Prussian government had not protected it by laws, almost as severe as the game laws of England, against the avidity of the poachers.

ON GUNPOWDERS AND DETONATING MATCHES.

By ANDREW URE, M.D., F.R.S., &c.

GUNPOWDER is a mechanical combination of nitre, sulphur, and charcoal; deriving the intensity of its explosiveness from the purity of its constituents, the proportion in which they are mixed, and the intimacy of the admixture.

1. *On the Nitre.*

Nitre may be readily purified, by solution in water and crystallization, from the muddy particles and foreign salts with which it is usually contaminated. In a saturated aqueous solution of nitre, boiling hot, the temperature is 340° Fahrenheit; and the relation of the salt to its solvent is in weight as three to one, by my experiments—not five to one, as MM. Bottée and Riffault have stated*. We must not, however,

* Traité de l'Art de fabriquer la Poudre à Canon, p. 78.

adopt the general language of chemists, and say that three parts of nitre are soluble in one of boiling water, since the liquid has a much higher heat and greater solvent power than this expression implies.

Water at 60° dissolves only one-fourth of its weight of nitre; or, more exactly, this saturated solution contains 21 per cent. of salt. Its specific gravity is 1.1415; 100 parts in volume of the two constituents occupy now 97.91 parts. From these data we may perceive that little advantage could be gained in refining crude nitre, by making a boiling-hot *saturated* solution of it; since, on cooling, the whole would concrete into a moist saline mass, consisting by weight of $2\frac{3}{4}$ parts of salt, mixed with 1 part of water holding $\frac{1}{4}$ of salt in solution, and in bulk of $1\frac{7}{8}$ of salt with about 1 of liquid: for the specific gravity of nitre is 2.005, or very nearly the double of water. It is better, therefore, to use equal weights of saltpetre and water in making the boiling-hot solution. When the filtered liquid is allowed to cool slowly, somewhat less than three-fourths of the nitre will separate in regular crystals; while the foreign salts that were present will remain with fully one-fourth of nitre in the mother liquor. On redissolving these crystals with heat, in about two-thirds of their weight of water, a solution will result, from which crystalline nitre, fit for every purpose, will concrete on cooling.

As the principal saline impurity of saltpetre is muriate of soda, (a substance scarcely more soluble in hot than in cold water,) a ready mode thence arises of separating that salt from the nitre in mother waters that contain them in nearly equal proportion. Place an iron ladle or basin, perforated with small holes, on the bottom of the boiler in which the solution is concentrating. The muriate, as it separates by the evaporation of the water, will fall down and fill the basin, and may be removed from time to time. When small nitrous needles begin to appear, the solution must be run off into the crystallizing cooler, in which moderately pure nitre will be obtained, to be refined by another similar operation.

At the Waltham Abbey gunpowder-works the nitre is rendered so pure by successive solutions and crystallizations, that it causes no opalescence in a solution of nitrate of silver. Such

crystals are dried, fused in an iron pot at a temperature of from 500° to 600° Fahrenheit, and cast into moulds. The cakes are preserved in casks.

About the period of 1794 and 1795, under the pressure of the first wars of their revolution, the French chemists employed by the government contrived an expeditious, economical, and sufficiently effective mode of purifying their nitre. It must be observed that this salt, as brought to the gunpowder-works in France, is in general a much cruder article than that imported into this country from India. It is extracted from the nitrous salts contained in the mortar-rubbish of old buildings, especially those of the lowest and filthiest descriptions. By their former methods the French could not refine their nitre in less time than eight or ten days; and the salt was obtained in great lumps, very difficult to dry and divide: whereas the new process was so easy and so quick, that in less than twenty-four hours, at one period of pressure, the crude saltpetre was converted into a pure salt, brought to perfect dryness, and in such a state of extreme division as to supersede the operations of grinding and sifting, whence also considerable waste was avoided.

The following is a brief outline of this method, with certain improvements, as now practised in the establishment of the *Administration des poudres et salpêtres*, in France.

The refining boiler is charged over night with 600 kilogrammes of water, and 1200 kilogrammes of saltpetre, as delivered by the *salpêtriers*. No more fire is applied than is adequate to effect the solution of this first charge of saltpetre. It may here be observed, that such an article contains several deliquescent salts, and is much more soluble than pure nitre. On the morrow morning the fire is increased, and the boiler is charged at different intervals with fresh doses of saltpetre, till the whole amounts to 3000 kilogrammes. During these additions, care is taken to stir the liquid very diligently, and to skim off the froth as it rises. When it has been for some time in ebullition, and when it may be presumed that the solution of the nitrous salts is effected, the muriate of soda is scooped out from the bottom of the boiler, and certain affusions or inspersions of cold water are made into the pot, to

quicken the precipitation of that portion which the boiling motion may have kept afloat. When no more is found to fall, one kilogramme of Flanders glue, dissolved in a sufficient quantity of hot water, is poured into the boiler; the mixture is thoroughly worked together, the froth being skimmed off, with several successive inspersions of cold water, till 400 additional kilogrammes have been introduced, constituting altogether 1000 kilogrammes.

When the refining liquor affords no more froth, and is grown perfectly clear, all manipulation must cease. The fire is withdrawn, with the exception of a mere kindling, so as to maintain the temperature till the next morning at about 88° C. = 190.4° Fahrenheit.

This liquid is now transferred by hand-basins into the crystallizing reservoirs, taking care to disturb the solution as little as possible, and to leave untouched the impure matter at the bottom. The contents of the long crystallizing cisterns are stirred backwards and forwards with wooden paddles, in order to quicken the cooling, and the consequent precipitation of the nitre in minute crystals, which is raked, as soon as it falls, to the upper ends of the doubly inclined bottom of the crystallizer. It is thence removed to the washing chests or boxes. By the incessant agitation of the liquor, no large crystals of nitre can possibly form. When the temperature has fallen to within 7° or 8° Fahrenheit of the apartment, that is, after seven or eight hours, all the saltpetre that it can yield will have been obtained. By means of the double slope given to the crystallizer, the supernatant liquid is collected in the middle of the breadth, and may be easily laded out.

The saltpetre is shovelled out of the crystallizer into the washing chests, and heaped up in them so as to stand about six or seven inches above their upper edges, in order to compensate for the subsidence which it must experience in the washing process. Each of these chests being thus filled, and their bottom holes being closed with plugs, the salt is besprinkled from the rose of a watering-can with successive quantities of water saturated with saltpetre, and also with pure water, till the liquor, when allowed to run off, indicates by the hydrometer a saturated solution. The water of each sprinkling

ought to remain on the salt for two or three hours; and then it may be suffered to drain off through the plug-holes below for about an hour.

All the liquor of drainage from the first watering, as well as a portion of the second, is set aside, as being considerably loaded with the foreign salts of the nitre, in order to be evaporated in the sequel with the mother waters. The last portions are preserved, because they contain almost nothing but nitre, and may therefore serve to wash another dose of that salt. It has been proved by experience, that the quantity of water employed in washing need never exceed thirty-six sprinklings in the whole, consisting of three waterings, of which the first two consist of fifteen, and the last of six pots; or, in other words, of fifteen sprinklings of water saturated with saltpetre, and twenty-one of pure water.

The saltpetre, after remaining five or six days in the washing chests, is transported into the drying reservoirs, heated by the flue of the nearest boiler; here it is stirred up from time to time with wooden shovels, to prevent its adhering to the bottom, or running into lumps, as well as to quicken the drying process. In the course of about four hours, it gets completely dry, in which state it no longer sticks to the shovel, and falls down into a soft powder by pressure in the hand. It is perfectly white and pulverulent. It is now passed through a brass sieve, to separate any small lumps or foreign particles accidentally present, and is then packed up in bags or barrels. Even in the shortest winter days, the drying basin may be twice charged, so as to dry 700 or 800 kilogrammes. By this operation, the nett produce of 3000 kilogrammes thus refined, amounts to from 1750 to 1800 kilogrammes of very pure nitre, quite ready for the manufacture of gunpowder.

The mother waters are next concentrated; but into their management it is needless to enter in this memoir.

On reviewing the above process as practised at present, it is obvious that, to meet the revolutionary crisis, its conductors must have shortened it greatly, and have been content with a brief period of drainage.

2. *On the Sulphur.*

The sulphur now imported into this country, from the volcanic districts of Sicily and Italy, for our manufactories of sulphuric acid, is much purer than the sulphur obtained by artificial heat from any variety of pyrites, and may, therefore, by simple processes, be rendered a fit constituent of the best gunpowder. As it is not my purpose here to repeat what may be found in common chemical compilations, I shall say nothing of the sublimation of sulphur; a process, moreover, much too wasteful for the gunpowder-maker.

Sulphur may be most easily analyzed even by the manufacturer himself; for I find it to be soluble in one-tenth of its weight of boiling oil of turpentine, at 316° Fahrenheit, forming a solution which remains clear at 180°. As it cools to the atmospheric temperature, beautiful crystalline needles form, which may be washed sufficiently with cold alcohol, or even tepid water. The usual impurities of the sulphur, which are carbonate and sulphate of zinc, oxide and sulphuret of iron, sulphuret of arsenic and silica, will remain unaffected by the volatile oil; and may be separately eliminated by the curious, though such separation is of little practical importance.

Two modes of refining sulphur for the gunpowder-works have been employed; the first is by fusion, the second by distillation. Since this combustible solid becomes as limpid as water at the temperature of about 230° Fahrenheit, a ready mode offers of removing at once its denser and lighter impurities, by subsidence and skimming. But I may take the liberty of observing that the French melting pot, as described in the elaborate work of MM. Bottée and Riffault, is singularly awkward, for the fire is kindled right under it, and plays on its bottom. Now a pot for subsidence ought to be *cold-set;* that is, should have its bottom part imbedded in clay or mortar for four or six inches up the side, and be exposed to the circulating flame of the fire only round its middle zone. This arrangement is adopted in many of our great chemical works, and is found to be very advantageous. With such a boiler, judiciously heated, I believe that crude sulphur might be made remarkably pure; whereas by directing the heat

against the bottom of the vessel, the crudities are tossed up and incorporated with the mass.

The sulphur of commerce occurs in three prevailing colours; lemon yellow verging on green, dark yellow, and brown yellow. As these different shades result from the different degrees of heat to which it has been exposed in its original extraction on the great scale, we may thereby judge to what point it may still be heated anew in the refinery melting. Whatever be the actual shade of the crude article, the art of the refiner consists in regulating the heat, so that after the operation it may possess a brilliant yellow hue, inclining somewhat to green.

In seeking to accomplish this purpose, the sulphur should first be sorted according to its shades; and if a greenish variety is to be purified, since this kind has been but little heated in its extraction, the fusion may be urged pretty smartly, or the fire may be kept up till every thing is melted but the uppermost layer.

Sulphur of a strong yellow tinge cannot bear so great a heat, and therefore the fire must be withdrawn whenever three-fourths of the whole mass have been melted.

Brown-coloured brimstone, having been already somewhat scorched, should be heated as little as possible, and the fire may be removed as soon as one-half of the mass is fused.

Instead of melting, separately, sulphurs of different shades, we would obtain a better result, by first filling up the pot to half its capacity, with the greenish-coloured, putting over this layer, one quarter volume of the deep yellow, and filling it to the brim with the brown-coloured. The fire must be extinguished as soon as the yellow is fused. The pot must then be closely covered for some time; after which the lighter impurities will be found on the surface in a black froth, which is skimmed off, and the heavier ones sink to the bottom. The sulphur itself must be left in the pot for ten or twelve hours, after which it is laded out into the crystallizing boxes or casks.

Distillation affords a more complete and very economical means of purifying sulphur, which was first introduced into the French gunpowder establishments, when their importation of the best Italian and Sicilian sulphur was obstructed by the British navy. Here the sulphur need not come over slowly in

a rare vapour, and be deposited in a pulverulent form called flowers; for the only object of the refiner is to bring over the whole of the pure sulphur into his condensing chamber, and to leave all its crudities in the body of the still. Hence a strong fire is applied to elevate a denser mass of vapours, of a yellowish colour, which passing over into the condenser, are deposited in a liquid state on its bottom, whilst only a few lighter particles attach themselves to the upper and lateral surfaces. The refiner must therefore give to the heat in this operation very considerable intensity; and at some height above the edge of the boiler, he should provide an inclined plane, which may let the first ebullition of the sulphur overflow into a safety recipient. The condensing chamber should be hot enough to maintain the distilled sulphur in a fluid state,—an object most readily procured by leading the pipes of several distilling pots into it; while the continuity of the operations is secured, by charging each of the stills alternately, or in succession. The heat of the recipient must be never so high as to bring the sulphur to a syrupy consistence, whereby its colour is darkened.

In the *sublimation* of sulphur, a pot containing about four cwt. can be worked off only once in twenty-four hours, from the requisite moderation of its temperature, and the precaution of an inclined plane, which restores to it the accidental ebullitions. But by *distillation*, a pot containing fully ten cwt. may complete one process in nine hours at most, with a very considerable saving of fuel. In the former plan of procedure, an interval must elapse between the successive charges; but in the latter, the operation must be continuous to prevent the apparatus from being cooled: in sublimation, moreover, where communication of atmospheric air to the condensing chamber is indispensable, explosive combustions of the sulphurous vapours frequently occur, with a copious production of sulphurous acid, and correspondent waste of the sulphur; disadvantages from which the distillatory process is in a great measure exempt.

I shall here give an outline of the form and dimensions of the distilling apparatus employed at Marseilles in purifying sulphur for the national gunpowder-works, which was found adequate to supply the wants of Napoleon's great empire. This apparatus consists of only two still-pots of cast iron, formed

like the large end of an egg, each about three feet in diameter two feet deep, and nearly half an inch thick at the bottom, but much thinner above, with a horizontal ledge four inches broad. A pot of good cast iron is capable of distilling 1000 tons of sulphur before it is rendered unserviceable, by the action of the brimstone on its substance aided by a strong red heat. The pot is covered in with a sloping roof of masonry, the upper end of which abuts on the masonry of the vaulted dome of condensation. A large door is formed in the masonry in front of the mouth of the pot through which it is charged and cleared out; and between the roof-space over the pot, and the cavity of the vault, a large passage is opened. At the back of the pot a stone-step is raised to prevent the sulphur boiling over into the condenser. The vault is about ten feet wide within, and fourteen feet from the bottom up to the middle of the dome, which is perforated, and carries a chimney about twelve feet high, and twelve inches diameter within.

As the dome is exposed to the expansive force of a strong heat, and to a very considerable pressure of gases and vapours, it must possess great solidity, and is therefore bound with iron straps. Between the still and the contiguous wall of the condensing chamber, a space must be left for the circulation of air, a precaution found indispensable by experience; for the contact of the furnaces produces on the wall of the chamber such a heat as to make it crack and form crevices for the liquid sulphur to escape. The sides of the chamber are constructed of solid masonry, forty inches thick, surmounted by a brick dome, covered with a layer of stones. The floor is paved with tiles, and the walls are lined with them up to the springing of the dome; a square hole being left in one side, furnished with a strong iron door, at which the liquid sulphur is drawn off at proper intervals. In the roof of the vault are two valve-holes, covered with light plates of sheet-iron, which turn freely on hinges at one end, so as to give way readily to any sudden expansion from within, and thus prevent dangerous explosions.

As the chamber is an oblong square, terminating upwards in an oblong vault, it consists of a parallelopiped below, and a semi-cylinder above, having the following dimensions:—

Length of the parallelopiped . . .	$16\frac{1}{2}$ feet.
Width	$10\frac{4}{5}$
Height	$7\frac{1}{4}$
Radius of the cylinder . . .	$5\frac{2}{5}$
Height or length of semi-cylinder .	$16\frac{1}{2}$

Whenever the workman has introduced into each pot its charge of ten or twelve hundred weight of crude sulphur, he closes the charging doors carefully with their iron plates and cross-bars, and lutes them tight with loam. He then kindles his fires, and makes the sulphur boil. One of his first duties (and the least neglect in its discharge may occasion serious accidents) is to inspect the roof-valves and to clean them, so that they may play freely before any expulsive force from within. By means of a cord and chain, connected with a crank attached to the valves, he can, from time to time, ascertain their state, without mounting on the roof. It is found proper to work one of the pots a certain time before fire is applied to the other. The more steadily vapours of sulphur are seen to issue from the valves, the less atmospherical air can exist in the chamber, and therefore the less danger there is of combustion. But if the air be cold, with a sharp north wind, and if no vapours be escaping, the operator should stand on his guard, for in such circumstances a serious explosion may ensue.

As soon as both the boilers are in full work the air is expelled, the fumes cease, and every hazard is at an end. He should bend his whole attention to cut off all communication with the atmosphere, securing simply the mobility of the valves, and a steady vigour of distillation. The conclusion of the process is ascertained by introducing his sounding-rod into the pot, through a small orifice made for its passage in the wall. A new charge must now be given.

By the above process, well conducted, sulphurs are brought to the most perfect state of purity that the arts can require; while not above four parts in a hundred of the sulphur itself are consumed; the crude, incombustible residuum varying from five to eight parts, according to the nature of the raw material. But in subliming sulphur, the frequent combustions inseparable from this operation carry the loss of weight in flowers to about twenty per cent.

The process by fusion, performed at some of the public works in this country, does not afford a return at all comparable with that of the above French process, though a much better article is operated upon in England. After two meltings of *grough* sulphur (as imported from Sicily or Italy), eighty-four per cent. is the maximum amount obtained, the average being probably under eighty; while the product is certainly inferior in quality to that by distillation.

3. *On the Charcoal.*

Tender and light woods, capable of affording a friable and porous charcoal, which burns rapidly away, leaving the smallest residuum of ashes, and containing therefore the largest proportion of carbon, ought to be preferred for charring in gunpowder-works.

After many trials, made long ago, black dogwood came to be preferred to every plant for this purpose; but modern experiments have proved, that many others afford an equally suitable charcoal. The woods of black alder, poplar, lime-tree, horse-chesnut, and chesnut-tree, were carbonized in exactly similar circumstances, and a similar gunpowder was made with each, which was proved by the same proof-mortar. The following results were obtained:—

	Toises.	Feet.
Poplar—mean range	113	2
Black alder	110	4
Lime	110	3
Horse-chesnut	110	3
Chesnut-tree	109	

By subsequent experiments confirmatory of the above, it has been further found, that the willow presents the same advantages as the poplar, and that several shrubs, such as the hazelnut, the spindle-tree, the dogberry, the elder-tree, the common sallow, and some others, may be as beneficially employed. But whichever wood be used, we should always cut it when full of sap, and never after it is dead; we should choose branches not more than five or six years old, and strip them carefully, because the old branches and the bark contain a larger proportion of earthy constituents. The branches ought not to exceed

three-quarters of an inch in thickness, and the larger ones should be divided lengthwise into four, so that the pith may be readily burned away.

Wood is commonly carbonized in this country into gunpowder-charcoal in cast-iron cylinders, with their axes laid horizontally, and built in brick-work, so that the flame of a furnace may circulate round them. One end of the cylinder is furnished with a door, for the introduction of the wood and the removal of the charcoal; the other end terminates in a pipe, connected with a worm-tube for condensing the pyrolignous acid, and giving vent to the carburetted hydrogen gases that are disengaged. Towards the end of the operation, the connexion of the cylinder with the pyrolignous acid cistern ought to be cut off, and a very free egress opened for the volatile matter, otherwise the charcoal is apt to get coated with a fuliginous varnish, and to be even penetrated with condensable matter, which materially injure its qualities.

In France, the wood is carbonized for the gunpowder-works either in oblong vaulted ovens, or in pits, lined with brick-work or cylinders of strong sheet-iron. In either case, the heat is derived from the imperfect combustion of the wood itself to be charred. In general, the product in charcoal by this method is from sixteen to seventeen parts by weight from one hundred of wood. The pit-process is supposed to afford a more productive return, and a better article; since the body of wood is much greater, and the fuliginous vapours are allowed a freer escape. The surface of a good charcoal should be smooth, but not glistening.

The charcoal is considered by the most scientific manufacturers to be the ingredient most influential, by its fluctuating qualities, on the composition of gunpowder; and, therefore, it ought always to be prepared under the vigilant and skilful eye of the director of the powder-establishment. If it has been kept for some time, or quenched at first with water, it is unsuitable for the present purpose. Charcoal extinguished in a close vessel by exclusion of air, and afterwards exposed to the atmosphere, absorbs only from three to four per cent. of moisture; while red-hot charcoal quenched with water may lose by drying twenty-nine per cent. When the latter sort of charcoal is used for gunpowder, a compensation in weight must be made

for the water present. But charcoal which has remained long impregnated with moisture, affords a most detrimental constituent to gunpowder.

4. *On Mixing the Constituents and forming the Powder.*

The three ingredients being thus prepared are ready for manufacturing into gunpowder. They are, i. Separately ground to a fine powder, which is passed through proper silk sieves or bolting machines; ii. They are mixed together in the proper proportions, which we shall afterwards discuss; iii. The composition is then sent to the gunpowder mill, which consists of two edge-stones of a calcareous kind, turning by means of a horizontal shaft, on a bed-stone of the same nature; incapable of affording sparks by collision with steel, as sandstones would do. On this bed-stone, the composition is spread, and moistened with as small a quantity of water as will, in conjunction with the weight of the revolving stones, bring it into a proper body of *cake*, but by no means to a pasty state. The line of contact of the rolling edge-stone is constantly preceded by a hard copper scraper, which goes round with the wheel, regularly collecting the caking-mass, and bringing it into the track of the stone. From fifty to sixty pounds of cake are usually worked at one operation, under each millstone. When the mass has thus been thoroughly kneaded and incorporated, it is sent to the corning-house, where a separate mill is employed to form the cake into grains or corns. Here it is first pressed into a hard firm mass, then broken into small lumps; after which the corning process is performed, by placing these lumps in sieves, on each of which is laid a disc or flat cake of *lignum vitæ*. The sieves are made of parchment skins, perforated with a multitude of round holes. Several such sieves are fixed in a frame, which, by proper machinery, has such a motion given to it, as to make the *lignum vitæ runner* in each sieve move about with considerable velocity, so as to break down the lumps of the cake, and force its substance through the holes, in grains of certain sizes. These granular particles are afterwards separated from the finer dust by proper sieves and reels.

The corned powder must now be hardened, and its rougher

angles removed, by causing it to revolve in a close reel or cask turning rapidly round its axis. This vessel resembles somewhat a barrel-churn, and is frequently furnished inside with square bars parallel to its axis, to aid the polish by attrition.

The gunpowder is finally dried, which is now done generally with a steam heat, or in some places by transmitting a current of air, previously heated in another chamber, over canvass shelves, covered with the damp grains of gunpowder.

5. *On the Proportion of the Constituents.*

A very extensive suite of experiments to determine the proportions of the constituents for producing the best gunpowder, was made at the Essonne works, by a commission of French chemists and artillerists, in 1794.

Powders in the five following proportions were prepared:—

	Nitre.	Charcoal.	Sulphur.	
1	76	14	10	Gunpowder of Bâle.
2	76	12	12	Gunpowder works of Grenelle.
3	76	15	9	M. Guyton de Morveau.
4	77.32	13.44	9.24	Idem.
5	77.5	15	7.5	M. Riffault.

The result of more than two hundred discharges with the *proof-mortar* shewed that the first and third gunpowders were the strongest, and the commissioners in consequence recommended the adoption of the third proportions. But a few years thereafter it was thought proper to substitute the first set of proportions, which had been found equal in force to the other, as they would have a better keeping quality, from containing a little more sulphur and less charcoal. More recently still, so strongly impressed have the French government been with the high value of durability in gunpowders, that they have returned to their ancient *dosage* of seventy-five nitre, twelve and a half charcoal, and twelve and a half sulphur. In this mixture, the proportion of the substance powerfully absorbent of moisture, viz., the charcoal, is still further reduced, and replaced by the sulphur, or the conservative ingredient.

If we inquire how the *maximum* gaseous volume is to be produced from the chemical reaction of the elements of nitre on charcoal and sulphur, we shall find it to be by the generation of carbonic oxide and sulphurous acid, with the disengage-

ment of nitrogen. This will lead us to the following proportions of these constituents :—

	Hydrogen = 1.	Per Cent.
1 prime equivalent of nitre	102	75.00
1 " " sulphur	16	11.77
3 " " charcoal	18	13.23
	136	100.00

The nitre contains five primes of oxygen, of which three, combining with the three of charcoal, will furnish three of carbonic oxide gas, while the remaining two will convert the one prime of sulphur into sulphurous acid gas. The single prime of nitrogen is, therefore, in this view, disengaged alone.

The gaseous volume, on this supposition, evolved from one hundred and thirty-six grains of gunpowder, equivalent in bulk to seventy-five grains and a half of water, or to three-tenths of a cubic inch, will be, at the atmospheric temperature, as follows :

	Grains.		Cubic Inches.
Carbonic oxide	42	=	141.6
Sulphurous acid	32	=	47.2
Nitrogen	14	=	47.4
			236.2

being an expansion of one volume into 787.3. But as the temperature of the gases at the instant of their combustive formation must be incandescent, this volume may be safely estimated at three times the above amount, or considerably upwards of two thousand times the bulk of the explosive solid.

But this theoretical account of the gases developed does not well accord with the experimental products usually assigned, though these are probably not altogether exact. Much carbonic acid is said to be disengaged, a large quantity of nitrogen, a little oxide of carbon, *steam of water, with carburetted and sulphuretted hydrogen.* From experiments to be presently detailed, I am convinced that the amount of these latter products printed in italics must be very inconsiderable indeed, and unworthy of ranking in the calculation ; for, in fact, fresh gunpowder does not contain above one *per cent.* of water, and can therefore yield little hydrogenated matter. Nor is the hydrogen in the carbon of any consequence.

It is obvious that the more sulphur is present, the more of

the dense sulphurous acid will be generated, and the less forcibly explosive will be the gunpowder. This is sufficiently confirmed by the trials at Essonne, where the gunpowder that contained twelve of sulphur and twelve of charcoal in one hundred parts, did not throw the *proof-shell* so far as that which contained only nine of sulphur and 15 of charcoal. The conservative property is, however, so capital, especially for the supply of our remote colonies and for humid climates, that it justifies a slight sacrifice of strength, which at any rate may be compensated by a small addition of charge.

Table of Composition of different Gunpowders.

	Nitre.	Charcoal.	Sulphur.
Royal Mills at Waltham Abbey	75	15	10
France, national establishment .	75	12. 5	12. 5
French, for sportsmen . .	78	12	10
—— for mining . . .	65	15	20
United States of America .	75	12. 5	12. 5
Prussia	75	12. 5	12. 5
Russia	73.78	13.59	12.63
Austria	76	11. 5	12. 5
Spain	76.47	10.78	12.75
Switzerland, (a round powder) .	76	14	10
Chinese	75	14. 4	9. 9
Theoretical proportions (as above)	75	13.23	11.77

6. *On the Chemical Examination of Gunpowders.*

I have treated five different samples; i. The Government powder made at Waltham Abbey; ii. Glass gunpowder made by John Hall, Dartford; iii. The treble strong gunpowder of Charles Lawrence and Son; iv. The Dartford gunpowder of Pigou and Wilks; v. Superfine treble strong sporting gunpowder of Curtis and Harvey. The first is coarse-grained, the others are all of considerable fineness. The specific gravity of each was taken in oil of turpentine: that of the first and last three was exactly the same, being 1.80; that of the second was 1.793, reduced to water as unity.

The above density for specimen first, may be calculated thus:

75 parts of nitre, specific gravity = 2.000
15 parts of charcoal, specific gr. = 1.154
10 parts of sulphur, specific gr. = 2.000

The volume of these constituents is 55.5; by which if their weight 100 be divided, the quotient is 1.80.

The specific gravity of the first and second of the above powders, including the interstices of their grains after being well shaken down in a phial, is 1.02. This is a curious result, as the size of the grains is extremely different. That of Pigou and Wilks similarly tried is only 0.99; that of the Battle powder is 1.03; and that of Curtis and Harvey is nearly 1.05. Gunpowders thus appear to have nearly the same weight as water, under an equal bulk; so that an imperial gallon will hold from ten pounds to ten pounds and a half, as above shewn.

The quantity of water that 100 grains of each part with on a steam bath, and absorb when placed for 24 hours under a moistened receiver standing in water, are as follows:

100 grains of Waltham Abbey, lose	1.1 by steam heat, gain	0.8 over water.
of Hall	0.5	2.2
Lawrence	1.0	1.1
Pigou and Wilks	0.6	2.2
Curtis and Harvey	0.9	1.7

Thus we perceive that the large-grained government powder resists the hygrometric influence better than the others; among which, however, Lawrence's ranks nearly as high. These two are therefore relatively the best keeping gunpowders of the series.

The process most commonly practised in the analysis of gunpowder seems to be tolerably exact. The nitre is first separated by hot distilled water, evaporated and weighed. A minute loss of salt may be counted on, from its known volatility with boiling water. I have evaporated always on a steam bath. It is probable that a small proportion of the lighter and looser constituent of gunpowder, the carbon, flies off in the operations of corning and dusting. Hence analysis may shew a small deficit of charcoal below the synthetic proportions originally mixed. The residuum of charcoal and sulphur left on the double filter-paper, being well dried by the heat of ordinary steam, is estimated as usual by the difference of weight of the inner and outer papers. This residuum is cleared off into a platina capsule with a tooth-brush, and digested in a dilute solution of potash at a boiling temperature. Three

parts of potash are fully sufficient to dissolve out one of sulphur. When the above solution is thrown on a filter, and washed first with a very dilute solution of potash boiling hot, then with boiling water and afterwards dried, the carbon will remain; the weight of which deducted from that of the mixed powder will shew the amount of sulphur.

I have tried many other modes of estimating the sulphur in gunpowder more directly, but with little satisfaction in the results. When a platina capsule, containing gunpowder spread on its bottom, is floated in oil heated to 400° Fahrenheit, a brisk exhalation of sulphur fumes rises, but, at the end of several hours, the loss does not amount to more than half the sulphur present.

The mixed residuum of charcoal and sulphur digested in hot oil of turpentine gives up the sulphur readily; but to separate again the last portions of the oil from the charcoal or sulphur is hardly possible.

When gunpowder is digested with chlorate of potash and dilute muriatic acid, at a moderate heat, in a retort, the sulphur is acidified; but this process is disagreeable and slow, and consumes much chlorate. The resulting sulphuric acid, being tested by nitrate of baryta, indicates of course the quantity of sulphur in the gunpowder. A curious fact occurred to me in this experiment. After the sulphur and charcoal of the gunpowder had been quite acidified, I poured some solution of the baryta salt into the mixture, but no cloud of sulphate ensued. On evaporating to dryness, however, and redissolving, the nitrate of baryta became effective, and enabled me to estimate the sulphuric acid generated; which was of course 10 for every 4 of sulphur.

The acidification of the sulphur by nitric or nitro-muriatic acid is likewise a slow and unpleasant operation.

By digesting gunpowder with potash water, so as to convert its sulphur into a sulphuret, mixing this with nitre in great excess, drying and igniting, I had hoped to convert the sulphur readily into sulphuric acid. But on treating the fused mass with dilute nitric acid, more or less *sulphurous* acid was exhaled. This occurred even though chlorate of potash had been mixed with the nitre, to aid the oxygenation.

The following are the results of my analyses conducted by the first described method:

100 grains afford, of	Nitre.	Charcoal.	Sulphur.	Water.	
Waltham Abbey	74.5	14.4	10.0	1.1	
Hall, Dartford	76.2	14.0	9.0	0.5	loss 0.3
Pigou and Wilks	77.4	13.5	8.5	0.6	
Curtis and Harvey	76.7	12.5	9.0	1.1	loss 0.7
Battle Gunpowder	77.0	13.5	8.0	0.8	loss 0.7

It is probable, for reasons already assigned, that the proportions mixed by the manufacturers may differ slightly from the above.

The English sporting gunpowders have long been an object of desire and emulation in France. Their great superiority for fowling-pieces, over the product of the French national manufactories, is indisputable. Unwilling to ascribe this superiority to any genuine cause, M. Vergnaud, Captain of French Artillery, in a little work on fulminating powders, lately published, asserts *positively*, that the English manufacturers of 'poudre de chasse' are guilty of the 'charlatanisme' of mixing fulminating mercury with it. To determine what truth was in this allegation, with regard at least to the above five celebrated gunpowders, I made the following experiments:

One grain of fulminating mercury, in crystalline particles, was mixed in water with 200 grains of the Waltham Abbey gunpowder, and the mixture was digested over a lamp with a very little muriatic acid. The filtered liquid gave manifest indications of the corrosive sublimate, into which fulminating mercury is instantly convertible by muriatic acid; for copper was quicksilvered by it; potash caused a white cloud in it, that became yellow, and sulphuretted hydrogen gas separated a dirty yellow-white precipitate of bisulphuret of mercury. When the Waltham Abbey powder was treated alone with dilute muriatic acid, no effect whatever was produced on the filtered liquid by the sulphuretted hydrogen gas.

Two hundred grains of each of the above sporting gunpowders were treated precisely in the same way, but no trace of mercury was obtained by the severest tests. Since, by this process, there is no doubt, but one 10,000th part of fulminating mercury could be detected, we may conclude that

Captain Vergnaud's charge is groundless. The superiority of our sporting gunpowders is due to the same cause as the superiority of our cotton fabrics—the care of our manufacturers in selecting the best materials, and their skill in combining them.

7. *On Detonating Matches.*

This subject has been so ably treated in the report of MM. Aubert, Pellissier, and Gay Lussac, that I shall confine myself to a few observations, the results chiefly of my own experience.

Mr. Howard's proportions of the ingredients for preparing his fulminate of mercury are,

Mercury		100 Grains.
Nitric acid, sp. gr. 1.3, $1\frac{1}{2}$ measured ounces	=	884
Strong alcohol, 2 measured ounces .	=	750

The mercury is dissolved by heat in the acid, the solution is allowed to cool to a blood-heat, and then poured into the alcohol. On heating the mixture slightly, an effervescence soon ensues, the commencement of which is the signal for removing the heat from the matrass or retort; for if it be continued for some time longer, the chemical action will become furious, and the fulminate will be injured by an admixture of subnitrate of mercury. After the crystalline powder precipitates, the whole is to be thrown on a filter, washed, and dried on a steam-bath.

The authors of the above report say the best proportions are those of Howard; but they appear to estimate them incorrectly, for they prescribe 12 of nitric acid and 12 of alcohol (by weight) to 1 of mercury. We may hence infer that considerable latitude may be used in the proportions of the materials. I consider the latter ones wasteful, since 100 of mercury, with 950 of nitric acid, 1.35 and 850 alcohol 0.835, produce about 120 parts of a perfect fulminate. The supernatant liquid retains nearly 5 per cent. of the mercury, for 5 grains of a dark-grey oxide may be obtained from it by ammonia.

I have analyzed the match-powder collected from fifty detonating caps of French manufacture, taken from a stock found to answer very well in practice. The whole weighed exactly

16.3 grains, being about one-third of a grain per cap. Treated with hot water, it yielded 8.5 grains of soluble matter, of which 7.0 grains were nitre, and 1.5 nitrate of mercury derived from the ill-made fulminate. By boiling again in water, this passed into a yellow subnitrate.

7.2 Grains of insoluble matter were brushed off the dried filter and heated with dilute muriatic acid. The solution being thrown on a filter, this retained 1 grain of carbon and sulphur, while 6.2 grains of fulminate of mercury passed through in the state of a bichloride. The proportions of this match-powder must have been, therefore, 8 grains of a kind of gunpowder, and about 8 of indifferent fulminate of mercury; and yet it exploded very well: it obviously contained more nitre than usually enters into gunpowder.

The proportions deduced by the French commissioners from their elaborate and able researches are 10 of fulminate, and 6 of pulverin (gunpowder meal).

100 grains of fulminate triturated with a wooden muller on marble, with 30 grains of water and 60 of gunpowder, are sufficient to mount four hundred detonating caps.

In describing the formation of fulminating mercury, I omitted a curious fact that lately occurred to me. Desirous of moderating the reaction of the mixture, which had been overheated, I added a little alcohol from time to time, till its quantity was increased by nearly one half. The fulminate being washed, and laid out on the filtering paper in the air, when nearly dry, minute brilliant points were observed to start up on different parts of its surface, which, becoming larger, were found to be globules of mercury. This metallization went silently and slowly on till nearly one-half of the powder disappeared. An ethereous hydro-carbonate was evidently the agent in this unexpected reduction*.

* To the relative conservative powers of different gunpowders, my attention was first drawn by my very intelligent friend, Major Moody, Commanding Royal Engineer of the Government Gunpowder Works; and through his co-operation I hope to be able, in another paper, to prosecute this subject, so interesting in a national point of view.

London, September, 1830.

ANALYSIS OF NEW BOOKS.

Commentaries on the Mining Ordinances of Spain. By* Don Francisco Xavier de Gamboa. *Translated from the Spanish, by* Richard Heathfield, Esq., *Barrister at Law.*

THIS work is a commentary on the mining laws of Spain and her colonies, having reference, more particularly, to the kingdoms and provinces now constituting the republic of Mexico, in which country it is the principal authority in questions concerning mines or mining. The greater part of the work, as might be supposed, is devoted to the discussion of legal topics; but it likewise contains, interspersed, and by way of digression, a variety of historical and scientific information, on most subjects connected with mining. The object of the author appears, in fact, to have been two-fold:—

First,—to give a complete view of the existing law of mining; which he illustrates by tracing it down from the earliest periods, and by reference to the civil law, the general law of Spain and the Indies, and to cases decided within his own experience†.

Second,—to give as much instruction and useful information as he could collect, on the various subjects connected with mining and the reduction of the metallic ores, of a nature to interest the practical miner and metallurgist, and to lead them to the attainment of greater perfection in their several departments.

Amongst the most interesting of the subjects discussed under this head, is that of the reduction of the ores of the precious metals; under which the author takes an opportunity to describe the processes employed for that purpose in Mexico, at the period when he wrote, and which are, with little or no variation, the same now practised in that country. The Mexicans are not so rude and unskilled in the art of reducing their ores,

* The Ordinances themselves have been translated into English by Mr. Thomson, and are before the public.

† The new code of mining laws, issued by Charles III. in 1783, very closely follows, in all that concerns the *working* of the mines, the ordinances illustrated by Gamboa, which it leaves in force where not directly at variance with the regulations of the former. Hence the authority of the work of Gamboa at the present day, in Mexico and the other new republics of America. Few alterations have been made in the laws of mining, by the legislatures of those countries, since the establishment of their independence, besides such as were rendered necessary by the altered form of the government.

as they have been erroneously supposed in this country to be. The processes employed by them, although conducted with little scientific knowledge, and, generally speaking, with no other guide than long practice and experience in the pursuit, are found, from the nature of the ores, and the circumstances of climate and other local accidents, to be better adapted for that country than any others, as is sufficiently proved by the complete failure of Sonneschnied and his colleagues, in their attempt, under the auspices of Charles III., to introduce into America the improvements of Europe *.

The smelting process is described by the author as follows :—

'*Of preparing and mixing the Ores, previous to their reduction by Smelting. Of the construction of the various Smelting Furnaces employed.*—All the ore raised from the mines is carried to the reduction works, where a receipt is signed on the memorandum brought by the carrier from the mine. The workmen at the reduction works, taught by experience, distinguish the ores adapted for smelting, from those proper for amalgamation, according to their nature, and arrange them separately in an office or store-room. The ore is pounded by beating with a pick or hammer, or more readily, and at less expense, in stamping mills ; and being reduced to fragments of a greater or less size, according to its tractability or obstinacy under the action of the fire, it is piled in heaps, or spread out at once for the purpose of making the *revoltura* or *revolturon*, which is the mixing together of several ingredients, namely, the principal ore, the assistant ore†, litharge, impregnated cupels or bottoms of furnaces, *plomillos*‡, *fierros*§, and slag.

'In making this mixture, the nature of the ore is attended to ; some ores requiring a mixture of all these ingredients, and others not. No general rule, however, can be given for these mixtures, but the miner must frame rules for his own government, founded on repeated experiments and long observation, making him familiar with the nature of the ore.

'The mixture, being prepared in the manner above described, is placed in the furnace to be smelted. There are many descriptions of furnaces ; some being made of stone, some of mud bricks, and

* See Sonneschnied's *Tratado de la Amalgamacion de Nueva España ;* in the preface to which the author acknowledges his inability, after ten years' labour, to introduce with effect, into Mexico, either the process of Baron Born, or any other, preferable to that of the *patio*, which, he says, in p. 91 of the work, 'has subsisted two centuries and a-half, and will subsist as long as the world endures.'

† 'Metal de Ayuda'—Ore of a more fusible character, mixed with the less tractable ores to assist their fusion.

‡ 'Plomillos'—Scoriæ charged with *lead.*

§ 'Fierros'—Slag or scum, being an unreduced mass of oxides and sulphurets, in which those of *iron* predominate.

some of clay. In some the smelting is performed with wood, in others with charcoal; in some the mouths or apertures are stopped up, and in others left open. In some, the ore and wood are mingled together; in others, the wood or charcoal is not in contact with the ore, but the flame only, whence they are called *reverberatory* furnaces.'

'*Of the smelting of Ores.*—Having made the proper mixture, and prepared the furnaces and the machines for supplying them with wind, the smelter must heat or anneal the furnace, if, from being new or newly repaired, it requires it; for, if the ore be thrown in whilst the furnace is cold, it is apt, upon getting warm, to fly or crack, with danger to the bystanders: and if it be moist, in the summer, the same thing will happen, and it will explode with very great force. During the first few hours, charcoal is first thrown in, then a basket of slags, then one of charcoal, and so on, until it be time to add the mixed ore. Half a basketful of this is then thrown in, and upon that a basket of charcoal, and so on, until the furnace begins to work, after which, alternate basketsful of mixed ore and charcoal are thrown in. One or two *cargas* of charcoal are consumed for each charge, according to the nature of the ore; some ores requiring the furnace to be moderately filled; others, that it should be filled to the top. If the ore be not earthy, but clean, the furnace may be charged freely.

'The furnace being thus arranged and brought into play, smelts four charges in twenty-four hours, the ingots being tapped off from time to time; for which purpose, an aperture is made below the bridge of the breast-pan, and the melted portion runs off into the float. The first ingot let off, after repairing the furnace, is called *calentadura*, and is smaller than the others, because the furnace becomes coated with vitrified ore adhering to it, and care is therefore taken not to throw in rich ores for the *calentadura*. The fused metal being let off, the bridge is stopped up, the breast-pan is cleared out, charcoal dust is thrown into and around it, and the furnace is again set to work. The portions which may have adhered to it are taken off last of all, and are mixed with the ores in future smeltings.

'After the smelting is performed, the furnace is uncharged, which is done in the following manner. The charges of ore being all finished, slags and charcoal alone are thrown in, until all the smelted ore has flowed into the breast-pan, when the furnace throws off a very beautiful flame. The wall of mud bricks and everything which may have adhered to it, are then broken down with a crow or iron bar of about twenty-five pounds weight. And here the unfortunate smelters suffer much, during an hour of great labour; for the furnace is hot in the extreme, the crow is heavy, and the incrusted matter adheres very closely. The smoke and vapour from the slag, which are quenched by pouring water upon them, and which are consequently carried down to the feet of the

workmen, are poisonous; and as they drink water incessantly to relieve their exhaustion, they lose the use of their hands and feet, and become bloated. They are subject also to violent pains in the stomach, occasioned by the coldness of the ore.'

After describing the mode of refining the silver, the author proceeds to describe the operation of cold amalgamation, or amalgamation by the *patio*, by which the greater part of the gold and silver now circulating over the whole globe has been reduced from the ore.

'*Of the reduction of Ores by Quicksilver.*—Nature, by exhibiting to mankind the effect of fire in fusing the surface of mountains, first suggested to them the idea of smelting the ores containing lead. Nature also, by setting before them the particles of quicksilver found amongst the ores, first guided them to the method of mixing the harsh ores with quicksilver, salt, and water; an operation which, although in the infancy of the discovery rude and troublesome in practice, requiring many months to effect the reduction of the gold and silver, has now, by the devices of art, and the lessons of experience (the best instructor in the hidden mysteries of physics), been carried to perfection; *magistral** and various other mixtures being employed, so that the ore may be reduced in twenty days or under—and the process has even been completed in twenty-four hours.

'The object of first importance, in the process of amalgamation, is to provide a skilful amalgamator, capable of distinguishing between smelting ores, and those adapted for amalgamation; who can make assays, in the small way, to ascertain what the *monton* will yield in gross; who understands the proper ingredients, temperatures, admixtures, and stirrings to be applied, and who can calculate and compare the probable amount of the expenses and of the metallic produce: for the bringing the silver to the proper point is not to be entrusted to a mere ignorant blockhead.

'Secondly, a due selection of the ores must be made, for the purpose, in performing the reduction by amalgamation, of making such mixtures as their nature may require; and such ores as require smelting, must be set apart for that operation.

'Third, the ore must be ground as fine as possible, that the quicksilver may combine more readily with the silver.

'Fourth, the ore being ground, it is the practice, in some districts, to roast such as is of a sulphureous or bituminous (?) nature, in furnaces adapted for that purpose; in which the criterion of being sufficiently purified, is the ceasing to give off vapour. The same treatment is also applied to the pyritous or resplendent ores, which, under the influence of fire, lose their splendour, and at the same time, get rid of their prejudicial qualities. Those which contain

* Sulphuret of copper, roasted and ground to powder.—*Trans.*

litharge or copperas should not be roasted, until they have been washed and thoroughly agitated in tubs of water, so as to separate the copperas ; for unless this precaution be taken, it will be increased in quantity by the action of the fire, instead of being driven off, and it will have the effect of destroying the quicksilver, and preventing its uniting with the silver. It is sometimes proper to roast the ore after grinding, and sometimes while in the rough. But the most usual course, in the mining districts of New Spain, is not to roast the ore at all, on account of the injurious effect of the operation, in rendering it dry, in diminishing its richness, and in augmenting its bad qualities.

'Fifth, the ore being ground, is thrown into heaps or *montons*, usually of 30 quintals ; but in some places of 18 quintals: and the *montons* are sometimes placed beneath a roof, but most frequently in a well-flagged yard or *patio*, whence this mode of reduction is called the reduction by the *patio*.

'Sixth, with each *monton* of 18 quintals, are mixed two barrels of brine, from impure salt ; six, eight, or ten pounds of *magistral*, as the nature of the ore may require, and from ten to twelve pounds of quicksilver. The *monton* thus prepared, is stirred and trodden, which is called *repasar*. After two or three days, the stirring and treading are repeated, and if it require more quicksilver, a further charge is thrown in, and it is again stirred, until found to require no more: and it is to be observed, that the more quicksilver it requires the better, as a proportionate quantity of silver may be expected.

'Seventh, the quicksilver must be added at different times, and not be thrown in all at once, so that it may by degrees take up the whole of the silver. The first stirrings must be performed with softness and gentleness, lest the quicksilver should become too minutely divided and form *lis*, which is the term applied when it divides into almost imperceptible particles. From the varying nature of the ore, and the diversity of circumstances which arise, no certain rules can be laid down for the course to be pursued in stirring in the quicksilver and *magistral*, and it will therefore be found, that it is sometimes necessary to excite heat by stirring, and at others to apply moisture. Neither is it possible to determine the precise moment at which the *montons* are in a state for washing, for though they may not make any *lis* of silver, nor require any more quicksilver, yet the quicksilver may be dispersed. The only rule is, to ascertain whether the proportion of silver taken up, corresponds with the result of the assay made at the commencement of the process ; and there is no way of ascertaining this, but by making a further trial, in a small way, whether the *monton* is in want of any addition, which in such case may be supplied, or whether it is complete, in which latter case the *monton* may be washed.

'Eight, the *monton* being ready for washing, is thrown into wooden vats of very large size, within each of which is contained a

mill. The mill is turned by a mule, and it is proper that it should not always go round in the same direction, but that the motion should be sometimes reversed: the object being, that the *lises* of silver may fall to the bottom, and that the quicksilver contained therein may not be lost by escaping with the slime or earthy residue, which contains a proportion of silver, and also of quicksilver in a minute state of division. To prevent this loss, it is therefore necessary that the mixture should be kept briskly stirred in every part. The slime being separated, the quicksilver remains at the bottom of the vat, combined with the silver, in which state it is called amalgam. The amalgam is taken out and placed in a linen bag, which being suspended from the beams, the uncombined quicksilver runs out. The part which remains in close combination is made up into small cakes, which are formed into one large cake or *piña* (pine apple), the size being adapted to the capacity of the brass cap or bell. The latter consists of two pieces, the first of which is in the form of a large basin, with a groove round the rim and a hole in the bottom. On the inner part of the rim are three rests, on which is placed a grating, made of iron bars, and upon that is set the *piña* or cake, which is covered over with the cap. The cap is bell-shaped, and fits into the groove of the vessel, which must be surrounded with earth, and have a pan of water beneath it. The cap or bell remains above, and is covered entirely with ignited charcoal, the heat from which, raising the quicksilver in vapour, it finds its way into the vessel, and passing through the hole in the bottom, is received in the pan of water, and brought back into the state of fluid quicksilver. Where caps of brass, copper, or iron, cannot be procured, they must be made of the finest clay, adapted to resist the fire.

'The proportion of silver returned, depends on the quality of the ore; sometimes the produce of silver is equal to an eighth part of the quicksilver mixed in with the *monton*, sometimes a sixth part, and sometimes a fifth part. The quicksilver separated in a liquid state, still contains minute particles of silver, and it is set apart to be used in working other *montons*, until consumed. This is the only part really consumed;* for the rest is either lost by being converted into *lis* in the *montons*, or escapes with the slime, from the agitation of the mill, being divided into the most minute and imperceptible particles. A quintal of quicksilver is not wholly consumed until after it has been employed seventeen times.'

When the ore is tolerably rich, and a more speedy return of the silver is desired, another process is sometimes resorted to, which is called the *beneficio por cazo*, or reduction by the *cazo*.

* In Mexico, the difference between the quantity of quicksilver employed in the process of reduction and the quantity recovered, is arbitrarily divided into quicksilver *consumed* and quicksilver *lost;* a quantity equal or proportionate in weight to the silver obtained, being said to be *consumed*, and the remainder of the deficient quicksilver to be *lost*.—*Trans.*

This process has the advantage of wasting very little quicksilver, and is thus described :—

'*Reduction by the cazo* (*pan*).—This method of reduction affords the most speedy means of extracting the silver. The ore being thoroughly ground, and a quintal being taken, the proper quantities of salt, water, and quicksilver, are mixed in, according to the nature of the ore. The mixture is then placed over the fire, and must be kept constantly stirred, and the act of ebullition further assists in keeping it in motion. It is tried from time to time, to ascertain whether it requires any further addition of quicksilver or salt. Each pan will reduce three charges per day. If the ore be rich, it will often yield a marc, a marc and a half, or two marcs per quintal: and provided the quality be not lower than six ounces, this mode of reduction is very advantageous ; but if the produce of silver be below that rate, it will not answer, from the great consumption of wood, quicksilver, and salt, together with the cost of the pans and coppers. The latter must be closely attended to, to see that there are no chinks or cracks in the bottom, through which the quicksilver might escape; to prevent which, they should be varnished with several coats of lime, slag, iron, and white of egg, well beaten up together. *Barba* expresses himself in highly approbatory terms of this method of reduction, both on account of the saving in quicksilver, and because fuel may be supplied from various trailing plants, which abound in the Indies, and may likewise be much economised by making one furnace heat four pans, as we have seen in several sugar mills in the kingdom of Mexico.

'The assays in the small way will indicate, exactly, what quantity of silver the boiling should yield; but this is more readily ascertained by inspecting the substance itself, which, being taken out with a ladle, and the slime being separated, the metal remains. The slime is separated by washing, in vats of water, supplied from a cistern appropriated to the purpose. This operation removes all the earthy matter and slime ; which, when a sufficiency is collected, are worked over in the process of reduction by cold amalgamation. The quicksilver settles, and is found at the bottom of the vat, combined with the silver. The quicksilver is then separated, in the manner described under the head of reduction by the *patio ;* but it always requires refining, never turning ont pure, like that from the *patio.*'

A third method depends on the employment of sulphate of copper, or *colpa.* This process, called the *beneficio por colpa,* is as follows :—

'*Of the reduction by colpa* (*sulphate of copper.*)—The plan or sketch of the new method of reducing the silver from all classes of ore, whether cold or warm, by means of *colpa,* or white or yellow copperas, was described by Don Lorenzo Phelipe de la Torre Barrio y Lima, a proprietor of mines in the district of

San Juan de Lucanas in Peru, and was printed at Lima in 1738, and reprinted at Madrid in 1743; where a summary of the discovery was likewise printed separately, in the same year, which met with commendation from the pen of Father Feyjoo*. The discovery consists in employing *colpa*, or copperas; the goodness of which is tried by reducing it to powder, moistening it with water, and throwing some globules of quicksilver into it. If the quicksilver spreads, or separates into minute particles, the *colpa* is good; and the like if the quicksilver, when placed on the *colpa*, and stirred in a cup or with the finger, assumes a bluish ash colour, or divides.

'The ore and the *colpa* being well ground, the latter is to be taken in an equal proportion to the salt used. The mixture is to be stirred, as in the ordinary process of reduction, four times a day, and is afterwards to be charged with about two quintals more of the *colpa*, and water is to be sprinkled uniformly over it. The quicksilver is then to be stirred in, in such quantity as the nature of the ore may require. After six days an assay is made, the stirring being continued; and if the ore be too warm, it is allowed to cool, or lime is thrown in; after which fresh charges of quicksilver are added from time to time. The slime must be washed without throwing in any quicksilver by way of *baño* †. When the quicksilver is driven off, it will be found that a greater proportion of silver is obtained, and that none of the quicksilver is consumed, except such part as is lost in the stirring, or from other accidental circumstances. This is the method pursued with the cold ores.

'For the warm ores it is said, that when ground, a basketful of lime is to be thrown uniformly over them. To twenty-five quintals of ore, ten *arrobas* ‡ of salt are to be added, with a sufficient quantity of water, and the mixture must undergo four stirrings. The next day, the *colpa*, being first well prepared, is to be added, in the proportion of one half, to the weight of salt used; and a sufficient quantity of water being added, the mass is to be stirred four times, and as often on the following day. The mass being spread abroad, another *arroba* of *colpa* is to be thrown in, distributing it uniformly, and the mixture is to be sprinkled with water. When thus moistened, the quicksilver is to be stirred in; and three days after, it must be ascertained, as in the ordinary mode of reduction, whether the *montons* are cold and require more stirring, or whether they are warm, and demand a further addition of lime.'

Other methods of reduction are likewise described, which, being in less general use, we pass over.

When on the subject of boundaries, the author describes, at

* Cartas eruditas, tom. ii., carta 19.

† A term applied to a supplementary proportion of quicksilver, usually thrown in by way of softening the slime preparatory to washing.—*Trans.*

‡ An *arroba* is 25lbs. Spanish.—*Trans.*

some length, the method of mine-surveying practised inNew Spain, and the simple instruments employed for that purpose; and he takes occasion to recommend the adoption of the method then practised in Europe, which he illustrates by descriptions of the instruments, figures, and diagrams. (Vol. i. p. 327, &c.) The latter method being, in principle, though not in all its details, the same which is now pursued in the Cornish mines, it is unnecessary to refer to it more particularly.

The various machinery employed in mining and the reduction of the ores, is also described and illustrated by faithful, though rude figures. (Vol. ii. p. 189, &c.)

In another part of his work, the author discusses the expediency of opening the quicksilver mines of New Spain, and the probability of their admitting of being worked with advantage. The trade in quicksilver being monopolized by the crown of Spain, no mines of that metal were allowed to be worked, but those of *El Almaden* in Old Spain, and Guancavelica in Peru, and hence no progress was ever made in turning to advantage the quicksilver veins of New Spain. But that there are such veins, and that they might be worked to much advantage, is evident from the following passages:—

'In stating above, that we have not met with any account of mines of quicksilver having been worked in the early times after the discovery of the kingdom of New Spain, we are to be understood as referring to the sixteenth century, the era of the conquest; but subsequent to that period, many instances may be found.

'First, some quicksilver mines were discovered in the jurisdiction of Chilapa, at sixty leagues distance from Mexico, to the southward*. Don Gonzalo Suarez de San Martin went over in August, 1676, to explore these mines, with a master smith and master bricklayer, and having set up a shed, a house, a smithy and furnaces, he had a part of the crest of the vein blasted away on the 14th of October, and commenced the works of San Mateo, San Joseph and Santa Catalina, all contiguous. He began three adits at a greater depth; but the hardness of the ground obliged him to remove half a league farther down, where, finding fair indications of success, he drove the work of la Concepcion. Here also he found very good ore, in a matrix of white spar, and drove a work, which he called los Reyes. He then drove an adit in a cross direction, and, at the distance of 47 *varas*, cut a vein of considerable size. Several assays were made of the ores from these works, both in the large and small way. Those from San Mateo yielded, by the minute assay, 12 ounces of quicksilver per quintal, those from Concepcion 25 ounces, those from the cross-cut 26 ounces.

* Villa Señor, Theatro Americano, tom. i., page 178.

'The second instance was during the viceroyalty of the Duke de la Conquista, who, in the year 1740, commissioned Don Philip Cayetano de Medina, an alderman of Mexico, and proprietor of the estate in which the Cerros of el Carro and el Picacho were situated, and Don Gregorio de Olloqui, an inhabitant of San Luis Potosi, to inspect some quicksilver mines in the aforesaid Cerros, which, according to Don Mathias de la Mota*, are in the jurisdiction of the Sierra de Pinos, in the kingdom of New Galicia. The result of this commission has not become known.

'The third instance is that stated above, as having occurred in respect to these very mines of el Carro and el Picacho, in the year 1745, when the working of a newly-discovered mine of quicksilver was taken up by Don Fermin de Echevers, the president of Guadalaxara. On this occasion, we know from very good authority, that the vein was found to be rich, abundant, and easily worked, and equal to the supply of the whole kingdom of New Spain; and also, that upon the result of the reduction of some of the ore, conducted under the president's orders, the cost of the quicksilver amounted to no more than 22 or 23 dollars per quintal.

'The fourth instance we shall mention, occurred previously to the last, being in the year 1743, early in the viceroyalty of Count Fuenclara, by whose order doctor Pedro Malo da Villavicencio, senior judge of the royal audiency, set out for the purpose of exploring some other quicksilver mines near Temascaltepec, the ores of which had been subjected to several experiments and assays at Mexico, by Don Manuel de Villegas Puente, factor of the royal stores, who now accompanied the senior judge; but their investigations failed of any beneficial result, and it appears that nothing but urgent necessity will ever induce the government to sanction the laws permitting mines of quicksilver to be worked, like those of silver, gold, or any other metal.

'Yet, as it is evident that there are within this kingdom mines of quicksilver, which the crown might at any moment order to be worked, nothing is easier than to demonstrate the expediency of adopting the same plan here, which has succeeded so well in the famous mines of Guancavelica in Peru †. For, first, whenever the supply of quicksilver fails, as has happened times without number, either in consequence of war, of losses at sea, or of the delay attendant upon procuring it from such a distance, the reduction of the ore in the amalgamation works is brought to a stand, the revenue is thrown into arrear, the whole kingdom suffers, the working of the mines is interfered with, and trade receives a check. By setting the quicksilver mines at work, all or most of these evils would be remedied, facilities would be afforded for reducing the silver in an expeditious manner, and the amount of the tenths, the one per cent. and the coinage duty would be augmented.'

* Mota, MS. History of New Galicia, c. 62, n. fin.

† Solorz. Polit. lib. 6, cap. 2.

In confirmation of the above, it may be added, that other veins of quicksilver, appearing, by the analysis of Professor Del Rio, to afford ores worth working, have recently been discovered in Mexico. Analyses of two specimens of the ore may be seen in the Philosophical Magazine for August, 1828.

The pits (shafts) and adits, by the aid of which the water is carried off from the mines, are then described.

These, with a chapter describing the operations of the mint of Mexico (vol. ii. p. 233), a vocabulary of mining terms (vol. ii. p. 320), and an enumeration of the mining districts of New Spain (vol. ii. p. 332), are the principal matters falling under the second head, which are treated by the author at length, and with these we shall conclude the present analysis; passing over the legal department of the subject, which, although forming the bulk of the work, might, we apprehend, be less interesting to the readers of this Journal.

Anatomische Untersuchungen über den Bau der Augen bei den Insekten und Crustaceen vom Dr. J. Müller zu Bonn. Mekel's Archiv für Anatomie und Physiologie. 1829.—(Anatomical Investigations of the Structure of the Eyes in Insects and Crustacea, by Dr. J. Müller, &c. &c.)

THE original observations of Dr. Müller, contained in his 'Beiträge zur vergleichenden Physiologie des Gesichtsinnes, Leipzig, 1826," of which the present paper is a continuation, and which have subsequently been confirmed by G. Treviranus, Huschke, and Straus Durckheim, have hitherto been unnoticed in this country. They are of interest, however, not only as furnishing more correct ideas of the structure and character of the eyes of Insects and Crustacea than those generally received, but also as serving to remove the apparent anomalies by which they were supposed to be separated from the corresponding organs in vertebral animals.

It may not be superfluous to state, that, according to the usually admitted opinions, the structure of these organs, whether simple, conglomerate, or compound, is essentially similar; consisting in pyramidal prolongations of the optic nerve, covered by a uniform stratum of black pigment, and externally by a transparent cornea; the existence of a crystalline or vitreous humour being expressly denied. Such an organization, whilst it presents no analogy with that of the higher animals, places

insuperable difficulties in the way of all attempts of explaining the nature of the function, and naturally enough has been quoted in support of the extravagant doctrine which refers the seat of vision in the eyes of animals to the choroid.

The observations of Dr. Müller refer to the four different forms of eyes as they occur in Insects and Crustacea, viz.:— 1. Simple Eyes. 2. Aggregates of Simple Eyes. 3. Compound Eyes with facets on the external surface. 4. Compound Eyes without facets.

1. *Simple Eyes.*—The eye of Scorpions and Solpugæ have all the parts of the eyes of higher animals, viz., a retina surrounded by a layer of black pigment, a lens and vitreous humour, and lastly a cornea, convex externally. The black pigment, surrounding the cup-shaped retina, forms at the anterior edge of the vitreous humour a projecting belt, closely embracing the greatest posterior convexity of the lens. In Scolopendra morsitans there are four such simple eyes on each side of the head, of which three are circular, and the fourth and largest, elliptical. In all there is a hard, amber-coloured, and almost circular lens, in immediate contact with the posterior surface of the cornea. Each lens is lodged in a cup-shaped retina, coated externally by black pigment. In these, as in most other simple eyes, there is either not any vitreous humour, or it is so small as to escape notice. In other cases, on the contrary, as Mantis religiosa, Gryllus hierogliphicus, and the larva of Dytiscus marginalis, there is reason to suppose it exists.

2. *Aggregates of Simple Eyes.*—Of this kind are the eyes of Oniscus, Julus, Lepisma, Cymothoa, &c. In a large species of Cymothoa, where the number of eyes thus aggregated was about forty, Dr. Müller found as many crystalline globes or lenses, one in contact with the posterior surface of each cornea; they were hard, transparent, and amber-coloured. Behind each lens was a larger globular mass, also transparent and amber-coloured, with a pit on its anterior surface, in which was lodged the posterior convexity of the lens. This larger mass was coated externally and posteriorly by a layer of black pigment, and in contact at its back part with a fibre from the common optic nerve, which probably is expanded into a cup-shaped retina, situated between it and the stratum of pigment.

3. *Compound Eyes with polygonal facets.*—In many Crustacea, the existence of crystalline cones or prisms between the facets of the cornea and the fibrils of the optic nerve has long been known. Such were described in Astacus fluviatilis, by

Leuwenhoek and Cavolini; in Pagurus Bernhardus, by Swammerdam; in Limulus Polyphemus, by André.

In Penæus sulcatus, Dr. Müller describes the cornea as subdivided into quadrangular facets, and in contact posteriorly with a stratum of short crystalline masses, the lateral surfaces of which are coated by a greenish opaque pigment, separating them from each other. The crystalline columns, or prisms, are quadrangular, perfectly transparent, very short, being about as long again as they are wide, and in contact posteriorly with the fibrils of the optic nerve.

In Lucanus cervus (Coleoptera), the cornea is exceedingly thick, its facets being elongated like prisms. The crystalline bodies are conical, the bases being almost in contact with the cornea, whilst the apices are in contact with the extremities of the fibrils of the optic nerve, each of which is coated externally by a violet pigment.

A similar structure with some minor variations is also to be found in Orthoptera, Hemiptera, Lepidoptera, Hymenoptera, Diptera, and Neuroptera. As the general result of such observations, Dr. Müller describes the structure of such compound eyes as follows:—Behind the facets of the cornea is situated a stratum of elongated transparent prisms, in close apposition to each other, cylindrical or conical,—and allowing the transmission of light in the direction of their longitudinal axis only, their lateral surfaces being coated with pigment. The proportion between their longitudinal and transverse diameters varies from 10 : 1, to 2 : 1. The anterior extremity, in contact with the cornea, is sometimes smooth, sometimes rounded. The pigment is sometimes black, as in Dytiscus, Blatta, Phalænæ, &c.; at others, as in Penæus, Locusta, Gryllus, &c. yellowish-white, greenish, &c. though still opaque.

In some few cases the transparent cones are wanting, though their place is even here supplied by a thin transparent membrane, subdivided like the cornea into facets; *e. g.* in Vespa crabro, Papilio rhamni, Libellula quadrimaculata, Æschna grandis. In Meloe maialis, the cornea is studded posteriorly with transparent projections, very convex, and almost parabolical.

4. *Compound Eyes without facets.*—In Monoculus apus the cornea, which is continuous with the common integuments, is smooth, and without facets; on removing it, the surface of the eye presents a dense aggregate of very small semicircular elevations, which terminate posteriorly in pointed cones, embedded in black pigment, and connected with the tuft-

shaped extremities of the optic nerve. A similar structure probably exists in all the Monoculi, and most of the inferior Crustacea. In the Daphniæ the crystalline bodies are pear-shaped, short, and few in number; such also is the case in Gammarus pulex. In all, the principal peculiarities, independent of the absence of facets on the cornea, consist in the anterior rounded extremities of the crystalline cones, and the manner in which they project anteriorly beyond the stratum of pigment in which their apices are immersed; to which, however, there are some approximations in insects. Are these peculiarities connected with the aquatic habits of these animals, rendering necessary a greater refractive power?

The pear-shaped masses in the Daphniæ and Gammarus pulex present an approach to the lenses of simple eyes, as they occur (aggregated) in Oniscus, &c. The latter, however, besides possessing a spherical lens, have a round vitreous humour, and never the transparent conical masses. The difference from these aggregates is still greater in Monoculus apus, the cones being elongated, small, and numerous. Hence it becomes necessary to discriminate the compound eyes without facets, of the inferior crustacea, as well from the compound eyes with facets of insects and crustacea, as from the aggregates of simple eyes in Millipedes and Onisci.

Ueber den Bau der Augen bei Murex tritonis, Linn., vom Dr. J. Müller zu Bonn. (Meckel's Archiv, No. 3, 1829. On the Structure of the Eyes in Murex tritonis.)

THE black points at the extremities of one of the pairs of feelers in Helix pomatia, were long ago described by Swammerdam as eyes, in which he recognised an aqueous humour and a crystalline lens. Subsequently, Stiebel ('Meckel's Archiv,' b. 5) examined the same parts in Helix pomatia and Cyclostoma viviparum, and found in them a choroid, an iris, and a crystalline. As the true nature of these supposed eyes of gasteropodous mollusca was, however, still by many considered problematical, Dr. Müller availed himself of an opportunity of deciding the question by examining them in Murex tritonis.

They are here placed at the outer side of the feeler, on a small eminence near its root, the axis of the organ being in the same direction as that of the feeler itself. The surface of the eye is convex, and surrounded by a prominent ridge formed

by the substance of the feeler. The eye itself is easily separable from the surrounding substance, and is then seen as a blackish sphere, with its greatest diameter in the longitudinal direction. A thin transparent lamella, continuous with the substance of the feeler, is expanded in front of the globe of the eye. This cornea, as it may be considered, is separated from the globe by a space extending over its anterior third, which in the recent state is probably occupied by a fluid (aqueous humour).

The posterior part of the globe, embedded in the substance of the feeler, is formed by a greyish-black membrane (choroid), which at its anterior part forms a narrow circular belt of a darker colour (iris), perforated in its centre by a circular pupil. The external margin of the cornea reaches somewhat farther back than the outer edge of the iris.

The optic nerve, which is a branch of the nerve running in the axis of the feeler, perforates the posterior part of the cup formed by the choroid, and probably expands on its inner surface into a retina, of which some imperfect traces were visible. The inner surface of the choroid is perfectly black; its cavity is almost completely occupied by a firm, round, amber-coloured mass, similar to those found in the eyes of spiders, and representing either a crystalline lens or vitreous humour.

As the most essential parts of an eye are here present, and of comparatively large size, we are warranted in supposing that there must be a corresponding power of vision. Experimental observations on this point are the more desirable, as in Helix and Cyclostoma, where there is a similar organization, the animals appear not to see, or at least not distinguish objects.

FOREIGN AND MISCELLANEOUS INTELLIGENCE.

§ I.—*MECHANICAL SCIENCE.*

1. Resistance opposed to Water moving in Pipes.—(*D'Aubuisson.*)

Notwithstanding the endeavours made to deduce formulæ from experiments on the passage of water through tubes, so as to assist and guide the engineer in laying down pipes to supply manufactories or towns, yet frequent mistakes have occurred: thus at Paris, at the Fontaine des Innocens, only two-thirds of the water calculated upon were obtained; whilst, in the faubourg St. Victor, only the half of that expected issued from the pipes. These differences appear to result from experiments made on too small a scale, or with apertures disproportionate to the areas of the tubes; for the results of practice come sufficiently near to the formulæ of MM. Prony and Eytelwein, when the velocity of motion in a pipe was small in consequence of a contracted aperture made in a plate of metal being used. When the contracting plate was altogether removed, then the product in water was a fourth or third less than that given by the formulæ, from which M. D'Aubuisson concludes that the resistance increases with the velocity in a greater ratio than that given to it in the calculations; where it is supposed to increase proportionably as $v^2 + m\,v$, m being nearly equal to 0.055, and v representing the mean velocity.

In consequence of the arrangement and state of the water-pipes at Toulouse, some large and accurate experiments have been made there by MM. Castel and D'Aubuisson, in systems of pipes of 4.7 inches and 10.63 inches in diameter, and 1434 and 1986 feet in length. In these experiments the quantity of water passed and the pressure were varied; the results were noted, and also calculated by the formulæ, so as to deduce the loss of pressure due to the resistance of the pipes: that by calculation came out 27., 25., 32.7, and 31.7 per cent. below the result of experiment. As the two latter were the principal experiments, it is concluded that, generally, calculation gives the resistance nearly one-third less than what is obtained by actual and careful practice *.

2. On the Resistance of Lead to Pressure, and on the Influence of a small Quantity of Oxide upon its Hardness.

The recent experiments of Mr. Bevan on the compression of lead †, and his proposal of applying balls of that metal to estimate the force of presses, screws, &c., must be well known to English readers.

* Annales de Chimie, xliii. p. 224.

† Quarterly Journal of Science, N. S., vol. vi., p. 392.

A similar investigation has been entered into by M. Coriolis, which, however, is much more refined as regards those circumstances that enable the lead to resist the force applied.

The points at first under investigation by the latter philosopher were temperature, time, impact, and state of the surfaces between which the lead was confined. The pieces of lead were cylinders 24 millimetres in diameter, and 19 in height; weighing each from 100 to 101 grammes. The arbitrary scale of measurement used gave 680 divisions for the 19 millimetres of height. The lead was pressed between two plates of iron in a kind of box, allowing lateral enlargement as the pressure was exerted, and the measurements of thickness were taken by means rendering the estimation very delicate.

To remove any irregularity resulting from differences in the times of pressure, it was in all ordinary cases limited to an exact minute. To ascertain the effect of impact, two pieces, which had been pressed equally, were then re-pressed, the one for two minutes, the other also for two minutes, but at eight different operations. On making thus the effect of impact eight times as much in one case as in the other, still the whole difference was only 19 divisions, which, divided amongst the extra 7 impacts, gives only about 3 divisions for each. As to the original temperature, its effect amounts to little or nothing; for when the cylinders were purposely cooled down, the mere effect of compression evolved so much heat that they could scarcely be touched, and this heat soon overpowered the original difference: experimentally no sensible difference was produced. In reference to the influence exerted by the state of the surfaces between which the lead was pressed, this also proved to be insensible.

In the experiments the results are always expressed by the number of divisions to which the thickness of the lead has been reduced from the original standard thickness of 680 parts; and in this abstract we shall only give the mean results. Under the following pressures the ordinary lead used in mints was reduced to the expressed thickness.

Kilogrammes .	1500	1824	1950	3175
Thickness . .	463	336	337	296

When this lead was re-fused and cast, it was found to have increased so much in hardness, as with 1500 kilogrammes to give 490 degrees.

Lead was then reduced from the carbonate, and tried after being fused and cast once, twice, thrice, &c., care being taken as much as possible to prevent oxidation by the use of tallow, charcoal, &c., upon the surface. By the pressure of 1950 kilogrammes it was, after the first fusion, reduced to 333 degrees; after the second to 351; after the third, to 398, always setting off from the standard thickness of 680.

This effect was referred to a small quantity of oxide introduced

into the lead at each time of pouring. To ascertain the truth of this opinion, a stopcock was attached to the bottom of the melting vessel so that the lead could be drawn off without any contact with the atmosphere, the surface above being covered all the time with a thick layer of charcoal powder. Then the former experiments being repeated, it was found that lead, after the first fusion, was reduced to 303, less than on any former occasion; after a second, to 311; and, after a third, to 301; so that now no repetition of fusion produced any effect. Some of the lead was also cast in this way, being first raised to a cherry-red heat, and others only to the lowest point necessary for liquefaction. The effects were the same in both: no influence had been exerted over the hardness of the metal, and the changes which usually occur are due to a little oxide introduced.

In experiments upon the influence of time it was found that, after a minute had elapsed, the effect of time was masked by the general effect of the metal, and nearly hidden. For a charge of 1950 kilogrammes the compressions were as follows:—

Time . .	30″	45″	60″	75″	90″	120″
Thickness	365	331	322	321	319	313

So that here, after a minute, 10″ produced an effect of only 2 degrees upon the scale. Still it was found the effect did proceed; for with a charge of 1760 kilogrammes the effect was as follows:—

Time . . .	1 minute	1 hour	24 hours
Thickness	317	245	223

So that, after 24 hours, the lead still continued to give way.

The most important conclusion from these experiments is, that lead fused and cast in the open air is of variable hardness, and that to obtain it with its true and constant power of resistance, it must be cast out of contact of air, and drawn off from the bottom of the mass *.

3. On the Power of Horses.—(*B. Bevan, Esq.*)

The following experimental data are from a letter written by Mr. Bevan to the Editors of the *Philosophical Magazine.*

"In the period from 1803 to 1809 I had the opportunity of ascertaining correctly the mean force exerted by good horses in drawing a plough; having had the superintendence of the experiments on that head at the various ploughing matches both at Woburn and Ashridge, under the patronage of the Duke of Bedford and the Earl of Bridgewater. I find among my memoranda the result of eight ploughing matches, at which there were seldom fewer than seven teams as competitors for the various prizes.

* Annales de Chimie, xliv. p. 103.

	Lbs.
The first result is from the mean force of each horse in six teams of two horses each team, upon light sandy soil . =	156
The second result is from seven teams of two horses each team, upon loamy ground, near Great Berkhampstead . =	154
The third result is from six teams of four horses each team, with old Hertfordshire ploughs . . . . =	127
The fourth result is from seven teams of four horses each team, upon strong stony land (improved ploughs) . . . . . . =	167
The fifth result is from seven teams of four horses each team, upon strong stony land (old Hertfordshire ploughs) . . =	193
The sixth result is from seven teams of two horses each team, upon light loam . . . . =	177
The seventh result is from five teams of two horses each, upon light sandy land . . . . =	170
The eighth result is from seven teams of two horses each team, upon sandy land . . . . =	160

"The mean force exerted by each horse from fifty-two teams, or one hundred and forty-four horses, = 163 pounds each horse; and although the speed was not particularly entered, it could not be less than at the rate of two miles and a half per hour.

"As these experiments were fairly made, and by horses of the common breed used by farmers, and upon ploughs from various counties, these numbers may be considered as a pretty accurate measure of the force actually exerted by horses at plough, and which they are able to do without injury for many weeks; but it should be remembered that if these horses had been put out of their usual walking pace, the result would have been very different. The mean power of the draught-horse, deduced from the above-mentioned experiments, exceeds the calculated power from the highest formula of Mr. Leslie;"—which is as follows: $(15 - v)^2$ = pounds avoirdupois for the power of traction of a strong horse, and $(12 - v)^2$ = pounds traction of the ordinary horse, v = velocity in miles per hour*.

4. On the Change of Volume occurring when Bodies combine together.

An experimental examination of the change of density induced by combination has been undertaken by M. P. Boullay, with a view to ascertain whether any general law could be deduced by which might be obtained an insight into the density of substances generally when in combination. His first care was to obtain the specific gravities of many bodies, simple and compound, to a high degree of accuracy, and in this respect every precaution appears to have been taken. Then comes the point principally under discussion: either the spe-

* Phil. Mag., N. S., viii. p. 22.

cific gravity of a compound is the sum of the specific gravities of its elements, or it is different in consequence of contraction or dilatation. In by far the greater number of cases it proves to be different: thus, in the sulphurets of mercury, lead, arsenic, antimony, tin, and iron, the specific gravity is increased; in the iodide of potassium it is also increased; in those of silver, mercury, and lead, it is diminished. Then endeavouring to determine whether the contraction was the same for bodies having similar atomic composition, no analogy was found; so that, though many sulphurets and iodides have been examined carefully, nothing can be deduced from them relative to other sulphurets and iodides: even constancy of contraction or expansion cannot be deduced, for the iodides present cases of both.

These results on the sulphurets and iodides appear to M. Boullay important, not only as adding facts to our knowledge, but as marking and destroying an error into which many philosophers, occupied with the same question, have fallen. They have endeavoured to determine the specific gravity of bodies brought to the same condition, (the solid state, for instance,) but have been stopped by those substances which cannot be brought into that form. Assuming the hypothesis, however, that in the union of two bodies in the solid state there was neither expansion nor contraction, or else that the negative element only was altered, they have thought themselves justified in deducing from the specific gravity of a binary compound and one of its elements, the specific gravity of the other: thus the densities of oxygen and chlorine have been calculated from the metallic oxides and chlorides. This assumption is entirely done away by the facts quoted.

Even admitting for a moment the hypothesis as good, calculation from it proves its own fallacy: thus the density of oxygen derived in this way from the oxides varies from 1.25 to 5.88, which, without experiment, would prove great modifications by expansion and contraction. The chlorides gave still more striking results for chlorine; and from the specific gravity of the chloride of potassium it would appear that a volume of this binary compound contains more than its volume of metal only, indicating an enormous contraction between that substance and the chlorine*.

5. Apparent Hydrostatic anomaly with Laurel-oil.

Dr. Hancock has remarked a curious apparent anomaly in the hydrostatic pressure of two fluids, the lighter of which, upon mixture, passed to the bottom, and the heavier to the top. One of the fluids is laurel-oil; the other a mixture of pure ether (*i. e.* free from alcohol) and proof spirit in equal proportions, or with a slight excess of ether. Such a mixture is lighter than the essential oil, but when the latter is poured upon the former it floats, and indeed whichever

* Annales de Chimie, xliii. 266.

is added last the same effect takes place; nor does the ultimate state of things differ, whether the mixture be made gently, or violent agitation be given to it.

Dr. Hancock concludes that these seemingly strange appearances result from the strong affinity of the essential oil for ether, by which it attracts it from the mixture with alcohol, combines with it, and so forms a mixture essentially lighter than the ether and spirit. He found, by trial, that though the essential oil would not mix with ether if at all adulterated, that with pure ether it dissolves in every proportion.

A remarkable circulating motion was also observed when laurel-oil and alcohol were brought together. 'Take a phial of the laurel-oil, and drop into it at different intervals some rectified spirits of wine, when the most interesting results will be observed to ensue—a circulation, presently commencing, of globules of alcohol up and down through the oil, which will last for many hours or for days, (how long is unknown.) A revolving or circulating motion also appears in the oil, carrying the alcoholic globules through a series of mutual attractions and repulsions; the round bodies moving freely through the fluid, turning short in a small eccentric curve at each extremity of their course, passing each other rapidly without touching, but after a time they seem to acquire a density approximating to that of the lower stratum, which appears to be an aqueous portion separated by the ethereal oil from the alcohol; and this assimilation taking place, the globules, after performing many revolutions, will fall flat upon the surface and unite with the lower or watery stratum. This experiment was performed with a small phial: perhaps a larger one would render the result more perspicuous*.'

6. On the Quantity of Light reflected by Metallic Specula at different Angles of Incidence.—(*R. Potter, Esq.*)

A paper upon this subject has been read to the Royal Society by Mr. Potter, in which he shews some curious departures in fact from generally received opinions. Sir Isaac Newton has stated, that metallic specula, in common with all other substances, reflect light most copiously when incident most obliquely. Some experiments made by the author, with specula of his own construction, having raised doubts in his mind as to the accuracy of the prevailing opinion on this subject, which accords with that of Newton and of Bouguer, he instituted a more exact inquiry into the proportions of incident and reflected light from specula at various angles of incidence. He used for this purpose a photometer resembling that of Bouguer, and consisting of an upright screen with a square aperture, across which a piece of thin tissue paper was extended, destined to receive on one compartment the reflected light from one lamp, and on another

* Brewster's Journal, 1830, 48, 51.

compartment the direct light from another lamp, employed as a standard of comparison. By adjusting the respective distances of the lamps, the lights on the paper were rendered sensibly equal in point of intensity, the equality being judged of by the eye viewing them from the other side. The measurements were taken alternately, first one of the direct, and then one of the reflected lights, until a sufficient number of uniform results were obtained. The author, after taking every precaution that occurred for insuring accuracy, invariably found that the proportion of light reflected from metallic substances, instead of increasing, diminished in pretty regular gradation, as the angle of incidence was augmented. Thus, in the first experiment, when the angle of incidence was 20°, the proportion of the reflected to the incident light was as 69.45 to 100; at 40° it was 66.79; and at 60° it was reduced to 64.91. Some irregularities occurred in the series of results deduced from different sets of experiments, arising partly from the variableness of the light given out by the lamps, and partly from the difficulty of preserving the metallic surface in the highest state of lustre which it has when newly polished. The author combats the opinion, that the quantities of light which metals are capable of reflecting when polished, are in the ratio of their densities; and finds that in those metals which were the subjects of his experiments, the quantities of light absorbed or lost by reflection at incidences nearly perpendicular are almost exactly in the ratio of their specific heats*.

7. On the apparent Projection of Stars upon the Moon's Disk.

The attention of astronomers has lately been called in a particular manner, by Sir James South, to the extraordinary effect which had often previously been observed of the apparent projection of the stars upon the moon at the time of occultation. The star, on coming up to the moon, in place of disappearing instantly behind its edge, appears (for several seconds occasionally) to advance a short space on to or before its disk, and then disappear. This effect does not always happen with the same occultation; some persons see it—others do not; it happens for variable periods of time, and upon both the dark and bright limb of the moon, though most frequently upon the latter.

A very curious letter upon this subject has been written by M. Gergonne to the editor of the *Bibliothèque Universelle*, in which he describes an example of this illusion, of rather an early date. 'Before 1789, or rather in 1786, I cannot say at what season, as I was coming after mid-day from the College of Nancy, where I studied, (it consequently was about a quarter to five o'clock,) I found about a dozen men and women in a group, at the bottom of the Rue St. Michel, very nearly in front of an Eglise de Pénitens

* Phil. Mag., N. S., viii. 60.

which has disappeared in the revolution—their eyes being attentively fixed on the sky. Inquiring of one of them what was the matter, he pointed with his finger to the object of their attention, at the same time saying, " A star on the moon !" and, in fact, I saw a star of considerable brilliancy on the edge of the enlightened part of the moon's disk. According to the position of the star relative to the sun, which was still far from setting, this should have happened in the spring or autumn, near the first quarter. I remained some instants considering the phenomenon, which gradually disappeared. I cannot now say positively whether the star disappeared behind the moon, or whether it separated from it.

' Although I was not at that time much versed in astronomy, I did not doubt that the supposed star was Venus, which I had sometimes observed in full daylight; and as I also knew that Venus was placed in the heavens as to us far beyond the moon, the phenomenon appeared very surprising to me; and hence, doubtless, the reason why I have preserved the recollection of it.

' This particular fact would have nothing more remarkable than many others which had been cited, if it did not establish, i. That the phenomenon may be seen in full day-light; ii. That it may be well observed with the naked eye, and that, consequently, the explication is not to be found in any action of the telescopes; iii. That it does not depend upon such a condition of the eye as, being purely accidental or exclusively proper to such and such persons, would prevent its uniformly affecting several persons collected together accidentally; iv. That its duration may much exceed that of some seconds, for on this occasion it certainly lasted above a minute. Such a prolonged effect should necessarily happen each time that the motion of the occulted star is, with respect to the moon, nearly a tangent to its disk; and if I had at hand the volumes of the *Connaissance des Tems* for that time, I should, without doubt, find that the occultation which I have described, and which I should then be able to refer precisely to the year and day, would be in this condition. It is also to occultation of this kind that the preference should be given, that leisure may be obtained for the correct observation of the appearances.' This letter is signed J. D. Gergonne, editor of the *Annales des Mathematiques*, and in a note to it, it is stated that in the *Connaissance des Tems* for 1788, p. 43, may be found mention of an occultation of Venus on the 9th April, 1788, about three hours fifty-four minutes after noon, which might be seen from many parts of the earth, but not at Paris*.

8. On the Production of Coloured Bands by Plane Mirrors.

If a person stand before a silvered mirror and observe the reflected image of a candle, he will see at its sides several very

* Bib. Univ., 1830, 345.

apparent coloured bands. The light may be held a few inches before the eye, and so that the incident and reflected rays may make but a small angle. This experiment is due to Mr. Whewell of Cambridge; but M. Quetelet, on repeating it, found that it was not constantly produced, and that the necessary condition was the presence of a slight film of vapour on the glass*. To make the experiment it is sufficient to breathe upon a cold mirror at the place where the image of the candle is to be reflected.

M. Quetelet has found that the experiment succeeds as well when the mirror is not silvered; even a piece of crown glass will do; but, from its irregularities, the bands are not so distinct. Day-light does not interfere with the observation. A drop of oil behind the glass makes the colours disappear. A line from the image of the eye to the image of the light is always perpendicular to the direction of these coloured lines. The bands affect the form of curved lines, which, in certain cases, degenerate into straight lines†. They do not extend far beyond the image of the light. The colours proceeding from the light are bluish-green, yellow, red; bluish-green, yellow, red, &c. Other circumstances being the same, the bands are larger as the observer is farther from the mirror, as the light is nearer to the eye, and in fact as the angle between the incident and the reflected rays is smaller.

This phenomenon does not appear to be related to that which Newton observed with concave mirrors. It appears, as to the colours, to have more connexion with the effect observed when the sun or a light is seen through a transparent plate upon which has been spread a very fine powder.

The breath forms but a transient haze; but M. Quetelet has found an easy mode of rendering the preparatory state of the glass permanent. It consists in extending a very thin regular film of fatty matter, as oil or tallow, over the glass; a soft cloth is to be pressed lightly or dabbed over the whole surface of the film to destroy the parallel lines otherwise existing, and then the effect is obtained as well as with the breath‡.

9. Size for Illuminators, Artists, &c.

Four ounces of Flanders glue and four ounces of white soap are to be dissolved on the fire in a pint of water, two ounces of powdered alum added, the whole stirred and left to cool. It is to be spread cold with a sponge or pencil on the paper to be prepared, and is much used by those who have to colour unsized paper, as artists, topographers, &c.§

* Many mirrors produce the effect without the film, in consequence of a slight granulation left upon the surface of the glass by the manufacturer.—*Ed.*

† We have never seen the bands in a flat piece of glass otherwise but straight. —*Ed.*

‡ Bull. Univ., A. xiii., 190, 192.

§ *Ibid.* E. xiv. 344.

§ II.—CHEMICAL SCIENCE.

1. Galvanic Currents during the Decomposition of Water.

The following description is from the personal observation of Professor Silliman. 'In the decomposition of water by the galvanic power, two tubes being filled with water, and inverted in a vessel filled with that fluid, their orifices being about one inch apart and the connexion established through the fluid by slips of platina, I had recently the satisfaction of observing distinctly the currents of gas as they took their departure to their respective poles. It has been a problem, whether the water is decomposed under one tube, or the other tube, or at some intermediate point; but, in the experiment referred to, ocular demonstration was exhibited, that the decomposition took place simultaneously, under both tubes, and not at any intermediate point. This appeared from the fact, that under each tube a current of gas rose vertically from the platina slip, and collected in the top of the tube, while another current shot off laterally and took up its march towards the opposite pole beneath the contiguous tube: as this process was going on at the same time under both tubes, it follows that there were opposite currents of gas, but they occasioned less mutual disturbance than might have been supposed; because the levity of the hydrogen and the gravity of the oxygen determined them to pass each other at different levels, and although many bubbles were buoyed up in the passage, and made their escape, and were lost by passing through the water intermediate between the two tubes, a large part of the gases was collected in the respective tubes. The process was continued for several hours with a large battery, and the currents were palpable to all the bystanders. With a magnifying-glass the appearance was beautiful, and nothing can exhibit more decisively the all-dominant power of the galvanic influence in causing even gaseous elements to separate at different points, and to pass horizontally, in opposition, through at least two inches of water, until they arrived at the poles by which they were respectively attracted: but, on examining the gases in the two tubes, so far from finding the oxygen gas in the one and the hydrogen in the other, there was found in both a highly explosive mixture, which gave a very sharp report when a flame was applied; and in fact the result was precisely the same as when the two tubes, standing in different vessels and furnished with metallic caps and depending platina wires, to connect them with the slips of the same metal below, are joined by a good conductor touching the caps.

Did the strong mechanical conflict of the two opposite currents cause the gases to be intermingled and thus to be in part carried into the stream? or did a portion of each gas fail to be expelled from the tube by the attractions and repulsions, and thus rise by mere

levity, to mingle with the gas appropriate to each particular pole * ?

We can by no means consider Professor Silliman's account as at all altering the state of our knowledge relative to where the decomposition of water occurs, between or at the voltaic poles. The Professor seems to imply that it takes place at both poles, quoting the two currents from each pole as the proof; but there is no proof that the two currents were not of the same gas, *i.e.*, both oxygen at the positive and both hydrogen at the negative pole; and, in fact, that is the only way of accounting for the mixture of both gases in both receiving tubes. There is great reason to believe that the arrangement of the gas at each pole into two currents, one internal and the other external to the receiving tube, was a mere consequence of the descending water carrying off the smaller bubbles with it.—*Ed.*

2. Power of Metallic Rods, or Wires, to decompose Water, after their connexion with the Galvanic Pile is broken.—(*Berzelius.*)

In the experiments which I undertook in 1806, 7, in company with Mr. Hisinger, we had found that rods of metal which were employed to decompose water by means of the galvanic pile, continued to develope gas after their connexion with the pile had ceased, a circumstance which seemed to indicate a continuance of electrical state, though these rods shewed no action upon any other portion of liquid, even of the same kind, than that in which they had been placed during their contact with the pile. This observation, which I had almost forgotten, has been lately confirmed by Pfaff, who has also added to it several others of a similar kind. We might suppose such effects to be produced by a residual polarity, both in the liquid and the metal, shewing itself, as long as it continues, by a continuation of chemical action; but some of Pfaff's experiments seem to oppose this idea, for he found that the addition of ammonia to the liquid, by which all its internal polarity was destroyed, did not deprive the wires of their effect. The metals which acquire this property in the highest degree are iron and zinc, next to which is gold. He attempts to explain the phenomenon, by supposing that the continued passage of the electrical stream had brought the elements of the water nearer to a state of separation, so that a very slight influence was sufficient to destroy their union. It must be confessed, however, that we cannot at present advance a satisfactory explanation†.

3. On Pyrophosphoric Acid and the Pyrophosphates.

Mr. Clarke first pointed out the singular change induced upon the phosphates by calcination, and, conceiving the acid was changed in its nature, gave it in its new condition the name of Pyrophos-

* Silliman's Journ., xviii. 199.

† Berzelius, Arsberättelse, 1829, p. 33.

phoric acid. Gay-Lussac then gave further light on the subject, and now M. Stromeyer has published an investigation of the subject, which adds very much to what was before known.

M. Stromeyer first compares the two salts of silver, namely, the phosphate and pyrosphosphate, as those compounds which most strikingly exhibit the new characters impressed on the acid. Both these salts are pulverulent, and, when well dried, anhydrous; the first is yellow, the second white; the first has a specific gravity of 7.321, the second of 5.306. The first fuses with great difficulty, requiring a very high temperature, and cools into a yellow mass. The second fuses beneath a red heat into a brown liquid, which, by cooling, becomes a colourless, crystalline mass. Both salts are insoluble in water, both dissolve in nitric and sulphuric acid, and are precipitated unchanged; but when the pyrophosphate is heated in solution, it beeomes ordinary phosphate. Muriatic acid decomposes it, but without changing the peculiar character of the acid.

All the metallic pyrophosphates, boiled with phosphate of soda, become phosphates, and form pyrophosphates of soda—the reverse does not take place. Hence pyrophosphoric acid should be placed after phosphoric acid in chemical affinity; and this alone establishes an important distinction between the two. Most of the pyrophosphates recently precipitated dissolve freely in the solution of pyrophosphate of soda. The same effect does not happen with the phosphates and phosphate of soda.

Hence, that a great difference exists between the phosphoric and the pyrophosphoric acid is evident, although the latter is obtained by calcining the former, or by burning phosphorus in oxygen; still there are plenty of reasons why the difference should not be due to either an excess or deficiency of oxygenation in this respect. M. Stromeyer shews that both are alike; neither does it depend upon more or less water combined, for the two salts of silver are both anhydrous, and yet their properties are distinct.

M. Stromeyer determined the composition of these two salts, and, by various modes of experimenting, proved that they contained different proportions of acid and base. The result of all his experiments was, that the proportions per cent. were as follows:—

	Oxide of Silver.	Acid.
In the phosphate . .	83.454 .	16.545
,, pyrophosphate .	75.390 .	24.610

for equal quantities of acid, therefore, the quantity of oxide of silver in the two salts is as 3 : 5. This great difference in saturating power is the cause why, when a neutral phosphate of soda is calcined, it becomes strongly alkaline, for the phosphoric acid present, by becoming pyrophosphoric acid, loses two-fifths of its neutralising power, and yet this extraordinary effect happens without any loss of acid, or any change in the quantity of its constituents. The whole difference depends upon the manner in which the elements combine, and it is one more added to the very few decisive cases previously known, in which the mere mode of combination, and that too in a

binary compound, produces such differences of properties as to constitute the products real and distinct substances*.

4. Production of Hydrocyanic (Prussic) Acid under uncommon circumstances.—(*A. A. Hayes.*)

Wishing to decompose some nitric acid containing about one-third its weight of dry acid, it was subjected to distillation with one-third of its weight of raw sugar; the distillation was attended by the production of vapours of nitrous and hyponitrous acids, as is usual in the decomposition of nitric acid. The fluid in the receiver was slightly acid, it was therefore returned to the retort still containing the residue of the first operation, and gentle heat applied; the strong and peculiar odour of hydrocyanic acid was developed, in such a quantity as to render the atmosphere of a small room irrespirable. After cooling the apparatus and decanting the distilled fluid, a few drops of ammonia were added, and the alkaline fluid, mixed with a solution of proto-sulphate of iron, and a few drops of acid, deposited a bulky precipitate, which, on exposure, became of a fine blue colour.—Rosebury Laboratory, March, 16th, 1830 †.

5. Action of Chlorine on Carburetted Hydrogen.—(*Morin.*)

A memoir upon the action of chlorine on carburetted hydrogen, consisting of single proportionals of carbon and hydrogen, has been read by M. Morin to the Société de Physique, &c., of Geneva, of which the following is a brief abstract. The investigation was rendered necessary in consequence of the conflicting statements put forth by different philosophers of the nature, composition, and production of the resulting substance.

When chlorine and olefiant gas are brought together over water, a compound sometimes called chloric ether, or hydrocarburet of chlorine, is formed, which was analysed several years since by MM. Robiquet and Colin: they concluded, from all their experiments, that it consisted of equal volumes of chlorine and olefiant gas combined together, and in fact, it was well ascertained, that in these proportions the substance was abundantly produced, and the gases disappeared.

M. Morin analysed it by passing its vapour through a tube heated to dull redness: carbon was left in the tube and a gaseous mixture obtained, containing two volumes of muriatic acid gas, and one volume of a peculiar carburetted hydrogen, containing twice its volume of hydrogen, in combination with 0.6 of a volume (as the hypothesists say) of the vapour of carbon; 3.7 parts of the hydrocarburet of chlorine were used, and, according to the received opinion of composition, a fourth more of muriatic acid, and a third less of the carburetted hydrogen gases ought to have been obtained. Hence it appeared, that a very considerable part of the chlorine

* Ann. de Chim., xliii. 364. † Silliman's Journal, xviii. 201.

had somehow disappeared. M. Morin found this in the water over which the compound had originally been formed; for although both gases may have been well purified, this water always becomes strongly acid, and in fact, being saturated with bi-carbonate of potassa, evaporated to dryness and ignited, the chloride of potassium produced was found to contain half the chlorine which had been employed in forming the oily fluid.

Hence the true theory of action is as follows: four atoms of carburetted hydrogen being acted upon by two atoms of chlorine (equal volumes), one of the former gave its hydrogen to one of the latter, to form one of muriatic acid, and its carbon to the other atom of chlorine, to form an atom of proto-chloride of carbon. This atom of proto-chloride, combined with the remaining three of carburetted hydrogen, forms the chloric ether *; and upon consideration it will be found, that such a compound would give by decomposition the proportion and kind of gases before stated to occur.

Action of Chlorine on Alcohol.—As alcohol and ether may be considered as hydrates of carburetted hydrogen, M. Morin then closely investigated the effect of chlorine upon them. In the alcohol experiment, the chlorine being disengaged in a matrass, then passed through a vessel containing chloride of lime, next through that containing the alcohol, next to this was a vessel containing water, and ultimately a fourth with a solution of chloride of lime; the third vessel was to absorb any muriatic acid formed, and the fourth to saturate any carbonic acid which might be disengaged. When the chlorine was passed very slowly, and the alcohol was very pure, the whole of the gas was absorbed, and a greenish oily liquid was deposited at the bottom of the vessel. Gradually the absorption of chlorine diminished, but did not cease until several days had passed, after which the bubbles were increased in bulk whilst traversing the liquid. There were then two liquids in the vessel, the lower third was oily, whilst the upper part was very acid and fuming. Either could be coloured green by a slight excess of chlorine. The increase in weight indicated the chlorine absorbed, and by saturating the acid liquor with bi-carbonate of potassa, the quantity of muriatic acid produced was easily determined. Of the two liquids, the lightest was found to precipitate by water, and to be a solution of the heavier in acid; the quantity thus dissolved was estimated by comparative experiments. The quantity of carbonic acid produced was as nothing, the trace existing probably came from the manganese. The experiment proved that chlorine combined with alcohol in a volume equal to that of the hydro-carbon present, estimated in the same state; that half the

* We have ventured to alter the number of atoms, &c., referred to by M. Morin in illustration, without, however, altering the sense of the statement. M. Morin doubles the atom of carbon, and calls it vapour, &c.; the consequence is, that in the very passage altered, the theoretical impropriety occurs of saying, that *bi*-carburetted hydrogen is composed of *two* atoms of hydrogen and *one* of carbon.—*Ed.*

chlorine became muriatic acid, and that the other half formed a substance of the same specific gravity as the hydro-chloride of carbon. Hence, it may be concluded that chlorine acts on alcohol as it does on olefiant gas; that the composition of the substances obtained in both cases is the same, and that the water of the alcohol is not concerned in the action. A good result can only be obtained in operating at temperatures close to 32°, in allowing only a very slow current of chlorine, and in effecting complete saturation. The operation will soon appear terminated, but in such cases a very variable oily product will be obtained.

Action of Chlorine on Ether.—The same kind of experiment, and with the same apparatus, was then made with ether, also a hydrated hydro-carbon. By keeping the temperature at 32°, or below; moderating the current of chlorine; and continuing the operation until the saturation was perfect; all the muriatic acid produced passed into the third vessel, or that containing the water. In place of the ether, nothing remained but a green liquid impregnated with chlorine, and of the specific gravity of chloric ether.

The muriatic acid produced represented half the chlorine: the oily matter was equal to what the hydro-carbon in the ether could have produced, as olefiant gas with chlorine. The quantity of carbonic acid evolved was quite insignificant; the water of the ether was inert during the action, and in fact, the action of chlorine is the same whether olefiant gas, alcohol, or ether be used. Although all proceeds successfully if every precaution be taken, yet inattention easily gives erroneous results. If the saturation be incomplete, the oily matter varies in density and quantity. If the current of chlorine be rapid, ether is carried off into the water and escapes the action. If the temperature rise, the muriatic acid and ether react upon each other, and muriatic ether is produced.

The substances thus produced, though alike in composition, vary in some properties, and principally in taste and odour; these differences, it is supposed, may be due to a little sweet oil of wine. That made with the gases has a sweet penetrating taste and agreeable odour; those with alcohol and ether have an acrid taste, resembling more that of peppermint; in colour and some other qualities they differ slightly. They agree, however, in specific gravity, which is between 1.22, and 1.24; in extreme solubility in alcohol and ether; in being almost insoluble directly in water, but soluble by means of muriatic acid, and remaining in solution after the acid is neutralised. All produce by combustion a green flame, and abundant vapours of muriatic acid*.

6. Bromide of Carbon.

The following account of this substance is extracted from a work on bromine and its chemical combination, by C. Lœwig.

* Ann. de Chimie, xliii. 225.

Bromide of carbon may be prepared in two ways; according to the first method, bromine is mixed with alcohol at 36° Baumé. The mixture heats strongly, and if bromine is still added, a moment of sudden effervescence supervenes, accompanied with disengagement of vapours of hydro-bromic acid and free bromine. After the liquid has cooled, there is added an alcoholic solution of caustic potash until discolouration is produced; water is then poured in, and the alcohol is evaporated at a gentle heat. When the liquid begins to cool, there separates a small quantity of a yellow oil, heavier than water, and immediately after a concrete crystalline matter. The alcoholic solution may also be diluted with a large quantity of water, and in this manner the concrete substance equally separates with the oil.

This combination, however, may be obtained in greater quantity by the following process. Bromine is put along with ether for a certain time, and the mixture is then distilled. At first there only passes hydrobromic acid, and then comes a very clear oil, which falls to the bottom of the liquid that has already passed. When the distillation has been continued for some time it is interrupted, pure potash is added to the residuum, and it is diluted with water. There is then deposited a voluminous white mass which is washed with water upon a filter. It is then melted at a very gentle heat, and allowed to harden by cooling.

This bromide of carbon forms white opaque scales, greasy to the touch, like camphor, and friable. Its smell is highly aromatic, resembling that of nitric ether; its taste is sharp, like that of peppermint. In the fluid state it is transparent and colourless. It burns as long as it is in contact with flame, and disengages vapours of hydro-bromic acid. It is heavier than water, melts at a slight degree of heat, evaporates at 212° F., and sublimes, forming acicular crystals, having a pearly lustre. It is but feebly dissolved by water, to which it communicates its smell and taste. When the water is at 122° F., it is dissolved, and at a higher degree it is in part evaporated with the vapour. Alcohol and ether easily dissolve it, and the solutions are not rendered turbid by nitrate of silver. Alkalies have no action upon it, even at the boiling temperature. Sulphuric, hydrochloric, and nitric acids have no effect upon it. When the melted bromide of carbon is submitted to a current of free gas, chloride of brome is immediately formed. On heating it with the oxides of iron, copper, zinc, &c., there are obtained metallic bromides, and carbonic acid gas. By making it pass these metals in the state of vapour, there are obtained metallic bromides and charcoal. It is to this latter property that M. Lœwig has had recourse for analysing the bromide of carbon, which is composed of 9.01 carbon, and 91.99 brome, the atomic weight of the latter being = 941.1*.

* Edin. Nat. Journal, ii. 233.

7. Preparation of Phosphuret of Lime.—(*Dr. Coxe.*) I employ two Hessian crucibles, some of the inner members of a nest. The larger of the two has a hole bored through its bottom, and a test tube of a suitable size luted in with clay. The phosphorus is put into the test tube, the top of which is loosely covered with a piece of broken crucible to prevent the small pieces of quicklime from running down into it. The lime is then put in so as to fill this crucible and partly fill the upper smaller one, which serves as a cover to it, and is luted on with some fine clay a little moistened. The cover has also a small hole in its top to afford an outlet for the air, or volatilised phosphorus, if there should be any occasion for it. The whole is now placed upon the grate of a furnace, with the test tube projecting through and appearing below, and a charcoal fire kindled around it. The phosphorus may be kept cool if it should be thought necessary, by making the tube dip into the water, contained in a tin cup attached to the end of a stick. When the crucibles and their contents are thoroughly red hot, a chafing dish is substituted for the tin cup, and the phosphorus rising in vapour produces the desired change. The phosphuret should be preserved in a sealed vial. The same crucibles may be used a number of times*.

8. Iodide of Potassium a good Test for Arsenic—curious Compound produced.

Professor Emmett of Virginia has recommended the iodide of potassium, or iodine alone occasionally, as a useful test for white arsenic. He found that when the iodide was added to a solution containing only 2.8 per cent. of arsenious acid, or 1.8 per cent. of arsenite of potassa, or when iodine alone was added to a solution containing 2.8 per cent. of arsenite of potassa, an immediate precipitation took place. If the precipitation be performed with drops upon a glass plate, then $\frac{1}{300}$dth of a grain of arsenic is sufficient for the purpose; the precipitate, when gradually formed, is white, adheres with great tenacity to the glass plate, and then may be thoroughly washed, and will present the following characters. Concentrated nitric acid changes the white colour to a dark brown, purple, or even black, from free iodine; and starch added at the same time, becomes deep blue. Strong hot sulphuric acid does the same; when cold, it merely produces a bright yellow, the latter effect is produced by strong muriatic acid. Metallic salts are not likely to cause errors in the use of this test, because, if originally present, they are separated by the carbonated alkali used to dissolve the arsenious acid. The presence of coffee, tea, milk, and other liquids, does not seem materially to retard the precipitation.

The substance thus formed appears to be a curious compound. It resembles arsenious acid in solubility and precipitation; thus,

* Silliman's Journal, xvii. 349.

hot water dissolves about 5.3 per cent. and deposits nearly one half on cooling. It requires a much higher heat than white arsenic for its volatilisation (550° Fahrenheit), and at 600° is decomposed, giving off first arsenical fumes and then evolving iodine. On analysing the substance it turned out to be a compound of

Arsenious acid . .	63.3
Iodide of potassium .	36.7
	100.0

Notwithstanding the novelty of such a compound, in which it is impossible to tell whether the white arsenic acts the part of acid or base, although it is present nearly to the extent of five atoms, and where no analogy to the composition of a double salt appears obvious; yet Professor Emmett observes there are facts from which its existence must be inferred. Thus iodide of potassium, even when added in great excess, does not precipitate the whole of the arsenite of potassa, nor is it capable of diminishing the alkaline reaction; on the contrary, when arsenite of potassa is so far neutralized by free acetic or arsenious acid as not to affect turmeric paper, it acquires this property by the addition of iodide of potassium, apparently in consequence of a union between the latter substance and the excess of arsenious acid, which while dissolved had the power of counteracting the alkaline effect: other considerations lead to the same result.

If subsequent experiments should establish the existence of such a compound, it will be a solitary but striking example of what may be considered a chemical hybrid *.

9. Ammonia in Native Oxide of Iron.—(*Boussingault.*)

Vauquelin shewed that rust of iron contained ammonia, and Chevallier shewed that the natural oxide of iron also contained the same alkali. As the oxides the latter worked with came from a distance, it might be urged that they had acquired ammonia by the way; for if rust formed within houses absorbed ammonia, so also might native oxides acquire that alkali in its transit from place to place. M. Boussingault, therefore, sought to ascertain whether the natural oxides of iron gave the substance immediately after their extraction from the earth.

In the mine of Cumba near Marmato, a large vein of hydrated oxide of iron in syenitic porphyry is worked as a gold ore. In a part of this mine, called *por a fuera*, where the work proceeds with activity, about a foot of mineral was broken down at the end of the excavation so as to expose a fresh surface, and then a hole was bored in the very middle of the vein; after having been carried eight inches deep, the powder of the ore was collected carefully in a basin, placed under the hole, and touched by nothing but the tool. Four ounces of this ore were then bruised and rubbed in distilled

* Silliman's Journal, xviii., 58.

water, the filtered liquid was acidified by muriatic acid and evaporated; it left fifteen grains of residue, which being introduced into a glass tube with a piece of quicklime slightly moistened and heated, gave ammonia sensible not only to test papers, but also by its strong odour. Hence it results, as M. Chevallier has stated, that the natural oxides of iron contain ammonia, and this fact, conjoined with that of Austin, that ammonia is formed by the oxidation of iron in contact with air and water, acquires a certain degree of geological importance*.

10. Atomic Weight of Titanium.—(*Rose.*)

M. Rose some time since endeavoured to ascertain the atomic weight of titanium from the analysis of its sulphuret, but finds, as he suspected, that the sulphuret often contains titanic acid, and therefore yields uncertain results. In fact, when chlorine was passed over the heated sulphuret, besides the chlorides of titanium and sulphur, titanic acid always appeared.

He has, therefore, resorted to the chloride of titanium as a more definite compound; a mixture of titanic acid and charcoal is heated and chlorine passed over it; the chloride of titanium formed is rectified from off mercury or potassium † several times to remove the excess of chlorine, and is then a clear limpid fluid like water, leaving no trace of chlorine when decomposed by water. If this chloride and water be brought together suddenly, heat is evolved, and the solution is milky; if the chloride is left in a moist atmosphere, the action takes place without the least formation of turbidness. After some time the titanic acid is precipitated by ammonia, carefully added so as not to be in great excess, exposed to a moderate temperature to dissipate the excess, and filtered to separate the titanic acid. The above liquor is then mixed with nitric acid, and the chlorine precipitated from it by a solution of nitrate of silver. The titanic acid and chloride of silver are then weighed and give data to determine the quantity of titanium and chlorine in the original compound. From the mean of many experiments thus made, it would appear that one hundred parts of the compound contain

Chlorine	74.46	71.461
Titanium	25.54	23.539

and as 74.46 chlorine correspond to 16.82 oxygen, that the titanic acid is composed per cent. of

Oxygen	39.71	36.130
Titanium	60.29	63.870

Dumas, some time since, endeavoured to ascertain the specific gravity of the vapours of the chloride of titanium, and found it to be 6.836, that of air being 1. This would give the composition of the above compounds as expressed in the second column of figures. The cause of this difference between the results obtained is, at

* Ann. de Chimie, xliii., 334.

† Potassium does not act upon the compound at boiling temperatures.

present, unknown, but unfortunately throws doubt upon both processes*.

11. On the Crystallization of Gold.—(*Professor Henslow, of Cambridge.*)

A small glass-stoppered phial, containing a solution of gold in a mixture of nitric and muriatic acids, had stood long neglected for a considerable time (perhaps four or five years) in a cupboard. Upon accidentally discovering it, I found a portion of the acid had escaped and the gold crystallized. This effect had probably been promoted by a flaw in the phial, which extended through the neck, and a little way down its length. The stopper, in consequence, must have been slightly loosened, and thus allowed more space for the formation of a thin dendritic crystallization of the gold. This was further continued down the inner surface of the phial, and was there sufficiently thick to admit the impression of minute but distinct crystalline facets. A small crystallized lump of gold lay at the bottom of the phial, but I believe this had been originally attached to the rest, and merely fallen by its weight, as I have since observed to be the case in another portion. Around the stopper, and along the flaw, there was a saline concretion, which tasted like sal ammoniac, and as ammonia was kept in the same cupboard, it had probably united with the muriatic acid as it exuded. Upon finding this specimen, I examined some other metallic solutions, and found a similar separation of the metal had taken place, in a phial containing a solution of platina, and in another containing a solution of palladium. In both these cases a thin, interrupted, and dendritic lamina of metal might be seen between the stopper and the neck, but the crystallization had proceeded no further. I unstoppered the phial containing the platina, and the lamina (as might have been expected) immediately disappeared in the form of a slight muddy film. The palladium I still possess. Probably this phenomenon may be of frequent occurrence; but as the separation of the metal does not often extend below the neck of the phial, it may have passed unnoticed. These facts, if multiplied, may perhaps serve to throw some light upon the mode in which the dendritic laminæ of native gold, silver, &c., are formed in rocks†.

It would have been satisfactory to know whether, in the case described by Professor Henslow, any lard, wax, or lubricating matter had been originally applied to the stopper of the phials, which could have caused or promoted the effect of reduction. The Professor has not before met with any cases of reduction in the crystalline form of gold from solution in acid. These, however, are not uncommon. We have specimens of gold finely crystallized, by gradual reduction and deposition, from an ethereal solution of its chloride; and both gold and silver, and also other metals, may be reduced

* Annalen der Physik, xv., 145.

† Mag. Nat. Hist. i., 146.

from these solutions in acid, and crystallized, by leaving pieces of charcoal, phosphorus, &c., in them.—*Ed.*

12. Salicine—its power as a Febrifuge.—(*Leroux.*)

A very important Memoir by M. Leroux, which was presented to the Academy of Sciences, has been most favourably reported upon by MM. Gay Lussac and Majendie. It relates to nothing less than the discovery of a principle in indigenous plants which may replace quinia and cinchonia as medical remedies. Being aware that the willow had been employed advantageously as a bitter and febrifuge, M. Leroux sought in it for some active principle, and ultimately sent two preparations to the Academy, one called salicine, the other sulphate of salicine. He at first thought the new principle was a vegeto-alkali, but when afterwards in Paris, he convinced himself that it had no power of neutralizing acids, did not combine with them, was rendered uncrystallizable by them, contained no nitrogen, and was not a vegeto-alkali. The sulphate was a mistake.

Salicine is in the form of very fine nacreous white crystals, very soluble in water and alcohol, but not in ether; it is very bitter, and partakes of the odour of willow bark. In order to obtain it, three pounds of the bark of the willow (*salix helix*), dried and pulverized, is to be boiled in fifteen pounds of water, with four ounces of carbonate of potash, for an hour; it is to be filtered, and, when cold, two pounds of solution of sub-acetate of lead added: when settled, it is to be filtered, treated with sulphuric acid, the rest of the lead precipitated by sulphuretted hydrogen, the excess of acid neutralized by carbonate of lime, again filtered, the liquid concentrated and saturated by dilute sulphuric acid, then boiled with animal charcoal to remove colour, filtered hot, crystallized repeatedly, and dried without access of light. About one ounce of salicine will be obtained in the large way; probably twice the quantity would result, for great loss is occasioned by the above numerous operations. It may be preserved in well-closed bottles, and does not attract moisture.

As to the medicinal powers of this substance, M. Majendie states, that his own experience of its effects in intermitting fevers is favourable, and that he has seen three doses, of six grains each, stop a fever. He quotes the experiments of MM. Miquel, Husson, Bally, Girardin, Cognon, &c., at the hospitals and elsewhere, in its favour: they all agree in its anti-febrile power, and in stating that from twenty-four to thirty grains of salicine will arrest the return of the fever, whatever may be its kind. This is nearly the same as the dose of the sulphate of quinia.

In concluding, the commissioners state, that M. Leroux has discovered in the willow (*salix helix*), a crystallizable principle which approaches sulphate of quinia in its anti-febrile power, and that this discovery is, without contradiction, one of the most

important that has been made for many years in pharmaceutical chemistry*.

13. Preparation and Composition of Malic Acid.

This curious vegetable acid has been obtained pure and crystallized by M. Liebeg, and carefully analysed, for the purpose of setting the discordant results of different chemists at rest. The expressed juice of the ripe fruit of the mountain ash was boiled with animal charcoal, which had previously been purified by muriatic acid; and a certain quantity of potash added, but so as to leave a great excess of acid; the whole evaporated till thick as syrup, then mixed with five or six times its volume of spirit of wine, and the clear, vinous liquor, after separation from the mucilaginous matter, distilled. The thick viscid residue of the distillation was again acted upon by alcohol, which entirely did away with the mucous state. Being again distilled, the residue was diluted with much water, precipitated by acetate of lead, and the malate of lead obtained, decomposed in water by sulphuretted hydrogen. The addition of potash and treatment by alcohol has for its object the separation of tartaric acid and tartrate of potash, which occurs in the original juice, and which otherwise would have given a mixture of tartaric acid with the malic. As directed, the malic acid can contain only citric acid, or traces of tartaric acid; when concentrated, therefore, ammonia is to be added in quantity insufficient to neutralize the liquor; alcohol, equal in volume to the liquid, is to be added also, and the whole allowed to cool, when quadrangular crystals of the acid malate of ammonia will be obtained, the salt being very little soluble in alcohol, even though diluted. These dissolved in water, precipitated by acetate of lead, and the precipitate decomposed by sulphuretted hydrogen, yield *pure malic acid;* which will be found to crystallize by evaporation in the air, forming, first, acicular crystals, and ultimately a solid crystalline mass.

A crystallized malate of zinc was then formed, resembling in properties that described by M. Braconnet. By a heat of 212° it loses ten per cent. of water, without change of form; at 248° it lost other ten per cent., then becoming a white coherent powder. By analysis, the salt gave 46.734 malic acid, 32.711 oxide of zinc, 20.555 water, the oxygen of the oxide, water and acid being as 1 : 3 : 4. Hence the equivalent of malic acid is 57.3, hydrogen being unity.

The malate of silver is anhydrous at 212°, and composed of 66.975 malic acid, 33.026 oxide of silver per cent., which gives the equivalent number of malic acid as 57.2. When the dry salt is decomposed by heat, it blackens only for an instant, and yields carbonic oxide gas, which burns like alcohol, and contains no empyreumatic matter.

* Ann. de Chimie, xliii., 440.

The acid malate of ammonia was then decomposed in the manner adopted by MM. Liebeg and Woehler, with the hippuric acid*. It gave azote and carbonic acid in the proportion of 1 : 8, indicating four atoms of carbon in the acid. The hydrogen was determined by burning the dry malate of zinc with oxide of copper, and collecting the water by chloride of calcium. The results came out as 4 atoms carbon, 24 ; 2 hydrogen, 2 ; 4 oxygen, 32 ; =58 : but as this was too high, as compared to the conclusions respecting the equivalent number, and as it was the same with the composition of dry citric acid, excess of hydrogen was suspected ; and as a trace of water in the salt used would account for this excess, other experiments were made with the anhydrous malate of silver. This salt gave little more than one atom of oxygen, and the composition of malic acid may therefore be considered as follows :—

4 atoms carbon . .	24
1 „ hydrogen . .	1
4 „ oxygen . .	32
Equivalent number .	57†

14. Ulmin, or Ulmic Acid, and Azulmic Acid.

The following points relative to the history of ulmin are abstracted from a thesis by M. P. Boullay on this subject. This substance derives its importance from the numerous circumstances which give rise to it, and the daily conversion of numerous vegetable matters, especially those in wood, into it. Its existence in vegetable earth, in manure, and in the sap of plants, shews the important part which it performs, and it is probably the most valuable compost known. It occurs in enormous quantities in brown earth, turf, &c., and Holland probably owes the superiority of its agricultural productions to the quantity which it naturally possesses.

M. Boullay has considered it as an acid, and gave it a corresponding name, because of its power of combining with bases. It was first found by Vauquelin in an exudation from the elm tree ; M. Braconnot formed it artificially. It is produced in the distillation of wood in soot, and may be formed by the action of sulphuric and muriatic acids upon many vegetable substances.

Ulmic acid differs from the substances produced by the action of air or oxygenizing bodies, on extracts, tannin, gallic acid, or gallates, both by its colour and solubility in alcohol. It is more probable, from the properties of the resulting substance, that when gallic acid or the gallate of ammonia is exposed to air, a new substance, not sufficiently examined, is produced.

The composition of ulmic acid is the same as that of dry gallic acid, but it has a much feebler saturating power ; its equivalent number is to that of gallic acid as 5 : 1. It has been analysed by

* Quart. Jour. of Science, vol. vii. p. 424. † Ann. de Chim. xliii. 259.

Boullay, and gallic acid by Berzelius; the proportions they obtain are as follow :—

	Ulmic Acid.	Gallic Acid.
Carbon . .	56.7	57.08
Water . .	43.3	42.92

equal to three proportions oxygen, three hydrogen, and six of carbon. Hence it was supposed, that gallic acid differed only in water of crystallization, but all attempts to deprive it of water, and convert it into ulmic acid, failed.

The ulmates of the metals, although insoluble in saline solutions and in excess of ammonia, are, when well washed, soluble in water, like the ferro-prussiate of iron. They take fire at a temperature much below a red heat, and burn. Three of them were found by experiment to be composed, per cent., as follows :—

	Oxide of Silver.	Of Lead.	Of Copper.
	28.57 .	26.86 .	10.5
Ulmic acid	71.43 .	73.14 .	89.5

Hence the equivalent of the acid consists of fifteen proportions of oxygen, fifteen of hydrogen, and thirty of carbon, which, taking hydrogen as unity, is 315. This is precisely five times the number of gallic acid.

The feeble capacity of saturation possessed by ulmin may, perhaps, be important in nature, for a large quantity of this food of plants may in consequence be transmitted to them from decomposing substances, by small quantities of alkali or ammonia. The earthy ulmates, and especially that of lime, are not quite insoluble, and withal are capable of being suspended so perfectly in fluids as to be useful in the nutrition of plants, whilst still they are not so likely to be washed away as the soluble ulmates.

Azulmic Acid.—By this name M. Boullay designates a substance which has the same kind of relation to ulmic acid that azoted organic matter has to such as is of vegetable origin. The carbonaceous product left by the spontaneous decomposition of hydrocyanic acid is azulmic acid, and not a carburet of azote. It contains hydrogen, and can combine with salifiable bases in the same manner as hydrocyanic acid itself. Azulmic acid is not soluble either in hot or cold water or alcohol: strong cold nitric acid dissolves it, forming a reddish solution, precipitable by water. The alkalies dissolve it very freely, producing deep-coloured solutions: the acids precipitate these solutions, as do also the metallic salts. By heat azulmic acid gives first hydrocyanate of ammonia, then cyanogen, and leaves carbon. When analysed, the proportion of azote to carbon was in volumes as 2 to 5. Hence, upon theory, it will consist by weight per cent. of 47.64 azote, 50.67 carbon, and 1.69 hydrogen.

Pursuing the analogy between ulmic and azulmic acid, M. Boullay endeavoured to form the latter by heating gelatine with potassa, in imitation of M. Braconnot's process for forming ulmin; and, in fact, azulmic acid appeared to be produced. Azulmic acid is pro-

duced also not only by the spontaneous decomposition of hydrocyanic acid, but by those of hydrocyanate of ammonia, of cyanogen dissolved in water, by the action of cyanogen upon bases, and indeed whenever compounds of this substance are experimented with. The action of weak nitric acid on cast iron, or the carbon it contains, produces a similar substance; and as azulmic acid appears to combine with concentrated nitric acid, there is reason to believe that artificial tannins are only combinations of this body with nitric acid, or at least that they contain an analogous substance*.

15. On Caseum and Milk.—(*Braconnot.*)

An excellent, because practical memoir on milk has been published by M. Braconnot, in the *Annales de Chimie*, xliii. 337, which offers many applications of a substance long but not thoroughly known, not a few of which we anticipate will hereafter come into use. This substance is caseous matter, or, as he has called it, *caseum.*

Soluble Caseum, and its Applications.—2500 parts (grammes) of the curd of new cheese, as sold in the market, were heated to 212° for some time: it contracted, and became a glutinous elastic mass, swimming in much serum. Being washed in boiling water, to remove the acid serum, and dried, it weighed 469 parts. It was a compound of caseum with acetic and lactic acids: being divided, put into sufficient water with 12.5 parts of crystallized bicarbonate of potassa, and heated, it dissolved with effervescence, producing a mucilaginous liquor, distinctly reddening litmus paper. Being evaporated carefully, with continual agitation, it left a soft portion, which, as it cooled, acquired consistency, was drawn out between the fingers into thin portions, and then dried in the air upon a sieve: it weighed 300 parts. This *soluble caseum* is a surcaseate of potassa, containing still butter and salts. It resembles isinglass, is of a yellow-white colour, translucent, and of a stale taste: it is perfectly soluble in hot or cold water, producing a fluid rendered milky by the presence of butter.

In this impure state the substance is easily prepared: instead of the bicarbonate, the potash or soda of commerce may be used. The following are hints for its application. Like gelatine, it may be preserved without alteration for any length of time, and may be obtained in enormous quantities, if required. Associated in various ways with food, it must prove of the greatest importance on board vessels for long voyages. Its aqueous solution, sugared and flavoured with a little lemon-peel, makes an agreeable and nourishing drink for invalids. It is a powerful cement: its solution, evaporated on glass or porcelain to dryness, cannot be removed without injury to the vessels; its hot concentrated solution has been applied with great success to join glass, porcelain, wood, and stone. The same solution forms a brilliant varnish: being applied to paper, it

* Annales de Chimie, xliii. 273.

makes labels, which, when moistened and attached, adhere with great force. It may be used instead of isinglass in dressing silks, ribands, gauze, preparing artificial flowers, &c. It has not answered in endeavours to clarify beer, but is equal to milk or cream in the clarification of table liqueurs, giving them the softness and qualities of age. It may be used in place of creamed milk in the clarification of beet-root, sugar, syrups, &c., in conjunction with animal charcoal, without exciting any fear regarding the presence of serum. M. Braconnot thinks also, that by the help of a little ammonia the greater part of the curd previously separated as above from its serum may be taken up and converted into a dry substance, which, with the help of earthy salts, will be of great service in clarification: for, having dissolved some of this preparation in water, a small quantity of muriate of lime, sulphate of magnesia, or even sulphate of lime in powder was added: the liquid remained clear whilst cold, but the slightest effect of heat made it coagulate uniformly throughout; the coagulum gradually contracted, and a perfectly clear liquid issued from it.

Milk has always been considered as a certain antidote in some cases of poisoning. The soluble caseum will perform the same office against most of the metallic salts, but there is reason to believe that white of egg is better than either against corrosive sublimate.

Chemical Properties of Caseum.—Caseum is an acid which, because of its tendency to combine with almost every substance, it is very difficult to obtain pure. The soluble caseum already described is to be dissolved in boiling water, put into a funnel, the aperture of which is stopped, and left until a layer of cream has collected on the surface. After removing this, a little sulphuric acid is to be added, which will form a clot of sulphate of caseum: this is to be well washed and then heated in water, with just enough carbonate of potash to dissolve it. The mucilaginous liquor formed is, whilst hot, to be mixed with its volume of alcohol. It is necessary that no deposit form at the moment; it should occur only in the course of twenty-four hours, and will include the butter, the sulphate of potash, and part of the caseum. All is to be placed on a cloth, and a clear transparent liquid will pass, which, evaporated to dryness, leaves caseum pure, except in retaining a minute portion of potash.

Caseum, or *caseic acid*, thus obtained, is a dry diaphanous substance, resembling gum arabic in appearance, and unalterable in the air. It reddens litmus paper, is soluble in hot or cold water, forming transparent viscid adhesive solutions, yielding by evaporation transparent pellicles, which again dissolve in water. The mineral acids, except the phosphoric, when added to the liquor, unite to the caseum, and produce white, opaque, coagulated, insoluble masses. Very weak solutions are not thus coagulated, as may be seen by adding a little diluted sulphuric acid to such; heat does not

cause the effect, but the moment a little lime is added it happens at once. Milk with twice its bulk of water is not coagulated by sulphuric acid cold, but apply heat and the effect is produced, because a little phosphate of lime in the milk then becomes sulphate, and acts as above. Generally, the combinations of cheesy matter with acids are imputrescent. Well washed sulphate of caseum was left with water for a long time: it gradually disappeared, but produced no putrid odour.

Vegetable acids precipitate caseum, unless in excess. Potash, soda, and ammonia produce very soluble compounds with it, which are perfectly transparent, unalterable by air, and resemble gum. All earthy bases and metallic oxides form insoluble compounds. All salts, except those with base of potash, soda, or ammonia, combine with caseum to form insoluble compounds. Even a little selenitic water put into a solution of caseum, though it causes no change at first, yet, when heat is applied, produces insoluble pellicles, which are a compound of the caseum and earthy salt. The same or still more striking coagulation happens with sulphate of magnesia and acetate of lime.

Strong alcohol does not affect caseum; weak alcohol dissolves it. Sugar renders a solution of caseum more liquid: gum arabic renders it quite insoluble, probably from the presence of earthy salts in it. Infusion of galls acts with it as with gelatine. M. Braconnot suspects that vegetable albumen is nothing more than caseum with some earthy salts present.

Improved Milk.—Besides caseum and butter, milk contains salts, &c. which are not particularly desirable. M. Braconnot took 2½ litres (4.4 pints) of milk, heated it to 113° F., gradually added dilute muriatic acid, and agitated the whole. The curd formed contained the caseum and butter, and, being separated from the whey, was gradually mixed with 5 grammes (77 grains) of crystallized sub-carbonate of soda, reduced to powder and warmed. No water was added, but the whole gradually dissolved. It had the weak acidity of recent milk, and formed about a half-litre of cream (a fifth of the first bulk), capable of numerous applications in domestic economy. If made up to its first bulk with water and a little sugar, it forms a milk more agreeable than the original; or it may be flavoured, &c., and used as cream. If it be heated with about its weight of sugar, it becomes remarkably fluid, and forms a perfectly homogeneous syrup of milk, which will keep for any length of time, and which, by the mere addition of a sufficient quantity of water, forms a perfectly homogeneous white opaque liquid, which is in every respect like sugared milk of improved flavour. The syrup diluted with water forms a nourishing drink for invalids. Carefully evaporated, but not beyond a certain limit, or the butter would separate, it gave, when cold, a soft confection, which left for a twelvemonth in a loosely stopped bottle, underwent no change. This, when exposed in thin portions to the air, was rendered quite dry, and could then be crushed

and kept for any length of time without change, being always reconvertible into useful states by the mere addition of water*.

16. Manufacture of Charcoal.

A new process, recommended in the *Journal des Forêts*, for this purpose, is to fill all the interstices in the heap of wood to be charred with powdered charcoal. The product obtained is equal, in every respect, to cylinder charcoal; and, independent of its quality, the quantity obtained is very much greater than that obtained by the ordinary method. The charcoal used to fill the interstices is that left on the earth after a previous burning. The effect is produced by preventing much of the access of air which occurs in the ordinary method. The volume of charcoal is increased a tenth, and its weight a fifth†.

Mr. Doolittle, of Birmington, United States, has lately charred wood in kilns constructed for the purpose. One was built of brickwork, thirty feet diameter and nine feet high, to the opening of the arch which inclosed the top. It had openings at the top and sides for the purpose of admitting air, charging, extracting, &c., all which openings were under regulation. The charcoal thus obtained was exceedingly good in quality, free from stones, earth, &c., and very abundant in quantity, the increase being, in the latter respect, sometimes half as much more as the old mode of burning would give‡.

17. Potash obtained commercially from Felspar.

According to M. Fuchs, this important alkali may be extracted from minerals containing it, by the following method:—They are to be calcined with lime, then left for some time in contact with water, and the liquor filtered and evaporated. M. Fuchs says he has thus obtained from nineteen to twenty parts of potash from felspar, and from fifteen to sixteen from mica, per cent§.

18. Sale of Selenium.

Perfectly pure selenium (free from sulphur) is announced for sale, at the price of four gold Frederics (ninety francs) per ounce of Cologne (446 grains). Applications, post paid, with the money, is to be made to the Ducal Office of the Mines of Harzgerode, in the duchy of Anhalt.

* Ann. de Chim. xliii. 337.
† Bull. Univ., D. xiv. 262.
‡ Silliman's Journal, xvii. 395.
§ Ann. de l'Industrie, v. 278.

§ III. *NATURAL HISTORY, &c.*

1. Mechanism of the Human Voice in Singing.

A memoir on this curious subject has been read to the Academy of Sciences by M. Bennati, and examined by MM. Cuvier, Prony, and Savart. The former of these three philosophers has reported thereon to the Academy. The principal object of the memoir is to make known the powers of an organ in effecting the modulations of the voice, which in this point of view has been little attended to by physiologists. This is the soft palate, or the narrow part of the gullet formed above by the uvula, at the sides by the arches, and at the bottom by the root of the tongue. M. Bennati has succeeded in constructing an instrument which can include three octaves. He points out in his memoir the precautions which should be taken in this respect for the instruction of young persons destined to be vocalists; amongst one of the principles, is, to interrupt the exercises at the period when the voice changes. M. Bennati concludes his memoir by this proposition, that it is not only the muscles of the larynx which serve to modulate the sounds, but also those of the os hyoides, of the tongue, and of the veil of the palate; without which all the degrees of modulation necessary in singing cannot be attained. From hence it results that the organ of voice is an organ *sui generis*, an instrument inimitable by art, because the materials of its mechanism are not at our disposal, and we cannot conceive how they are appropriated to the kind of sonorousness which they produce. This result, although not entirely new to science, appears to the reporters to be proved by M. Bennati by new facts and observations, and to have acquired such development as to fix the attention of physiologists*.

2. Globules in the Humours of the Eye.

MM. Ribes and Donne have lately discovered globules in the humours of the eye, of a smaller size than those of the blood. There are three orders of them: the first are in sinuous chaplets, and very apparent; the second are isolated, larger than the others, aud surrounded by a black circle; the third are least distinct, and resemble a kind of mist. The authors are disposed to question the utility of so many parts of the visual organ in the production of impressions on the retina. It is known that the removal of the crystalline lens by extraction does not destroy vision. The rays of light must be considerably modified by the globules of the humours †.

* Revue Ency. xlvi. 502.

† Archiv. General. Medical Journal, v. 148.

3. Use of Nitrogen in Respiration—Cyanogen in the Blood.

Dr. Rich, Professor of Chemistry in the Vermont Academy of Medicine, has put forth a view of the part which nitrogen performs in respiration, to produce cyanogen, which then exists in the blood as cyanide of iron. He quotes the observations of others, by which the nitrogen of the atmosphere is shewn to be absorbed in respiration, and also occasionally given out again in the lungs, and he thinks there is no more difficulty in conceiving that it should enter into the blood in the pulmonary vessels, and combine with the carbon in the blood, just as oxygen does. Cyanogen would probably result; and then, referring to the ordinary processes by which Prussian blue is obtained from dried blood, Dr. Rich seems to consider it just as likely that the process should merely transfer the cyanogen already existing, as that they should cause its formation from the carbon and nitrogen present. This view appears to him to explain the difference which has existed amongst chemists relative to the presence of iron in the blood. Englehart's process of detecting iron in the fluid blood, or rather in the colouring matter of the blood, namely, by passing chlorine through it for a time, and then testing the clear solution, he conceives to depend upon the chlorine taking away the cyanogen from the iron, and so bringing the latter into a state indicative by the usual tests *.

Dr. Rich has not had the opportunity of supporting his views by any experiment, although he suggests some. We cannot help observing that the idea of the cyanogen obtained by the Prussian blue maker being merely that which pre-existed in the blood, appears to be a very violent one. The quantity he can obtain from dry blood is enormous, many times surpassing the weight of the colouring matter in it. Further, the colourless serum will yield plenty; and now, in fact, blood is but seldom resorted to for it, but hoofs, horns, and other sources of animal matter, are used for the purpose.

4. Action of the Pile on Living Animal Substances.

Being desirous of testing by experiment the opinion often entertained and advanced, that secretions in the living body are the result of electrical decomposition, M. C. Matteucci applied the poles of a voltaic pile containing fifteen pairs of plates, to two wounds made on the lateral parts of the abdomen of a rabbit, so as to leave the peritoneum bare. The poles were of gold, and it was soon found that a yellow alkaline liquor, containing many bubbles of air, collected at the negative pole, whilst a yellow liquid with few bubbles and slightly acid, collected at the positive pole. When the positive pole was copper, it became covered with a green coat slightly acid; the same results were obtained by acting upon other parts of the

* Silliman's Journal, xviii. 52.

body, as the liver, intestines, &c. The substance obtained at the negative pole besides alkali, contained much albumen and coagulated by heat; the fluid at the positive pole also contained a highly azotated substance.

These experiments M. Matteucci considers as supporting the opinion advanced above; and considering the secreting viscera in different feeble electric states, it is easy to conceive the production of acid and alkaline substances characterizing the secretions, and to understand the formation of new substances by the combination of the nascent elements. The electric state of the organ secreting particular fluids may also be deduced; and still further it might be expected, that alkaline secretions would contain substances in which hydrogen and carbon formed the principal part; whilst acid secretions would contain bodies abounding more in oxygen and azote. A brief consideration of the analysis of those substances which are found in the urine, milk, bile, saliva, &c., will shew generally the truth of this deduction *.

5. On the Disorders arising from the long-continued use of Iodine.—(*Dr. Jahn.*)

The following is the account which Dr. Jahn gives of that diseased state of the system, which results from a long continued or excessive use of iodine, and which it will be found differs much, as do also the explanations of the effects, from the descriptions of MM. Coindet, Gardiner, Sceter, &c.

When introduced into the organic fluids, iodine acts firstly and principally upon the process of nutrition. The first evident effect is an absorption of the fat, so that a gradual leanness is remarked. At the same time, we may observe with a little attention, an augmentation of all the excretions. The skin, in consequence of an increased deposition of carbon upon it, appears dull and of a livid hue; there is great and clammy perspiration; respiration is obstructed, the urine is increased in quantity, and the surface of it is often covered with an oily pellicle. The alvine evacuations are increased, and the fæces are loaded with bilious matter and contain but little mucus; the seminal secretion is increased, and also the menstrual discharge. 'It is clear,' says M. Jahn, 'that in this state the vitality of the veins and lymphatic vessels is exalted, and the predominance of venous excitement is shewn, by the swollen state of the superficial veins, and the blue colour of the lips. The blood, it may be inferred from the diminished redness of the skin, and the feebleness of the arterial pulsations, has acquired a more serous character, and is more liquefied, so that the quantity of serum is greater in proportion to the cruor and fibrine. The energy of the irritable tissues is comparatively diminished. Hence the patient is more easily fatigued than before; digestion is

* Ann. de Chimie, xliii. 259.

irregular, the saliva and mucus are diminished, and complaint is made of dryness of the mouth and throat. The nervous power is also materially affected, and symptoms resembling hysteria and hypochondriasis arise, morbid sensibility, lowness of spirits, timidity, sensation of weakness, trembling of the limbs, similar to that produced by mercury, agitated sleep, with disagreeable dreams, &c.

At this period irregular and transient febrile attacks announce a reaction of the constitution. If now the morbid condition be not opposed, and if the iodine be continued, the above symptoms increase in severity, and shortly the glandular tissues, the breasts, testicles, and thyroid gland are diminished in substance. At length, all those symptoms arise, which are said to constitute nervous consumption.

M. Jahn has examined two bodies, which presented the traces of the action of iodine. A woman, who having misused the remedy, was attacked with enteritis, which proved fatal; and a man affected with cancer of the stomach, who was treated by the internal and external use of iodine, and who took very large doses of the tincture secretly, in hopes of a more speedy cure.

In the bodies of these patients the fat had disappeared, the various tissues had a withered and flabby appearance, the glands were shrunk and soft, and also the mesenteric ganglia (which are usually much developed in cancer of the stomach), the thyroid and supra-renal glands, the liver, spleen and ovaries.

Notwithstanding the mischief sometimes inflicted by the use of iodine, M. Jahn considers it one of the most valuable remedies which has been recently discovered*.

6. Chlorine an Antidote to Hydrocyanic Acid.

MM. Persoz and Nonat have verified the favourable results which M. Simeon had obtained relative to the remedy which chlorine affords against prussic acid. They operated upon three dogs, upon the eyes of which a drop of prussic acid had been placed. Dividing the symptoms into three periods, namely; i. uneasiness, ii. tetanus, iii. interrupted respiration: they found that when chlorine was applied in the first period, the relief was immediate, the respiration became regular, vomitings and alvine discharges occurred, the animal gradually regained its strength, rose unsteadily, and, in about half an hour, was as lively as at first. Applied at the second period, the symptoms were arrested, but the restlessness continued awhile; and though respiration was less painful, the convulsive movements continued for ten minutes, then occurred vomitings, &c., as before, and, at the end of an hour, the animal was perfectly well. The two dogs thus treated being tried next day with the same quantity of prussic acid, but without chlorine, died in a few minutes.

* Med. Jour., xlix., p. 72.

In the third case, all the effects of the prussic acid were produced before the chlorine was applied; the respiration had ceased for twenty-five seconds, and the animal was rapidly perishing; but the chlorine not only recalled it to life, but ultimately restored it to full vigour: the full effect only occurred, however, after some hours. Ten days after it was quite well, and the paralysis of the abdominal parts, which occurred in all, had, in this case, entirely disappeared.

After this, MM. Persoz and Nonat sought to ascertain whether the prussic acid, being absorbed into the vessels and tissues, the chlorine would follow and decompose it. Two dogs of equal strength were taken, the crural veins laid bare, and separated from the neighbouring parts, and especially the accompanying nervous fibres; then a drop of prussic acid was put upon each vessel. The effects were instantaneous; a few drops of chlorine (solution) were let fall on to one of the crural veins—the other animal was left alone. The first was as immediately recovered as it was injured; the second died directly. The first felt no inconvenience after some hours, except from the wound. Endeavours were then made to kill him, by putting prussic acid upon the eye and upon the crural vein of the opposite side; but the animal only felt temporary inconvenience and a few convulsive movements, and was very quickly at ease. Hence it appears that the chlorine administered beforehand is taken into the circulation, and is then an effectual remedy against prussic acid.

Trials made with the chlorids of lime and soda, in place of chlorine, shewed that they possessed no corresponding powers, being quite inert as antagonists to the hydro-cyanic acid *.

7. On the Cure of Animal Poisons, and probably Hydrophobia, by the local Application of Common Salt.—(*Rev. J. Fischer.*)

The Rev. J. G. Fischer was formerly a missionary in South America, and is anxious to call the attention of the public to the probable utility of common salt, as a remedy in cases of hydrophobia, if at least the opinion be correct, that what will cure the bites of venomous serpents will be efficacious in the former class of cases. He says, ' I actually and effectually cured all kinds of very painful and dangerous serpents' bites, after they had been inflicted for many hours; for immediately after I had applied my remedy the pain subsided, and the patient calmed, which remedy was nothing else than common table salt; and I kept it on the place or wound, moistened with water, till all was healed, within several days, without ever any bad effect occurring afterwards. I, for my part, never had an opportunity to meet with a mad dog, or any person who was bitten by a mad dog; I cannot, therefore, speak from experience, as to

* Ann. de Chimie., xliii., 324.

hydrophobia, but that I have cured serpents' bites always, without fail, I can declare in truth.'

Mr. Fischer then quotes Dr. Urban's practice from Hufeland's German Medical Journal. He had six methods, but his most successful was to apply a thick pledget, soaked in any saline solution, to each wound, or to each place where the teeth had made a mark without breaking the skin, and retain them there by bandages. The best solution is of salt, one ounce, or one ounce and a half, to a pound of plain water, and the wounds are to be kept constantly moistened with it. The lint is to be renewed and soaked twice a day; the places wetted every two hours, and even washed by the patient, especially if any indications of relapse, as itching or pain, should manifest themselves.

A case is then quoted from the Kent Herald, and Morning Herald of July 28, 1827, as follows: 'A friend of ours was some years since bitten by a dog, which a few hours afterwards died raving mad. Immediately upon receiving the bite, he rubbed salt for some time into the wound, and, in consequence, never experienced the least inconvenience from the bite, the saline qualities of the salt having evidently neutralized the venom, and prevented, in all probability, a melancholy death by hydrophobia.'

That which induced Mr. Fischer to try the above remedy, in the case of serpents, was 'a page of the late Bishop Loskiell's (with whom I was personally acquainted), in his History of the Missions of the Moravian Church in North America, which says, as far as I recollect, that at least among some tribes, they were not at all alarmed about the bites of serpents, having always in use such a sure remedy as salt for the cure of them, so much so, that they would suffer a bite for the sake of a glass of rum. It was this that induced me to try the cure of venomous bites with salt, and the trial has exceeded my expectations.' 'P. S. The advice of killing all dogs is neither practicable nor necessary: apply salt to man and dog, the bitten and the biter, all will be most probably well*.' &c.

8. On Restoration from Drowning by insufflation of the Lungs.

At the sitting of the 22d May of the Royal Academy of Medicine, M. Piorry reported the results of his experiments on the insufflation of the lungs of living rabbits, of the lungs of sheep, and man, after death. He concluded, first, that insufflation seldom causes rupture of the lungs unless too long and too violently continued; that death is caused by a mixture of air and blood in the heart, or by a double hydrothorax, or by the distension of the abdomen; that this insufflation may cause subpleural but not interlobular emphysema; and that insufflation of the digestive tube is almost as promptly mortal as that of the lungs by preventing the descent of the diaphragm and impeding respiration. Secondly, that crepitation always indicates

* Med. Journal, v. 49.

disease, and depends on froth in the bronchi, or on the mixture of air with an effused fluid, giving rise to *rale*, and causing asphyxia or death. 3dly. That the effusion of blood into the trachea from a wound is dangerous, as it is expectorated or absorbed with difficulty, and is disposed to be converted into froth. 4th. If water pass into the lungs during submersion, it is easily poured off by giving a declining position to the superior parts of the body; but if a person respire on the surface of the water, the water which passes into the trachea will be frothy and not easily removed. It is, therefore, necessary to remove all water before we commence insufflation. 5th. We should remember that the fluid effused during the agony (death) may be the sole cause of extinguishing life. Many members presented confirmatory reflections on the opinion of M. Piorry as to the innocuity of insufflation in a great majority of cases*.

MM. Leroy, Magendie and Dumeril are opposed to M. Piorry's opinion†.

9. Surgical Recovery of an Eye.

M. Maunoir, professor of surgery at Geneva, having performed the operation for cataract, by extraction, upon a man eighty-two years of age, weakened by an operation for hernia, which he had endured six weeks before, perceived to his regret that, although the pupil remained beautifully black and perfectly intact, the anterior and posterior chambers of the eye were not replenished, the cornea became sunk and wrinkled, a few bubbles of air penetrated the anterior chamber, and the patient had no vision. Without yielding to the first melancholy impression, the operator, by a happy presence of mind, conceived the hopes of filling the cavity: he sent immediately for some distilled water, warmed it, placed the patient on his back, and filled the external orbit of the eye with the water, opened the eyelid, and raised the flap of the cornea. The water then penetrated into all the accessible cavities, the folds of the cornea disappeared, and its convexity was restored. Having kept the eye shut for some minutes, he then directed the patient to open it, and found it in the most satisfactory condition; the patient distinguished all the objects presented to him as well as after the most successful operation. A slight pain was felt after the introduction of the water, which went off after a short time. From that time the eye healed without difficulty, and when opened a week after the operation it was free from swelling and inflammation; the cornea was perfectly united, but the pupil was a little obscure, the sight feeble, and the patient complained that he did not see so well as immediately after the operation. But six days after the bandage was removed the shade of the pupil was much diminished, the sight grew stronger from day to day, and no doubt was entertained that the patient would soon be able to read common print‡.

* Archives General. † Med. Jour. v. 73. ‡ Bib. Universelle, Oct. 1829.

10. On the Means of improving both the Quality and Quantity of Wool.—(*M. Petri.*)

A memoir upon this subject has been presented to the Academy of Sciences, and reported upon by M. Coquebert Montbret. In the sheep, says M. Petri, the nourishing fluids are naturally distributed between the flesh, the fat, and the wool. By frequent shearings, made when the animal is young, these fluids may be determined in greater abundance towards the skin, and will then nourish the woollen fibre. This theory, he says, he has applied with great success, and he finds that, besides increasing the quantity of wool, its quality is also very much improved, and the staple rendered finer. This improvement may be transmitted from one generation to another, so that whole flocks may in this way be converted into fine wool animals, only by taking care to reserve those animals for reproduction which yield the most improved produce, and paying attention, at the same time, to the choice of food, and to the other circumstances and cares which are necessary. It appears that M. Petri has not as yet had time to prove the result of prolonged trials conducted upon these principles*.

11. Vision of Birds of Prey.—(*Dr. J. Johnson.*)

It always appeared to us most extraordinary, indeed unaccountable, that birds of prey could scent carcasses at such immense distances as they are said to do. We were led to scepticism on this subject some twenty years ago, while observing the concourse of birds of prey from every point of the horizon to a corpse floating down the river Ganges, and that during the north-east monsoon, when the wind blew steadily from one point of the compass for months in succession. It was extremely difficult to imagine that the effluvia from a putrefying body in the water could emanate in direct opposition to the current of air, and impinge on the olfactories of birds many miles distant. Such, however, were the *dicta* of natural history, and we could only submit to the general opinion. We have no doubt, now that we know the general opinion to be something wrong, that it was by means of the optic rather than the olfactory nerves 'that the said birds smelled out their suit.'

The toucan is a bird which ranks next to the vulture in discerning, whether by smell or by sight, the carrion on which it feeds. The immense size of its bill, which is many times larger than its head, was supposed to present in its honeycomb texture an extensive prolongation of the olfactory nerve, and thus to account for its power of smelling at great distances; but on accurate examination, the texture above mentioned in the bill is found to be mere diploe, to give the bill strength. Now the eye of this bird is somewhat larger

* Revue Encyclopédique, xlvi. 499.

than the whole brain; and it has been ascertained, by direct experiments, that where very putrid carrion was inclosed in a basket from which effluvia could freely emanate, but which concealed the offal from sight, it attracted no attention from vultures and other birds of prey till it was exposed to their view, when they immediately recognised their object, and others came rapidly from different quarters of the horizon where they were invisible a few minutes before. This sudden appearance of birds of prey from immense distances and in every direction, however the wind may blow, is accounted for by their soaring to an altitude. In this situation their prey on the ground is seen by them, however minute it may be; and therefore their appearance in our sight is merely their descent from high regions of the atmosphere to within the scope of our optics. The toucan in India generally arrives a little in the rear of the vulture, and remains till the larger bird is glutted; while smaller birds of prey, at a still more retired distance, pay similar homage to the toucan*.

12. New Species of British Snake.

Mr. T. M. Simmons has discovered, near Dumfries, in Scotland, a species of snake which seems to be new to our naturalists, and which has been appropriately called Coluber natrix: it has no ridged line on the middle of its dorsal scales, which are extremely simple and smooth. The number of scales under the tail is about eighty, and the plates on the belly one hundred and sixty-two. The only specimen hitherto found measured five inches, was of a pale colour, with pairs of reddish-brown stripes from side to side over the back, somewhat zig-zag, with intervening spots on the sides. It comes nearest in character to a species of snake (Coluber austriacus, *Linn.*) which is common in France and Germany, and which has smooth dorsal scales, like the Dumfries snake. The latter, also, if the figure published by Sowerby be correct, has large scales on the head, which proves that it cannot be the young of the common viper, which, however, had also ridged scales.—J. R. †

13. On the Existence of Animalcula in Snow.—(*Dr. Mure.*)

The following account was sent by Dr. J. E. Mure in a letter to Dr. Silliman. 'When the winter had made a considerable progress without much frost, there happened a heavy fall of snow. Apprehending that I might not have an opportunity of filling my house with ice, I threw in snow, perhaps enough to half fill it. There was afterwards severely cold weather, and I filled the remainder with ice. About August the waste and consumption of the ice brought us down to the snow, when it was discovered that a glass of water, which was cooled with it, contained hundreds of animal-

* Medico-Chirurgical Review. Nat. Mag., ii. 473.
† Mag. Nat. Hist., ii. 458.

cules. I then examined another glass of water, out of the same pitcher, and with the aid of a microscope, before the snow was put into it, found it perfectly clear and pure: the snow was then thrown into it, and on solution the water again exhibited the same phenomenon—hundreds of animalcules, visible to the naked eye with acute attention, and, when viewed through the microscope, resembling most diminutive shrimps, and, wholly unlike the eels discovered in the acetous acid, were seen in the full enjoyment of animated nature.

'I caused holes to be dug in several parts of the mass of snow in the ice-house, and to the centre of it, and in the most unequivocal and repeated experiments had similar results; so that my family did not again venture to introduce the snow-ice into the water they drank, which had been a favourite method, but used it as an external refrigerant for the pitcher.

'These little animals may class with the *amphibia* which have cold blood, and are generally capable, in a low temperature, of a torpid state of existence. Hence their icy immersion did no violence to their constitution, and the possibility of their revival by heat is well sustained by analogy; but their *generation*, their *parentage*, and their *extraordinary transmigration*, are to me subjects of profound astonishment*.'

14. Antipathy of the Chameleon to Black.

Whatever may be the cause, the fact seems to be certain, that the chameleon has an antipathy to things of a black colour. One which Forbes kept uniformly avoided a black board which was hung up in the chamber; and, what is most remarkable, when it was forcibly brought before the black board, it trembled violently, and assumed a black colour †. It may be something of the same kind which makes bulls and turkey-cocks dislike the colour of scarlet; a fact of which there can be no doubt ‡.

15. Phosphorescence of the Sea in the Gulf of St. Lawrence.

Captain Bonnycastle, R.E., whilst coming up the gulf on the 7th September, 1826, observed this phenomenon under the following interesting circumstances. At two o'clock, A.M., the mate, whose watch it was on deck, suddenly aroused the captain in great alarm, from an unusual appearance on the lee bow. The night was starlight; but suddenly the sky became overcast in the direction of the high land of Cornwallis county, and a rapid, instantaneous, and immensely brilliant light, resembling the aurora borealis, shot out of the hitherto gloomy and dark sea on the lee bow, and was so vivid that it lighted

* Silliman's Journal, xviii. 57.

† Oriental Memoirs, p. 350. ‡ J. R., Nat. Mag., ii. 269.

everything distinctly, even to the masthead. The mate having alarmed the master, put the helm down, took in sail, and called all hands up. The light now spread over the whole sea between the two shores; and the waves, which before had been tranquil, now began to be agitated. Captain Bonnycastle describes the scene as that of a blazing sheet of awful and most brilliant light. A long and vivid line of light, superior in brightness to the parts of the sea not immediately near the vessel, shewed us the base of the high, frowning, and dark land abreast of us; the sky became lowering and most intensely obscure. The oldest sailors on board had never seen anything of the kind to compare with it, except the captain, who said he had observed something of the kind in the trades. Long tortuous lines of light, in a contrary direction to the sea, shewed us immense numbers of very large fish darting about as if in consternation at the scene. The spritsail-yard and mizen-boom were lighted by the reflection as though gas-lights had been burning immediately under them; and until just before daybreak, at four o'clock, the most minute objects in a watch were distinctly visible. Day broke very slowly, and the sun rose of a fiery and threatening aspect. Rain followed.

Captain Bonnycastle caused a bucket of this fiery water to be drawn up: it was one mass of light when stirred by the hand, and not in sparkles as usual, but in actual coruscations. A portion of this water, kept in an open jug, preserved its luminosity for seven nights.

On the third night the scintillations of the sea reappeared, and were rendered beautifully visible by throwing a line overboard and towing it along astern of the vessel. On this evening the sun went down very singularly, exhibiting in its descent a double sun, and, when only a few degrees above the horizon, its spherical figure changed into that of a long cylinder, which reached the horizon. In the night the sea became nearly as luminous as before. On the fifth night the luminous appearance nearly ceased.

Captain Bonnycastle is unwilling to attribute the above effect to living animalcula; but suggests the idea that it depends upon some compound of phosphorus suddenly evolved and dispersed over the surface of the sea. In such a compound he conceives the phosphorus or phosphoric acid to be afforded by exuviæ or secretions of fish, and the other constituents to be in some way connected with those abundant oceanic salts, the muriate of soda and sulphate of magnesia *.

16. Rending of Timber by Lightning.

Some pieces of an oak struck by lightning have been presented to the Academy of Sciences by M. Arago, from the Duke de Chartres. One was about three feet in length, and was split into lathes from two to three lines in thickness and eight or ten lines in width; the

* Silliman's Journal, xviii. p. 187.

other from twelve to fifteen lines (query, inches?) was divided into a multitude of longitudinal fragments so as to resemble a broom. M. Arago, on this occasion, referred to two other cases in which carpentry had been disintegrated in a similar manner. Lavoisier said, relative to the latter, that one piece was split into longitudinal fragments so thin and numerous as to resemble perfectly a box of alumettes. These observations, made on dry wood, shew that that explication should be rejected which applied only to living wood, and which supposed that the electric fluid descended along the vessels containing the sap *.

It is well known that there is powerful expansion in the space through which an electric discharge passes. The old instrument called Kinnersley's electrometer is founded upon this effect. Now, supposing lightning to strike a tree, the mere difference of cohesion of the wood in different directions would account for the splitting into fibres, without reference to the direction of the electricity. In living or moist wood the conversion of the aqueous parts present into vapour, by increasing the expansive power, would tend to increase the rending effect; but still the wood would give way in the same manner. If, therefore, the force be enough to split the wood, but yet not sufficient to tear it to atoms, it would of necessity rend it into lathes or fibres.—*Ed.*

17. Protraction of Vegetable Life in a dry State.

Medico-Botanical Society.—Mr. Houlton produced a bulbous root which was discovered in the hand of an Egyptian mummy, in which it probably had remained for two thousand years. It germinated on exposure to the atmosphere; when placed in earth it grew with great rapidity †.

18. Market State of Hyosciamus.

According to Mr. Houlton hyosciamus, as usually sold in the markets, is of the first year's growth, and is inert; that of the second year's growth, collected in June or July, is alone to be depended upon as a remedy. Hence, probably, much of the uncertainty attending the use of this plant in practice.

19. Snow of the Winters 1829-1830.

M. Huber-Burnand was induced to pay particular attention to the character of the snow which fell last winter at Yverdun, during the months of January and February, in consequence of certain singular appearances which had not before been observed. He had also remarked the same character on the 21st, 22d, 23d, and 24th of January, 1829, which were very cold days. This snow was crys-

* Revue Ency., xlvi. p. 498.

† Med. Journ. v. 79.

tallized in stellar pallets, with six rays, along which were disposed other filaments arranged as in feathers, and these again supporting other finer filaments similarly arranged. The angles were sixty degrees, the pallets were extremely thin, perfectly plane, and quite regular in form.

Previous to the 2d of January of the present year, the quantity of snow of this kind which had fallen was but small, but on the 2d, 3d, and 4th of January the quantity was so great, all of the same kind, as to attract general attention; every body was talking of it, and comparing it to feathers. M. Huber-Burnand ventured to call it *Polar Snow*, from its corresponding to the description given of such, and it retained the name. Whenever this snow fell during the winter, it was found to be of the same kind. Five or six inches of this snow fell in the three days mentioned. It was extremely light, very dry, and without adhesiveness. Instead of presenting a swan-like whiteness, it had more the silvery appearance of feathers of the colymbus, in consequence of the high polish of its crystalline facets. When this snow was dropped freely into a basin, measured, and then melted, it gave one-forty-fifth its volume of water.

This snow fell on various occasions during the winter. In the intervals another kind fell, which was called *elementary snow*. It fell only on foggy days, and was supposed to be formed near the earth. The particles were excessively fine, not regularly crystallized. It fell as a fine powder, but only rarely. Both these kinds of snow fell at temperatures much below that of ordinary snow, namely, at ten or fifteen degrees below the freezing point.

On the 23d and 24th November, 1829, the temperature being two or three degrees above freezing, it snowed continually for twenty-four or thirty hours, nevertheless it did not accumulate on the ground to a height of more than eight inches, because much of it melted as it fell. The water derived from it amounted to 31 lines, that enormous quantity being collected in the rain-guage. The wind passed during the time from being violent at south-west to the north-west, where it remained. The snow was heavy, and full of water; it broke the branches of the trees in the neighbourhood, especially the upper ones, upon which it frequently rested to the height of more than a foot.

The hoar frost of last winter was also abundant and peculiar at Yverdun. It each day affected a different form, being sometimes in parallel fillets, or groupes, sometimes resembling leaves, at others spines, occasionally spines terminated by a flat rosette, with six divisions, &c., the spines being sometimes an inch in length. These arrangements were all alike on the *same* day. Such effects shew us, that circumstances probably occur with the air of which we are ignorant, although they are sufficiently powerful to have a strong influence in certain phenomena which occur in that elastic fluid *.

* Bib. Univ., 1830, 355.

20. Peculiar Fall of Snow.—(*Mr. Sherriff.*)

On Saturday, the 20th instant, (Feb. 1830,) it commenced snowing here (East Lothian) about eight o'clock P.M., and continued till twelve, about which time there arose a very violent storm of wind, accompanied with a heavy shower of sleet and rain, after which another fall of snow occurred. On the morning of Sunday (21st instant), the frost was pretty keen, and there was a slight crust found on the surface of the fallen snow.

The fields presented a very uncommon appearance, being thickly studded with snow-balls varying from a foot to a foot and a half in diameter. The field, in which I first observed them, has a gentle declivity from south to north, but this I think is inadequate to afford a satisfactory explanation of their formation, as the hollow tract which they had formed in the snow I observed to be from *west* to *east;* the wind was from the west.

I afterwards observed them in fields quite level. In one village in particular, which had an exposure to the west, they were exceedingly numerous, being not above a yard and a half separate from each other. I did not minutely examine the internal structure of them, but I saw one which had been cut through the middle by the wheel of a gig, and it did not appear to be composed of any thing but snow, having no hard body for an internal nucleus *.

21. Electricity of the Winds.

In the Mediterranean. Mr. Black ascertained by numerous observations, that winds or currents of vapour of some continuance from an extent of sea, are *negatively* charged with electricity; while those from the land, especially from hilly countries, are relatively in a *positive* condition. When opposite winds, such as north and south, are differently charged with electricity, and meet, a transfer of the electric matter is always the consequence †.

22. Irised Aurora Borealis.

The following particulars of a phenomenon of this kind are from an account sent by James Bowdoin, Esq. to Professor Silliman. About nine o'clock on the 8th of September, 1827, (place, Augusta in Maine,) a bright and well defined arch appeared extending towards the east and west, whose crown was about 45° above the northern horizon. It almost instantly disappeared. Then pencils or rather columns, *perfectly irised*, were seen very strongly resembling regular segments of a fine rainbow in the disposition and arrangement of colours and in shape, although in some other particulars having the appearances of clouds so illuminated. Each of these pencils or columns, the sides of which were parallel, and their ends regularly

* Edin. Nat. Journ., ii., 58. † Mag. Nat. Hist., ii., 468.

and smoothly truncated perpendicularly to these sides, was somewhere about half a degree in width, and in length about eight degrees, though varying in both particulars. They were not radii from the north, but parallel to each other, running from a little east of north, their lower extremities being about 20° from the horizon. The bearings of these columns differed much from that of the arch before mentioned. They soon became *merry dancers*, were then bent rapidly, and continued nimbly playing into curves of small circles, sometimes looking as if gracefully folded or twisted like the most delicate gauze. The order of the colours, whether the same with those of the rainbow, the same in all the columns, and the red towards east or west, was not noted at the time.

The iris-like appearance continued only a few minutes. The sky soon became pure; all clouds disappeared, and long bright streamers shot up from the north to the zenith; some of them continued half a minute, and were occasionally tinged red or yellow. After these streamers, feeble lights from the north rose like faintly luminous puffs of smoke, then occurred the common lights, and in about fifteen or twenty minutes from the first all was over.

The moon, nearly full, rose about eight o'clock, and shone the whole time, but nothing justifies the reference of the colours to her light, on the contrary many circumstances oppose it. The air had towards evening become rapidly cool, and the atmosphere during the day had been smoky from the burning wood, but these are considered to have no relation to the colours *.

23. Influence of the Age of Parents on the Sex of Children.

The following results are extracted from a letter written by Professor Hosacker to the Editor of the *Medical Gazette* of Inspruck:—

i. In those marriages where the mother is older than the father the number of boys is to that of girls as 90.6 to 100. ii. The parents being of the same age, the boys were to girls as 90 to 100. iii. The father being from three to six years older than the mother, the proportion of boys to girls was as 103.4 to 100. iv. The father being from six to nine years older than the mother, the boys were to the girls as 124.7 to 100. v. The father being from nine to twelve years older than the mother, the male children were to the female as 123.7 to 100. vi. The father being eighteen years or more older than the mother, the boys were to the girls as 200 to 100†.

24. Precautions in the Planting of Potatoes.

It would appear from experiments made in Holland, that when potatoes are planted, the germs of which are developed, as happens occasionally in late operations, or after mild winters, that the produce

* Silliman's Journ., xviii., 72. † Bib. Univ., 1830, 456.

differs in quantity by more than a third to what it would be if potatoes which had not advanced had been used, and further, that besides this diminished product, the quality also is very inferior*.

25. Preservation of frozen Potatoes.

In time of frost the only precaution necessary is to retain the potatoes in a perfectly dark place for some days after the thaw has commenced. In America, where they are sometimes frozen as hard as stones, they rot if thawed in open day, but if thawed in darkness they do not rot, and lose very little of their natural odour and properties†.

26. Cure of Wounds in Elm Trees.

Those elms which have running places, or ulcers, may be cured as follows:—Each wound is to have a hole bored in it with an auger, and then a tube, penetrating an inch or less, is to be fixed in each. Healthy trees which are thus pierced give no fluid, but those which are unhealthy yield fluid, which increases in abundance with the serenity of the sky and exposure to the south. Stormy and windy weather interrupts the effect. It has been remarked that in from twenty-four to forty-eight hours the running stops, the place dries up, and is cured‡.

27. Preservation of Fruit Trees from Hares.

According to M. Bus, young fruit trees may be preserved from the bites of hares by rubbing them with fat, and especially hogs'-lard. Apple and pear trees thus protected gave no signs of the attacks of these animals, though their feet marks were abundant on the snow beneath them§.

28. Waterspout on the Lake of Neufchatel.

On the 9th of June, at nine o'clock in the morning, the weather being moist and the thermometer at 64° F., a waterspout was seen at Neufchatel, on the other side of the lake, about a league from the port. From a fixed black cloud, about eighty feet above the surface, descended perpendicularly a dark grey cylindrical column, touching the surface of the lake. Much agitation was seen at the foot and top of the column, a dull heavy sound was heard, and the waters of the lake were seen to mount rapidly along this sort of syphon to the cloud, which gradually became white as it received them. After seven or eight minutes had elapsed a north-east wind pressed upon the column, so that it bent in the middle, still however raising water, until at last it separated. At the same moment the cloud above, agitated and compressed by the wind, burst and let fall

* Bib. Phys. Econ., 1829.

† Recueil Industriel, xiv. 81.

‡ Journal des Forêts, 1829.

§ Bull. Univ. D. xiv. 381.

a deluge of rain. This appearance was neither preceded nor followed by any lightning or explosion; the column was vertical and immobile, no rotary movement being observed*.

29. Mirage of Central India.

The following account of the Indian mirage is from Colonel Tod's *Ragasthan*. It is only in the cold season that the *mirage* is visible. The sojourners of Maroo call it the *see-kote*, or 'castles in the air.' In the deep desert, to the westward, the herdsmen and travellers through these regions style it *chittrám*, 'the picture;' while about the plains of the Chumbol and Jumna they term it *dessasúr*, 'the omen of the quarter.' This optical deception has been noticed from the remotest times. The prophet Isaiah alludes to it when he says 'and the parched ground shall become a pool;' which the critic has justly rendered 'and the *sehráb* shall become real water.' Quintus Curtius, describing the *mirage* in the Sogdian desert, says that 'for the space of four hundred furlongs not a drop of water is to be found, and the sun's heat being very vehement in summer, kindles such a fire in the sands that everything is burnt up. There also arises such an exhalation that the plains wear the appearance of a vast and deep sea,' which is an exact description of the *chittrám* of the Indian desert. But the *sehráb* and *chittrám*, the true *mirage* of Isaiah, differ from that illusion called the *see-kote*, and though the traveller will hasten to it in order to obtain a night's lodging, I do not think he would expect to slake his thirst there.

When we witnessed this phenomenon, at first the eye was attracted by a lofty opaque wall of lurid smoke, which seemed to be bounded by or to rise from the very verge of the horizon. By slow degrees, the dense mass became more transparent, and assumed a reflecting or refracting power; shrubs were magnified into trees; the dwarf *khyre* appeared ten times larger than the gigantic *amli* of the forest. A ray of light suddenly broke the line of continuity of this yet smoky barrier, and, as if touched by the enchanter's wand, castles, towers, and trees were seen in an aggregated cluster, partly obscured by magnificent foliage. Every accession of light produced a change in the *chittram*, which, from the dense wall that it first exhibited, had now faded into a thin transparent film broken into a thousand masses, each mass being a huge lens, until at length the too vivid power of the sun dissolved the vision; castles, towers, and foliage melted like the enchantment of Prospero into 'thin air.'

But the difference between the *sehrab* or *chittrám* and the *see-kote* or *dessasúr* is, that the latter is never visible but in the cold season, when the gross vapours cannot rise, and that the rarefaction which gives existence to the other destroys this whenever the sun has attained 20° of elevation.

* Bib. Univ., June, 1830.

A high wind is alike adverse to the phenomenon, and it will mostly be observed that it covets shelter, and its general appearance is a long line, which is sure to be sustained by some height, such as if it required support. The first time I observed it was in the Jesipoor country: none of the party had ever witnessed it in the British provinces. It appeared like an immense walled town, with bastions; nor could we give credit to our guides when they talked of the *see-kote*, and assured us that the objects were merely 'castles in the air.' I have since seen, though but once, this panoramic scene in motion, and nothing can be imagined more beautiful.

It was in Kotah, just as the sun rose, whilst walking on the terraced roof of the garden-house of my residence, as I looked towards the low range which bounds the sight to the south-east, the hills appeared in motion, sweeping with an undulating or rotatory movement along the horizon, trees and buildings were magnified, and all seemed a kind of enchantment. Some minutes elapsed before I could account for this wonder, until I determined that it must be the masses of a floating *mirage*, which had attained its most attenuated form, and being carried by a gentle current of air past the tops and sides of the hills while it was itself imperceptible, made them appear in motion. But, although this was novel and pleasing, it wanted the splendour of the scene of the morning, which I never saw equalled but once. This occurred at Hissar, on the terrace of James Lumsdaine's house, built amidst the ruins of the castle of Fero, in the centre of one extended waste, where the lion was the sole inhabitant, that I saw the most perfect specimen of this phenomenon. It was really sublime. Let the reader fancy himself in the midst of a desert plain, with nothing to impede the wide scope of vision; his horizon bounded by a lofty black wall encompassing him on all sides; let him watch the first sunbeam break upon this barrier, and at once, as by a touch of magic, shiver it into a thousand fantastic forms, leaving a splintered pinnacle in one place, a tower in another, an arch in a third,—these in turn, undergoing more than kaleidoscopic changes until the '*fairy fabric*' vanishes. Here it was emphatically called *Hurchuna Raja ca poori*, or 'the city of Raja Hurchuna,' a celebrated prince of the brazen age of India. The power of reflection shewn by this phenomenon cannot be better described than by stating that it brought the very ancient *Aggaroa*, which is thirteen miles distant, with its fort and bastions, close to my view.

The difference, then, between the *mirage* and the *see-kote* is that the former exhibits a horizontal, the latter a columnar or vertical stratification, and in the latter case likewise, a contrast to the other, its maximum of translucency is the last stage of its existence. In this stage it is only an eye accustomed to the phenomenon that can perceive it at all. I have passed over the plains of Meerut with a friend who had been thirty years in India, and he did not observe a *see-kote* then before our eyes; in fact so complete was the illusion, that we only saw the town and fort considerably nearer.

Indeed, whoever notices while at sea the atmospheric phenomena of these southern latitudes, will be struck by the deformity of objects as they pass through this medium; what the sailors term a fog-bank is the first stage of our *see-kote*. I observed it on my voyage home, but more especially in my passage out. About six o'clock on a dark evening, while we were dancing on the water, I perceived a ship bearing down with full sail upon us so distinctly, that I gave the alarm in expectation of a collision; so far as I recollect, the helm was instantly up, and in a second no ship was to be seen. The laugh was against me. I had seen the 'Flying Dutchman,' according to the opinion of the experienced officer on deck, and I believed it was really a vision of the mind; but I now feel convinced it was either the reflection of our own ship in a passing cloud of this vapour, or a more distant object therein refracted *.

30. VILLAGE LIGHTED BY NATURAL GAS.

The village of Fredonia in the western part of the state of New York presents this singular phenomenon. I was detained there a day in October of last year, and had an opportunity of examining it at leisure. The village is forty miles from Buffalo, and about two from lake Erie; a small but rapid stream called the Canadaway passes through it, and after turning several mills discharges itself into the lake below; near the mouth is a small harbour with a lighthouse. While removing an old mill which stood partly over this stream in Fredonia, three years since, some bubbles were observed to break frequently from the water, and on trial were found to be inflammable. A company was formed, and a hole an inch and a half in diameter, being bored through the rock, a soft fetid limestone, the gas left its natural channel and ascended through this. A gasometer was then constructed, with a small house for its protection, and pipes being laid, the gas is conveyed through the whole village. One hundred lights are fed from it more or less, at an expense of one dollar and a half yearly for each. The flame is large, but not so strong or brilliant as that from gas in our cities: it is, however, in high favour with the inhabitants. The gasometer I found on measurement collected eighty-eight cubic feet in twelve hours during the day; but the man who has charge of it told me that more might be procured with a larger apparatus. About a mile from the village, and in the same stream, it comes up in quantities four or five times as great. The contractor for the lighthouse purchased the right to it, and laid pipes to the lake; but found it impossible to make it descend, the difference in elevation being very great. It preferred its old natural channels, and bubbled up beyond the reach of his gasometer. The gas is carburetted hydrogen, and is supposed to come from beds of bituminous coal: the only rock visible, however, both here, and to a great extent on both sides along the southern shore of the lake, is fetid limestone †.

* Silliman's Journal, xvii. 398.

† Brewster's Journal, 1830, p. 265.

31. Singular Natural Sound.

'In the autumn of 1828, when on a tour through Les Hautes Pyrenées,' says a recent traveller, 'I quitted Bagneses de Luchon at midnight, with an intention of reaching the heights of Porte de Venasque—one of the wildest and most romantic boundaries between the French and Spanish frontier, from the summit of which, the spectator looks at once upon the inaccessible ridges of the Maladetta, the most lofty point of the Pyrenean range. After winding our way through the deep woods and ravines, constantly ascending above the valley of Luchon, we gained the Hospice about two in the morning, and after remaining there a short time, proceeded with the first blush of dawn, to encounter the very steep gorge terminating in the pass itself, a narrow vertical fissure through a wall of massive and perpendicular rock. It is not my intention to detail the features of the magnificent scene which burst upon our view, as we emerged from this splendid portal, and stood upon Spanish ground—neither to describe the feelings of awe which riveted us to the spot, as we gazed, in speechless admiration, on the lone, desolate, and (if the term may be applied to a mountain) the ghastly form of the appropriately named Maladetta. I allude to it solely for the purpose of observing, that we were most forcibly struck with a dull, low, moaning, Æolian sound, which alone broke upon the deathly silence, evidently proceeding from the body of this mighty mass, though we in vain attempted to connect it with any particular spot, or assign any adequate cause for these solemn strains. The air was perfectly calm; the sky was cloudless; and the atmosphere clear to that extraordinary degree, conceivable only by those who are familiar with the elevated regions of southern climates; so clear, and pure indeed, that at noon a bright star which had attracted our notice through the grey of the morning, still remained visible in the zenith. By the naked eye, therefore, and still more with the assistance of a telescope, any waterfalls of sufficient magnitude would have been distinguishable on a front base, and exposed before us; but not a stream was to be detected, and the bed of what gave evident tokens of being occasionally a strong torrent, intersecting the valley at its foot, was then nearly dry. I will not presume to assert, that the sun's rays, though at that moment impinging in all their glory on every point and peak of the snowy heights, had any share in vibrating these mountain chords; but on a subsequent visit, a few days afterwards, when I went alone to explore this wild scenery, and at the same hour stood on the same spot, I listened in vain for the moaning sounds: the air was equally calm, but the sun was hidden by clouds, and a cap of dense mist hung over the greater portion of the mountain*.'

* N. M. Mag. xxx. 341.

JOURNAL

OF

THE ROYAL INSTITUTION

OF

GREAT BRITAIN.

ON A PECULIAR CLASS OF OPTICAL DECEPTIONS.

By M. FARADAY, F.R.S.

Director of the Laboratory of the Royal Institution, &c. &c.

THE pre-eminent importance of the eye as an organ of perception confers an interest upon the various modes in which it performs its office, the circumstances which modify its indications, and the deceptions to which it is liable, far beyond what they otherwise would possess. The following account of a peculiar ocular deception, which, in a greater or smaller degree, is not uncommon, and which, if looked for, may be observed with the utmost facility, may, therefore, prove worthy of attention; and I am the more inclined to hope so, because in some points it associates with an account and explanation of an ocular deception given by Dr. Roget in the Philosophical Transactions for 1825, page 121.

The following are some cases of the appearance in question. Being at the magnificent lead mills of Messrs. Maltby, two cog wheels were shewn me moving with such velocity, that if the eye were retained immovable, no distinct appearance of the cogs in either could be observed; but, upon standing in such a position that one wheel appeared behind the other, there was immediately the distinct, though shadowy resemblance of cogs moving slowly in one direction.

Mr. Brunel, junr. described to me two small similar wheels at the Thames Tunnel: an endless rope which passed over, and was carried by one of them, immediately returned and passed in the opposite direction over the other, and conse-

quently moved the two wheels in opposite directions with great but equal velocities. When looked at from a particular position, they presented the appearance of a wheel with immovable radii.

When the two wheels of a gig or carriage in motion are looked at from an oblique position, so that the line of sight crosses the axle, the space through which the wheels overlap appears to be divided into a number of fixed curved lines, passing from the axle of one wheel to the axle of the other, in form and arrangement resembling the lines described by iron filings between the opposite poles of a magnet. The effect may be obtained at pleasure by cutting two equal wheels out of white cardboard (Fig. 1. Plate 3.), each having from twelve to twenty or thirty radii, sticking them on a large needle two or three inches apart, revolving them between the fingers, and looking at them in the right direction against a dark or black ground; the greater the velocity of the wheels the more perfect will be the appearance (Fig. 2.)

When the dark-coloured wheel of a carriage is moving on a good light-coloured road, so that the sun shines almost directly on its broadside, and the wheel and its shadow are looked at obliquely, so that the one overlaps the other in part, then, in the overlapping part, luminous or light lines will be perceived curved more or less, and conjoining the axle and its shadow, if the wheel and shadow are superposed sufficiently; or, tending to do so, if they are superposed only in part: the more rapid the motion the more perfect is the appearance. The effect may be easily observed by making a pasteboard wheel like one of those just described, blackening it, sticking it on a pin, and revolving it in the sunshine, or in candle light, before a sheet of white paper (Fig. 3.) If the wheel be converted into a tetotum or top, by having a pin thrust through its centre, and spun upon a sheet of white paper, the effect produced by the wheel and its shadow will be obtained with facility, and in form will resemble Fig. 2. In all these cases no rims are required; the spokes or radii produce the effect.

If a carriage wheel running rapidly before upright bars, as a palisade or railing, be observed, the attention being fixed upon the wheel, peculiar stationary lines will appear: those perpen-

dicular to the nave or axis will be straight, but the others curved; and the curve will be greatest in those which are farthest from the upper straight line. These curves are the same in form as those already described and explained by Dr. Roget *, and the appearance itself is produced in a similar manner; but the phenomena are distinct and the causes different. The effect at present referred to is best observed when the velocities are great, whereas that explained by Dr. Roget takes place only when the velocities are moderate. It is probable that some of the appearances briefly mentioned by an anonymous writer in the Quarterly Journal of Science, first series, vol. x. p. 282, and already referred to by Dr. Roget, were of the kind now to be explained; for though the description is not accurate either for the effects which form the object of this paper, or that explained by Dr. Roget,—and is, indeed, inconsistent with the observation or explanation of any of the phenomena,—it probably had its origin in the occurrence of some of both kinds under the eyes of the writer. The effect is easily obtained by revolving a white pasteboard wheel before a black or dark ground, and then, whilst regarding the wheel fixedly, traversing the space before it with a grate also cut out of white pasteboard. By altering the position of the grate and direction of its motion, it will be seen that the straight lines in the wheel are always parallel to the bars of the grate, and that the convexity of the curved lines is always towards that side of the grate where its motion coincides in direction with the motion of the radii of the wheel. By varying the velocity of the wheel and grate, the curves change in their appearance, and the whole or any part of the system, as described and figured by Dr. Roget, may be obtained at pleasure.

I have had a very simple apparatus constructed, by which these and many other analogous appearances can be shewn in great perfection and variety. One board was fixed upright upon the middle of another, serving as a base; the upright board was cut into the shape represented in Fig. 4.; the middle, and the two extreme projections, forming points of support, were supplied with little caps cut out of copper-plate and bent into shape (Fig. 5.), so that, when in their places, they offer

* *Philosophical Transactions*, 1825, *p.* 121.

four bearings for the support of two axes, one on each side the middle. The axes are small pieces of steel wire tapered at the extremities ; each has upon it a little roller or disc of soft wood, which, though it can be moved by force from one part of the axis to another, still has friction sufficient to carry the latter with it when turned round. These axes are made to revolve in the following manner: A circular copper plate about four inches in diameter has three pullies of different diameter fixed upon its upper surface, whilst its lower surface is covered with a piece of fine sand-paper attached by cement. A hole is made through the centre of the plate and pullies, and guarded by brass tube, so fitted as to move steadily but freely upon an upright steel pin fixed in the middle of the centre wooden support; hence when the plate is in its place, it rests upon the two rollers belonging to the horizontal axes, whilst it is rendered steady by the upright pin. It can easily be turned round in a horizontal plane, and it then causes the two axes with their rollers to revolve in opposite directions, and the velocities of these can be made either equal to each other, or to differ in almost any ratio by shifting the rollers upon the horizontal axes nearer to, or farther from the centre of the stand.

To produce motions of the axes in the same direction, an aperture was cut in the lower part of the upright board ; a roller, turned for it, which loosely fitted within the aperture; and a steel pin or rod passed as an axis through the roller. The roller hangs in its place by endless lines made of thread, passing under it, and over little pullies fixed on the horizontal axes; when, therefore, it is turned by the projecting pin, it causes the revolution of the axes. The variation in velocities is obtained by having the roller of different diameters in different parts, and by having pullies of different dimensions. This description will be easily understood by reference to the figures.

This apparatus had to carry wheels either with cogs or spokes; which was contrived in the following manner. The wheels were cut out of cardboard, were about seven inches in diameter, and were formed with cogs or spokes at pleasure. A piece of cork, being the end of a phial cork, about the tenth of an inch in thickness, was then fastened by a little soft cement

to the middle of the wheel, and a needle run through both, and then withdrawn. These wheels could at any time be put upon the axes, and, being held sufficiently firm by the friction of the cork, turned with them. By these arrangements the axes could be changed, or the wheels shifted, or the velocities altered without the least delay.

The beauty of many of the effects obtained by this apparatus has induced me to describe it more particularly than I otherwise should have done. The appearance which I first had shewn to me by Mr. Maltby was exhibited very perfectly; two equal cog wheels were mounted (Fig. 6.), so as to have equal opposite velocities; when put into motion, which was easily done by the thumb and finger applied to the upper pully of the horizontal copper plate, they presented each the appearance of an uniform tint at the part corresponding to the series of cogs or teeth, provided that the eye was so placed as to see the whole of both wheels; but when a position was taken up, so that the wheels were visually superposed, then in place of an uniform tint, the appearance of teeth or cogs was seen—misty, but perfectly stationary, whatever the degree of velocity given to the wheel. By cutting the cogs or teeth in the wheel nearest to the eye, deeper (Fig. 7.), the eye could be brought into the prolongation of the axes of the wheels, and then the spectral cog wheel appeared perfect (Fig. 8). The number of intervals thus occurring was exactly double the number of teeth in either wheel: thus a wheel with twelve teeth produced twenty-four black, and twenty-four white alternations. When one wheel was made to move a little faster than the other, by shifting the wooden roller on its axis, then the spectrum travelled in the direction of that wheel having the greatest velocity; and with more rapidity the greater the difference between the velocities of the two wheels. When the wheels were looked at so that they only partly visually superposed each other, the effect took place only in those parts: and it was striking and extraordinary to observe, as it were, two uniform tints mingling, and instantly breaking out into the alternations of light and shade which I have described. There are many variations in the curvature and other appearances obtained by altering the position of the eye, which will be immediately understood when

observed, and which for brevity's sake I refrain from describing.

Wheels were then fixed on the machine, consisting of radii or spokes, each having twelve, equal in length and width (Fig. 1). When revolved alone, each wheel gave, with a certain velocity, a perfectly uniform tint; but when visually superposed, there appeared a fixed wheel, having twenty-four spokes, equal in dimensions to the original spokes. Variations of the position of the eye, or of the relative velocity of the two wheels, caused alterations similar to those I have referred to with the cog wheels.

In observing these effects, either the wheels should be black or in shade, whilst the part beyond is illuminated; or else the wheels should be white and enlightened, whilst the part beyond is in deep shade. The cog wheels present nearly a similar appearance in both cases, though in reality the parts of the spectrum which appear darkest by the one method are lightest by the other. The spoke wheels give a spectrum having white radii, in the first method, and dark radii in the second. Placing the wheels between the eye and the clouds, or a white wall, or a lunar lamp, answers well for the first method; and for the second, merely reversing the position, and allowing the light to shine on the parts of the wheel towards the eye, whilst the back ground is black, or in obscurity, is all that is required. Strictly, the phenomena should be viewed with one eye only, but it is not often that vision with two eyes disturbs the effects to any extent.

The cause of these appearances, when pointed out, is sufficiently obvious, and immediately indicates many other effects of a similar kind, and equally striking, which are dependent upon it. The eye has the power, as is well known, of retaining visual impressions for a sensible period of time; and in this way, recurring actions, made sufficiently near to each other, are perceptibly connected, and made to appear as a continued impression. The luminous circle visible when a lighted coal or taper is whirled round—the beautiful appearances of the kaleidophone—the uniform tint spread by the revolution of one of the spoke or cog wheels already described —are a few of the many effects of this kind which are well known.

But during such impressions, the eye, although to the mind occupied by an object, is still open, for a large proportion of time, to receive impressions from other sources; for the original object looked at is not in the way to act as a screen, and shut out all else from sight; the result is, that two or more objects may seem to exist before the eye at once, being visually superposed. The schoolboy experiment of seeing both sides of a whirling halfpenny at the same moment,—the appearances produced by the thaumatrope,—and the transparency of the revolving cog or spoke wheels referred to, in consequence of which other objects are seen through the shaded parts,—are all effects of this kind; two or more distinct impressions, or sets of impressions, being made upon the eye, but appearing to the perception as one.

So it is in the appearances particularly referred to in this paper: they are the natural result of two or more impressions upon the eye, really, but not sensibly, distinct from each other. If, whilst the eye is stationary, a series of cogs like those represented by the continuous outline (Fig. 9.) pass rapidly before it, they produce a uniform tint to the eye; and for the purpose of following out the description, let it be supposed the cogs are in shade between the eye and a white back-ground; the tint is then a hazy, semitransparent grey. If another series of cogs represented by the dotted outline, and close to the first, so as to give no sensible angular difference in the dimensions of the cogs, pass with equal velocity in the same direction, it will produce its corresponding tint. If the two sets of cogs be visually superposed in part, as in the figure, there will be no alteration in the uniformity of the tint. If the cogs of one set be more or less to the right or left of the other, then the superposed part will approach more or less to the tint of the shaded and uncut part of the cardboard wheel, and be less transparent. But if, instead of the motion being equal, the velocities are unequal, then total changes of the appearance supervene; the spectrum (if I may so call it) of the superposed parts becomes alternately light and dark, and the alternations take place more or less rapidly as the velocities of the two sets of cogs differ more or less from each other.

When the cogs move in opposite directions, the uniform tint

which each alone can produce is soon broken up in the superposed parts into lighter and darker portions, and when the velocities of both are equal, the spectrum is resolved into a certain number of light and dark alternations, which are perfectly fixed, and which, to the mind, offer a singular contrast to the rapidly moving state of the wheels, and to the variations which their velocity may undergo without altering the visible result.

This effect, strange as it at first appears, will be easily understood by reference to Fig. 9. Suppose the eye directed to the part *l* beyond the cogs, and the sets of cogs to be moving with equal velocities in the opposite directions, indicated by the arrow heads: the part *l* will be eclipsed by the cogs *a* and *b* simultaneously, and for exactly the same time, for they begin to cover it and they leave it together; *l* therefore is alternately open to and shut from the eye for equal times; for what these cogs have done, will be performed by all the other cogs in turn, and the cogs are equal in area to the spaces between: half the light, therefore, from that part of the back-ground comes to the eye, and produces a corresponding impression. But with respect to the point *d*, although the cog *b* is just leaving it exposed, the cog *a* is just beginning to eclipse it; and by the time the latter has passed over, the edge of the cog *e* will be upon the spot, and that cog will therefore hide it until *f* comes up; so that in fact the point *d* is always hidden, no light comes from that part of the back-ground, and it consequently appears dark—*l′* is circumstanced just as *l* was, for the cogs *a* and *e* cover it simultaneously, and so do all the other cogs in pairs; it is therefore a light space in the spectrum: *d′* is a repetition in everything of *d*, and is a dark space. The parts intermediate between the maxima of light and darkness will, by examination, be found to be eclipsed for intermediate periods, and to appear more or less dark in consequence, so that the appearance of the spectrum belonging to the visually superposed parts of the two sets of cogs is as in Fig. 10.

In the case of equal wheels with radii, the fixed spectrum produced when the wheels superpose each other has twice the number of radii of either wheel, that being of course the number of times which the radii coincide with each other in one

revolution. Fig. 11. represents the fixed spectrum produced by two equal wheels of eight radii each. When the radii or spokes are narrow, the difference in the intensity of tint between the middle and the edges of each image of a spoke is so slight as to be scarcely perceptible. But as this circumstance and many others will explain themselves immediately they are experimentally observed, it is unnecessary to dwell minutely upon them here.

A very simple experiment will render the whole of these effects perfectly intelligible. If a little rod of white cardboard five or six inches long, and one-thirtieth of an inch wide, be moved to and fro from right to left before the eye, an obscure or black back-ground being beyond, it will spread a tint, as it were, over the space through which it moves (Fig. 12.) A similar rod held and moved in the other hand will produce the same effect; but if these be visually superposed, *i. e.*, if one be moved to and fro behind the other, also moving, then in the quadrangular space included within the intersection of the two tints will be seen a black line sometimes straight, and connecting the opposite angles of the quadrangle; at other times oval or round, or even square, according to the motions given to the two cardboard rods (Fig. 13.)

This appearance is visible even when the rods are several inches or a foot apart from each other, provided they are visually superposed. It is produced exactly as in the former case, and the black line is in fact the path of the intersecting point of the moving rods. As their motions vary, so does the course of this point change, and wherever it occurs, there is less eclipse of the black ground beyond than in the other parts, and consequently less light from that spot to the eye than from the other portions of the compound spectrum produced by the moving rods.

In this experiment the eye should be fixed, and the part looked at should be between the planes in which the rods are moved. The variation produced by using black rods, and looking at a white ground, will suggest itself. Those who find it difficult to observe the effect at first, will instantly be able to do so if the rod nearest the eye is black, or held so as to throw a deep shade: the line is then much more distinct; but the

explanation is not quite the same, though nearly so—it will suggest itself. Two bright pins or needles produce the effect very well in diffuse day light; and the line produced by the shadow of one on the other, and that belonging to the intersection, are easily distinguished and separated.

If, whilst a single bar is moved in one hand, several bars or a grate is moved in the other, then spectral lines, equal to the number of bars in the grate, are produced. If one grate is moved before another, then the lines are proportionably numerous; or if the distances are equal, and the velocity the same, so that many spectral lines may coincide in one, that one is so much the more strongly marked. If the bars used be serpentine or curved, the lines produced may be either straight or curved at pleasure, according as the positions and motions are arranged, so as to make the intersecting point travel in a straight, or a curved, or in any other line.

The cause of the curious appearance produced, when spoke or cog-wheels revolve before each other, already described, will now be easily understood; the spokes and cogs of the wheels produce precisely the same effect as the bars held in the hand, and the fixedness of the position of the spectrum depends upon the recurrence of the intersecting or hiding positions, exactly in the same place with equal wheels, provided the opposite motions of each be of equal velocity, and the eye be fixed.

When wheels were used in the little machine described (Fig. 4.), having equal but oblique teeth, and the obliquity in the same direction, the spectrum was also marked obliquely; but when the obliquity was in opposite directions, the spectrum was marked as with straight teeth.

When equal wheels were revolved with opposite motions, one rather faster than the other, the spectrum travelled slowly in the direction of the fastest wheel; when the difference in velocity between the two wheels was made greater, the spectrum travelled faster. These effects are the necessary consequence of the transference of the intersecting points already described, in the direction of the motion of the fastest wheel.

When one wheel contained more cogs than the other, as, for instance, twenty-four and twenty-two, then with equal motions

the spectrum was clear and distinct, but travelled in the direction of the wheel having the greatest number of teeth. When the other wheel was made to move so much faster as to bring an equal number of cogs before the eye or rather any one part of the eye, in the same time as the other, the spectrum became stationary again. The explanations of these variations will suggest themselves immediately the effects are witnessed.

When the motion of the wheels upon the machine is in the same direction, the velocities equal, and the eye placed in the prolongation of the axis of the wheels, no particular effect takes place. If it so happens that the cogs of one coincide with those of the other, the uniform tint belonging to one wheel only is produced. If they project by the side of each other, it is as if the cogs were larger, and the tint is therefore stronger. But when the velocities vary, the appearances are very curious; the spectrum then becomes altogether alternately light and dark, and the alternations succeed each other more rapidly as the velocities differ more from each other.

When wheels with radii are put upon the machine, it is easy to observe, in perfection, the optical appearance already referred to, as exhibited by carriage wheels, &c. (Fig. 2.) They should be looked at obliquely, so as to be visually superposed only in part; and provided the wheels are alike, and both revolving in the same direction with equal velocity, they immediately assume the form described, passing in curves from the axis of one wheel to the axis of the other, and much resembling in disposition those curves formed by iron filings between two opposite poles of a magnet.

If the wheels revolve in opposite directions, then the spectral lines, originating at each axis as a pole, have another disposition, and, instead of running the one set into the other, are disposed generally like the filings about two similar magnetic poles, as if a repulsion existed: not that the curves or the cause are the same, but the appearances are similar. A very little attention will shew that all these lines are the necessary consequence of the travelling of successive intersecting points; and any one of them may be followed out by experimenting with the two pasteboard rods already described, these being moved in the hand as if each were the spoke of a wheel.

All these effects may be simply exhibited by cutting out two equal pasteboard wheels without rims, passing a pin as an axis through each, spinning one upon a mahogany or dark table, and then spinning the other between the fingers over it, so that the two may be visually superposed. If the appearances are observed by a lamp or candle, the wheels should be so held to the light that the shadow of the upper may not fall upon the lower, otherwise the effects are complicated by similar sets of lines which appear upon the lower wheel, and are produced by the shadow of the upper. These are the same in form and disposition as the former, and are even more distinct; they should be viewed, not through the upper wheel, but directly upon the lower; their explanation has in part been given, and will be sufficiently evident.

The form which the appearance occasionally assumes when a carriage wheel is revolving before upright bars, is exceedingly well shewn by the little machine described (Fig. 4.), when mounted with a single wheel carrying several equal radii at equal distances. The bars of the grate should be equidistant, the intervals between them being about that between the extremities of two contiguous spokes of the wheel. The varied appearances produced by varying the motion of the wheel and grate, both in direction and velocity, will be better understood from a few easy experiments than from any description.

The lines which thus occur may any one of them be imitated by the two cardboard bars held and moved in the hand; the whole system may then be obtained at once if one of the independent wheels (Fig. 1.) be revolved by the pin between the fingers, and a single pasteboard bar (of equal width with the radii) passed once, not too rapidly, before it; by returning the bar the lines are seen a second time. Should the eye not readily catch the appearance, a black instead of a white single bar may be used, or a shadow be thrown by an opaque bar from a candle, or the sun, upon the revolving wheel; and then, to extend and follow out the forms, the bar should be moved to and fro slowly before the revolving wheel, to the extent of one half or the whole length of a radius, when it will immediately be seen, that all the lines produced, even when a grate is used, are merely the courses of so many points of in-

tersection between the radii of the wheel and the bars passing before or behind it.

A variation in the mode of observing many of these curious spectra, but which still further supports the explication given, is to cast the shadows of the revolving wheels, either by sun or candle-light, upon a screen, and observe their appearance. The way in which the cogs or radii of the wheels shut out more or less of a back-ground from the eye, as already described, will enable them, to an equal degree, to intercept light, which would otherwise fall upon a screen. When the two equal cog wheels are revolved so as to have the shadows cast upon a white screen, that shadow exhibits all the appearances and variations observed when the eye is looking by the wheels in shade at a white back-ground. The shadow is light where the wheels appear dark, for there the light has passed by the cogs; and dark where the wheels appear light, for there the cogs have intercepted most of the rays. The screen should be near to the wheels, that the shadow may be sharp; and it is convenient to have one wheel of rather smaller radius than the other, or else to place them obliquely to the sun for the purpose of distinguishing the shadow of each wheel, and shewing how beautifully the spectrum breaks out where they superpose. When the spoke-wheels are revolved they also cast a shadow, presenting either the appearance of fixed or moving radii according to the circumstances already described. When the two small spoke-wheels upon one pin are revolved in an oblique direction, their shadow exhibits very beautifully the lines often seen in the wheels of carriages.

During these experiments the attention cannot but be drawn to the observation of the figures produced by the shadow of one wheel upon the face of the other. These are frequently very beautiful, and combining as they often do with the designs produced, as already described, are occasionally more striking than any of the appearances yet spoken of. Mr. Wheatstone is, however, engaged in an inquiry of a much more general and important kind, which includes these effects, and which, I trust, he will soon give to the public.

Several of the effects with wheels already described, and

some new ones, may be obtained with great simplicity, by means of reflection, in a very striking manner. If a white cardboard wheel, with equal radii, be fixed upon a pin, and rotated between the fingers before a glass, so that the wheel and its reflected image may visually superpose in part, the fixed lines will be seen, like those of Fig. 2, passing in curves between the axis of the wheel and the reflected image. If the person gradually recede from the glass, but still look through the wheel in his hand at the reflected image, *i. e.*, still retain them superposed, which is best done by bringing the revolving wheel close to the eye, he will see the lines or radii of the reflected image gradually become straight, and when from three feet to any greater distance from the glass, will see the spectrum of the reflected image, having as many dark radii upon it as there are radii in the wheel he is revolving. Whatever the velocity, or however irregular the motion of the wheel, these lines are perfectly stationary. The explanation of the change of form and ultimate appearance of the whole, and of the number and fixed position of the lines, will be so evident when the experiment is made, in conjunction with what has been said, as to require no further statement here.

A very striking deception may be obtained in this way, by revolving a single cog wheel (Fig. 6.) between the fingers before the glass, when from twelve to fifteen or eighteen feet from it. It is easy to revolve the wheel before the face so that the eyes may see the glass through or between the cogs, and then the reflected image appears as if it were the image of a cog-wheel, having the same number of cogs, but perfectly still and every cog distinct; instead of being the image of one in such rapid motion, that by direct vision the cogs cannot be distinguished from each other, or their existence ascertained. The effect is very striking at night if a candle be placed just before the face, and near to it, but shaded by the wheel; in the reflection the wheel is then well illuminated, and the reflected face or shadow forms a good back-ground against which to observe the effect.

I have, perhaps, already rendered this paper longer than necessary, but the singularity of the appearances and the facility with which they may be observed, have induced me

to suppose that many persons would like to repeat the experiments, and must be my excuse for some further variations in the mode of experimenting.

A disc of cardboard, about two inches and a half in diameter, was cut into a wheel like Fig. 16.; another disc, rather larger, was cut into a similar wheel, and then the radii of one were twisted obliquely like the wings of a ventilator, and the radii of the other similarly set, but in the opposite direction: a small hole being made in the centre of each, a large pin was passed through that of the smaller wheel, and then a small piece of cork passed on to the pin to hold the wheel near the head, but free to turn; two or three beads were then added, the second wheel put on, and then a second piece of cork; the end of the pin was then stuck into a quill or a pencil, and thus was formed an apparatus very like a child's windmill, except that it had two sets of vanes, each revolving in opposite directions. On walking across a room towards a window, or a candle, with this little toy in the hand, or blowing at it slightly from the mouth, the lines were beautifully seen, being either stationary or moving, according to the relative velocity of the two wheels. This could be altered at pleasure by inclining the vanes more or less, or by blowing towards the centre of the wheels, or towards the edges when the larger hind wheel received more propulsive force.

Spinners or whirligigs formed of discs of cardboard stuck upon pins, and upon which radii either straight or curved, or other forms, had been drawn in bold lines with black ink, when spun upon a sheet of paper, and then looked at through the moving fingers or through equidistant bars of pasteboard moved before them, shew a great many of the effects.

Finally, a couple of open radial wheels (Fig. 1.) upon pins or wires, if revolved between the fingers in different positions and directions, shew a great many of these effects extremely well. Their shadows may be thrown upon each other, or upon the wall; one may be held near the eye, when it acts like a grate with parallel bars; and if one side of each wheel is black whilst the other is white, still greater variety may be obtained. They will be quite sufficient, when employed in a few experiments, to make, in this description, anything clear which I may have left obscure.

The curious appearance exhibited by the wheel animalcule has such a resemblance to some of those described in this paper, that they inevitably associate in the mind of a person who has witnessed both effects. This little insect has been well described by Mr. Baker * and others, and can only be viewed distinctly under a high magnifying power; it then presents an elongated sack-like form (Fig. 17.), either attached by the posterior part to the side of the vessel containing the water in which it exists, or else floating in the fluid. When the effect in question is observable, there is seen the appearance of two wheels, one on each side of the head; they seem formed of deep teeth or short radii, perhaps fourteen or fifteen in number; the form of these teeth is not sharp or well defined, but hazy at the edges; the interval between them is perhaps rather more than the width of the teeth; the teeth are not distinctly set on to a nave or axis, but appear sometimes even to melt away or attenuate at the part toward the centre, and sometimes appear, as independent portions, *i. e.* as much separated from the centre part or supposed place of attachment as from the neighbouring teeth.

These parts are never seen as wheels, except in motion; the animal is sometimes seen without them; the parts which produce the appearance being then either retracted and drawn inwards, or disposed in other forms, for the animal is of a very changeable nature. The motion of the wheels is continuous, as if they were spinning constantly in one direction upon their axis; the velocity is such as to carry the teeth rapidly before the eye, but is not enough to confound the impression of one tooth with that of its neighbours, and therefore they may be distinctly seen. Both wheels move usually in the *same direction;* and when the head of the animal is towards the observer, the direction is generally the same as that of the hands of a clock. Baker states, however, that he has seen them move in opposite directions, and also has seen the motion first discontinued, and then reversed, in the same wheel. The velocity is not always the same, but varies with the efforts of the animal to catch its food. Whatever the

* Baker on the Microscope, vol. ii. p. 266; see also Leeuwenhoek, Phil. Trans., Nos. 283, 295, 337; and Adams on the Microscope, p. 548.

mechanism of the parts, the result is, that currents are established in the water towards the head of the animal, which currents pass off outward from the edges of the apparent wheels; and little particles floating in the water may be seen to pass towards the head, and be suddenly thrown off at the edges of the wheels with considerable force.

So striking are the appearances of these animalcula, that men of much practice in microscopical observation are at this day convinced they do possess wheels, which actually revolve continuously in one direction. The struggle in Mr. Baker's mind between the evidence of his senses and his judgment, illustrates this point in so lively a manner, that I may be excused quoting his account of it:—'As I call these parts *wheels*, I also term the motion of them a rotation, because it has exactly the appearance of being such. But some gentlemen have imagined there may be a deception in the case, and that they do not really turn round, though indeed they seem to do so. The doubt of these gentlemen arises from the difficulty they find in conceiving how or in what manner a wheel or any other form, as part of a living animal, can possibly turn upon an axis supposed to be another part of the same living animal, since the wheel must be a part absolutely distinct and separate from the axis whereon it turns; and then say they, how can this living wheel be nourished, as there cannot be any vessels of communication between that and the part it goes round upon, and which it must be separate and distinct from? To this I can only answer that, place the object in whatever light or manner you please, when the wheels are fully protruded they never fail to shew all the visible marks imaginable of a regular turning round; which I think no less difficult to account for, if they do not really do so. Nay, in some positions you may, with your eye, follow the same cogs or teeth whilst they seem to make a complete revolution; for the other parts of the insect being very transparent, they are easily distinguished through it. As for the machinery, I shall only say, that no true judgment can be formed of the structure and parts of minute insects by imaginary comparisons between them and larger animals, to which they bear not the least similitude. However, as a man can move his arms or his legs

circularly as long and as often as he pleases by the articulation of a ball and socket, may not there possibly be some sort of articulation in this creature whereby its wheels or funnels are enabled to turn themselves quite round?

'It is certain all appearances are so much on this side the question, that I never met with any who did not, on seeing it, call it a *rotation;* though, from a difficulty concerning how it can be effected, some have imagined they might be deceived. M. Leeuwenhoek also declared them to be *wheels* that *turn round, vide Phil. Trans.,* No. 295. But I shall contend with nobody about this matter: it is very easy for me, I know, to be mistaken, and so far possible for others to be so too, that I am persuaded some have mistaken the *animal itself,* which perhaps they never saw; whilst, instead thereof, they have been examining one or other of the several *water-animalcules* that are furnished with an apparatus commonly called *wheels,* though they turn not round, but excite a current by the *mere vibration of fibrillæ* about their edges.'

Notwithstanding the evidence adduced by Mr. Baker, which, as I have said, is admitted by some at the present day, it must be evident, from a consideration of the nature of muscular force, and the condition of continuity under which all animals exist, that the rotation cannot really occur. The appearances are altogether so like some of those exhibited in the experiments already described, that I feel no doubt the wheels must be considered not as having any real existence, but merely as spectra, produced by parts too minute, or else having too great a velocity when in use by the animal to be themselves recognized. It is not meant that they are produced by toothed or radiated wheels; for that supposition would take for granted what has already been considered as impossible—continual revolution of one part of an animal whilst another part is fixed; but arrangements may be conceived, which are perfectly consistent with the usual animal organization, and yet competent to produce all the effects and appearances observed. Thus, if that part of the head of the animal were surrounded by fibrillæ, endowed each with muscular power, and projecting on all sides, so as to form a kind of wheel; and if these fibrils were successively moved in a tangental direction rapidly

the one way, and more slowly back again, it is evident that currents would be formed in the fluid, of the kind apparently required to bring food to the mouth of the animal; and it is also evident, that if the fibrils, either alone or grouped many together, had any power of affecting the sight, so as to be visible, they would be less visible at the part through which they were rapidly moving, than that through which they were slowly returning; and at that place, therefore, an interval would appear, which would seem to travel round the wheel, in consequence of the successive action of the fibrils. But, if instead of the whole group of fibrils acting in succession as one series, they were to be divided by the will or powers of the animal into fifteen or sixteen groups, the action being in every other respect the same, then there would be the appearance of fifteen or sixteen dark spaces, and as many light ones disposed as a wheel; and these would continue to travel round in one direction, so long as the animal continued the alternate action of the fibrils. This may be illustrated by supposing Fig. 14 to represent a fixed circular brush, with long hairs, and the little dots to be the sections of so many wires, forming the arms of a frame which, when turned round, shall carry the hairs of the brush forward a little, and then, letting them go, allow them to return quickly to their first position. If this frame be turned continually round, it would cause the brush, when looked at from a distance, to appear as a revolving toothed wheel, although in reality it had no circular motion. Now, what is performed here by the wire-arms at the outer extremity of the hairs, and the natural elasticity of the latter, may, in the wheel animalcula, be effected at the roots of the fibrillæ by muscular power; and in this or some similar way the animal may have the power of urging the current necessary to supply food, and, at the same time, producing the spectrum of a continually revolving wheel, or even the more complicated forms discovered by Leeuwenhoek (Fig. 15), without requiring any powers beyond those which are within the understood laws of Nature, and known to exist in the animal structure.

Royal Institution, Dec. 10, 1830.

DESCRIPTION OF A MODE OF ERECTING LIGHT VAULTS OVER CHURCHES AND SIMILAR SPACES.

By M. de LASSAUX.

(Communicated by Professor Whewell, of Cambridge.)

M. DE LASSAUX, of Coblentz, architect to the King of Prussia, is the discoverer and restorer of this process, and gives the following account of his investigations.

He had arrived in various ways at the conviction that what are called the *gothic* and *ante-gothic* styles of architecture (the pointed arch and round arch styles), are not only the most appropriate for churches, but also the cheapest. He had attempted to discover some easy means of erecting stone vaults in such cases, thinking them highly desirable, whenever the funds at the builder's disposal will permit them. Vaults which are at the same time wide and light, belong incontestably to the boldest and most ingenious of human inventions: they are peculiarly suitable in religious edifices; they are secure from the devastations of fire; and, when introduced in public buildings, they correspond to the spirit of the celebrated decree of the republic of Florence, enacted in the year 1294, that all which is executed for the commonwealth should bear the lofty impress of the common will.

M. de Lassaux was also aware, that at Vienna, at the present time, very wide and flat domes are erected almost entirely *free-handed* (*i. e.* without centering), and that in the neighbourhood of that city, very flat ovens and wide mantlepieces * are constructed almost in the same manner, and with the help only of a few slight posts or poles. He endeavoured, therefore, to discover some mode of facilitating, by similar means, the execution of wide vaults in churches.

His attempts for some time led him to nothing bearing on

* Brunelleschi constructed the cupola of the church of Santa Maria del Fiore at Florence, without a centre.—J. S. 'At Bassora, where they have no timber but wood of the date tree, which is like a cabbage-stalk, they make arches without any frame. The mason, with a nail and a bit of string, describes a semicircle on the ground, lays his bricks, fastened together with a gypsum cement, on the lines thus traced, and having thus formed his arch, except the crown brick, it is carefully raised, and in two parts placed on the walls. They proceed thus till the whole arch is finished; this part is only half a brick thick, but it serves to turn a stronger arch over it.'—*Eton's Survey of the Turkish Empire.*—J. S.

the point in question, except the usual methods of laying down the vaulting lines, and some historical notices, which will be mentioned subsequently. In the old church vaults which are extant, there was little to be seen, as they are in almost all cases covered with a coat of mortar or plaster.

About six years ago, however, happening to go into the space above the vault of the fine church at Ahrweiler, he observed in the *extrados* of the vaults so remarkable a dissimilarity in their height and curvature, that the thought in an instant struck him, that it was impossible these could have been built upon a regular centering. On a closer examination, it appeared impossible to entertain any further doubt on this subject; and in various places, where the rubble work had been laid bare, the whole mode and manner was exhibited of the process which had been employed, and the opinion thus formed was more and more confirmed by subsequent examination of a number of other vaults.

The whole mystery resides in this, that these pointed-arch cross-vaultings consist of separate, generally horizontal, courses; of which courses each has a small concavity, and consequently forms a small vault by itself, as soon as its terminating points have their due counterpoise. Now, as the bed-faces of the individual courses of a regular pointed arch, that is, of one which is described about an equilateral triangle, recede very slowly from the horizontal line, and even at the summit make with it an angle of only 60°, the adhesion of each individual vaulting-stone of moderate dimensions, such as brick and similar stones generally have, to the layer of mortar, is sufficient to prevent the sliding of the stone before the termination of the course; and hence there is no difficulty in executing each individual course *free-handed* and independently, and in locking it against its counterpoise. Against each course already locked, and consequently fixed and immoveable, we may begin a new one, and so continue to the final termination of the whole vault. All that is required, therefore, is a solid resistance for the terminating points of each course. Now, such a resistance may be supplied not only by solid obstacles, as the external walls, but equally well by the reaction of a contiguous course. Hence, if the groining-ribs or diagonal lines of the

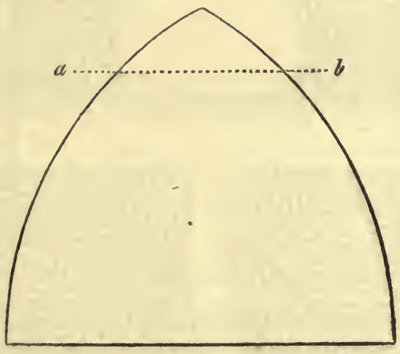
separate compartments are properly supported beneath, the courses which rest on the same point perpetually keep each other in equilibrium, and consequently no further contrivance is needed than to execute the whole courses in the individual horizontal planes at the same time, or nearly at the same time; that is, to carry the courses all the way round; consequently the process in such cross-vaulting is the same fundamentally as in domes, when each *course* is locked by itself as a ring, one ring is gradually laid upon another, and thus finally the dome itself is locked, except that in these domes the upper courses have steeper bed surfaces, and consequently the stones will no longer remain in their places without the application of other auxiliary means, but would slip down as soon as they were laid, if not prevented in some other way. This is now done in Vienna in a very simple manner, by means of some strong ends of rope, which are fastened above, and somewhat backwards, from the course to be vaulted, and hang down like plummets, being loaded below by some stones tied to the rope. As soon as a stone is laid, and by a moderate blow with the hammer pressed against the preceding stone, one of these ropes is brought over the stone, and the pressure produced by the weight of the appended stone, combined with the adhesion of the mortar, is sufficient to hold the stone till it is sufficiently supported by the contact of the next stone; and this in its turn is prevented from slipping down by the pressure of the cord upon it.

We very often find, however, over ancient churches, cross-vaults, where the diagonal lines consist of semicircles, and consequently the transverse and longitudinal lines, and also the lines of subdivision in compound vaults, form somewhat depressed pointed arches, of which the radii are usually three-fourths, but sometimes only two-thirds of the diameter;—here the same difficulty occurs in the upper courses, and probably has been met by the same or similar methods. We sometimes observe also a *back-vaulting* of those courses, of which we shall have to speak again in describing the locking of the vault.

The only difference between these old cross vaults and the usual ones, consists in this; that the latter are formed by the

motion of two horizontal straight lines over four arches set opposite two and two, and consequently all the horizontal lines in the vault are straight lines; and each course must be constructed as a vault in an upright position, so as to support itself freely, which requires a very careful shaping of all the individual stones, and a considerable thickness; whereas in the ancient vaults no horizontal line whatever occurs, and each course is laid in a line somewhat curved outwards; and consequently incomparably less thickness and less labour is requisite, and yet a far stronger arch is produced. A single cross vault of the first kind, according to the profile Fig. 1, cut

Fig. 1.



horizontally, in the line *a b*, forms a rectangular composition of straight lines, as Fig. 2, while the other, according to Fig. 3, consists of curves, which push with their extremities *e e* against the outer walls; and, resting with their other extremities at *d d* in the diagonal lines upon two centerings which cross each other, keep each other mutually in equilibrium, till by closing up, or locking the vault, complete diagonal arches are obtained, which now support themselves, and enable us to remove both the centerings.

The way in which the separate divisions are closed up in the old vaults is represented in Fig. 4, and the last courses are generally *back-vaulted;* that is, the joints are flatter and not so steep as those which the regular joint-section requires, and as the above derivation of the forms of the stones would produce. At the same time, the two sides of the pointed arch

Fig. 2.

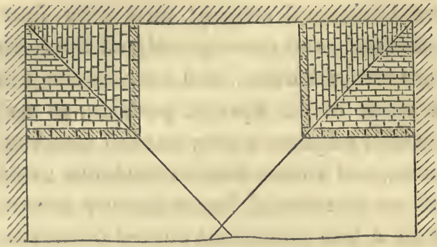


Fig. 3.

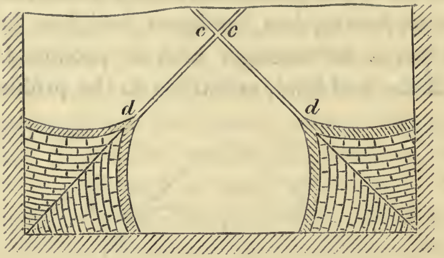
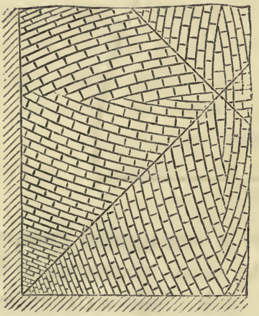


Fig. 4.



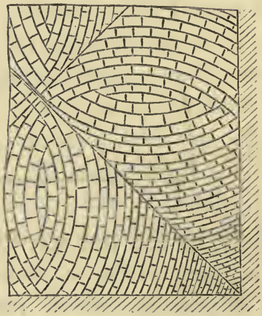


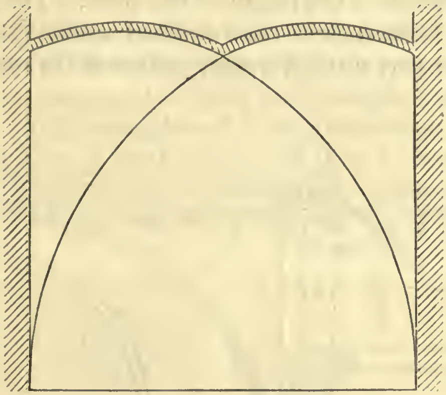
Fig. 5.

unite in a point, as in a simple-pointed arch, at the ribs or groins only, and not in the intermediate spaces. In the latter portions they form rather a very acute ellipse; and the *wall-scutcheon* * also has this form not unfrequently, indeed almost universally, where there is not introduced in this part some ornamented band or moulding. Hence the vertex of these intermediate compartments is higher than the vertex of the diagonal ribs, and forms a flat arch from their point to the wall, as is manifest from the section in Fig. 6.

* The portion of the wall, bounded by the arch, resembling an inverted escutcheon.

But again, the separate courses are not always horizontal; they often ascend, according to Fig. 5, from the diagonal ribs by a tolerably steep inclination towards the wall, sometimes even at an angle of 45°. This, probably, was done for the sake of concentrating more completely the push from the latter upon the centerings of the former, and then changing it into a nearly perpendicular pressure; perhaps also for the sake of giving a greater concavity, and, consequently, a greater strength

Fig. 6.

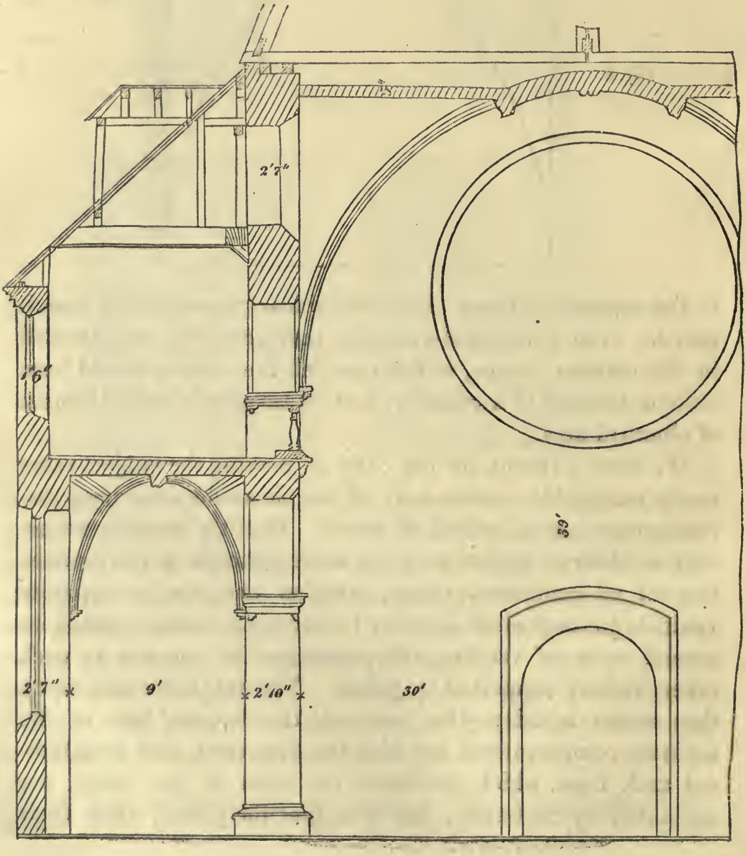


to the separate courses. Even rectilinear cross-vaulting, and, it may be, even cylindrical-vaulting, may probably be executed in this manner; since, in this case, all the courses would form oblique sections of a cylinder, and consequently would consist of elliptical arcs.

We have hitherto, for the sake of making the subject more easily intelligible, spoken only of simple vaults covering separate square spaces, walled all round. But it is scarcely necessary to observe, that exactly the same principle is the foundation of all compound vaults, whether one row of separate vaults is juxta-posited, in order to cover an oblong space; or several rows of vaulting compartments lie opposite to each other, and are supported by pillars. The only difference which then occurs is this:—that not only the diagonal lines of the separate compartments, but also the transverse and longitudinal arch lines, which determine the form of the vaults, are supported by centering; and also that the pillars, when their

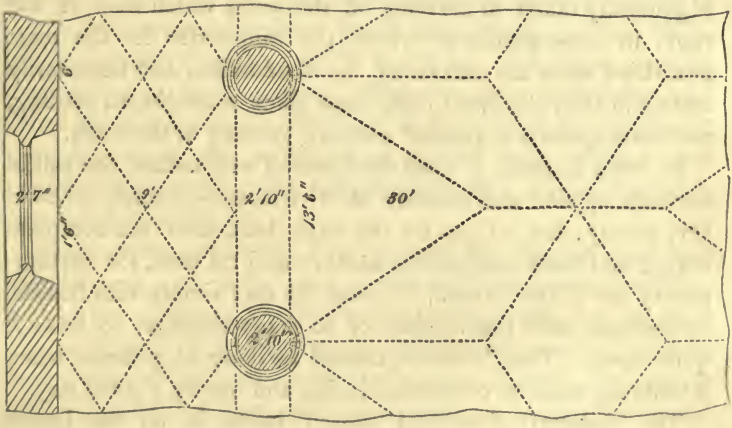
distances from each other, or from the outer walls, are unequal, as is the case in almost all churches, must have a certain stability before they are vaulted over, since the push of the surrounding vaults is unequal, and, consequently, they no longer hold each other in equilibrium; their push not being in this case resolvable into a simple perpendicular pressure upon the pillars. This, then, is the use of that massive wall which is carried by the arches, and which binds the pillars together in the direction of the length of the nave. This wall finds a resistance to its ends in outer walls of suitable strength; and at its upper part sustains a great portion of the roof; and con-

Fig. 7.



sequently, by means of its own weight, in combination with that of the roof, exercises a powerful pressure upon the pillars, which is more than sufficient to give them the stability requisite to enable them to counteract the unequal push of light vaults. The profile, Fig. 7, of the Church of the Jesuits in this place (built in 1615) will make this clear, since here we have the still more unfavourable case, in which the vaults are not only very unequal, but their points of incidence are not opposite each other, being at different heights; and consequently the whole push of the vault (which is not pointed, but semicircular) operates upon the pillar, which has no support on the other side; nor are the outer walls, though only two feet seven inches thick, strengthened by any buttresses; while the side windows have a width of only six feet for the intermediate piers: perhaps one of the boldest constructions of a vault which is known. See the plan of the vaulting, Fig. 8.

Fig. 8.



In reality, indeed, the push of such thin vaults is not anything which we need fear, since, according to the formulæ of Bondelet, or even those of Belidor, piers are sufficient for them which have a smaller thickness than that which is usual in the outer walls of high and wide buildings. Hence the *ribs* and the filling in behind of the spring of the vault arches, supply all the advantages which Bondelet, with truth, attributes to vaults, of which the flanks are filled in behind, and which

are thicker than those at the summit. 'Voutes extradossées moitié de niveau et moitié d'inégale épaisseur.'—Art. de Batir, vol. iii., p. 328 to 332, and p. 380 to the end of the vol. The author knows several old churches, with semicircular cross or domical vaulting, where the side-walls have declined considerably from the perpendicular; and where the vaults, having separated in the middle, the chinks have been at various times again filled in; and thus the semicircle has gradually become a depressed arch. If it had, in these cases, been the arch which pushed the walls asunder, it must necessarily have fallen in as soon as the cracks began to *gape;* that is, when their edges no longer touched each other. That this did not happen, gives the most convincing proof that no push at all had taken place; but that rather each half of the vault was detached, and hung to the wall, and that its yielding was produced by other causes. These lie often in a deficient construction of the roof, but most commonly in the small care which is generally taken in carrying off the water which falls on the roof; in consequence of which, the rain-water for the most part drips upon the outside of the foundation, and necessarily causes in this, the most dangerous place, a continued settling, and consequently a gradual outward yielding of the walls.

In order to make a small thickness of wall suffice, the skilful ancients applied also another effective means—namely, to build very slowly, and to put on the vault late, after the complete drying and hardening of the mortar; and till then, for the temporary use of the church, to cover the roof simply with boards, or perhaps, after the manner of so many basilicæ, to leave it quite open. This, however, cannot be done at present, when a building must be completed inside and out by a fixed day.

The material of ancient church vaults is, on the Lower Rhine, everywhere, the well-known tuf, which is manufactured as Trass, modelled to the size of common tiles, and three or four inches thick. On the Upper Rhine, beginning from about Bingen, it is brick of small size: the thickness of the vaults varies from four to eight inches: the execution is often very slovenly, and different in almost every vault; that is, the concavity is sometimes very flat, sometimes very strong, sometimes again uncommonly neat and resembling an assemblage

of eggshells. In the vaulting of the Minster at Ulm *, the tile materials are known to have been mixed up with chopped straw, which, in the burning, naturally was reduced to ashes, and thus gave to the stone a degree of porosity, and consequently of lightness.

The author found the same thing in the church at Kirchberg, near Zimmern; but he observed that here, in an experiment made with different mixtures, tiles of this kind lose very much in strength, which, for such vaults, is far more injurious than the small excess of weight of common solid stone would be; even the latter does not amount to half the weight of a common *grouting* (filling-in of the inside of a wall); and, consequently, operates rather advantageously, than prejudicially, to the stability of a substantial building.

We find this kind of vaulting applied also in domes; in which case the circle is divided by *ribs* into compartments, and compounded of several flat rounds. Among others, the author found in an old tower at Andernach, a domed vault, of which the horizontal section forms, not a circle, but four shell-shaped pieces of circles, which rest upon two edges, cutting each other at right angles. When, in later times, the ribs were multipled, and a network of various forms was extended over the vault, the intermediate spaces were smaller, and could consequently, with still more facility, be vaulted with *free-handed* vaulting.

The advantage of this kind of vaulting without centering consists, not only in the very considerable saving of boarding, and of the greatest part of the centering arches, but it gives also a firmer vault; since the settling takes place gradually before the usual closing of the vault: indeed, the author almost doubts, whether such thin vaults could be constructed at all upon a boarded centering. Except this is supported by scaffolding to an immoderate degree, the mere motion of the labourers, in the course of the vaulting, must cause a perpetual shaking, and, consequently, separations in the vault after it is begun; and even when the vault is brought to its closing, and it is wished to loosen the centering, which is so extremely

* Haffner, Description of the Minster at Ulm, p. 11.

advantageous for the uniform closing of all vaults, the inevitable consequence is, bellying and cracking. If, on the other hand, we wish to leave the centering standing till the complete drying of the vault, the wasting of the mortar would cause all the joints to open and crack. But the network formed by the mortar in all the joints, gives to a thin vault of heavy stone a peculiar strength; as the author very clearly ascertained by an experiment for the purpose. There is in this place a kind of stone, which is used very advantageously for the lining of walls, the pannels of ceilings, and the construction of chimneys. It is a conglomerate of loose pumiceous sand, cemented into a coherent mass by a loamy earth; and is naturally so tender, that it may be rubbed to pieces in the hand; and, indeed, has hardly more consistence than a swallow's nest. This mass lies in layers some feet under the surface, in the country between Engers and Bendorf: it is worked in stages, cut into loaves of 13 in. long, 6 in. broad, and 4 or 5 in. thick, and dried in the air. With these stones set on the long narrow side, he ordered a very flat vault (31 feet span, and $4\frac{1}{4}$ feet spring, and about 4 feet broad) to be thrown across between two old walls. Its thickness was thus only 6 inches, or the 124th part of the diameter of the semicircle, of which the arch was part; consequently, the stones suffered a pressure equal to that right upon each stone of a semicircular vault of 62 feet diameter, and therefore far greater than that which the stones could have borne on the ground; and yet this arch, although it could be put in strong vibration with the hand, had so much strength that a man could walk over it.

That the old vaults were built *free-handed,* and not upon a boarded centering, no one can doubt. Who would have given himself the trouble, so disproportionate to its object, of making such a boarding vaulted according to each arch of the centering, when he might obtain the same end with one which was quite common? Besides, the unequal convexity in all such old vaults shews that no gage or model was ever applied, but the observation of the proper form was left to the choice and practice of the mason. We often see, as has already been said, a strong convexity pass into a flat one, or reversely; when probably it had suddenly struck the mason, that he was

vaulting too round or too flat, and when he set about correcting his mistake too suddenly.

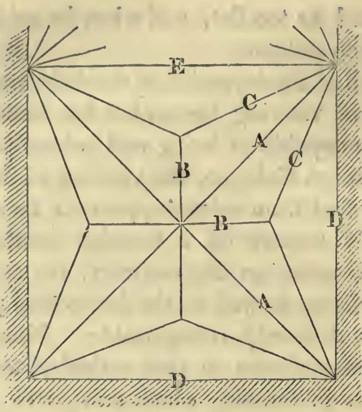
As to the epoch of the invention of this bold and uncommonly ingenious mode of vaulting, the author has hitherto discovered nothing exact or capable of being well substantiated.

In the cathedral at Cologne, the vaulting of the choir, so far as can be discovered from below, appears to be still rectilinear, and consequently vaulted on a boarded centering. In the north aisle of the nave, on the contrary, the curvature of the intermediate surfaces, as well as the horizontal position of the courses, is very distinctly recognizable. Now, as we may presume that the workmen in this cathedral were acquainted with the practices in building which existed in their time, we may place the invention of this kind of vaulting between the completion of the choir and that of this side-aisle; and, consequently, according to Boisserée, between 1322 and the beginning of the sixteenth century.

In books, the author, as has already been said, has been able to discover nothing concerning the practical art of building. There is, however, in De l'Orme (*Œuvres de Philibert de l'Orme, Rouen*, 1648; the first edition appeared in 1568, during his lifetime; he died 1570, or, according to others, 1577), in the 8th chapter of the 4th book, a passage historically very remarkable. These old church vaults are there called 'voutes modernes et à la mode Française, que les maîtres maçons ont accoustumé de faire aux églises et logis des grands seigneurs.' He says further—' Ces façons de voutes ont esté trouvées fort belles, et s'en void de bien exécutées et mises en œuvre en divers lieux de ce Royaume et signamment en ceste ville de Paris, comme aussi en plusieurs autres. Aujourd'huy ceux qui ont quelque cognoissance de lavraye architecture ne suivent plus ceste façon de voûte, appelée entre les ouvriers la mode Françoise; laquelle veritablement je ne veux depriser, ains plutost confesser que l'on y a faict et pratiquer de fort bons traicts et difficiles.'

The separate ribs have here all particular names: thus in Fig. 9, which occurs in De l'Orme, the ribs A are called *Croissée d'ogives*; B, *Liernes*; C, *Tiercerons ou tiercerets*; D, *Formerets*, when they lie against the wall and have only a half

Fig. 9.



profile; but *arcs doubleaux* when, as at E, they divide two compartments of vaulting from one another, and thus acquire a stronger profile. He then gives a drawing, with an explanation for laying down these different lines, and concludes very naïvely—

'Si quelques-uns desirent en scavoir davantage pour le pratiquer, faut qu'ils s'addressent aux architectes ou maistres maçons qui l'entendent. Car il est mal-aisé de le pouvoir mieux expliquer, que par œuvre et effect, c'est à dire en demonstrant au doit et à l'œil, comme les pierres se doivent trasser et assembler.' Subsequently, in the 9th chapter, De l'Orme gives a design for a simple vault over a church; recommends the laying the lines of the ribs exactly in the circle, that no chink (*aucun jouet*) may occur; the not driving any large wedge into the joints of the ribs; the vaulting the pannels of the vaulting (pendentifs) with brick or small stones; the laying the courses horizontally and according to the proper section of the joints; also the putting little thin mortar in the joints; and insures the durability of such vaults in the words—'telles voûtes faîctes ainsi dureront long temps.'

In the 10th chapter he adds a design out of his *Nouvelles Inventions de Charpenterie*, published in 1561, in order to shew that similar vaults may be constructed with wooden ribs and hanging keystones; but he himself is of opinion that it is better to execute these in hewn stone. He then speaks of the

various kind of ornaments, by means of hanging keystones, which occur so commonly in similar vaults, and concludes with the following remarkable words—

'Les ouvriers ne font pas seulement une clef suspendue au droict de le *croissee d'ogives*, mais aussi plusieurs, quand ils veulent rendre plus riches leurs voutes, comme aux clefs on s'assemblent les *tiercerons* et *liernes*, et lieux ou ils ont mis quelque fois des *rampants*, qui vont d'une branche à une autre, et tombent sur les clefs suspenduës, les unes estant circulaires, les autres en façon de *soufflet*, avec de *guimberges*, *mouchettes*, *claire-voyes*, *feuillages*, *crestes de choux*, et plusieurs bestiaux et animaux: qui estoient trouvez fort beaux du temps qu'on faisoit telles sortes de voutes, pour lors appellés des ouvriers (ainsi que nous avons dict) voutes à la mode Française. Et jaçoit qu'aujourd'huy l'on ne s'en ayde gueres, et qu'elles soient bien peu en usage si est-ce qu'elles sont tres difficiles, signamment quand on les accompagne de pendentifs de pierre de taille. Qui ne sont autre chose ainsi que nous disions cydevant que la maçonnerie qu'on met par dessus les branches. Comme vous le pouvez cognoistre et remarquer en le figure ensuyvant, au lieu de A B. Quand les dicts pendentifs sont faicts de brique ou petites pierres de maçonnerie, ils ne sont tant difficiles: mais les faisant de pierre de taille qui touche justement sur les branches, les pieces s'y trouvent des gauchées, biaises, d'estrange figure, selon l'œuvre qu'on faict, qui se monstre fort belle et tres difficile a conduire *.'

* Similar wooden vaults, that is, vault-shaped coverings extended between wooden ribs, or filled up with hurdle-work, and subsequently ornamented, are, in the district of Coblentz, frequent in small village churches, of which some are very old; on a larger scale, and decorated with a net work of ribs, they occur in the Church of the Jesuits at Münstereifel, built between 1612 and 1658. This kind of make-believe architecture is therefore older than De l'Orme thinks, but nevertheless has recently been produced as an ingenious and novel invention.

Similar apparent vaults, without visible and ornamented groin-edges, would have at least the modern merit of cheapness. With moulded ribs, on the contrary, they become very dear, when they are executed in a good and durable manner, and consequently with careful labour, and of good wood properly dry. While the author was unacquainted with the ancient mode of vaulting, and thought, like so many other persons, that stone vaulting required immoderate expense, he had proposed a ceiling of this kind, that is without ribs, for a church, which, fortunately, was not executed. In fact, the idea of a simplified construction had misled him. As, however, such a process might in other places be advantageous, a short description may not be superfluous.

It is known that all horizontal lines in cylindrical and cross-vaulting are

In the 11th chapter, De l'Orme speaks at length of domes over quadrangular spaces as a new invention (invention fort ingenieuse pour couper un globe quarrement), and boasts, with justice, that it is cheaper, because it requires no ribs, and is easier to execute because the section of the joints is simpler; and, finally, he describes several kinds of this vaulting, according to the quadrilateral circle, triangle, and oblique line.

Now, an invention which, in the time of De l'Orme, was still called à la moderne, cannot easily be much older, and, compared with the statement in Boisserée's great work on the cathedral at Cologne, p. 16, according to which the north aisle was vaulted after the year 1500, is, perhaps, to be placed only in the beginning of the fourteenth century.

According to De l'Orme, Mathurin Fousse, Derand, and De la Rue also wrote on vaults: M. de Lassaux is not acquainted with their works; but it would appear that they limit themselves to stone-cutting, to which the French, as we know, began at that time to attribute perhaps too great a weight. Roland de Valois also, who certainly was acquainted with the above work and used it, repeats, in his 'Dictionnaire d'Architecture,' only the names of the separate ribs, without adding anything with respect to the practical execution of the vaults themselves. Rondalet, in his excellent *Art de batir*, limits himself in the same manner to the rules for the section of the joints of the ribs.

The author has also endeavoured in vain to obtain oral information. One mason, indeed, remembered to have heard from his grandfather, that in *keying* these vaults it was very necessary to avoid driving in the key-stone too hard, because if that were done the sides would rise and belly. The last vault of

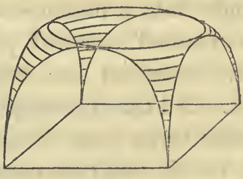
straight lines, and the vertical or oblique lines only are curved. Every light ceiling is composed of boarding on a frame, on which the former is nailed in a perpendicular direction. Now, as in general a surface of boarding is not capable of being bent into a sharp curvature, the boards must be fastened in a horizontal position on a frame or skeleton formed of curves. If, on the other hand, we would have another flexible material for the covering of the frame, for instance hoops, which are in all respects preferable, the frame may be of straight wood, and consequently may be prepared at a much smaller expense; and the curved surfaces may be, by means of hoops nailed on perpendicularly or obliquely, of such a kind as may best be laid on, and ornamented, without further preparation, with hair mortar (which ought to be mixed with hog's bristles), and consequently prove a considerable saving in the boarding and reeding.

this kind, so far as the author is aware, exists in the church of Niederbriessig on the Rhine, a building of the year 1718. Another opportunity must be taken of describing how the author was so fortunate as to be able to vault two churches of his own building with solid stone—how what has here been said was applied in practice—and how so many other difficulties of all kinds which occurred were happily obviated. In the mean time a hasty statement of the dimensions may serve to shew the applicability of this mode of vaulting in all cases. The greater of these churches, which is entirely in the pointed-arch style, is, in the clear, 57 feet wide and 48 feet high, is divided by two rows of pillars, 17 feet from axis to axis, 3 feet thick, and 25 feet high, into a middle-nave of 30 feet clear, and two aisles; and is vaulted over with pointed cross-vaulting of the sandstone above described, and with intermediate ribs of only 6 inches thick. All the centerings under this vault are of wood from 4 inches to at most 5 inches scantling. The greatest of these did not in any case consist of entire arches, but only of segmental arches fastened together. The outer walls, executed in irregular broken stones, are 3 feet thick, 50 feet high, and are strengthened by buttresses of the same thickness, separate 17 feet from one another, projecting 4 feet, and having a height of 30 feet. The tower, $21\frac{1}{2}$ feet square, and in the wall-work 110 feet high, carries an octagonal spire 124 feet high, built of wood and covered with slates.

In the small church, in the round arch style, the buttresses are in the interior, and are rounded into niches. The interval of these is also 17 feet, and the breadth of the middle-nave, between the pillars (which are $2\frac{1}{2}$ feet thick and 22 feet high) is $22\frac{1}{2}$ feet; the whole clear breadth between the buttresses is 42 feet. The pillars and walls are bound together both in a longitudinal and transverse direction by semicircular ribs, and the compartments thus formed are covered with cupola ceilings 6 feet thick, which were vaulted *freehanded*, without any mechanical assistance.

At a later period, on the occasion of the vaulting of the choir in another church, with a similar cupola of 24 feet square, there occurred to the author a simple method of preserving the complete accuracy of the form of the cupola. He caused a very light pole, of the length of half the diagonal, and conse-

quently of the radius of the sphere of the cupola, to be fastened to its middle point by a double hinge, in such a manner that it could be carried round in all directions, and consequently could touch each point of the interior surface of the dome of the assumed diameter*. The four *gussets* were then vaulted, one after the other, in horizontal courses, and each stone pushed forward so far that it could be touched with the end of the moveable pole. In this way a circular ring was obtained of the breadth of the square, on which were vaulted in a second, third, and so forth, to the completion of the cupola. From time to time, by the application of the pole, the complete regularity of the spherical form was secured and properly preserved.



M. de Lassaux adds, that if he should hereafter have the good fortune to erect other vaulted churches, he would limit himself rigorously to the round-arch style. This style possesses a superiority in the simplicity and completeness of its forms. Moreover its spherical vaults, in consequence of the saving of the ribs and their centerings, are considerably cheaper; and we can, therefore, with given funds, make our churches larger, which, in consequence of the universally limited means, are always built on too small a scale. This style is, moreover, the more agreeable, because less that is fine in it has come down to us; while, on the contrary, our buildings in the pointed style, compared with those of the ancients, always appear more or less as a miserable subterfuge.

* Eton, in his 'Survey of the Turkish Empire,' says, 'I have seen *cupolas* of a considerable size built without any kind of timber support. They fix firmly in the middle a post about the height of the perpendicular wall, more or less, as the cupola is to be a larger or a smaller portion of a sphere; to the top of this is fastened a strong pole, so as to move in all directions, and the end of it describes the inner parts of the cupola.'

ACCOUNT OF A NEW COMET OBSERVED BY M. DABADIE,

Professor of Mathematics at the College of Port Louis.

[Communicated by Sir ALEXANDER JOHNSTON from Sir CHARLES COLVILLE, Governor, &c. &c.]

Port Louis, 25th August, 1830.

[MEMORANDUM.]

INCLOSED is a statement of the observations and calculations of our professor of mathematics at the Royal College of Port Louis, showing the elements by which the orbit of the comet of April, 1830, may be ascertained. It is probable that his Excellency might be desirous of transmitting this paper to Sir Alexander Johnston, as it will be valuable for connecting the series of distances which may have been observed in New South Wales and the Cape of Good Hope, on the visit of this stranger; for it does not appear that any notice exists of this comet having before appeared, at least none of the books we possess designate the same extent or inclination of the orbit.

Ever yours, &c.

(*Signed*) C. TELFAIR.

Note.—The original observations are preserved in the College.

Comet of the 11*th of April,* 1830.

At seven o'clock, P. M. on the 16th of March, Mlle. ——— asked me the name of a round nebulosity which she perceived between the constellations of the Camelion and the greater cloud. I was immediately convinced that it was a comet. By the next day the nucleus had advanced nearly five degrees towards the north, and it continued that direction with

a diminishing apparent velocity till it reached the eastern wing of the Swan, where it disappeared toward the end of May. The length of its tail never exceeded five degrees.

Not having an observatory in which to fix the instruments that would have given at once the right ascension and declination, I made a great many observations of its distance from different stars during its progress. The following are those which I have used in the calculation of the six elements of the comet.

March 19th, at 8 h. 45 m. 50 s. of true time, at Port Louis, the distance of the comet from Canopus was 36° 11′; at 9 h. 2 m. its distance from α Centauri was 34° 50′.

April 1st, at 16 h. 48 m. its distance from α Centauri was 69° 34′; at 17 h. 11 m. its distance from α Aquilæ was 43° 50′.

April 15th, at 16 h. 25 m. 50 s. its distance from α Aquilæ was 21° 50′; at 16 h. 40 m. 50 s. its distance from α Centauri was 97° 39′ 30″.

Longitude of the ascending node.	Inclination of the orbit.	Place of the perihelion.	Perihelion distance, that of the sun being 1.	Passage of the perihelion in mean time of Port Louis.	Motion.
S. D. M. 7 18 31	D. M. 49 46	S. D. M. 7 28 13	0·897.	April 11. 21 h.	Direct.

ON THE PERMANENCE OF THE MAGNETISM IN STEEL BARS.

By S. H. CHRISTIE, Esq., M.A., F.R.S., &c.

IN the course of some magnetical experiments, made in China, on the deviations of a magnetised needle due to the action of an iron shell, Captain Wilson found, that when the magnetism of the needle was disturbed, by applying the pole of a magnet to the similar pole of the needle, considerable changes were produced in its deviations; and, on Captain Wilson's return to England, the experiments were repeated and extended, and the results classed by Mr. Barlow. These* were considered as quite decisive against a law which I had several years before stated, that the deviations of a magnetised needle, due to the action of iron, followed†. I was, therefore, induced to repeat the experiments; and having determined the situations of the magnetic centres, the intensity of the magnetism in different points, and the points of greatest intensity, in needles having their magnetism unequally distributed in their two branches, that is, in which the symmetrical distribution of magnetism had been disturbed, which had been omitted to be done in the former experiments, I showed that the results of those experiments were not only consistent with the views which I had previously taken, but were such as I had anticipated from the law referred to ‡.

While I was engaged in making these experiments, it became a question with me, how far the deviations of a needle, having its magnetism unequally distributed, observed at the beginning of any set of observations, could be compared with those observed towards the end of the same set, in consequence of a tendency which might exist in the magnetism of the needle to return to a state of symmetrical distribution. My first object was, therefore, to determine whether any change that could influence the results took place in the time occupied in making a set of experiments, an interval of 3 or 4 hours.

* Phil. Trans. 1827.

† Camb. Phil. Trans. 1820; Phil. Trans. 1825.

‡ Phil. Trans. 1828.

Finding that not only no change took place in this time, but that in much longer intervals scarcely any appreciable change could be observed, I was led to make observations with the view of determining whether, during a very long interval of time, the magnetism unequally distributed in a steel bar would return to a state of symmetrical distribution.

For this purpose I made use of four bars of steel, which had been somewhat softened, in order that they might be reduced to the same thickness by filing, and which I had not afterwards hardened, as I considered that with such bars changes would be most likely to occur. These, for the sake of distinction, I marked I., II., III., IV. Each of them is 0·15 inch in breadth, and 0·1 inch in thickness; I. and II. are 8·91 inches, and III. and IV. 5·94 inches in length. I. and II. were placed by the side of each other, and strongly and carefully magnetised by double touch, by means of two twelve-inch bar magnets; and the same was done to III. and IV. As usual, the south pole of each was indicated by a mark on that end of the bar; and in order to avoid any ambiguity which might arise from a change of position in the bars, when I determined their magnetic centres and poles, I, in all cases, made use of the terms, marked and unmarked ends, instead of south and north poles. The magnetism of I. and III. was disturbed by passing the marked end of a twelve-inch bar magnet from their centres to their marked ends twice; and that of II. and IV. was disturbed by a similar operation with the unmarked end of the bar magnet, from their centres to their unmarked ends.

In order to determine the position of the magnetic centre in each bar, I placed it on a rectangular wooden scale, parallel to, and equally distant from, the sides, the scale being fixed so that the axis of the bar was in the magnetic meridian, and its marked end towards the north. A compass, with a small trial needle, an inch in length, was fixed on another rectangular piece of wood furnished with a vernier; so that the scale being graduated across to tenths of an inch, and the side of the rectangle having the vernier being applied to it, the position of the point of the magnetised bar opposite to the centre of the trial needle, could be determined to the hundredth of an inch. The side of the rectangle carrying the trial needle and that

of the scale being in contact, the former was passed along the side of the latter, until the position of the trial needle was exactly reversed; that is, until its marked end pointed accurately south: the point of the bar then opposite to the centre of the needle, I considered as the zero, or magnetic centre of the bar. The distance of this point from the centre of figure of the bar was determined by passing the needle along each side of the scale, and a mean of the two distances taken; and this distance I designated M or U, according as it was towards the marked or unmarked end of the bar.

The points in the bars towards which the small needle, if uninfluenced by terrestrial magnetism, would be directed, when near to them, and which are nearly those where the magnetic intensity is the greatest, I considered as their poles; and in order to determine their positions in each bar, the scale was placed at right angles to the meridian, so that the marked end of the bar was towards the west. The trial needle with its vernier was then passed along the north side of the scale until the position of the needle was exactly reversed; and then again until it assumed its natural direction: the point of the bar opposite to the centre of the needle, in the first case, I considered to be its north or unmarked pole; and that opposite to it in the second, its south or marked pole. The distances of the poles from the centre of figure of the bar were similarly determined by passing the trial needle along the south side of the scale, the needle being, in this case, in its natural direction when opposite to the unmarked pole, and having its direction reversed when opposite to the marked pole. I took a mean of the distances thus determined for each pole, as the distance of that pole from the centre of figure, although I considered that the position of the needle, when its direction was reversed, was most accurately determined, since in this case the slightest change in its position caused a very sensible change in its direction. I should here remark, that in these bars there were no indications of other poles besides those whose positions I determined, magnetism of the same character predominating from the zero, or near that point, to one end of the bar, and the contrary magnetism predominating from the same point to the other extremity.

The observations are contained in the following Table:—

Bar on which the Observations were made.	Date of the Observations.	Distance of the unmarked or North Pole from the centre of figure of the Bar.	Distance by which the more deteriorated Pole receded from, or the less deteriorated Pole approached the centre, in the interval between the observations.	Distance of the magnetic centre or Zero from the centre of figure of the Bar.	Distance by which the magnetic centre approached the centre of figure in the interval between the observations.	Distance of the Marked or South Pole from the centre of figure of the Bar.	Distance by which the less deteriorated Pole approached, or the more deteriorated Pole receded from the centre in the interval between the observations.
		Inches.		Inches.		Inches.	
I.	1828, July 18.	1·24		2·18 M		4·24	
		1·24		2·16		4·28	
	Means	1.24		2·17		4·26	
	1828, Sept. 18.	1·26	+0·02	2·17	+0·005	4·24	+0·005
		1·26		2·16		4·27	
	Means	1·26		2·165		4·255	
	1828, Nov. 18.	1·24	0·00	2·16	+0·02	4·25	0·00
		1·28		2·13		4·26	
	Means	1·26		2·145		4·255	
	1829, Jan. 18.	1·23	+0·01	2·15	0·00	4·25	−0·01
		1·31		2·14		4·28	
	Means	1·27		2·145		4·265	
	1829, Mar. 19.	1·23	+0·025	2·15	+0·005	4·25	0·00
		1·36		2·13		4·28	
	Means	1·295		2·14		4·265	
	1829, May 20.	1·26	+0·01	2·14	0·00	4·25	0·00
		1·35		2·14		4·28	
	Means	1·305		2·14		4·265	
	1829, July 20.	1·30	+0·015	2·14	0·00	4·23	+0·005
		1·34		2·14		4·29	
	Means	1·32		2·14		4·26	
	1830, June 3.	1·35	+0·035	2·14	+0·01	4·22	0·00
		1·36		2·12		4·30	
	Means	1·355		2·13		4·26	
	1830, Dec. 2.	1·30	+0·005	2·13	+0·02	4·25	+0·005
		1·42		2·09		4·26	
	Means	1·36		2·11		4·255	
II.	1828, July 18.	4·17		1·98 U		0·88	
		4·17		1·98		0·92	
	Means	4·17		1·98		0·90	
	1828, Sept. 18.	4·17	0·00	1·98	−0·005	0·89	+0·005
		4·17		1·99		0·92	
	Means	4·17		1·985		0·905	
	1828, Nov. 18.	4·17	−0·005	1·98	+0·005	0·88	−0·005
		4·18		1·98		0·92	
	Means	4·175		1·98		0·90	
	1829, Jan. 18.	4·16	+0·02	1·95	+0·02	0·92	+0·035
		4·15		1·97		0·95	
	Means	4·155		1·96		0·935	
	1829, Mar. 19.	4·16	−0·02	1·96	−0·01	0·89	−0·015
		4·19		1·98		0·95	
	Means	4·175		1·97		0·92	
	1829, May 20.	4·16	0·00	1·96	+0·005	0·92	+0·01
		4·19		1·97		0·94	
	Means	4·175		1·965		0·93	
	1829, July 20.	4·18	−0·005	1·97	0·00	0·84	−0·04
		4·18		1·96		0·94	
	Means	4·18		1·965		0·89	
	1830, June 3.	4·16	+0·015	1·98	0·00	0·85	+0·035
		4·17		1·95		1·00	
	Means	4·165		1·965		0·925	
	1830, Dec. 2.	4·10	+0·035	1·61	+0·36	1·70	+0·78
		4·16		1·60		1·71	
	Means	4·13		1·605		1·705	

Bar on which the Observations were made.	Date of the Observations.	Distance of the unmarked or North Pole from the centre of figure of the Bar.	Distance by which the more deteriorated Pole receded from, or the less deteriorated Pole approached the centre in the interval between the observations.	Distance of the magnetic centre or Zero from the centre of figure of the Bar.	Distance by which the magnetic centre approached the centre of figure in the interval between the Observations.	Distance of the Marked or South Pole from the centre of figure of the Bar.	Distance by which the less deteriorated Pole approached, or the more deteriorated Pole receded from the centre in the interval between the Observations.
		Inches.		Inches.		Inches.	
III.	1828, July 18.	0·76		1·28 M		2·89	
		0·78		1·27		2·91	
	Means	0·77		1·275		2·90	
	1828, Sept. 18.	0·77	+0·005	1·29	−0·005	2·88	0·00
		0·78		1·27		2·92	
	Means	0·775		1·28		2·90	
	1828, Nov. 18.	0·78	+0·015	1·28	+0·01	2·89	−0·005
		0·80		1·26		2·92	
	Means	0·79		1·27		2·905	
	1829, Jan. 18.	0·78	0·00	1·27	+0·005	2·90	−0·01
		0·80		1·26		2·93	
	Means	0·79		1·265		2·915	
	1829, Mar. 19.	0·79	+0·305	1·27	+0·005	2·89	+0·015
		0·86		1·25		2·91	
	Means	0·825		1·26		2·90	
	1829, May 20.	0·80	−0·005	1·26	0·00	2·89	−0·005
		0·84		1·26		2·92	
	Means	0·82		1·26		2·905	
	1829, July 20.	0·85	+0·03	1·26	+0·005	2·87	+0·015
		0·85		1·25		2·91	
	Means	0·85		1·255		2·89	
	1830, June 3.	0·80	−0·01	1·26	+0·01	2·85	0·00
		0·88		1·23		2·93	
	Means	0·84		1·245		2·89	
	1830, Dec. 2.	0·83	+0·045	1·24	+0·005	2·88	0·00
		0·94		1·20		2·90	
	Means	0·885		1·22		2·89	
IV.	1828, July 18.	2·84		0·99 U		1·40	
		2·84		0·98		1·47	
	Means	2·84		0·985		1·435	
	1828, Sept. 18.	2·83	+0·005	0·97	+0·01	1·40	+0·01
		2·84		0·98		1·49	
	Means	2·835		0·975		1·445	
	1828, Nov. 18.	2·83	0·00	0·97	+0·01	1·43	+0·015
		2·84		0·96		1·49	
	Means	2·835		0·965		1·46	
	1829, Jan. 18.	2·82	+0·005	0·97	+0·005	1·47	+0·045
		2·84		0·95		1·54	
	Means	2·83		0·96		1·505	
	1829, Mar. 19.	2·81	+0·005	0·95	+0·01	1·47	+0·005
		2·84		0·95		1·55	
	Means	2·825		0·95		1·51	
	1829, May 20.	2·82	−0·005	0·95	0·00	1·50	+0·015
		2·84		0·95		1·55	
	Means	2·83		0·95		1·525	
	1829, July 20.	2·84	−0·005	0·94	+0·015	1·48	0·00
		2·83		0·93		1·57	
	Means	2·835		0·935		1·525	
	1830, June 3.	2·80	+0·01	0·95	+0·01	1·48	+0·015
		2·85		0·90		1·60	
	Means	2·825		0·925		1·54	
	1830, Dec. 2.	2·79	0·00	0·93	−0·01	1·52	−0·005
		2·86		0·94		1·55	
	Means	2·825		0·935		1·535	

In order to compare these results, it is necessary to consider the effects produced on the situations of the poles and magnetic centre of a bar by the disturbance of the symmetrical distribution of magnetism in it. When a bar has been carefully magnetised by double touch from its centre towards its extremities, the magnetic centre is very nearly in its centre of figure, and its poles are very nearly at equal distances from that centre*. If to either pole of such a bar the similar pole of a magnet be applied, the effect will be, to drive the pole at that end nearer to the centre, the other pole further from it, and the magnetic centre towards the other end of the bar. At the same time, the intensity of each pole will be diminished, but that of the pole in the branch to which the magnet has been applied considerably more than that of the pole at the untouched end; and in the former branch, the magnetism will likewise be less concentrated than in the other, having nearly the same degree of intensity over a considerable space. So that any tendency in the magnetism of the bar to return to a state of symmetrical distribution in the two branches, would be shewn by the approach of the untouched, or more intense pole, and of the magnetic centre towards the centre of figure, and the receding of the more diffused pole from that centre; and as, in the disturbance, the effect is greatest on the positions of the magnetic centre and the pole at the end to which the magnet has been applied, so this tendency will be most conspicuous in the change of position of these points.

In the foregoing table, the changes which took place from one observation to another, in the positions of the poles and magnetic centre, are exhibited in the fourth, eighth, and sixth columns, the sign *plus* indicating that the change was in the direction corresponding to the resumption of a state of symmetrical distribution of the magnetism in the bars, and the sign *minus* that it was in a contrary direction. Now although the changes in all the bars are, upon the whole, of the former character, yet, as they are the reverse in some cases from one

* The bars I. and II. having been thus magnetised, the positions of these points were—

	Unmarked Pole.	Zero.	Marked Pole.
I.	3·85 inches.	0·58 inch U.	3·71 inches.
II.	3·77	0·00	3·73

observation to another, and are besides but small, it is extremely doubtful whether, notwithstanding the care taken in adjusting the bars for examination and replacing them afterwards, they were not due to accidental derangement, when the bars were moved at the several times of observation, rather than to a tendency in the magnetism to resume a state of symmetrical distribution. Indeed it is possible that the needle made use of to determine the positions of the poles and magnetic centre may have slightly modified the magnetic state of the bar, at each observation, since it was necessary, for this purpose, to bring it within a very small distance; and such influence it is scarcely possible to prevent, especially in the case of rather soft bars, as these purposely were.

The changes in the bars I., III., IV., are in general much greater from July, 1828, to July, 1829, than subsequently, and are the most indicative of the mutual action of the poles upon each other, since the changes in the positions of the more diffused poles are much greater than could have arisen from errors of adjustment or observation; but even these, admitting them to be entirely due to such action, are so small, considering the time during which they took place, that the force which produced them must have been almost evanescent as compared with the coercive force of the steel.

In the bar II., the changes are extremely small from July, 1828, to July, 1829, and also from the latter date to June, 1830, and, varying much in their direction, do not indicate that they were caused by internal action; but from June to December, 1830, a decided change in the positions of the poles and magnetic centre took place. This, however, I have no doubt was entirely due to accident. When I removed this bar in December, for the purpose of making observations on it, in order to see which was the marked end, I incautiously approached a candle unfortunately placed upon a box containing a very powerful magnet, and, from the small amount of all the other changes, I have no hesitation in attributing the considerable change here observable to the action of this large magnet upon the poles of the bar. Those who have not been extensively engaged in delicate magnetical experiments can scarcely be aware of the difficulty of guarding against such accidental

influence, when surrounded by apparatus which is a source of disturbance. Independent of such disturbance, there can, I think, be no doubt that the magnetism in these bars would remain very nearly, if not precisely, in the same state for almost an indefinite period.

We may therefore conclude, from these observations, that, after the action of a magnet upon a bar which determines the position of its poles, has ceased, if any effect is produced by reciprocal action, the forces tending to produce this effect are almost evanescent when compared with the other forces acting upon the magnetism of the bar. Upon the whole, I am disposed to think, that into whatever state the magnetism of a steel bar may be placed by the application of a magnet to it, almost immediately after the removal of the magnet, the internal forces are in a state of equilibrium, or nearly so; and that, therefore, whatever may be the arrangement of the magnetism, it is, if not absolutely permanent, liable to scarcely any disturbance from internal action.

Royal Military Academy, 27*th Dec.* 1830.

ON THE ELECTRO-CHEMICAL DECOMPOSITION OF THE VEGETO-ALKALINE SALTS.

By W. T. BRANDE, F.R.S., Prof. Chem. R. I.

I AM not aware that any experiments have been recorded in relation to the phenomena presented by the salts of the vegetable alkaline bases, when subjected to the action of voltaic electricity; and as, under such circumstances, they exhibit appearances identical with those of ordinary salts, a further analogy is thus established between those curious compounds and the other salifiable bases.

Shortly after the discovery of a method of obtaining morphia in pure state, I remember that Sir Humphry Davy suggested the possibility of its affording, when electrised in contact with mercury, results corresponding with those which Berzelius had observed in respect to ammonia: he thought that the nascent elements of the morphia, as liberated by electrical decomposition, might, under such circumstances, effect a similar apparent

amalgam of the mercury, and he spoke of the subject as likely to throw some light upon the corresponding ammoniacal combinations. He made, I believe, a few experiments upon the subject, but the results were not as he expected, and they were, nowhere, I believe, recorded.

Since that period the subject generally has acquired much additional interest, by the discovery of several other bodies appertaining to the same class, and especially of quinia and cinchonia, the medicinal preparations of which have rendered these substances so generally known.

I repeated the experiment of the electrisation of moistened morphia and mercury, a globule of which, in contact with the vegetable base, was rendered negative; feebly at first, and afterwards by a more powerful voltaic combination. The morphia, I had reason to believe, was perfectly pure; but although the process was continued for a due time, in one instance exceeding twenty minutes, I did not observe any change in the fluidity of the metal, nor did it, on being transferred to a glass of pure water, exhibit any action upon that liquid, or any appearance of having united to foreign metallic matter.

Crystals of pure cinchonia reduced to powder, moistened, and subjected in the same way to the action of negatively electrified mercury, were equally inert, and exhibited no symptoms of contributing anything metallic to the mercury.

When mercury was similarly electrised in contact with quinia, moistened and placed upon a positive disc of platinum, it exhibited, in the course of a few minutes, appearances very different from those exhibited with it, when electrised in contact with morphia and cinchonia; the metal became filmy, and after a time appeared to acquire a tendency to a butyraceous appearance, and evidently had its fluidity diminished. When transferred into a tall glass of distilled water, a peculiar motion was perceptible upon its surface, and ultimately some small globules of gas were liberated, and it regained, though slowly, its usual aspect.

This experiment first led me to suspect that something like a metallization of the elements of the quinia had been effected, but I could not satisfy myself that it was reproduced by the action of water on the globule, nor could I, by carrying on the

process of electrism for a longer time, produce a much greater effect than ensued in the first five or ten minutes. Aware of the influence which very minute quantities of foreign matter, and especially of the fixed alkalies, or of lime, might have in producing some such appearances, and more especially recollecting the singular results of Mr. Herschel's experiments upon this subject, it became important to ascertain the absolute purity of the quinia employed. I therefore examined it with this view, and found it entirely soluble in strong alcohol; when dissolved in dilute muriatic acid the solution afforded no traces whatever of lime to the usual tests; but on burning a portion of the above quinia in a platinum crucible, and dissolving the ashes in muriatic acid, traces of lime were readily recognized in the latter solution. I treated the morphia and cinchonia which I had employed in the same way, but in them no traces either of fixed alkali or of lime could in any way be discovered. I am, therefore, induced to refer all the appearances which quinia exhibits to the obstinate adhesion of a very minute quantity of lime, which I have not yet been able entirely to deprive it of.

The electro-chemical decomposition of the salts of the vegeto-alkalies is very characteristic, in consequence of the difficult solubility of their bases. If, for instance, a solution of sulphate of morphia be placed in the voltaic circuit, so as to be decomposed between two plates of platinum, the negative plate, if the solution be strong, becomes presently covered with a white crust of morphia, which gradually falls off in films; if the solution be more dilute, the morphia falls in the form of a white cloud from the negative conductor.

The appearances are nearly similar with the solutions of sulphate of cinchonia and of sulphate of quinia.

Supposing that some more decided results than those above mentioned might be obtained by electrising mercury negatively in contact of the soluble salts of morphia, cinchonia, and quinia, the experiment was made with the respective sulphates of those alkalies, but no further appearances of metallization ensued provided the salts were pure—whereas any alkaline impurity, though in very minute quantity, gave the same equivocal appearances as had been previously obtained with quinia containing a little lime.

The appearances afforded by the electro-chemical decomposition of these salts led to the question, how far the bases might be discovered by the voltaic test in the infusions of opium and bark; but when these are treated in the usual way, there is no distinct separation of difficulty soluble alkaline matter, as might have been expected, in consequence, probably, of the multiplicity of substances that are present: nor is strichnine separable in this way from the infusion of nux vomica.

SOME OBSERVATIONS ON DR. ARNOTT'S EXPLANATION OF THE NATURE OF STAMMERING.

By MARSHALL HALL, M.D., F.R.S.E., &c., &c.

I WAS much struck, in the first instance, with the simplicity of Dr. Arnott's explanation of the defect in speech termed stammering, in his interesting and popular work, entitled 'Elements of Physics.' I, however, soon perceived its fallacy; and as this has not hitherto, I believe, been pointed out, it may not be amiss for me to do so briefly in this place.

I will first copy Dr. Arnott's view in his own words. That gentleman states, that * 'The most common case of stuttering is not (as has been almost universally believed), where the individual has a difficulty in respect to some particular letter or articulation, by the disobedience, to the will or power of association, of the parts of the mouth which should form it, but where the spasmodic interruption occurs altogether behind or beyond the mouth, *viz.*, in the glottis, so as to affect all the articulations equally. To a person ignorant of anatomy, and therefore knowing not what or where the glottis is, it may be sufficient explanation to say, that it is the slit or narrow opening at the top of the windpipe, by which the air passes to and from the lungs—being situated just behind the root of the tongue. It is that which is felt to close suddenly in hiccup, arresting the ingress of air, and that which closes, to prevent the egress of air from the chest of a person lifting a heavy

* Elements of Physics, vol. ii. Part I. Appendix pp. v—viii.

weight, or making any straining exertion; it is that also, by the repeated shutting of which, a person divides the sound in pronouncing several times, in distinct and rapid succession, any vowel, as o, o, o, o. Now the glottis, during common speech, need never be closed, and a stutterer is instantly cured, if, by having his attention properly directed to it, he can keep it open. Had the edges or thin lips of the glottis been visible, like the external lips of the mouth, the nature of stuttering would not so long have remained a mystery, and the effort necessary to the cure would have forced itself upon the attention of the most careless observer; but because hidden, and professional men had not detected in how far they were concerned, and the patient himself had only a vague feeling of some difficulty, which, after straining, grimace, gesticulation, and sometimes almost general convulsion of the body, gave way, the uncertainty with respect to the subject has remained. Even many persons, who by attention and much labour had overcome the defect in themselves, as Demosthenes did, have not been able to describe to others the nature of their efforts, so as to ensure imitation; and the author doubts much whether the quacks who have succeeded in relieving many cases, but in many also have failed, or have given only temporary relief, really understood what precise end in the action of the organs their imperfect directions were accomplishing.

'Now a stutterer, understanding of anatomy only what is stated above, will comprehend what he is to aim at, by being further told, that when any sound is continuing, as when he is humming a single note or a tune, the glottis is necessarily open, and, therefore, that when he chooses to begin pronouncing or droning any simple sound, as the *e* of the English word *berry* (to do which at once no stutterer has difficulty), he thereby opens the glottis, and renders the pronunciation of any other sound easy. If, then, in speaking or reading, he joins his words together, as if each phrase formed but one long word, or nearly as a person joins them in singing (and this may be done without its being at all noted as a peculiarity of speech, for all persons do it more or less in their ordinary conversation), the voice never stops, the glottis never closes, and there is of course no stutter. The author has given this explanation

or lesson, with an example, to a person, who before would have required half an hour to read a page, but who immediately afterwards read it almost as smoothly as was possible for any one to do; and who then, on transferring the lesson to the speech, by continued practice and attention, obtained the same facility with respect to it. There are many persons not accounted peculiar in their speech, who, in seeking words to express themselves, often rest long between them on the simple sound of *e* mentioned above, saying, for instance, hesitatingly, "e I e . . . . . .think e . . . . . . you may,"—the sound never ceasing until the end of the phrase, however long the person may require to pronounce it. Now a stutterer who, to open his glottis at the beginning of a phrase, or to open it in the middle after any interruption, uses such a sound, would not even at first be more remarkable than a drawling speaker, and he would only require to drawl for a little while, until practice facilitated his command of the other sounds. Although producing the simple sound which we call the *e* of *berry*, or of the French words *de* or *que*, is a means of opening the glottis, which by stutterers is found very generally to answer, there are many cases in which other means are more suitable, as the intelligent preceptor soon discovers. Were it possible to divide the nerves of the muscles which close the glottis, without at the same time destroying the faculty of producing voice, such an operation would be the most immediate and certain cure of stuttering; and the loss of the faculty of closing the glottis would be of no moment.

'The view given above of the nature of stuttering and its cure, explains the following facts, which to many persons have hitherto appeared extraordinary. Stutterers often can sing well, and without the least interruption,—for the tune being continued, the glottis does not close. Many stutterers also can read poetry well, or any declamatory composition, in which the uninterrupted tone is almost as remarkable as in singing. The cause of stuttering being so simple as above described, one rule given and explained may, in certain cases, instantly cure the defect, however aggravated, as has been observed in not a few instances; and this explains also why an ignorant pretender may occasionally succeed in curing, by giving a rule

of which he knows not the reason, and which he cannot modify to the peculiarities of other cases. The same view of the subject explains why the speech of a stutterer has been correctly compared to the escape of liquid from a bottle with a long narrow neck, coming—"either as a hurried gush or not at all:" for when the glottis is once opened, and the stutterer feels that he has the power of utterance, he is glad to hurry out as many words as he can, before the interruption again occurs.'

This view of the subject is so far from being correct, that it is quite plain that it is only in the articulation of certain letters, that expiration is interrupted, and, even in this case, the interruption is not in the larynx, the organ of voice, but in some part of the mouth, or organ of speech. It will assist us in the determination of the question, to take a review of the influence which the natural articulation has upon respiration, or rather upon expiration. It may be ascertained, by the simplest experiment, that in the pronunciation of the short word BAT, we adopt a mechanism, by which not only the different letters are formed, but the respiration is twice completely arrested;—and that, in the pronounciation of the equally short word FAN, we first interrupt the flow of the air through the nostrils, whilst it is forced between the teeth and lower lip, and then intercept the course of the air through the mouth, whilst we allow it to pass only through the nostrils.

It is on their influence on the respiration, that I formed the division and arrangement of the consonants, published in the nineteenth volume of this Journal; their sub-division was founded on the respective mode or mechanism of their enunciation. I divided them—

1. Into those, in the articulation of which both the mouth and the nostrils are closed, and the respiration, of course, completely arrested:

2. Into those, in the enunciation of which the nostrils are closed, but the mouth left more or less open, for the exit of the air, which is compressed, but not interrupted, in its expiration:

3. Into those, not requiring even the nostrils to be closed, and in the enunciation of which the air is still less compressed in its course from the lungs: and,

4. Into those, in the articulation of which the expired air is not interrupted, and scarcely impeded at all.

Of the *first* class, are

$$\frac{B}{P};\ \frac{D}{T};\ \frac{G^{*}}{K}.$$

In tracing these letters into their sub-divisions, we may observe, that the first pair are labials, being formed by the lips compressed together; the second pair are linguo-dentals, formed by pressing the point of the tongue against the posterior and upper part of the upper teeth; and the third pair are linguo-palatal, being effected by pressing the middle part of the tongue against the palate. In all, the posterior apertures of the nostrils are effectually closed by the pendulous vail of the palate being drawn upwards, and accurately applied to their posterior apertures. And of course, those persons whose palate is perforated, or in whom the pendulous vail of the palate is imperfect, as sometimes arises from disease, are more or less incapacitated from pronouncing these letters, the expired air being no longer intercepted, as it ought to be, in its course.

Of the *second* class, are

$$\frac{F}{V};\ \text{the TH}\ \dagger;\ \text{and}\ \frac{S}{Z}.$$

In the articulation of these letters, the posterior orifices of the nostrils are required to be closed, whilst, in the first pair, the compressed air is continually forced between the upper teeth and under lip; in the second, between the teeth and the tongue; and in the third, between the point of the tongue and the anterior part of the palate.

From this view of the subject, it will be readily apprehended how the substitution of D or T for the TH, by foreigners, is so remarkable; for it is no less than the substitution of a total interruption, for a mere compression of the air, in its exit from the chest.

Of the *third* class of letters, are

M; N; L; R.

In the enunciation of these letters, the expired air is only very slightly compressed, the nostrils being left freely open. It is for this very reason, probably, that these letters have been

* i. e. the *hard* G. † *Hard* and *soft*.

termed *liquids,* as flowing without obstacle. And it is by this circumstance, principally, extraordinary as it may appear, that the letter M differs from the letters B and P, for they are all equally labial; and that the letter N differs from T and D, for they are all equally formed by placing the point of the tongue near the roots of the upper teeth.

Of the *fourth* and last class, are

H; the Greek X; Y; and W.

In the enunciation of these consonants, the air appears to be scarcely compressed or impeded in its exit at all. This fact may, I think, account for the circumstance, that it has even been doubted, whether the two last letters be really consonants or not; and for the remarkable fact, that they cannot, as *consonants,* form the termination of any word. Their mechanism is guttural, double dental, and labial, respectively.

These letters, preceded as they are in this arrangement, by the liquids, lead us almost insensibly to the class of letters to be next noticed, namely, the *vowels.*

These are so called, from having been supposed to relate to the *voice* alone*. This, however, is obviously an error. The different parts forming the mouth, or organ of *speech,* are not less necessary to the enunciation of the vowels, than to that of the consonants, or their function less appreciable, on carefully making the experiment. Thus, the French U is entirely labial; the letter E is dental; O, palatal; whilst the diphthong AW, and the vowels marked in the French language by the circumflex (ʌ), are guttural.

Now let any one carefully examine the effort made by the stammerer in his attempts at the enunciation of these various letters. It will be obvious that the malady is but an exaggeration of the natural effort. In attempting to pronounce the letters of the first class, violent efforts are made, yet expiration —articulation—is not effected; but there is frequently, nay generally, a peculiar noise heard in the larynx, although its full enunciation is prevented by the action of the muscles of the mouth. But if the letters of the second class are pronounced with stammering, there is a perpetual hissing from the escape

* Blumenbachii Institutiónes Physiologiæ, Ed. MDCCCX, Sectio IX.

of compressed air, in the case of the letters F and V, between the lips, in that of the T H, between the tongue and upper teeth, and in that of the letters S and Z between the teeth. In the stammering enunciation of the letters of the third class, there is frequently a state of laborious respiration. In all these cases, then, it is plain that the larynx is open; any considerable effort applied to the parts concerned in the articulation of the first class of letters,—the least noise,—the least escape of air, alike demonstrate this fact. In the natural, and in the stammering articulation, there is the same total or partial interruption of the expiration, at the same parts, not of the larynx, but of the proper organs of articulation, only in different degrees. Let the larynx be really closed, which may be done after a little trial, and it will immediately be discovered that stammering is, in fact, impossible; the effort made by the force of the expired air against the parts of the mouth called into action in the articulation of the first class of letters,—all escape of air,—all noise, become totally interrupted.

I have just attentively watched the attempts of a stammerer to articulate the various letters.

In the effort to pronounce the first class of letters, especially the letter T, still more if two T's come together, as in the words THAT TREE, the face became flushed even from interrupted expiration; yet there was, at every repetition of the effort, a noise audible in the larynx, proving that this part was unclosed.

In pronouncing the letters of the second class, a repeated hissing noise was distinctly produced by the flow of the compressed air, in one case, (F, V,) between the under lip and upper teeth; in the second, (TH,) between the tongue and upper teeth; and in the third, (S, Z,) between the teeth.

In attempting the articulation of some of the letters of the third and fourth classes, and of some of the vowels, the breath was sometimes lost, as it were, in a full and exhausting expiration, altogether peculiar.

All these results prove that the larynx is not closed in stammering, and indeed that its closure and stammering are totally incompatible with each other. When expiration is interrupted,

it is by the co-operation, the coadaptation, of parts anterior to the larynx; it is, in a word, not an interruption in the organ of voice, but in that of speech. The paralysis of the laryngal muscles could not, therefore, effect the good which Dr. Arnott ingeniously supposes.

But would no evil really result from this paralysis of the muscles of the larynx? Would the 'loss of the faculty of closing the larynx" really 'be of no moment'? On the contrary, the accurate closure of the larynx, not by the epiglottis, but by means of its own muscles, is essential to the act of deglutition. This is demonstratively proved by M. Magendie, in his interesting memoir, 'Sur l'usage de l'Epiglotte dans la Deglutition.' The fact is further proved by cases of actual paralysis of the laryngal muscles occurring in the human body, and by the effects of inflammation and contraction, and of ulceration, of the internal parts of the larynx, in inducing defective deglutition.

The rule proposed by Dr. Arnott for remedying stammering, does not attach itself exclusively to the view which that gentleman has taken of the subject. On the contrary, the very same rule was proposed by myself, in the paper to which allusion has already been made, in the following words:—'Let a stammerer observe this rule: always to speak in a continuous or *flowing* manner, avoiding carefully all positive interruption in his speech; and if he cannot effect his purpose in this way, let him even half sing what he says, until he shall, by long habit and effort, have overcome his impediment; then let him *gradually*, as he may be able, resume the more usual mode of speaking, by interrupted enunciation. I am persuaded, that this is the principal means employed by those gentlemen who have undertaken to correct impediments in the speech, and it is, undoubtedly, the most rational. In addition to this rule, let the stammerer endeavour to speak in as calm and soft a tone as possible; for in this way the muscles of speech will be called least forcibly into action, and that action will be least liable to those violent checks or interruptions, in which stammering appears to consist. It would, of course, be irrelevant to the object of this essay, to allude to those other principles

connected with stammering, such as nervousness, of which it would be necessary to treat, in an essay written expressly on this important and interesting subject.'

I am persuaded that I need not apologize to Dr. Arnott for this free and plain discussion of his views relative to stammering, our mutual object being the discovery and establishment of truth.

OBSERVATIONS ON MR. RENNIE'S PAPER ON THE PECULIAR HABITS OF CLEANLINESS IN SOME ANIMALS.

By WILLIAM AINSWORTH, Esq.

IN the first number of the Journal of the Royal Institution, I observe a paper on the cleanliness of animals by Mr. Rennie, in which it is advanced upon the authority of Wilson, the author of the American Ornithology, that the serrated structure of the claw of the goat-sucker is employed as a comb to rid the plumage of vermin—an erroneous opinion as to its use having been held by Swainson, White, &c.

It is a fact, not generally known, that the claws of most birds are used for similar purposes; and thus birds which have short legs, as the swift, are most infested by insects. The expedients which birds characterized by short feet—the waders which, from the inflexible nature of their legs, and the geese tribe, from the opposition to scratching, offered by the membrane extending between the toes, are put to, in order to get rid of their vermin, are well deserving of attention, as illustrating the ingenuity of animals, and the curious provisions made by nature for their cleanliness. When birds, by accident or imprisonment, are deprived of the natural means of ridding themselves of vermin, they often fall victims to these attacks. Walking one day along the shore of Holy-Island, off the coast of Northumberland, I disturbed an ash-coloured sanderling (Calidris Islandica, Step.), which flew heedlessly, and as if injured. On shooting the bird, I found that it was covered with vermin, more especially about the head; so much so, that the poor thing must have fallen a victim to their tormenting ravages: on further examination, I found that it had lost

one of its legs, so that it was from its incapability to rid itself of these insects that their extraordinary increase was to be attributed. A circumstance of a similar kind also came under my notice connected with a swallow's nest. After the young birds had been hatched, and had attained a certain size, a change was made in the arrangement of the window, which frightened the parents: from that time they continued to feed their offspring, but never entered the nest; and I soon observed that the young ones were sick, and one by one they perished. I then took the nest down, and found it crowded with acari, which were of a very great size compared with that of the bird itself. I could only attribute this fatal increase of vermin to the old birds having been prevented cleaning out the abode of their family.

Poultry which run about in stony or paved yards, wear away the points of their claws by friction and digging, which renders them unfit to penetrate their coating of feathers; they are, therefore, more covered with vermin, and in consequence more sickly than fowls from the country.

On the AURORA BOREALIS of the 7th of JANUARY, 1831.

By S. H. CHRISTIE, Esq., M.A., F.R.S., &c.

[To the Editor of Q. J. of Science, &c.]

Woolwich Common, 7th January, 1831.

YOU will not, I think, be sorry to have some account of the appearance of the very beautiful aurora of this evening, in this neighbourhood, where, perhaps, I had a better opportunity of viewing it than you might have in town. I was not, however, under very favourable circumstances for making remarks upon it, as I was, for a considerable part of the time during which it appeared, travelling, being outside of our coach. I first observed it at 5h. 30m., being then on Blackheath, about half a mile S.W. from the observatory. At this time I observed a strong white light, resembling the tail of a comet, but denser, like a light cloud illuminated by the moon, proceeding from near Betelguex in the east towards Aldebaran,

It very quickly spread in this direction towards the south, and was soon joined by a similar band of light proceeding from the west, the whole now forming a strongly-marked arch, about 5° in breadth, pretty well defined on the upper side, but not so well on the lower. The highest point of the arch passed over the planet Mars, about 45° above the horizon. Towards the west the arch was lost in what appeared to be the London smoke, about 10° above the horizon; and towards the east, about the same portion was lost in haze; or rather it appeared to proceed from the smoky fog at this height on the west, and the haze on the east. The whole had the white appearance of a thin cloud illuminated by the moon. The brightest parts were to the S.E. and S.W. This arch faded gradually away, but was visible for nearly a quarter of an hour. Before it had disappeared, I observed a strong light in the N.E. (at this time I was about three-quarters of a mile S.E. from the observatory) of a brilliant rose colour. This increased rapidly in brilliancy, and sent coruscations up to the zenith. Very shortly afterwards the whole of the northern horizon became illuminated, and brilliant coruscations shot from every part towards the zenith. Some of these were very thin and well defined; but were not much tinged with red, excepting towards the N.E. and N.W. At the same time large detached masses of light, resembling floating clouds, were seen on the southern side from east to west; and similar ones, though not so strongly enlightened, appeared towards the north. These, towards the north, after a short time, assumed the appearance of an irregular inverted arch, the lowest point being, as near as I could judge, in the magnetic north, and brilliant coruscations proceeded rapidly from every part, some being slightly tinged with red. Shortly after six o'clock, when I arrived here, these gradually diminished in brightness; and at half past six, little more could be observed than a general light diffused over the northern side of the heavens. At 7h. 30m. I observed a distinct arch of light towards the north, the centre of the arch being very nearly in the magnetic north, and its highest point about 25° or 30° above the horizon. The eastern and western ends of this arch, like that which first appeared towards the south, were not visible near to the horizon.

8th January.—Later in the evening the northern arch increased in distinctness, the upper circumference being very well defined, and below there was another luminous arch, in the interior of which was a dark segment 10° or 12° in altitude. The magnetic north still appeared to be the centre of these arches; only very faint streams proceeded from the upper arch. The strongest light was at the two ends, towards the east and west. This was the appearance at 9h. 30m.; at 9h. 50m. three concentric arches were distinctly visible, their highest points being still in the magnetic meridian; at 10h. 45m. there was a single broad well-defined arch in the north, the lowest side of which, resting on the dark segment, was particularly well defined. The appearance was, very shortly after this, that of the dark segment breaking through the arch of light in various places, and sending through it dark streams, narrowing as they ascended. The luminous arch was now soon broken up, when there appeared in the N.E. a stream of pale rose-coloured light. The light in the N.W. shortly after assumed the same colour; and then the light in various intermediate places was tinged with the same, that in the N.E. becoming of greater intensity. Distinct pencils of light, streaming upwards, appeared now to be propagated from E. to W.; and brilliant coruscations, tinged with red, proceeded from every part. The phenomena were now nearly the same as at six o'clock, but of very inferior brilliancy. The streams of light gradually faded, and at 11h. 20m. the luminous arch, resting upon the dark segment, was formed as before, but the streams of light were very faint. At 11h. 45m. the upper side of the luminous arch could be traced; but the lower side was quite broken, and there were no streams from any part. From 10h. 45m. to 11h. 45m. I observed the star Deneb (α Cygni) very distinctly visible through the border of the dark segment, but it was not visible after this. At 11h. 20m. this star occupied the highest part of the arc, which must, therefore, have been about 10° in altitude, and still nearly in the magnetic meridian. The arch, though faint, was visible for some time after midnight; and at six this morning, the whole of the northern horizon was illuminated, and I thought the light somewhat tinged, but the moon being up, rendered this doubtful.

I regret much that I had not a magnetic needle so adjusted, that I might have observed whether the aurora had any magnetic influence. As it is not improbable that we may have a repetition of the phenomenon, I shall take care to be prepared in this way. I am not aware that, before this, any luminous arch has been observed towards the south, in this part of the globe, and I am very anxious to hear what observations have been made on this arch farther to the south. Dr. Gregory is at present at Hastings, and I intend inquiring of him, whether an arch was observed to the south of that place; if so, at what elevation at 5h. 30m.; and if not, whether the arch appeared to the north at that time: its elevation at these two places would pretty nearly determine its absolute height above the earth's surface. As I saw them, the phenomena between five and six o'clock, and between ten and eleven, were repeated nearly in the same order: the strongest red light appearing first in the N.E., and the coruscations succeeding the disappearance of the luminous arch in both cases. The temperature was rather low, but steady, about 24° F. during the whole time; the barometer high and steady (last night at 12h. 45m. 30·64; now 2h. P.M. 30·59).

ON THE MECHANISM OF THE ACT OF VOMITING.

By MARSHALL HALL, M.D., F.R.S.E., &c. &c.

I WAS greatly interested by the following extract from the valuable Report of cases in the Meath Hospital, just published by Drs. Graves and Stokes, in the fifth volume of the 'Dublin Hospital Reports and Communications.'

'A man about forty years of age died of tubercular phthisis.

'The œsophagus, after passing through the usual opening in the diaphragm, was found to re-enter the thorax by another very large opening in the tendinous portion towards the left side. The stomach occupied the inferior portion of the left thoracic cavity, its cardiac and pyloric extremities both lying in the opening.

'The man vomited frequently while under observation in the hospital. Now, as the stomach was placed entirely out of

the reach of being compressed by the contractions of the diaphragm, and as this contraction completely defended it from the influence of the abdominal muscles, it is clear that, in this case, vomiting must have occurred independently of compression, either of the diaphragm or of the abdominal muscles. This fact, worth a thousand experiments, *completely decides the question, that vomiting may be produced by the action of the stomach itself, unassisted by any external compressing force,* notwithstanding what Le Gallois and late physiologists have said to the contrary.'*

The authors of the report do not appear to have seen the paper† which I published in the number of this Journal for April to July, 1828; the object of which was—first, to expose the fallacy, both of that view of the nature of the act of vomiting, which refers it to a contraction of the stomach itself, and of that other view lately advocated by M. Magendie, which refers this act to the simultaneous contraction of the diaphragm and abdominal muscles; and, secondly, to propose a new view of this disputed question. As this last view has never been controverted—as it has, on the other hand, been generally admitted—and as it alone explains the various difficulties which beset each or both of the other two—it may not be amiss to reproduce its broad outlines here, in connexion with the interesting case of Dr. Graves and Dr. Stokes. They are these :—

1. During the act of vomiting, the larynx is closed;

2. The diaphragm, and its various apertures, are relaxed; and,

3. All the muscles of *expiration* are called into action; but,

4. Actual expiration being prevented by the closure of the larynx, the force of the *effort* is expended upon the stomach, the cardia being open from the relaxed condition of the diaphragm,—and vomiting is effected.

It is plain, from this view of the subject, that the thorax and abdomen become one cavity, as it were, the diaphragm lying loose and inert between them. It is also obvious, that it is

* Pp. 85—87.

† In this Paper I have referred to cases similar to that given by the authors of the Report.

quite indifferent on which side of the diaphragm the stomach may be placed, whether above, as in the case of hernia, or below, in its natural situation.

The view of the act of vomiting which I have taken, appears to me to be the only one which at once explains this act, as it occurs in the case of hernia of the stomach through the diaphragm, such as the one detailed by Dr. Graves and Dr. Stokes; and the experiment of M. Magendie, in which a bladder was substituted in the place of the stomach. The first establishes the fact, that the diaphragm, the second, that the stomach, has no necessary part in vomiting. It remained, therefore, to shew in what other manner the act of vomiting, and both of these facts, would admit of explanation. This is done in the manner already detailed. And the truth of the explanation is proved by two decisive experiments, related in the paper to which I have already referred.

FURTHER EXPERIMENTS ON THE COMMUNICATION OF PHOSPHORESCENCE AND COLOUR TO BODIES BY ELECTRICITY.

By THOMAS J. PEARSALL,
Chemical Assistant in the Laboratory of the Royal Institution.

IN a former communication (page 77 of the Royal Institution Journal) I observed, that those phosphori which are distinguished for their property of evolving light when heated, and which, under ordinary circumstances, allow of no repetition of this effect, could have the property restored to them by the agency of the electric discharge. I now purpose to offer some additional experiments and remarks upon this induction of phosphorescence.

From the results obtained, there was reason to anticipate that not only all such phosphorescent bodies might have this property modified, increased, restored, or imparted to them by the agency of ordinary electricity, and the effects be alternately produced and destroyed any number of times, but also that

bodies, not as yet known to possess this power, might have it conferred upon them.

The restored effects which I have described, as produced upon some known phosphori, in the former paper, I found to occur with the class generally; and the following ordinary substances will afford illustrations of the facility with which phosphori may, as it were, be created in ordinary substances by the means in question. The fragments used were placed in the cavity of a piece of ivory, into which were inserted two wires, and regulated discharges passed through them from a Leyden jar, having about two square feet of coated surface. The electrified portions were usually submitted immediately afterwards to a strong heat, so as to exhibit the whole effect of the phosphoric light with the utmost intensity.

White statuary marble yielded no light in its natural state; after twelve discharges it was heated on platina, and gave a dull orange light.

The same marble, calcined at a red heat, and electrified by twelve discharges, gave a clear *orange* and *violet* light by heat.

The carbonaceous part having been dissipated from ivory calcined, a *lilac* coloured light was conferred by fourteen discharges of electricity. This substance was, however, feebly luminous when heated in its natural state.

Mother of pearl calcined, and twelve times electrified, when heated, gave a strong light of *pink*, *violet*, and *light blue* colours, the whole of which were occasionally visible upon different parts of the same fragment.

Calcined oyster-shells, heated, after fifteen discharges emitted a strong light, of considerable duration, with *orange*, *yellow*, and *lemon-green* colours.

Cuttle-fish bone calcined, after six discharges, was capable of evolving by heat a bright *lilac* and *violet* coloured light; twelve discharges gave distinct *pink*, *purple*, and *yellow* phosphorescent light.

Common scollop shells were calcined and subjected to twelve discharges of electricity: the application of heat disengaged a strong light of considerable permanence, blending *salmon*, *pink*,

and intense *azure blue* tints. The phosphorescence of these specimens had light and colour of exquisite delicacy.

Chalk gave an orange light, rather dull, when heated in its natural state; but if made red hot, allowed to cool, and then twelve times electrified, it evolved a bright *orange* light when heated.

Common egg-shells gave no light; but twelve discharges of the electrical jar conferred a *bright purple* light.

The preceding experiments were made with substances which did not possess this peculiarity in their ordinary state, yet exhibited the conferred phenomena, with such beauty, variety, and intensity of colour, as to surpass very many natural specimens.

The results obtained with such varieties of fluor spars as I had access to, appeared in a tabular form in my preceding communication; the examination of other specimens presented similar effects.

The probable localities of the following varieties of fluors have been assigned by Mr. Sowerby:—

[The natural phosphorescence is shown in the second column; the third column states the number of explosions sustained by the calcined mineral, and also the phosphorescent appearances by heat subsequently applied.]

1. Green fluor (probably from Cornwall)	*Pink*, ending with *orange*	6 to 12, a *bright green*, ending with *purple*. 36. The *green* is increased nearly to the intensity of the natural phosphorescence of chlorophane.
2. Green fluor (cubes with bevilled edges), from Wear-dale, Cumberland	Deep *blue* and *purple* ..	20 to 40. First *green tinted*, then violet and *strong purple* light, very beautiful.
3. Pale yellow cubic fluor (Gersdorff)..	*Green* and *violet* light..	12, 24, 36. Yellowish light, of short duration, terminating with purple.
4. Cubic fluor (pale green) from Cumberland	*Light green*, changing to *pink* and *violet*...	12. *Green* and rich *purple* light. 24. *Green* and rich *purple* light, ending with *orange* colour.
5. Cubic fluor (pale green) Cumberland	Rich *purple*..........	12. *Green* and rich *purple* light. 36. Green light and other tints, rapidly changing.
6. Dark purple fluor, Derbyshire	Intense light, variegated with *greenish*, *lilac*, *purple*, and *orange* colours	12. A fragment emitted intense light, *pale greenish tint*, *almost white*. 50. Intensely *rich green light* of short duration.
7. Crystalline massive fluor, Derbyshire..	Dull *green* and *pink*, of short duration	24. *Yellowish light.*

8. Part of the crystallized surface of dark fluor formed in radiated concretions (Derbyshire)	Violet, changing to *pink* and *blue*...........	12. Light of short duration. 60. Strong light, *nearly white*.

Other Examples of Fluors may be added.

9. Cubic fluor, transparent violet crystals	Rich purple	12. No light. 24. Faint purple light.
10. White cubic fluor.	*Blue* and *pinkish* tints..	12. Faint *blue* and *pink*, changing to *yellowish light;* strong light towards the close.
11. Green fluor......	*Violet, pale yellow, pink* and *pale blue* tints ...	12. Bright *green*, changing to *purple*, good light.
12. White portion, from massive purple fluor	*Purple*	12. *Violet*, changing to steady *lemon-coloured light*.

On comparing the native phosphorescence with that induced by electricity in the same substance, the series of colours appeared to differ in nearly every specimen subjected to experiment. While some of the natural fluors can exhibit light of different colours, only a single colour may be conferred by electric action; or, on the contrary, a coloured light, naturally held, may be replaced by a variegated phosphorescence, in which the original tint does not appear.

As the communicated light, in many specimens, obviously increased in variety, beauty, and intensity of tints, when subjected to repeated electrical explosions, the following experiments were made to observe the progression of this property. The green fluor, from Wear-dale, Cumberland, No. 2 of the preceding table, was selected on account of the deep colour of the light given to it. After calcination it was placed in the influence of the following explosions, which were regulated by a discharging electrometer attached to the jar.

The variety of fluor was *naturally* phosphorescent, with deep *blue* and *purple* light, and the following experiments were made upon the same portions:—

1st	Discharge	—faint *purple* phosphorescence when heated.
2nd	„	faint *green*, succeeded by *purple*.
3rd	„	the same colours strengthened, and extended in duration.
4th	„	the purple increased.
6th	„	green light is brighter and stronger.
10th	„	strong green light, the purple rich, and increased in duration.

20th Discharge—both colours deeper, the light remains longer.
40th „ the colours are very rich, purple inclining to red towards the last.
100 explosions rendered the *green* tint very vivid and yellower, the *purple* was increased to intense richness.
160 discharges gave *intense light nearly white* when heated, succeeded by *brilliant green, rich purple,* of long duration; *then yellow,* and tints of *violet.*

This portion has been heated and electrified above fifteen times, the shocks and the temperature being very variable and intense, but the substance does not seem to have undergone any deterioration in its capacity for producing phosphoric light.

The fact of the communication and restoration of phosphorescence may be regarded as established by the examples adduced.

The following tabular view will show the permanence of the property so *communicated.* The minerals selected are the FLUORS enumerated in the preceding table: they were calcined, electrified, and divided into two portions; one of which was exposed to sunlight in glass tubes; the other was wrapt in papers, and kept in darkness.

The portions were heated after they had been exposed to, or secluded from daylight during the following periods:—

No.	After 21 days in Light.	After 21 days in Darkness.	Portions kept in the dark for three months.
1.	Very faint *purple* light	Good light, *green* and *purple*	*Yellow,* ending with *bright purple,* good light.
2.	*Yellow* and *bright purple* light	*Greenish* and intense *purple light*	Variegated tints, *green* changing to *purple,* which predominated.
3.	*Orange* light, some portions had *purple*	*Orange light*	Dull tints, rapidly changing, of *orange* and purple.
4.	Strong *yellow* and *greenish* colour.	Pale *yellow, green, violet,* and *purple,* strong light	Tints of *yellow, orange, pale-green,* and *purple.*
5.	Slightly phosphorescent	*Yellow, green,* and bright *purple*	Dull *green* and *purple.*
6.	*Yellow* and *orange*	Yellow	Strong light, *yellow* and *orange.*
7.	*Orange yellow*	*Pale-yellow,* strong light, ending with *purple*	*Yellow light.*
8.	Yellow light, confined to spots	Strong light, pale *yellow* .	*Green* and *violet.*
9.	*No light*	Chiefly feeble purple light	Fleeting tints of *purple.*

No.	After 21 days in Light.	After 21 days in Darkness.	Portions kept in the dark for three months.
10.	*No light*	Green and purple light ..	*Green* and *purple* light of short duration.
11.	Very feeble on some portions only	Changing tints, ending with purple	*Yellow* and *purple* light.
12.	Faint light	Vivid *yellow*, ending with *purple*	*Yellow* and *purple* light.
	Dark green and yellow tints ...	Brighter *green*, remained with Apatite, which also has been calcined and electrified.	

It appeared that during twenty-one days' exposure to sunlight, that the portions of Nos. 1, 5, 11, and 12, lost nearly all, and Nos. 9 and 10 all their phosphorescence; also that Nos. 1, 4, 6, 7, 8, and 12, had the colours modified during the time of exposure, as compared with their phosphorescence given in Table 1: orange and purple tints seem to be assumed by time.

The third column gives the phosphorescence conferred by electricity, which remained after an interval of nearly three months.

The effects detailed in this and in my previous communication are those produced after the destruction of the phosphorescence, which naturally existed in the minerals, by the application of a strong heat: another class of effects are now to be introduced, resulting from the electrization of bodies still retaining their native phosphorescence.

The result of this obvious extension of the inquiry was a series of magnificent colours, and an exaltation of the original phosphorescence, of which it is very difficult to convey an idea.

Crystals of the FLUORS, whose localities are given in Table 1. (p. 269), were used in the following experiments, upon the induction of additional phosphorescence by electricity. The same order of the substances is preserved in the present table.

Minerals.	Colour of Natural Phosphorescence.	No. of Electrical Discharges.	Heated to decrepitation—colour of superinduced Phosphoric Light.
1. Green fluor. Yellow green portion ...	*Pink* and *orange*..	24	*Green*, bright *blue*, intense rich *purple*, with after tints of pink; very strong light.
Bluish green portion ...	Light, nearly white, then *lilac*, *pink*, and *orange*	16	Vivid *emerald green*, then *purple*, finally *pink*.
2. Green fluor ...	*Cobalt*, *blue* and *purple* light....	20	Intense *purple*, several portions gave deep *orange* light; after it had been heated to redness for some time, it was still luminous with a *bluish* light.

Minerals.	Colour of Natural Phosphorescence.	No. of Electrical Discharges.	Heated to decrepitation—colour of superinduced Phosphoric Light.
3. Yellow fluor ..	*Violet light*, rather feeble	16	*Lemon-yellow*, *violet*, and several colours changing during decrepitation.
4. Light green fluor	Pale *green*, *pink*, and *purple*	16	*Green*, *straw-yellow*, *purple*, *orange*, and several other colours.
5. Light green fluor	Rich *purple*	20	Intensely vivid *blue*, *pink*, and *purple* light.
6. Dark purple fluor	*Green*, *pink*, *purple*, and *orange*.....	20	Dark *green*, *lemon-yellow*, *purple*, and *orange;* the light from some portions was very strong, and *nearly white*.
7. Dark fluor ...	*Greenish* and *pink tints*	14	Peculiar intense light of *straw whiteness*, then *greenish*, dull *orange*, and *pink* tints.
8. Dark fluor ...	Faint *violet* and *pink*	12	*Greenish yellow* light, *yellow*, *pink*, and *orange* colours.
9. Cubic violet fluor	*Purple*	12	Intense *azure-blue*, (some *yellow*,) very vivid light *nearly white*, from points of the fragments.
11. Green fluor ...	*Violet*, and *orange-yellow*	12	Brilliant *emerald-green*, *violet* and *orange*, very strong light, finally faint purple; these were striking changes.
Compact dark purple fluor .	*Violet* and *pink* ...	12	*Green*, *yellow*, *pink*, and *orange* light.
Apatite (phosphate of lime)	Brilliant *yellow-green*	12	Green, *yellower*, *olive* and *orange* tints, very strong light.

The additional phosphorescence appears to differ in colour from the natural quality, and to be evolved at a lower temperature, and blends into the previous natural phosphorescence, which also is increased in strength and duration.

These experiments may suffice to show that minerals, which naturally are phosphorescent when heated, do not necessarily exhibit the maximum degree of this peculiarity, but may have it exalted by artificial means. The phenomena are so far increased, that specimens of fluor, which held dull or undefined phosphorescence, have been rendered, by electricity, equal to the most eminent class of phosphorescent fluors; some varieties, indeed, rivalling the intensity of chlorophane, or Siberian fluor. The means of increasing the natural phosphorescence in bodies has not, as far as I know, been heretofore pointed out.

Portions of these electrified minerals were kept in darkness,

and examined after a lapse of fifty days. They still possessed increased phosphorescence: in some the order of tints was still the same; in others a change was observed, and the orange tints evidently prevailed.

On the Influence of Structure upon Phosphorescent Bodies.

As the mineral phosphate of lime, called APATITE, possesses naturally an intense degree of phosphorescence, other forms of this chemical compound were experimented with.

Phosphate of lime was precipitated by alkalies from solution in muriatic acid: it was collected, and allowed to aggregate by carefully drying it; the temperature was afterwards raised, but there was no appearance of light. It was then calcined: small compact hard lumps and powder were electrified by twenty discharges from two feet of surface, but no phosphorescence was induced.

Apatite was in like manner dissolved, precipitated, dried, calcined, and electrified, but no phosphorescence was induced.

Phosphate of lime calculus was electrified and heated, *no light* appeared: being calcined to redness, twelve discharges were made; the fragments when heated evolved differently coloured light; the *yellow*, *green*, and *orange* colours were increased by twenty discharges, the light also being rendered stronger. It is evident that change of texture would result in this case by the destruction of the organic matter diffused through the earthy mass.

As these bodies might be regarded as identical in chemical respects, their great difference in phosphorescent power is due, in some way, to the mechanical condition of the bodies: cohesion, arrangement of particles, texture, and extent of surface, are all circumstances which may influence the result.

The solidity of fluor spar was destroyed by levigation, but phosphorescence was evident when the powder was heated.

Crystallized fluor spar (*fluoride of calcium*) was powdered and dissolved in muriatic acid, from which it was precipitated by ammonia, then dried, and calcined at a red heat, without exhibiting light. Electricity also did not confer phosphorescence.

This muriatic acid solution deposited after some time small fragile crystals of *fluoride of calcium;* these, when collected and dried, lost their form; when heated, they slightly decrepitated, and were phosphorescent.

There are certain classes of bodies which exhibit decided differences in this relation to light. All the calcareous minerals, as the carbonates of lime and the fluor spars, may be rendered phosphorescent, while none of the specimens of quartz, siliceous, and aluminous minerals resorted to, either possessed naturally, or would receive phosphorescence.

I ought to mention, that in several instances I have observed a slight return of phosphorescence by time after it has disappeared. One example was in a crystal of fluor spar which had been calcined entire: after it had been deposited in darkness for some months, it was found to have regained feeble phosphorescence; and others, which gave no signs of light when heated after the calcination, yet appeared luminous when heated after a long seclusion from light. Other substances, besides these, might be adduced, whose feeble but constant phosphorescence cannot be the result of accidental circumstances alone. Common scollop-shells seem to inherit a remarkable phosphoric structure, as also the substance of calcined oyster-shells and cuttle-fish bones, especially when exposed to light for a short time; instances have occurred, when, after a strong calcination of these substances, they appeared visible, although they were heated many times and kept in darkness. These degrees of luminosity, although not likely to be confounded with the previous experiments, are yet pointed out, because they were guarded against in the following experiments. After all, the effects of temperature may be far more influential than can be traced at present, being perhaps as capable of disposing structure, as requisite in the ultimate development of the phenomena.

From these assigned reasons I conclude, that phosphorescence is dependent upon and modified by the structure and mechanical conditions of the substances under investigation.

The beautiful results thus produced by electricity naturally led me to vary its mode of application; and as, in the experiments described, the electric discharge had been passed directly over the substances, I now inclosed them in glass

tubes, with the view of deducing, if possible, the proportion of effect due to radiant matter passing off from the sparks: I found that phosphorescence was effected, although glass intervened, as the following facts will prove.

1. Portions of calcined scollop and oyster-shells were hermetically sealed up in small glass tubes placed within a longer tube, and the electric discharge effected its passage over the outsides of these little tubes.

After one hundred and sixty discharges of a jar, these substances were found, when heated, to be phosphorescent.

2. Six small tubes, sealed at both extremities, containing calcined chlorophane, calcined cuttle-fish bone, and calcined scollop-shells, were introduced into a glass cylinder, open at both ends: large shot were rolled in to keep the small tubes together, and to conduct the electricity.

This glass cylinder was then introduced into a glass tube of larger diameter, the space between being filled with portions of calcined oyster-shells and different fluors; the glass cylinders were placed horizontally.

Two hundred and twenty-five discharges of a jar were then made through the inner tube; the fragments contained between the two cylinders were decidedly phosphorescent when heated.

The tubes which had been thus strongly electrified had their contents examined.

The chlorophane fluor in two tubes was *not* phosphorescent.

The calcined oyster-shells had acquired an *orange-pink* and *bluish* light.

Other two tubes had calcined scollop-shells, which instantly gave, when heated, a *flame-coloured* phosphorescence, with *pink* and *purple* colours.

These experiments were necessarily very laborious; fewer explosions produced degrees of the effects; but I did not feel satisfied with giving the less decided results of thirty or forty explosions. The two experiments, given above, required about 3000 revolutions of a large cylinder machine in good action.

I then resorted to voltaic electricity, as a source of phosphorescent power, although, at first, any effect might be supposed to be precluded, either by the insulating power of the mineral, or when the quantity and the intensity of the electricity

were increased: for, unless the intense heat at the interruption of the circuit were avoided, it would destroy the phosphorescence which might be produced by the vivid light and continuous current.

Portions of calcined oyster and scollop-shells were submitted to the voltaic light from points of charcoal attached to the extremities of a voltaic battery in good action, of a hundred pairs of four-inch plates; the discharge being repeatedly intermitted, so as to resemble a series of ordinary sparks, guarding against the elevation of the temperature of the tube and the inclosed fragments. After ten minutes' exposure, they seemed to have acquired phosphorescence through the glass; for, when heated, they appeared faintly luminous.

Common calcined purple fluor did not appear affected by close proximity to the voltaic discharge.

Calcined oyster-shell powder, exposing a large surface to the direct light, was phosphorescent when heated.

Calcined purple fluor was placed in a tube, the voltaic discharge likewise effected in the tube over and through the fragments, which were thus influenced by the voltaic discharge and currents, from charcoal and from metal poles, but no phosphorescence appeared when the substance was heated.

A silver capsule, forming the termination of one pole, was strewed with calcined fluor spar; the charcoal extremity of the other pole traversed the metal plate, causing sparks and silent discharges to pass repeatedly through the portions of mineral; but the fluor was not luminous when heated.

Calcined scollop-shell, by the same arrangement, was rendered phosphorescent upon the subsequent application of heat.

So that there are great differences with respect to the induction of phosphorescence in these bodies by ordinary and by voltaic electricity.

On the Colouration of Fluor Spars by the Action of Electricity.

It was announced in the former communication, that certain fluor spars, rendered white by calcination, became coloured after they had been electrified—a distinct blue colour appearing upon specimens which originally were deep purple. As the cause of colour in these minerals has often been a subject

of *chemical investigation,* I may be allowed to give some experiments, which present this subject in a new point of view.

The fluors are those used in the former experiments upon phosphorescence; they were all rendered white by heat.

Green fluor from Cornwall, after calcination, was colourless, nearly transparent, and in very small splinters, which obtained a *pink* tint after thirty-two discharges of a large jar.

Crystal of No. 2, in the first Table, appeared naturally pale green by transmitted light, but blue by reflected light; heated to redness, it became colourless and opalescent: forty discharges caused *blue* tints upon the edges.

Large lemon-coloured crystal of fluor was opaque white after calcination; thirty-six discharges produced decided *blue* and *lilac* colours.

Cumberland cubic fluor (No. 5.) was purple by reflected light; the white opaque calcined fragments were rendered decidedly *pink* by thirty-six discharges.

No. 6. The purple cubic fluor from Berealston, Cumberland, when viewed by transmitted light, showed bands of blue and violet; by calcination a difference in structure was evident, by the alternating opaque planes: fifty discharges gave a faint *blue* to some portions only.

Dark purple fluor became white by calcination, and received a bluish tint by twenty-four discharges.

Twelve discharges rendered No. 8 bluish; sixty electrical explosions upon the calcined fluor caused it to appear *blue.*

No. 9 was opalescent by calcination; it acquired a faint *pink tint* by twenty-four discharges of electricity.

These various tints preclude the idea of fallacy, from the deposition of foreign matter by the electrical discharges: in one experiment, when nearly one hundred discharges had been made over fragments, and metal was deposited in the track of the discharge, it still preserved its metallic lustre. Hence there appears every reason to conclude, that the colour induced is the effect of structure alone.

These tints so produced were not permanent; some portions in the light lost all colour in a few days; other portions, kept in darkness, showed these external tints after two months.

The pink tints are strongest upon the edges, and soften upon the planes.

The blue tints are strongest upon the angles of the fragments, and upon the solid angles of the fissures.

And I may call attention to a similar distribution of colours to be observed in large crystals and specimens of massive dark purple fluor, which have their colours unequally conferred upon the surface, some portions being nearly white, other parts having faint tints of violet, purple, or blue; while towards the edges and solid angles of the crystals, the colours increase to intensity.

If massive dark fluor be broken into fragments, some of those may be selected which are scarcely tinted, except upon the edges and surfaces of the differently crystallized portions just separated, and upon these parts intense colour resides.

I took a large mass of purple fluor, weighing several pounds, and a portion was broken from a large cubic crystal, which was deep purple in the solid edges and angles, while the internal part near to the centre of the external planes was nearly white, the crystals having a mottled appearance; the white portion was highly phosphorescent: calcined in a crucible to redness, and subjected to electricity, *no colour* was produced, although it became highly phosphorescent.

Fluor spars with different colours were electrified in their natural state, but no alteration or addition of colour was remarked, excepting the dark purple fluor, whose depth of tint was increased.

It is a curious circumstance, that those portions of fluor which are naturally the most coloured, are also, when rendered white by heat, recoloured with the greatest facility by electricity; and as the latter power would appear to confer colour only by modifying in some way the arrangement of the particles, may not natural fluors owe their colours to structure? And may we not be allowed to suppose that nature used the same means, and that ELECTRICITY confers colour and phosphorescence in the first instance? Both the natural and the induced colours are destroyed by heat; and the colour, like the phosphorescence, may be repeatedly restored by electricity.

I may now, perhaps, venture to draw the following conclusions from the experimental details advanced, which have proved electricity to be efficient in the restoration of phosphorescence.

From the very feeble phosphorescent effects obtained by exposing substances to the intense light of the discharge, and also to the constant current of voltaic electricity, it is inferred that light, and great quantity of electricity, are not essentially necessary, but that the effects are due to electricity of great intensity, and hence the influence of the discharges of ordinary electricity.

As electricity itself does not permeate glass, the effects upon substances hermetically sealed up may be thus explained: when the outsides of the glass tubes are electrified by the intenseness of the discharge, a corresponding state is simultaneously induced upon the interior surface, and the contiguous substances are rendered phosphorescent by the so excited electricity.

The colours of bodies, generally, are believed to be due to peculiar structures capable of decomposing light, and reflecting particular coloured rays.

Since by experiment I have shown that colorific structure obtains in certain varieties of colourless fluors, as the result of intense electrization; and as electricity, under various conditions, manifestly commands the relations of molecules and masses of matter, by effecting, destroying, or suspending their combinations, may it not be advanced, that when matter (such as calcined fluor) which is not phosphorescent, is exposed to electric discharges, that they cause vibrations of the particles, which, being repeated with every discharge, gradually modify the structure, and bring it into a peculiar state? May not the action of heat allow this state to return to what it was originally; and from the vibrations of the atoms of matter in changes of structure proceed the undulations fitted to produce light?

This explanation appears to me to be in perfect conformity with the received laws and actions of light, heat, and electricity; and also with the conditions of the earthy substances.

Other causes, competent to these alternating changes of structure, may exist besides heat and electricity, but the above view seems to apply to the phenomena of phosphorescence generally. The alteration of phosphoric colours after some time may be regarded as consequent to the variations of atmospheric temperature having been sufficient so far to alter

the position of the particles, that when heat is ultimately applied, the vibrations produced are fewer and comparatively weaker.

Note.—Since my previous communication, I have been informed of a work devoted to phosphorescence, and also of an article in Gmelin's Chemistry, both in the German language *. On referring to the abstract contained in parts of the latter work, it appears that electricity has been employed with phosphori; and that certain bodies, phosphorescent by heat, whose property had been destroyed by calcination, had the property restored by electric shocks: any doubt upon the subject might perhaps be decided by consulting the original authority. My attention has also been called to some experiments by Mr. Skrimshire (Encycl. Metrop., Art. *Electricity*, §. 177), in which transient phosphorescence was conferred upon different substances by drawing sparks from them, or passing electrical discharges over them. The eyes were kept closed until the sound of the discharge was heard, and the light then observed. I am not acquainted with the detail of these experiments, and my own train of investigation was conducted independent of them, and was nearly concluded before I became aware of any similar inquiry.

ON THE DARKNESS BETWEEN THE PRIMARY AND SECONDARY RAINBOWS.

By Mr. AINGER.

[In a Letter to M. Faraday, Esq., F.R.S., &c.]

My dear Sir, 10, *Doughty-street, Oct.* 1830.

In consequence of your remark a few days since, that you had not seen a satisfactory explanation of the darkness between the primary and secondary rainbows, I have referred to several of the most accessible works on the subject, and I find that the phenomena of the rainbow are in general very imperfectly, and in many cases very incorrectly, described. I do not discover that the darkness in question, though sufficiently obvious, is ever alluded to; nor does it appear to me

* Placidus Heinrich, Phosphorescenz der Korper, vol. iv. Gmelin's Handbuch der Chemie, part i.

that its existence is easily deducible from the descriptions usually given of the circumstances under which the rainbow is produced.

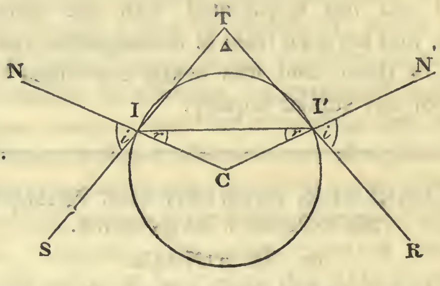
I will not occupy your time with an enumeration of the various mistakes which seem to exist on this subject, but refer you at once to the Traité de Physique of M. Biot, whose description and analysis, as far as I am able to appreciate them, are the best I have seen. I think, however, with great deference, that even these are, to a certain extent, imperfect and incomplete. M. Biot says—

'The phenomenon of the rainbow is produced by the coloured spectra which issue from different drops of water after two refractions, separated by one or two intermediate reflexions. But how,' he proceeds, 'does the superposition of these partial spectra compose the colours of the bow and determine its magnitude? This is what we have to examine.

'To do this simply, let us first consider a single incident ray of simple colour—for example, red; then, supposing that it emerges from the drop after a certain number of reflexions and refractions, let us calculate the angle it forms with its primitive direction.

'Let S I, Fig. 1. be such a ray entering at I and escaping at I′ after a

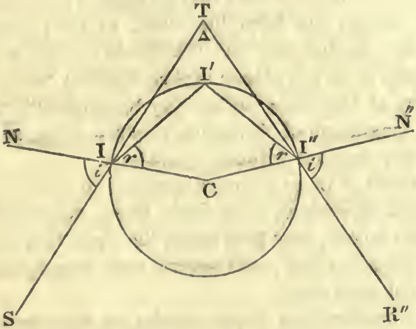
Fig. 1.



second refraction, without intermediate reflexion. From the centre of the globe draw C I N, C I′ N′, which will be perpendicular to the surface. Then S I N will be the angle of incidence, which we will call i; and C I I′ will be the angle of reflexion, which we will call r. Further, in consequence of the symmetry of the figure, the interior incidence at I′ will also be r, and the emergence will be i. Prolong the incident and emergent rays till they meet at T, forming the angle I T I′, which will be the deviation produced by refraction; we will call this Δ. Now it will be easy to find its value in functions of the angles i and r; for in the quadrilateral C I T I′, all the angles are known, except Δ. In short, the angles at I and I′ are both equal to i; further, the triangle C I I′ being isosceles, the angle I C I′ is equal to $180° - 2r$; then, since the sum of the angles of a quadrilateral are equal to four right angles, we have $\Delta + 2i + 180° - 2r = 2, 180°$, or $\Delta = 180° + 2r - 2i$.

'Let us next consider two refractions separated by one reflexion; the same construction (see Fig. 2.) and reasoning will apply: only the angle

Fig. 2.



I C I″ will be double I C I′, that is, equal to 2 (180° − 2 *r*); thus we have Δ + 2 *i* + 2 (180° − 2 *r*) = 2, 180°, and Δ = 4 *r* − 2 *i*.

'Generally, if the ray have *n* successive incidences in the interior of the globe, the angle I C I″ becomes *n* (180° − 2 *r*).

'The angular deviation will be constant for all rays of the same nature, which penetrate the globe under the same incidence; but the incidence changing, that also will change. To form a clear idea of these variations, let us first consider the case in which the ray suffers but one internal reflexion; after which it escapes from the globule into the air. Then, if we calculate the amount of deviation for several parallel rays, incident at small distances on various parts of the surface, it will be found that the deviation is nothing under a perpendicular incidence, in which the ray passes through the centre of the globule. The deviation gradually increases to a certain limit of incidence, which is about 54½° for the red rays, so that a pencil of these rays entering parallel at I (see Fig. 3.) under this incidence, and being once reflected from

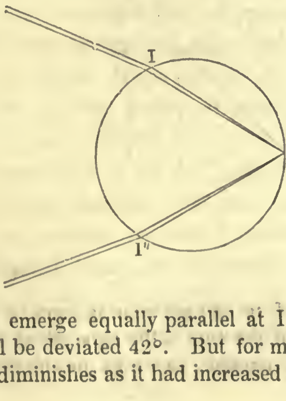


Fig. 3.

the inner surface, will emerge equally parallel at I″, though the general direction of the pencil be deviated 42°. But for more considerable incidences, the deviation diminishes as it had increased; and this diminution

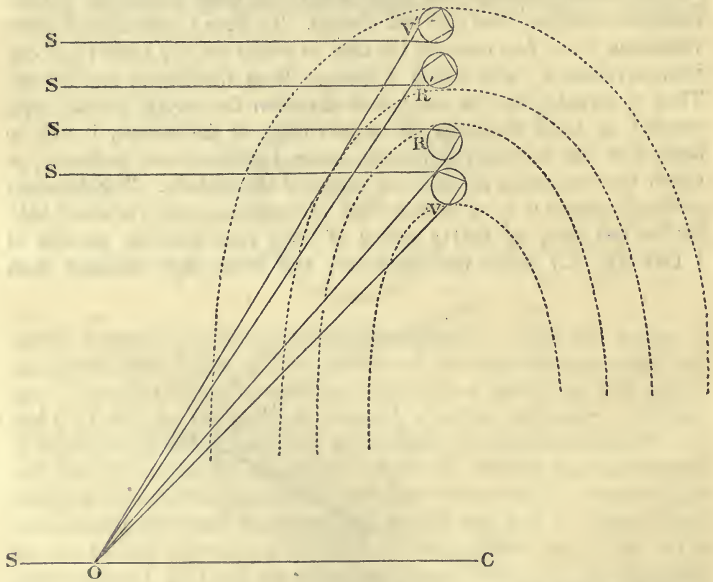
continues as far as the last rays tangent to the globule. Now if these rays are received at such a distance from the globule, that this last may be considered as a point, it is clear that all those which belong to unequal deviations will diverge one from the other, as their distance from the globule increases; so that they will become too feeble to give a perception of the globule to an eye placed in their course; while that eye would be affected even at the same distance by the emergent rays, which correspond to the maximum of deviation, because, being parallel, they are transmitted to any distance without separation.

'Suppose a series of these globules disposed circularly in such manner that the refracted rays which issue from them, and which are supposed to be of the same colour, may thus reach the eye; they will produce the sensation of a luminous line; and several such series placed side by side will produce a coloured band.

'The same considerations apply equally to the cases in which the refractions and reflexions are more numerous; there is always for each a limit at which the rays of a small pencil will emerge sensibly parallel, and will be transmitted without becoming enfeebled.

'To develop the consequences of these results, suppose that an observer placed at O (Fig. 4) views a large cloud composed of spherical

Fig. 4.



drops of water; draw from the centre of the sun through the eye the line S O C, to designate the direction of the rays, which we will for the present suppose to be exactly parallel, as would be the case if the sun were a

point infinitely distant. This being granted, there will be, from the anterior surface of the drops, a partial reflexion of all the colours which compose the incident light, and which will form a whitish tint, spread over the whole surface of the cloud; but, besides this, there will be seen two concentric arcs, coloured with all the colours of the spectrum. For if through the eye O be drawn a right line O V, forming with O C an angle of 40° 17′, and that it be supposed to revolve round O C, describing a conical surface, all the drops which are found in this surface will have the position in which the most refrangible violet rays, after having suffered two refractions and one reflexion, will emerge parallel, and will reach the eye at O, and this will not take place in any other part of the cloud; in virtue, therefore, of these rays alone, the spectator will see upon the cloud a violet bow, of which O C will be the axis, and O the centre. There will, in like manner, be an infinite number of concentric arcs exterior to the last, each formed by one description of simple rays; and as the rays become less refrangible, their arcs will be of larger diameter, so that the largest, composed of the extreme red, will subtend an angle R O C of 42° 2′. Thus the total extent of the coloured band will be 42° 2′ – 40° 17′, or 1° 45′, the red being without, and the violet within.

'It will be the contrary after two reflexions. If the lines O R′, O V′, be drawn, making with O C angles of 50° 59′ and 54° 9′, and then made to revolve round O C as an axis, the first will intersect all the drops, which, after two refractions and two reflexions of the red rays, will transmit them in parallel lines to the eye; and the second will determine the analogous limit for the extreme violet rays. Between these two arcs there will be others of all the intermediate colours of the prism, and they will form a second coloured band, having a width of 54° 9′ – 50° 59′, or 3° 10′. This band will have its colours in an order the inverse of the first, that is to say, the red will be inside, and the violet outside; and the distance between the two red arcs will be 8° 57′.'

In this account it is assumed that no sensible effect is produced on the eye of the spectator, after one or two internal reflexions, except by those drops which are included within the angles subtended by the coloured bands; although it is said that the whole expanse of the shower will exhibit a degree of whiteness, in consequence of the reflexion of the sun's rays from the anterior surfaces of the drops. These two statements are, to a certain extent, I think, irreconcilable; for if the rays reflected from the internal surfaces would be rendered insensible by their divergence, so also, I conceive, would those reflected from the external surfaces. The dispersion of the former will not account for the supposed differ-

ence, because each drop in the shower (with an exception to be presently noticed) would transmit rays of every coloured light, producing by superposition with themselves, and with the rays from other drops, the sensation of white light, differing only in brilliancy from that reflected at the outer surfaces. Mere divergence will not, I think, affect, to the extent supposed, the apparent quantity of light derived from numerous points at a great distance. It is true that a parallel pencil would appear very bright at a distance, which would render a divergent pencil, of equal magnitude, quite insensible. But, in the case under consideration, it is not a single pencil of parallel rays which is compared with another of divergent rays; the eye views a luminous space, part of which is so distant, that a thousand drops might be contained in a line having an inappreciable angular value. If the light from each of these thousand drops proceeded in parallel lines, the eye, although it would receive all the light transmitted by some one drop, would lose all that was reflected by the others. If, on the contrary, the light diverged from the drops, the eye would receive only a very small portion of the light from the one drop, but it would now receive an equal portion of the light reflected from each of the remaining nine hundred and ninety-nine drops; the whole of which proceeding from a space of no sensible magnitude, would produce a general impression of illumination, notwithstanding that the light from any single drop might have been invisible. An instance of the effect produced by numerous simultaneous impressions, each individually imperceptible, is furnished by a room in which silkworms are feeding. A hundred of these animals emit no sound that the ear can detect; but the noise of a very large number in the act of eating has considerable intensity. In a large and crowded theatre no individual is heard to open a play-bill, or turn the leaf of a book; but if any circumstance occasions a large portion of the audience to do either of these nearly at the same instant, a noise is produced like the rushing of a torrent. The correct statement, therefore, I think, would be, that in addition to the reflexion from the anterior surfaces, which is common to all the drops in the shower, every drop, with the exception before alluded to, is rendered visible by

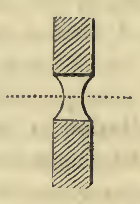
light twice refracted and once or twice reflected. The drops which are not thus made sensible, are those contained in the space subtending the angle between the red edges of the primary and secondary bows, which space is therefore comparatively dark, and constitutes the hitherto unexplained part of the phenomena. The rainbow, it will have been observed in the account of M. Biot, and I believe in all others, is described as an insulated coloured band, rendered visible by the parallelism of the rays, which emerge under a particular angle of incidence, without any allusion to the illuminated space, of which it is the coloured edge; the colour being nearly analogous to the coloured edges given to luminous objects when viewed through lenses or prisms. The parallelism of the rays at the maximum angle of deviation adds greatly to the brilliancy of the bow, as compared with the other parts of the illuminated space, and contributes materially to its distinctness, but is not, I think, properly called the *cause* of the bow. The brilliancy not only of the bow, but also of the illuminated space to some distance within it, is further increased by the circumstance, that the drops in this situation return two sets of rays to the eye, arising from light incident on both sides of the angle, producing the maximum deviation.

The only parts of the theory of the rainbow, which appear to be generally understood, are the circumstances which determine the limits and arrangement of the coloured bands, and the various angles at which the several colours arrive at their maxima or minima * of deviation. It is not, I think, commonly imagined that the circular streak of red is merely part of a circular red space, the interior of which is rendered undistinguishable by the superposition of other colours. That this is the case may be made evident by observing the progress of the rays through a sphere or cylinder of water, placed at various angles, with a luminous body, and the eye. If it be desired to

* Hitherto nothing has been said about a minimum of deviation; but it will presently be seen that the primary bow is formed by those rays which suffer a maximum of deviation as compared with its illuminated space, while the secondary bow is formed by those rays which suffer a minimum deviation as compared with its illuminated space. This distinction must be borne in mind, because the rays which, in the secondary bow, are, by comparison with the rest of the illuminated space, said to be suffering a minimum of deviation, do, in fact, deviate more than those which, in the primary bow, are said to be suffering a maximum.

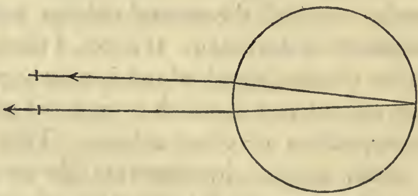
make this experiment without the intervention of glass, as it is not easy to obtain either a sphere or cylinder of water unsupported by a containing vessel, an equivalent form may be procured by placing a drop between two small surfaces, as in Fig. 5., near the middle of which the tangents of the opposite

Fig. 5.



surfaces will be parallel, which is, of course, all that is required. I used the ends of two black lead pencils for this purpose; but, finding that a thin glass bulb did not sensibly affect the results, I made the observations with that as being more convenient and manageable. In this way it will be seen, that, although the rays transmitted near the maximum and minimum angles of deviation are more brilliant than the others, yet the difference is one only of degree, and is not sufficient to render it probable that the latter are made invisible by mere divergence. The ray, after one internal reflexion, becomes distinctly visible as soon as it ceases to be confounded with the ray obtained by reflexion from the first surface, as in Fig. 6.;

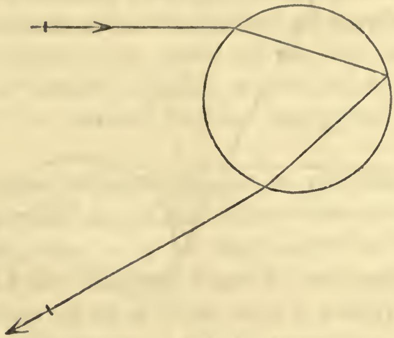
Fig. 6.



and it continues to increase in brightness, but without sensible colour, till the bulb arrives at the maximum angle of deviation, for the violet rays of the primary bow, as in Fig. 7.; after which the light becomes more feeble and coloured, till it vanishes altogether in a faint red. The same sort of progression is observed by commencing with a position of the bulb directly between the eye and the light, in which there will be a

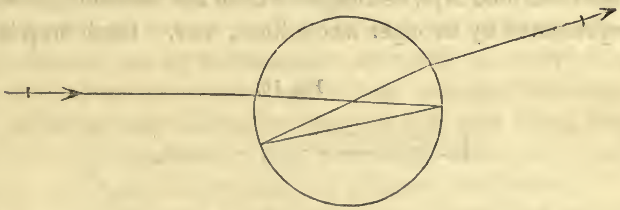
transmission of rays after two refractions and two reflexions, though they will be rendered insensible by the superior

Fig. 7.



quantity of light immediately refracted. But so soon as the angle is altered to avoid this, an image of the luminous body is perceived, which arrives by the course shown in Fig. 8. The

Fig. 8.



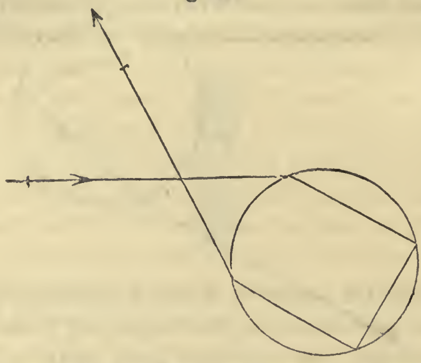
deviation goes on diminishing* till the bulb approaches an angle equal to that formed by the blue edge of a secondary bow; the light then changes to the various colours of the spectrum, and escapes as before in a faint red, Fig. 9.

I have said, that in the preceding observations the image of the luminous body was uncoloured, except when the bulb occupied angular positions similar to those of the coloured parts of the primary or secondary bows. It is, nevertheless, certain that in all the other positions the light must have been more or

* The expression is liable to be misunderstood. The deviation is said to be nothing when the ray returns exactly upon its own path; consequently the deviation is a maximum, or 180°, when it preserves its direction perfectly unchanged.

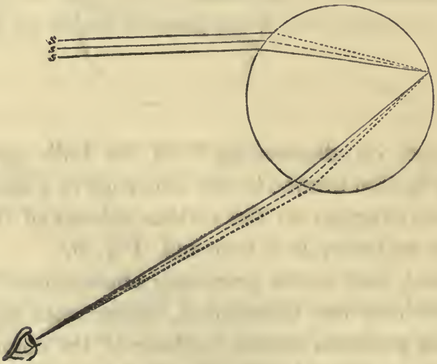
less dispersed, and therefore that the images are rendered colourless by the superposition of various coloured spectra

Fig. 9.



formed by the same drop, but so nearly coincident as to leave exposed no sensible colour. The course of the rays must have been something like that shown in Fig. 10., where the faint whole lines may represent red, the broken lines yellow, and the dotted lines blue rays, decomposed from the incident light which is represented by stronger whole lines, *s s s*. Each drop in the

Fig. 10.



luminous space would return spectra of these three colours, although they would be decomposed from different rays of white light, arriving at the drop under different angles of incidence. Their spectra would not perfectly coincide; but, if their

failure to do this be not perceptible in a bulb one inch in diameter and near to the eye, it is not surprising that the light from the rain-drops should appear colourless. The light at length becomes coloured, partly in consequence of the coloured rays arriving successively at the position of parallel emergence and greatest brilliancy, as stated by M. Biot, and partly in consequence of the drops failing successively to send to the eye certain of the colours, the blue failing first, and the red last.

From these observations it appears to me, that the phenomena of this beautiful apparition are no where detailed as perfectly and comprehensively as they might be; and I have endeavoured, in the diagram, Plate 4., to convey a clearer and more popular notion of them than is usually to be found. Here the irregular figure A B C D represents a section of a shower of rain, taken in any plane passing through the eye of the spectator and the sun; the former being at E, the latter infinitely distant on the line E C. Under these circumstances, coloured rays, formed by two refractions and one internal reflexion, will reach the spectator from every drop in the cone whose section is E F G; but the colours will be nearly neutralized by superposition everywhere, except at and near the surface of the cone, where they will give the impression of the primary bow; the cone of red rays being larger than that of the orange rays, this larger than the cone of yellow, this again larger than that of green, and so on.

Coloured rays, formed by two internal reflexions and two refractions, will reach the spectator from every part of the shower, except the cone E H I, the colour being, as before, neutralized by superposition in every part, except at and near the extreme edges of the illuminated space, where each colour will successively overlap the last, in the same order as before, producing the secondary bow.

The space between the two cones, E F G and E H I, returns no light after one or two internal reflexions, and is therefore comparatively dark, though the difference is by no means so great as, for the sake of distinctness, it has been made in the engraving.

In confirmation of the view here taken of the causes which

produce the comparative darkness in question, it may be noticed that the violet edge of the bow is extremely ill defined as contrasted with the red edge. In some cases, the colour can with difficulty be traced beyond an indistinct green, the remainder seeming to be merely the commencement of the blue colour of the atmosphere. According to the descriptions usually given, this difference would not exist; the parallel emergence of the violet rays ought to produce a very distinct line of violet light, because the red and yellow rays are in that situation subject to considerable divergence. But the fact is, that notwithstanding their divergence, they are far from imperceptible; and, mixing with the parallel violet rays, they confound and almost obliterate them, or rather unite with them to produce the impression of common compound light.

The superior brilliancy of the primary bow is not, I think, quite accurately accounted for when it is ascribed to the circumstance of its rays having suffered but one reflexion; for the double reflexions are made at angles so favourable, as nearly to counterbalance this difference. I apprehend that the faintness in the latter case is owing to the following causes:—

1. That the rays which suffer the maximum deviation in the primary bow arrive at the surface under a much smaller angle of incidence than those which suffer the minimum deviation in the secondary bow; the latter, therefore, are more copiously reflected from the first surface, and enter the drop in much smaller quantities.

2. That the angle at which the ray is afterwards refracted from the inner surface to the air, is, in the secondary bow, similarly favourable to reflexion, and unfavourable to refraction, so that only a small portion of the already reduced quantity of admitted light is refracted.

3. That the extent of the dispersion is increased by the second reflexion, as is shewn by the greater width of the secondary bow.

The last circumstance may, perhaps, be considered as included in the expression that the faintness is owing to the second reflexion, though it is not very obvious that such is the meaning.

These observations having been suggested by your remark, I beg leave to address them to you, and to place them at your disposal.

Remaining, my dear Sir, yours very respectfully,

ALFRED AINGER.

To M. Faraday, Esq.
&c. &c. &c.

ON THE MODE OF ASCERTAINING THE COMMERCIAL VALUE OF ORES OF MANGANESE.

By EDWARD TURNER, M.D., F.R.S. L. and E., Sec. G.S.
Professor of Chemistry in the University of London.

THE analysis of the ores of manganese, when pure, is exceedingly simple. The operator need only, by well known methods, determine the water which the ore contains, and the oxygen which it loses in being converted into the red oxide. Its degree of oxidation, on which the commercial value of ores of manganese so essentially depends, may then be readily inferred.

But when impurities prevail, as they almost always do, more or less, in commercial manganese, the analytic process is complex and troublesome; and the presence of iron, which is rarely absent, renders an exact result by the ordinary modes of analysis almost impracticable. For, as I have elsewhere stated*, when peroxide of iron is strongly heated in mixture with peroxide or deutoxide of manganese, oxygen is given out by the former as well as by the latter; and, accordingly, the oxygen lost by heat ceases to indicate the nature of the manganese. A moderately correct allowance for the quantity of oxygen emitted by the iron under these circumstances would be difficult, even after ascertaining in the moist way the quantity of iron contained in the ore; since the constitution of the resulting oxide of iron, as well as its uniformity, is probably variable, and, at all events, is undetermined. The chemist would, therefore, have to ascertain separately each constituent

* Brewster's Journal of Science, N.S. ii. 213.

of the ore, and consider the loss as oxygen belonging to the manganese,—a method not to be trusted in a complicated analysis, and which would be wholly inapplicable if the iron, as contained in the ore, should happen not to be uniformly oxidized.

I was led to reflect on these difficulties in consequence of being requested, some months ago, to examine a considerable number of different ores of manganese, the object being solely to ascertain the relative quantities of chlorine which an equal weight of each ore was capable of supplying; and as the method to which I had recourse gives such information with rapidity and precision, I have drawn up a short description of the process; not from any novelty being attached to it, but in the belief that it may be useful to persons engaged in a similar inquiry.

The method, in principle, consists in dissolving a given weight of the ore in muriatic acid, condensing the chlorine in water, and, by some uniform measure, estimating the quantity of chlorine relatively to an equal weight of pure peroxide of manganese, selected as a standard of comparison. The substance first used with this intention was a solution of indigo; but a weak solution of green vitriol, employed by Mr. Dalton for ascertaining the strength of bleaching powder, was found to be more precise in its indications.

The method of manipulating is as follows:—About ten grains of the ore in fine powder is introduced into a flask capable of containing about an ounce of water, and into its neck is fitted by grinding a bent tube about two inches long, which conducts the chlorine from the flask into a tube about sixteen inches in length, and five-eighths of an inch wide, full of water, and inverted in a small evaporating capsule, employed as a pneumatic trough. The apparatus being adjusted, the flask is half filled with concentrated muriatic acid, the conducting tube instantly inserted, and heat applied by means of a spirit-lamp. The air of the flask, together with the chlorine, is then collected, the greater part of the latter, if the gas is not very rapidly disengaged, being absorbed in its passage; and, consequently, the receiving tube, at the close of the process, will be about half full of gas. When the ore is completely dissolved, the last

traces of the chlorine are expelled from the flask by muriatic acid gas. In order that the chlorine thus collected may be entirely absorbed, the aperture is closed by a ground stopper, or, still more conveniently, with the finger, and the gas is well agitated until the chlorine is wholly absorbed. As the solution in the inverted tube may become too saturated to dissolve all the chlorine, it is convenient to fill a pipette with pure water, and, with the aid of the mouth, force a current to ascend into the tube, and thereby cause the heavier solution to flow out into the capsule.

The absorption being complete, the solution of chlorine is introduced into a six or eight ounce stoppered bottle, and a dilute solution of green vitriol, made, for example, with a hundred grains of the crystallized salt and a pint of water, is added in successive small quantities until the odour of chlorine just ceases to be perceptible. The quantity of liquid required for the purpose may be conveniently measured in a tube about sixteen inches long, and three-quarters of an inch in diameter, divided into two hundred parts of equal capacity, and supplied with a lip, so that a liquid may be poured from it, without being spilled. In conducting this part of the process, the operator will perceive two odours:—at first, the characteristic odour of chlorine, accompanied with the peculiar irritation of that gas;—and subsequently an agreeable, somewhat aromatic odour, unattended with the slightest irritation. The object is, to add exactly so much solution of iron as suffices to destroy the former of these odours, without attempting to remove the latter; a point which, with a little practice, may be readily attained. The whole of the iron is thus brought into the state of peroxide.

The first trial is generally accompanied with some loss of chlorine, and should only be used as a guide to a second and more precise experiment. Accordingly, a weighed portion of the same ore is dissolved, and the chlorine collected as before, except that the solution of green vitriol, in quantity rather less than sufficient, is at once introduced into the inverted tube and capsule. A more ready and perfect absorption of the chlorine is thus effected, and the subsequent addition of a small quantity of sulphate of iron suffices for completing the process.

The principal sources of error in this method are the two following:—loss of chlorine, by smelling repeatedly, and exposure to the air when the gas is absorbed by pure water; and oxidation by the air when the absorption is made directly by means of the solution of iron. The small flask and inverted tube are apt to retain the odour of chorine, and should therefore be rinsed out with the absorbing liquid. It should be remembered, also, that a given quantity of chlorine will emit a more or less distinct odour, according as it is less or more diluted. But by operating always in the same manner, and employing such weights of different ores, that equal quantities of the solution may contain nearly equal quantities of chlorine, it is easy to be independent of these errors of manipulation, by causing them to affect each experiment to the same degree. It will accordingly be found, with a little practice, that results of surprising uniformity may be thus obtained; and even the constitution of pure oxides of manganese may be ascertained by this method, almost with the same accuracy as by directly determining the quantity of oxygen.

PHENOMENA OBSERVED AT THE LAST ERUPTION OF MOUNT VESUVIUS IN 1828.

By Dr. E. DONATI.

AFTER the tremendous eruption of 1822, Vesuvius remained silent and apparently calm, until the 14th of March, 1828. At this time the volcano presented to the eye of the curious a truncated cone, steep and difficult of ascent, two hundred toises* in height; and a vast crater, half a mile in diameter, but of which the periphery, owing to the many irregularities of its outline, was nearly three miles in extent. The depth of the interior, which resembled an in-

* The celebrated Humboldt, on the 25th of November, 1822, took the barometrical measurement of the greatest cone, and found that the point Del Palo, was at an elevation of 223.6 toises above the plane of the cone, where travellers usually leave their horses to proceed on foot. This height is now diminished by fifteen toises; the materials which formed the summit having fallen into the interior of the crater.

verted cone, of a somewhat elliptical form, was about one hundred and sixty-six toises; the surface of the upper part consisting of semivitrified lava, containing much amphigene and pyroxene; and from the south-west to the north, divided here and there, like basalt, by vertical fissures, to the depth of two toises. Other varieties of lava which occur here present occasionally capillary amphibole, of a reddish brown colour, produced by the eruption of 1822.

Many small volcanic mouths (*fummaioli*) in the interior of the crater, exhaling aqueous vapour, together with sulphureous and muriatic gas, had generated sublimations of muriate of soda and of copper. These apertures, which, during the eruption of 1822, presented a scene completely volcanic, appeared to be re-animated in November, 1824, by an increase of temperature, and emitting dry vapours, produced the corneous muriate of lead (cotunnia) already described in the catalogue of Vesuvian productions.

Active smoking apertures and broad clefts began to be visible in April, 1826, in the interior of the crater facing the north; from these arose aqueous vapour united with sulphureous gas, which attacked the lavas, decomposed them, and generated considerable quantities of sulphate of lime in various forms, acicular, radiated, dendritic, and *à bouquet*. Lower down, but not far from these apertures, were others which afforded sublimations of a blue colour, semi-crystallized, which, upon examination, proved to be the bisulphuret of copper. The last-mentioned apertures no longer exist, having been precipitated into the bottom of the crater, together with part of the sides, to which they were attached. At the bottom of the crater was a large funnel-shaped opening about three toises in depth; and in June, 1826, appeared two new and active volcanic mouths to the east and north of it. I descended boldly; but the excessive heat and acidulous vapours impeded respiration; I could not approach near to the eastern one. The northern one afforded sublimations of trisulphuret of iron, brown and confusedly crystallized in small rhombs; and abundance of sulphate and persulphate of iron and manganese, and also muriatic salts. Every step and stroke, however slight, on the

lower part of the funnel, resounded in a manner which showed that the ground was hollow; the lava forming only a superficial volcanic crust. Signor Monticelli, who, together with Professors Corelli, Petagno and Costa*, was a spectator of my adventurous exploit, gave an account of it to the Royal Academy of Sciences. I was able to measure, by approximation, the depth of the interior of the crater, which was the same as I have already indicated (one hundred and sixty-six toises). A large quantity of materials rolling from the inside of the crater had filled the funnel at the bottom—the volcanic aperture to the north was exhausted, and that to the east had so far diminished in activity, that scarcely any vapour proceeded from it. But towards the end of 1827, others broke out on the southern side of the interior of the crater, which produced much peroxide of iron, in brilliant laminæ of a fine deep red colour; muriate of copper resembling lichen, and large stalactites of muriate of soda. The sides of the crater were much split near this part; and after a few months these, also, were precipitated into the bottom of the abyss.

On the 14th of March, 1828, suddenly, and without any previous notice, either by the disappearance of water or trembling of the earth, at about two o'clock in the afternoon, the above mentioned aperture near the bottom of the crater on the eastern side, although apparently exhausted, gave a tremendous shock, which not only shook the cone of the volcano, but was felt as far as the Hermitage—the forerunner of a new eruption †! The air now resounded with thunder and hollow bellowings; and from time to time shocks succeeded each other with increasing violence. All kinds of loose and detached substances which formed the covering of the aperture, were projected into the air; and

* Signor Costa found at the same time, near the mouth which produced the bisulphuret of copper, some insects of the family Cloteropti, which existed in a temperature of 60° Reaumur, and in an atmosphere loaded with scorching dust.

† On the 14th of March, occupied in mineralogical researches, I traversed the Fossa Grande, after having visited the Atrio del Cavallo, where a stratum of vitreous trachyte occurs, in which small laminæ of brown mica are disseminated. This rock is exactly similar to that called by the Italians *Occhi di Pernici;* and to another found in the Isle of *Paneira.* Scarcely had I felt the first shock, when, expecting an eruption, I mounted the very summit of the cone; and was a spectator of the first changes which took place.

falling again into the middle of the crater, in less than half an hour formed a small cone, which vomited globes of bluish white smoke and sparks and flashes of fire. The shocks, which resembled the discharge of immense cannons, were now repeated every minute, the last always exceeding the former ones in violence. The entire surface of the bottom of the crater, and the sides adjacent, exhibited constantly a heaving motion; and at the moment when the fused and red-hot materials were thrown into the air, the bottom as well as part of the interior sides which were already moved from their position, sunk and rose again. This phenomenon was repeated every time that the subterranean detonations were felt, and the heaving continued until the moment of the expulsion of the lava, which scarcely reached the edge of the little cone. This, as if on the point of disgorging itself entirely, projected impetuously into the air a stream of materials accompanied by dense white and red smoke.

It seemed as if the axis of the volcanic funnel were in the centre of the new cone; for all the substances were ejected perpendicularly from it, dispersed through the air, and fell again in various forms.

As the evening advanced, these phenomena augmented. The wind blew from the south, and spires of smoke resembling pine-trees, and scarcely rising through the air, inclined towards the most elevated part of the cone, called *il Palo.* The sky was serene, and experienced no interruption of its tranquillity, although between the eruptions were heard loud explosions and a bellowing sound, whilst electric sparks rose in the air.

On the 15th, the summit of the cone appeared covered with globes of dense smoke, rising one above the other; the bottom of the interior was entirely covered with scoria ejected from the funnel: the shocks were not so frequent, and the rumbling noises had considerably decreased. At noon, however, all their former violence returned, and they appeared even louder than before; their intensity increased from three till seven o'clock in the evening; so that the accumulation of melted substances had, by this time, elevated the bottom of the crater.

In this state, without any sensible variation in the pheno-

mena which it exhibited, the volcano remained until the night of the 20th of March. But on the morning of the 21st, the scene was entirely changed, and became very striking. Two additional volcanic apertures opened; one to the north, about twenty feet in diameter; the other very small, and situated between this and the one which gave the first signal of volcanic action. Loud explosions and violent shocks reverberated, not only through the interior of Vesuvius, but were felt as far as the city of Naples. The eastern mouth incessantly ejected matter in a state of fusion, pependicularly to the height of forty or fifty feet in the air; and when a current of fire was thrown out by one it was immediately succeeded by a jet from the other. A constant motion of ascent and descent was felt, and volumes of smoke were disengaged, of various colours—white, reddish, blue and black.

The northern mouth, perfectly circular in form, gave from ten to fifteen violent shocks every minute; and threw into the air melted substances, sometimes with brownish white, and sometimes with azure-blue smoke: the liquid lava now flowed from the margin in various directions, and spreading itself in the air, formed a hemisphere of transparent glass, which, either from the impossibility of extending itself, or from the pressure of the air, or because the internal vapours forced themselves a passage through it, broke in many directions, and fell again into the fiery fountain whence it arose.

The little mouth, every one or two minutes, gave a shock considerably stronger than those of the larger one. Opening itself into an ample basin, it ejected the very materials of which it was formed, and those which were continually falling on it from the lateral apertures. The scoriaceous lava, brown on the surface, but liquid within, beat against the interior sides of the cone, rising by degrees with a waving motion. The phenomena did not preserve the same character as on the preceding days, but increased with gigantic progress; at every shock the whole cone trembled, and an undulatory motion extended as far as to Resina and other parts of the surrounding country; the smoke continued to rise in large globes at the summit of the crater.

At two o'clock I went round the perimeter of the cone, to

observe what part would be most likely to give way under the impetus of the shocks. To the south I found many wide openings and deep fissures; but the suffocating acid vapours and the rain of scoriæ, which was then falling, obliged me immediately to quit my position, which was, in fact, becoming extremely dangerous. I met with other fissures at Il Palo, where there appeared no present danger.

The inhabitants of Naples, on the 21st, began to perceive the spires of smoke, and in the evening the jets of fire, and the country around to fear the effects of such appearances. There was much electricity in the air; the electric fire was in constant motion, and appeared to incline to the negative pole; the barometer and thermometer indicated no sensible variation; the apertures at the base (la pedamentina) and along the cone exhaled but little vapour, and remained at the temperature of 70° Reaum.*

These phenomena remained in a state of continual paroxysm till the evening of the 22d; and far from abating in violence, they increased so much, that two new apertures broke out, which, together with those already existing, detonated loudly, and ejected the melted lava to the distance of two thousand feet. Although the force acted obliquely, the melted matter reached a height exceeding that of the crater and of Monte Somma; and fell at various times in the night at Ottajano, in large and small pieces of perfect scoriaceous lava.

The morning of the 23d, the bottom of the crater was raised one-fourth, and seventeen volcanic mouths were in full action, exploding, shaking the earth, and throwing out melted matter in various directions, obliquely and even horizontally; vomiting also globes of smoke of various colours, which from time to time filled the vast area of the crater, and then, rising like huge machines into the air, assumed the appearance of magnificent columns. A mouth, which opened on the southern part from midnight till two o'clock the following day (24th), ejected fused substances, and the scoria of the lava which was within it, obliquely to the point *del Palo,* and they fell on the external part of the cone in large and small masses. All the spectators now abandoned the place; but in a few hours after this crisis, the activity of the volcano began to diminish and

eight apertures closed. In the evening, the three first only, and with detonations less loud than before, threw into the air jets of sparks. The first, to the east, was in constant action; that to the north, and the one between them, exploded from time to time like the first; and all the other smaller ones, sometimes exploding, sometimes throwing up showers and columns of sparks, presented the appearance of most brilliant fireworks.

The lava which filled the bottom of the crater was in some parts semicircularly divided by concentric strata of fire, which, in the northern and south-eastern parts, appeared as if animated by a subterraneous current of air; and from time to time, in some places, globular masses of the fused matter rose on the surface and rolled towards the centre. The barometers lowered; and the rain which fell during the night brought some degree of calm.

On the night of the 25th the northern aperture only remained in action. It ejected, with a loud bellowing, (at intervals of half an hour, and sometimes an hour,) smoke and flames: the latter were visible even from Naples; and the smoke rose in the form of pine trees, inclining to the north. The great aperture to the east, and the smaller ones, were in a state of tranquillity. Rain fell again in the night; and on the morning of the 26th with hail. Vesuvius and the Monti di Somma were covered with the latter; but during the day it disappeared. In the evening, the explosions of the northern mouth recommenced, so that every two minutes this aperture vomited globes of dense black smoke; and large quantities of very fine sand, of a dark brown colour, rose in the air and fell towards the north-west, on the summit of the cone and within the crater, throughout the greater part of the night. All the other mouths were entirely spent. The same aperture, during the whole of the 27th, becoming gradually exhausted, gave very slight shocks, resembling in sound the discharge of a musket; and in the evening, reviving for a few minutes, gave five or six as loud as the report of a cannon, discharging at the same time flames and smoke, and during the night, ceased.

On the morning of the 28th, Vesuvius remained in a state

of perfect calm; but the examination of the crater was still interesting. The whole of the interior appeared as if lined with black velvet, in consequence of the great quantity of sand which fell on the 26th. The bottom of it, by the frequent rising of the substance which formed it, and by a mass of scoriaceous lava which accumulated on its surface, was raised above its former level about forty toises, or perhaps still more, adjoining the sides. It appeared like the scum which lines a vessel when filled with a liquid in a state of fermentation.

On the north-east the lava had already formed an oblique eminence, supported against the interior surface of the crater, where there still remained vertical fissures, animated with living fire, and exhaling much vaporous smoke. This eruption was exceedingly beautiful and interesting; for every volcanic or igneous phenomenon which took place was observable, without danger, within the vast area of the existing crater. The shortness of the volcanic crisis can be attributed only to two causes; firstly, because the volcanic funnel was very superficial, and very little resistance was opposed to the igneous power, which, being exhausted, could scarcely furnish from itself more combustible materials, because deficient in them; secondly, because subterraneous currents of air prevented the fire from receiving the inflammable materials from strata beneath. This opinion may be thought a bold supposition; but in observing the mode of action of the volcanic mouths, the same appearance was visible as when a fire is blown with a pair of bellows. This idea has occurred to me from what I have many times noticed, and which may be verified at the present time, that in some fissures in the western part, the air enters with a loud whistling sound, but perfectly kalophonious. During the short time of the eruption, the water did not decrease in any of the wells in the neighbourhood of Vesuvius.

The pumiceous scoriæ, which fell on the 21st on the southern part of the summit of the cone, were of a greenish colour, and filamentous; some filaments not thicker than the finest hair, and others an inch in diameter. The result of the mechanical analysis,—a method adopted by Professor Cordier,—showed them to consist of an intimate mixture of pyroxene

and vitreous amphigene, imbedded in a vitreous paste. The heavy scoriaceous lava, which fell in Ottajano, and on the point *del Palo,* from one to eight inches in diameter, consist also of pyroxene and vitreous amphigene. The great mass of lava is very tenacious, slaggy, and brown; the scoriaceous part rough and uneven, with crystallized pyroxene (which is usually the bisunitain variety), covered with brown fused coating, and internally of a deep green colour. These scoriæ contain but little lime, and a large proportion of iron. The brown sand, which fell on the night of the 26th, consisted of microscopic particles of scoriæ, pumice, a mixture of amphigene and vitreous pyroxene, laminæ of mica, and a large quantity of magnetic iron in a state of the finest powder, a large proportion of permuriate of iron, some muriate of soda, and a small portion of sulphate of soda. I could not at this time descend to the bottom of the crater to observe the sublimations in the funnel, because, although extinguished, there was yet danger.

APPENDIX.

Vesuvius, after the 28th of March, presented no visible alteration till the 3d of July, except in the enlargement and extinction of the vertical fissures on the north-east, which had glowed so vividly. An undulatory trembling of the earth was sensibly felt at Marsala in Sicily, about the 15th of June; and towards the end of the month an explosion of gas took place in the isle of Ischia, which considerably damaged the surrounding land. It was observed very distinctly at Castellamare, and along the coast as far as Plantelleria; and the barometers and thermometers rose and fell in a very extraordinary manner.

On the abovementioned day (July 3), at three in the afternoon, one of the volcanic mouths near the centre of the crater re-opened, and, dislodging all the extraneous materials that lay upon and around it, threw into the air, with rumbling and bellowing sounds, brilliant fire, with dense smoke of various colours, and very shortly spread itself into a large basin; the fused substances which it furnished, falling again upon itself,

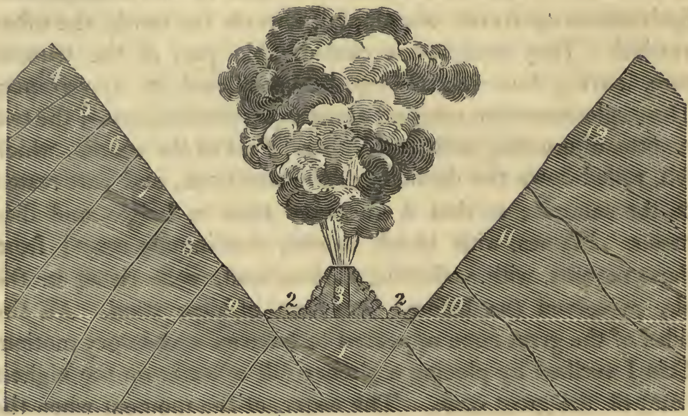
began in a little time to form a cone, which, on the 4th, had risen to the height of nearly one hundred feet, truncated horizontally at the summit, where it appeared about eighteen feet in diameter. This cone became the base of two semicircular, ignivomous apertures, one sloping towards the north, the other vertical. They occupied a considerable part of the truncature, leaving, however, a large part unaltered in appearance. The explosions were renewed about every three minutes; the two mouths alternating with each other. Part of the scoriæ, which fell, rolled down the declivity of the new cone, and part rested on the summit; so that it increased both in height and diameter. Flames, with bluish smoke, continually issued from these mouths, with a whirling motion, and, while rising in the air, preserved (as before) the form of pine trees. All the sides of the great cone appeared to have an undulatory motion. This I verified by placing a pitcher full of water on the highest point of the great cone. The water, at the moment when the subterranean detonations were felt, manifested an undulating motion, and fell from the sides of the pitcher. I repeated the experiment several times, and the same undulation constantly took place; so that, although the present eruption was nothing in comparison with so many others which had taken place, it appeared conclusive that it was sufficient to impart an undulatory motion to the whole of the cone.

The rain which fell at three o'clock on the night of the 5th of July seemed to cause a cessation of the igneous explosions; but instead of these, cinders were ejected, of a grey or brown colour, which fell on nearly all the hills in the neighbourhood of Vesuvius, and on the plains and gardens of the Torre dell' Annunciata. The husbandmen were not sorry to find their lands covered with this heated sand, for experience had taught them its utility in the culture of cotton and rice.

On that morning Vesuvius appeared most interesting, and particularly at the rising of the sun, which was immediately preceded by a rainbow—a magnificent spectacle, and which I have never before known to have been observed.

To attempt an explanation of this very short eruption would be to immerse oneself in a sea of conjectures, without deriving

any profit from them. I have thought it more useful to give a vertical section of the great cone, and of the small one produced in the centre of it.



The section of the cone of Mount Vesuvius seen from the interior, looking toward the W.N.W. The point at which visiters generally place themselves to see the interior of the crater is at S.S.E.

1.—Bottom of the crater before the eruption.
2.—Scoriæ and lava thrown up during the eruption.
3.—Cone formed during the lesser eruption at the beginning of July.
4.—Amphigenous pyroxenic vitrified lava, of the eruption of 1822; in some parts this lava, when it has beneath it smoke vents which exhale fumes, divides itself into coarse prisms.
5.—Stratum of Rapilli (i. e. white pumice) and labbie (dust).
6.—Stratum of pumice, and small fragments of scoriæ tinged with oxide of iron.
7.—Current of lava *Leveitica* (greenstone).
8.—Current of lava mixed with pyroxene. Lava of the same kind is found in the veins of the great dike (*Fossa Grande*).
9.—Current of lava mixed with pyroxene. It contains (in the *geodes*) very small crystals of pseudo-nesseline, and a small quantity of breislakite. The fragments of this lava crumbles into large prisms, like that at Scala near Portici, which is termed *basaltica da Brocchi.*
10.—Stratum of *rapilli* and *labbie*, impregnated with the fumes which exhale from the *fummaioli* smoke in that part, with efflorescence and sublimation of sulphur, muriate of soda, and copper. The sublimation makes this spot appear like a cultivated garden.
11.—Current of lava, divided into great prisms. This place is inaccessible.
12.—Rapilli and pozzolana, with globules of the Vesuvian dolomite of Thomson and of lava. These same materials are found far from the external walls, as well on one side as the other.

A MODE OF REGULATING THE SUPPLY OF WATER BETWEEN INTERSECTING RIVULETS AND CANALS.

DEVISED BY THE LATE ROBERT ALMOND, Esq.
of Nottingham.

[Communicated by MARSHALL HALL, M.D., F.R.S.E., &c. &c.]

THE Nottingham and Erewash canals diverge from the same point, at Langley Mill, in the county of Derby, and are terminated at the distance of a few miles from each other, by the river Trent. In consequence of this relative situation, the Nottingham, which was cut most recently, intersected some of the rivulets which had previously fallen into the Erewash. To compensate for this injury, an eminent mathematician devised the following ingenious plan of delivering from a reservoir of the Nottingham Canal, a given quantity of water per minute, under every variation of the height of water in the reservoir.

The water is brought into a small cistern, of which A (Fig. 1.) represents part of the end. *b* is an aperture, parallel with the horizon, which would of itself deliver the stipulated quantity when the water in the cistern is at its greatest height. *a* is a vertical aperture, connected with the former, and is quite closed by the shuttle B, when the cistern is full. Its sides are of that peculiar curvature, that as the shuttle is raised by the action of the buoy C, descending with the surface of the water in the cistern, the additional part of the aperture disclosed exactly compensates for the diminution of pressure. This plan, however, though correct in theory, proved altogether abortive in practice, on account of the excessive friction which is produced, partly by the motion of the shuttle in a groove, and partly by the lateral pressure of that portion of the water which is above the disclosed part of the orifice*.

A dispute afterwards arising respecting the Gilt Brook, which the Erewash Company deemed valuable in the dry summer months, and which had formerly been one of their feeders, they demanded a regular supply of water, according to the average quantity which the brook should be found to deliver in the months of June, July, and August. This quan-

* The investigation of the curve proper for the sides of the aperture, is furnished by the inventor in the 'Gents. Diary for 1799.'

Fig. 2

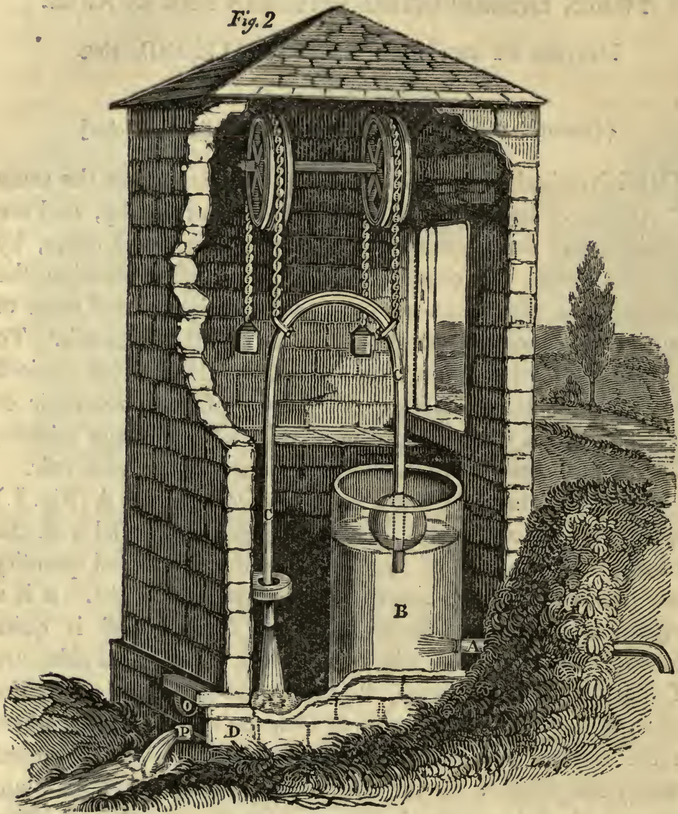
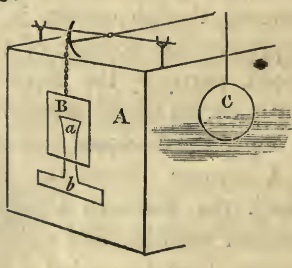


Fig. 3

Fig. 1



tity proved to be 11.25 cubic feet per minute; but a great difficulty now arose respecting an impartial mode of supply, and this difficulty appeared greater in consequence of the failure of the former plan. At length the dispute was amicably adjusted by the following method, which was invented and carried into execution by Mr. Robert Almond, of Nottingham, then one of the proprietors of the Nottingham Canal.

A (Fig. 2.) represents a pipe placed under the haling path of the Nottingham Canal, having the end, which communicates with the canal, turned downwards to prevent stones and dirt from falling into it. Its other extremity is connected with a large cast iron vessel B, in which the water necessarily rises to the level of that in the canal. C is a copper syphon, balanced in such a manner, partly by means of a hollow copper sphere, through which it passes, and partly by two weights passing over the wheels, as represented in the drawing, that it rises and falls freely with the water in the canal. By this contrivance, the discharging orifice of the syphon will always be at equal depths below the surface of the water in the canal, and must therefore constantly deliver equal quantities in equal times. D is a stone cistern, into which the water runs, after being discharged from the syphon, and which serves as a gauge, open to the inspection of passengers. On its interior side, a plate of copper is placed perpendicular to the plane of the section, and which is made visible by the stone being cut down to its edge. The water always remains level with this plate, whilst a discharge is taking place from the pipe P.

It must appear to every one versed in hydraulics, that, owing to the friction of the water against the sides of the syphon, its velocity must be retarded, and that the discharging leg must be longer than the theory of emptying vessels would lead us to suppose. This remark is verified by the case before us. The internal diameter of the syphon is 2.45 inches, and the lower orifice is 21.03 inches beneath the level of the canal; but, according to the theory, which supposes that the velocity at the orifice is that acquired by a body falling from rest through a space equal to half the depth of fluid above it, the depth of the lower orifice $= \dfrac{\overline{\text{quantity discharged per } 1''}|^2}{\overline{\text{area of orifice}}|^2 \times 386} =$ 12.22, &c., each term being expressed in inches. It is

therefore advisable, in the construction of such an apparatus, to make the difference of the legs of the syphon double that which the theory requires, and then to reduce the longer to its proper accuracy by absolute experiment.

The above equation is of more use in the construction of the gauge cistern. The depth above the pipe being assumed greater than is wished, the area of the pipe may be calculated, and the stone be afterwards cut down to the level at which the water is observed to remain.

When the apparatus is once regulated, the syphon and weights should preserve an exact balance in every point of ascent and descent; as the accuracy of the discharge depends in a great measure upon that circumstance. If strong catgut, or any light cord, be used to connect the syphon with the weights, the equilibrium may not be sensibly affected by the motion of the syphon. In the present case light chains are used; and, as the wheels are about two feet in diameter, half a revolution, or a variation of three feet in the level of the canal will take the weight of six feet of chain from one side of the axle, and add it to the other. To obviate this inconvenience, one of the wheels has a piece of lead attached to its side, which is narrower at its extremities than at its centre. W (Fig. 3.) represents the wheel thus loaded, and in its situation when the syphon is at its lowest point.

It only remains to observe, that this simple apparatus, which is so easily regulated by a little increase or diminution of the weights, has now been at work for more than fourteen years, without any alteration in its adjustment, and to the perfect satisfaction of all parties. A very intense frost has once or twice suspended its operation, but the succeeding thaw has enabled it to resume its function of a constant arbitrator.

Nottingham, 1826. R. W. A.

Note.—A floating syphon, with wheels and balance weights, is, we believe, fully described in the article 'Hydrodynamics,' in Brewster's Encyclopædia; but the date of the article is by much posterior to the first erection of the instrument described above, and which has the authority of twenty years constant use.—*Editor.*

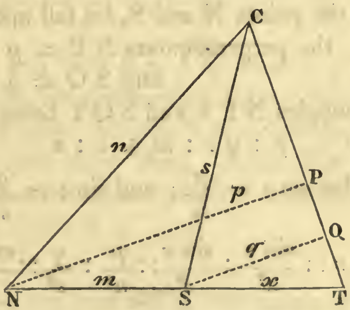
ON THE GEOMETRIC PROPERTIES OF THE MAGNETIC CURVE, WITH AN ACCOUNT OF AN INSTRUMENT FOR ITS MECHANICAL DESCRIPTION.

By P. M. ROGET, M.D., Sec. R.S.

THE properties of the magnetic curve being interesting to the geometrician, as well as important in their connexion with the theory of magnetism, I am induced to offer the following demonstrations of the two fundamental propositions respecting them, derived directly from the law of magnetic forces, as being more simple than any of those given by Professors Robison, Playfair, or Leslie. I have also added an account of a method I have devised for the mechanical description of these curves.

The principal problem relating to the magnetic curves is to find the direction, C T, Fig. 1. of the tangent to the curve

Fig. 1.



which passes through any given point C, when the situations N and S of the two poles are given. This direction indicates the position which an infinitely small compass needle, placed at C, and at liberty to turn freely round its centre, in a plane passing through N and S, will assume by the action of the magnet N S. This position must be such, that the rotatory forces exerted on both poles of the needle by each pole of the magnet shall exactly balance one another.

The forces themselves, according to the established law of magnetic action, are inversely as the squares of the distances

of the acting poles; which distances, in the case before us, are the lines C N and C S. But that part of each force which is effective in producing rotation, is, by the resolution of forces, as the sine of the angle which the direction of the force makes with the radius of rotation. Taking both these circumstances into account, the rotatory forces exerted by the two poles are to one another in a ratio compounded of the sines of the angles N C T and S C T, and of the reciprocals of the squares of C N and C S. For the convenience of notation, let these rotatory forces be denoted respectively by the letters R and r.

Let C N $= n$
C S $= s$
The angle N C T $= \nu$
The angle S C T $= \sigma$
The length of the magnet, or N S $= m$.
The portion of the produced axis S T intercepted between S and the line C T $= x$
From the points N and S, let fall upon C T
the perpendiculars N P $= p$
and S Q $= q$.
The triangles N P T and S Q T being similar

$$p : q :: m + x : x$$

Also $\sin \nu = \frac{p}{n}$; and $\sin \sigma = \frac{q}{s}$:

$$\text{Then, } R : r :: \frac{\sin \nu}{n^2} : \frac{\sin \sigma}{s^2} :: \frac{p}{n^3} : \frac{q}{s^3} :: \frac{m+x}{n^3} : \frac{x}{s^3}$$

But in the present case $R = r$; therefore $\frac{m+x}{n^3} = \frac{x}{s^3}$;

Or, $x = \frac{s^3 m}{n^3 - s^3}$: hence $n^3 - s^3 : s^3 :: m : x$.

That is, $CN^3 - CS^3 : CS^3 :: NS : ST$.

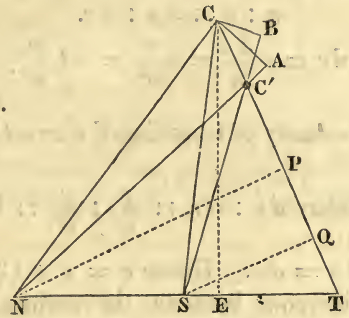
Hence, in order to determine geometrically the point T, in the axis N S produced, and thereby the direction of the line C T, which is the position of equilibrium for the infinitely short needle C, and the tangent to the magnetic curve at that point, we must take on that axis a distance S T such that it

may be a fourth proportional to the difference of the cubes of C N and C S, the cube of C S, and the line N S *.

The most remarkable property of the magnetic curve is, that the difference of the cosines of the angles, which lines drawn from any point in the curve to the two poles make with the axis, taken on the same side, is a constant quantity.

In order to demonstrate this proposition, we must take another point, C′, in the curve, Fig. 3. exceedingly near to the

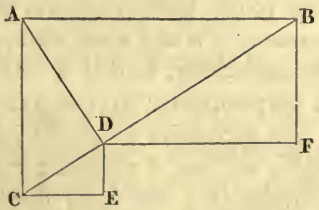
Fig. 3.



former point C; and from N and S draw to it the lines N C′, S C′, which should be produced a little beyond C′; and describe from the centres N and S, respectively, the small arcs C A and C B to meet these lines. Draw also C E perpendicular to N T. Let the same notation as before be preserved with regard to *n*, *s*, *p* and *q*.

* The following is a convenient way of obtaining, geometrically, lines which are in the ratio of the cubes of two given lines, A B and A C, Fig. 2. Set them,

Fig. 2.



as in the Figure, at right angles to one another. Join B C, and draw A D perpendicular to it. Draw C E, and D F, parallel to A B; and D E parallel to A C. The lines D F, D E, will be to one another in the ratio of the cubes of A B and A C.

Let $CE = e$	The angle $CNT = \alpha$
$CA = a$	The angle $CST = \beta$
$CB = b$	Cosine of $\alpha = c$
$CC' = t$	Cosine of $\beta = \varkappa$

Then $d\alpha = \frac{a}{n}$: and the triangle CAC′ being similar to NPC

$$n : p :: t : a; \text{ or } a = t\frac{p}{n}$$

$$n : e :: d\alpha : dc.$$

$$dc = e\frac{d\alpha}{n} = e\frac{a}{n^2} = e\,t\,\frac{p}{n^3}.$$

By a parity of reasoning $d\varkappa = e\,t\,\frac{q}{s^3}$

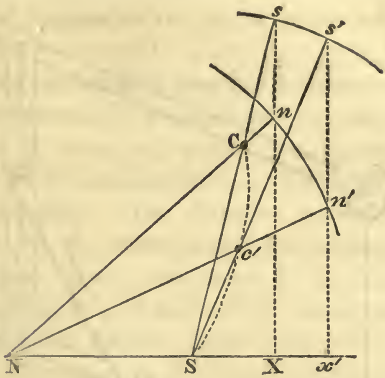
Therefore $dc : d\varkappa :: \frac{p}{n^3} : \frac{q}{s^3} :: R : r.$

When $R = r$, $dc = d\varkappa$. Hence $c = \varkappa + C$, or $c - \varkappa = C$. That is, the difference between the cosines of the polar angles C N T, C S T, is a constant quantity.

When the angle C S T exceeds a right angle, its cosine being then negative, the proposition will be changed to the following; namely, that the *sum* (instead of the difference) of the cosines of the polar angles is constant. When the angle C N T is also obtuse, both the cosines being negative, it is again the difference of the cosines that is constant.

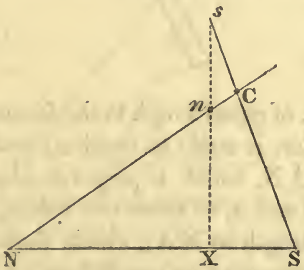
The following method of describing this curve is derived from the property above demonstrated. Let the two radii N*n*, S *s*, Fig. 4. be taken of equal length, and be made to revolve in the same direction round their respective centres N and S, while their other extremities *n* and *s* are always kept in such a position relatively to each other, as that a line drawn through them shall remain perpendicular to the axis N X; then the line constituted by the successive points of intersection C, *c′*, &c. of the two radii, will be a magnetic curve. This will appear from the consideration that with the equal radii N *n* and S *n*, the cosines of the angles C N X and C S X are the lines N X and S X, of which the difference is N S. In every other position of the radii, as N *n′*, S *s′*, where the line *s′ n′ x′*

Fig. 4.



is perpendicular to the axis N x', N S is the constant difference between the cosines of the polar angles c' N x' and c' S x'. When C S X is an obtuse angle, as in Fig. 5. the cosine of its

Fig. 5.

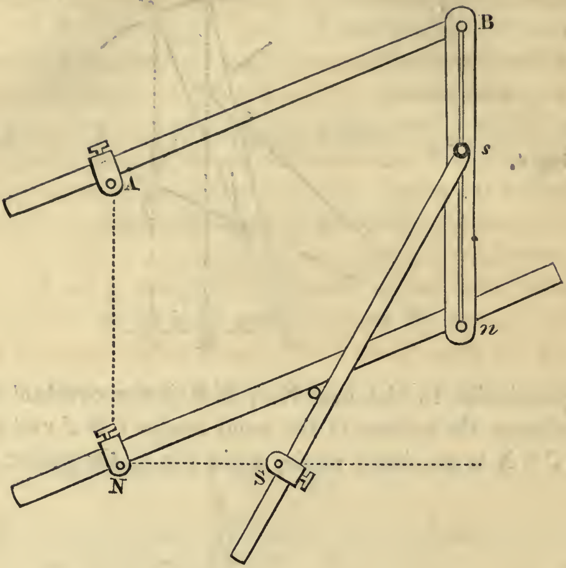


supplement C S N (or S X) *added* to that of C N S (or N X), will, in like manner, be found to give the constant sum N S, provided the other extremities, *n* and *s*, of the revolving radii continue to be in a line perpendicular to the axis; as will readily appear by inspecting the figure.

On this principle I have contrived the following instrument for describing mechanically the magnetic curve.

The ruler N *n*, Fig. 6. is furnished with a sliding collar, which, by means of a screw, may be fixed at any point in its length. The collar has a hole at the edge of the ruler, for the passage of a pin, which fixes it to a board (previously covered with a sheet of paper), at the point intended as the pole N; so that the ruler may turn round this point as a centre.

Fig. 6.

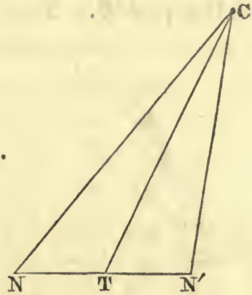


Another ruler AB, of equal length to the former, being furnished with a similar collar, is made to revolve round A, at the extremity of the line A N, which is perpendicular to the axis N S. The other ends, B and *n*, of these two rulers, are connected by a third (equal in length to N A), which, during the movements of N *n* and A B, preserves its parallelism to N A. The ruler B *n* has a groove, running its whole length, in which a button, projecting from the extremity of the rule S *s*, slides. The ruler S *s* is also furnished with a collar similar to the former, which fixes it to the board at S, or at any other required point in the line N S. A pencil, following the intersections of the rulers N *n* and S *s* with each other, as they turn round their respective centres N and S, will describe a magnetic curve. In adjusting the several collars, care must be taken that the distances of A, N, and S from B, *n*, and *s*, respectively, are all equal; and, in order to effect this, it will be convenient to have the rulers graduated to a scale of equal parts. The greater these distances, the larger will be the curve described by the instrument. When the ruler N *n* is brought down so as to coincide with the axis N S, the other ruler S *s* is in the

position of the tangent to the curve at its origin from the pole S.

When the two poles which give rise to the magnetic curves are of the same, instead of being of different, denominations, a different system of curves is produced, which have been termed the *divergent*, in contradistinction to the former, which are *convergent* to the poles. The divergent curves preserve, with slight modifications, the same geometrical relations to the axis as the convergent curves, and admit of a similar mode of mechanical description. Instead of the south pole S, in the preceding figures, let another north pole N′ be substituted; that is, let the north poles N, N′, Fig. 7, of two different mag-

Fig. 7.



nets, be placed so as to front each other; and let the actions of their remote south poles be neglected. In the former case, where the actions of the two poles of the magnet were of an opposite kind, the resultant of their joint action, or the line C T, Figs. 1 and 3, passed in a direction intermediate between N C prolonged, and S C (the former line being the direction of the repulsion, and the latter that of the attraction): it therefore cut the axis N X at some point in the prolongation of N S. But in the present case, the two magnetic poles being of the same kind, their action is similar, and their resultant is a force, of which the direction is intermediate to the lines C N and C N′, Fig. 7.; and this line produced, must cut the axis somewhere between N and N′. The angle C N′ T being reversed from the situation with respect to C N′, which it had in the former case, the sign of its cosine must be changed, and the equation becomes

$$c + \varkappa = C.$$

This applies to the case, in which the angle formed by C N′ with the produced axis is acute, and its cosine positive. When it is obtuse (or C N′ N acute), its cosine being negative, the equation is

$$c - \varkappa = C.$$

When the two poles are similar, and consequently the curves divergent, the two radii, which, during their revolution, generate them by their intersections, revolve in opposite directions; and the points in each which preserve the same perpendicular position with relation to one another, will be found to lie on opposite sides of the axis. The intersections of N *n* are made with that portion of the line S *s*, which is produced on the other side of the pole S. This is shown in Fig. 8,

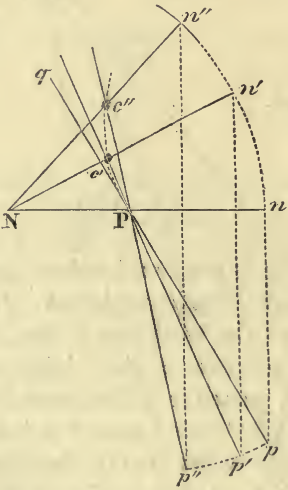


Fig. 8.

where N, P, are the two similar poles, and N *n*, P *p*, the two revolving radii; the latter being produced beyond P to *q*. In this position, when N *n* coincides with the axis, P *q* is the direction of the tangent to the divergent curve at the pole P. In their positions N *n′* and P *p′*, the radii intersect one another at the point *c′*; when they arrive at *n″* and *p″*, they intersect at *c″*; and so on; P, *c′*, *c″*, &c., being so many successive points of the curve. When N *n* and P *p* become parallel, they indicate the ultimate direction of the curve.

The divergent magnetic curves are capable of being described by an instrument of a similar construction to the one already explained; only the ruler B *n*, Fig. 6, must be of twice the length of the former; and in order to obtain a sufficient extent of curve, the revolving rulers, N *n*, and S *s*, must be prolonged in those parts where the intersections are to take place.

ON THE FIRST INVENTION OF TELESCOPES, COLLECTED FROM THE NOTES AND PAPERS OF THE LATE PROFESSOR VAN SWINDEN.

By Dr. G. MOLL, OF UTRECHT.

[Communicated by Professor Moll.]

THE late Professor Van Swinden had been at considerable pains to illustrate some important points in the history of natural philosophy. The first invention of telescopes in Holland attracted a considerable share of his attention, and he had the good fortune to meet with some official documents, which are calculated to throw some light on the mystery in which the early history of this celebrated invention is involved.

Mr. Van Swinden exposed the result of his labours in several public lectures, and he intended to publish a paper on the subject: his death prevented the accomplishment of this purpose. He left, however, the sketches of his lectures, together with extensive notes, and abstracts from various writers, which he had collected with great industry. These papers were committed to my hands, and the result of what I collected from them has been ordered to be printed by the Royal Institute of the Netherlands.

The little which is known of the first invention of telescopes in this country has been principally derived from two sources: first, from the book which the French physician, Pierre Borel, wrote on the subject in 1655, probably at the request, and certainly with the assistance, of William Boreel, at that time ambassador of the States at the court of France*. The second

* De vero Telescopii inventore, cum brevi omnium Conspicilliorum historia, authore Petro Borello, Regis Christianissimi consiliario et medico ordinario; Hagæ Comit. ex typogr. Adriani Vlacq. 4to., 1655.

source from which information is generally derived, is a passage in Descartes's Dioptrics*, in which he attributes the invention to a citizen of Alkmar, called James Metiús. Both the versions of Borel and Descartes are usually given in books written on this part of natural philosophy, and very recently they were repeated in the very excellent account of the life of Galileo published in England, and in the still more recent and capital work of Professor Littrow on Dioptrics.

The real name of this Metiús, of whom Descartes speaks, and who is also mentioned by Huygens, was Jacob Adriaansz. His father Adriaan Anthonisz was a man of considerable knowledge for his time; he possessed a great influence, and took a principal part in the struggle with Spain. In consequence, he was banished by the Duke of Alva, and his property confiscated. He contributed very essentially to the glorious defence of his native town against the Spaniards in 1592. He was created afterwards inspector of fortifications, and many towns were fortified on his plans. As a mathematician he is celebrated for his expression of the ratio of the diameter and circumference of the circle, by the numbers 113 and 355. At that time Ludolf van Ceulen had not given his celebrated number, and the ratio of Archimedes, of 7 and 22, was in general use. The numbers of Anthonisz have the merit of being easily kept in memory, and of being as accurate as almost any purpose requires. If no logarithms are used, it is easier to calculate than Ludolf's number.

There is another problem remaining of this Anthonisz, which shows his ability as a mathematician: it is recorded in one of the writings of his son Adrian, and Delambre notices it in his history of astronomy. The problem was solved by Nicholas des Muliers of Bruges, then professor of mathematics in Groningen.

All the four sons of Adriaan Anthonisz were mathematicians like their father. The eldest, Dirk or Theodore, was an engineer and surveyor in the service of the States. He sailed in

Pierre Borel was a native of Chartres, and author of several other books: he died 1689. A copy of this very rare tract has been recently added to the library of the Royal Institution. It contains a portrait of Lippershey.

* Cartesii Dioptrica, p. 49.

that capacity in the expedition against the Spanish colonies in the West Indies and the coast of Africa, sent out under Admiral Peter Van der Does in 1599. He died in that ill-fated expedition.

The second son, Adrian, whilst at the University, had the nickname of *Metiús* given to him by his fellow-students, on account of his propensity to mathematics. He became generally known under that name, and wore it through life. His father sent him to Hueen to study astronomy under the celebrated Tycho, and afterwards he visited several universities of Germany. He filled the astronomical chair at the university of Franeker with great credit, and died in that place in 1635. His works were very numerous and celebrated in their time, being considered the best elementary works then extant. Delambre seems to have known only one of Metiús's books, of which a complete catalogue is to be found in Vriemoet*.

The fourth son, Anthony, did not rise to such extensive fame: however, he also served his country as an engineer.

The third son is the person whom Descartes designates as the inventor of the telescope. His name was Jacob Adriaansz; and sometimes the name of Metiús, which properly belonged to his brother, was given to him. This Jacob, or James, died between 1624 and 1631. Contemporary writers describe him as a person of eccentric and fanciful habits, buried incessantly in deep meditations, and of a temper so little communicative, that he very seldom spoke to any one about the subject of his studies. It is well known that such an eccentric turn of mind is not incompatible with mechanical genius, and in England and elsewhere the most consummate skill has often been blended with most singular habits. It appears, from the evidence of writers of that time, that this Jacob had acquired considerable skill in working glass, and excelled, amongst other things, in the construction of large burning lenses. It is said that he once placed a large lens on the walls of Alkmar, and predicted that at a certain hour of the day it would set fire to a tree standing at a great distance on the other side of the moat. At the request of Prince Maurice of Nassau, who was

* Athenæ Frisiacæ.

a great proficient in mechanics and mathematics, many and pressing solicitations were made to make Jacob explain how this and other apparatus which he contrived were executed; but he obstinately rejected all offers, and always refused to give the least information, even on his death-bed, when strongly urged by a clergyman, at the request of his relatives. It must be allowed that at that time, and even now, the construction of a burning lens of such power was a matter of great difficulty, and even at present very few artists would be capable of doing the same. So strongly was his desire to conceal his inventions, that before his death he caused his apparatus and tools to be destroyed.

This eccentric character sent a petition to the States General of the United Provinces, dated the 17th of October, 1608. An original copy of this document, made by a public notary in the most authentic form, is existing in the library of the university of Leyden, amongst the manuscripts of Huygens. In this document it is distinctly asserted that this person actually invented the telescope. He calls himself Jacob Adriaanszoon, son of Mr. Adriaan Anthoniszoon, and he goes on to state, 'that since two years, he employed all the time which he could spare in inquiring into some occult or secret arts connected with glass-making. That he found that, by means of a certain instrument, which he was making for another purpose, the sight of persons using it might be extended, so as to make objects which, on account of their distance, could not be seen or only distinguished with great difficulty, appear near and distinct. That since that time he applied himself to bring this invention to greater perfection, in which he succeeded so far as to make an object appear as visible and distinct by his instrument as can be done with *that which was lately offered to the States by a citizen and spectacle-maker of Middelburg.* That his Excellency (Prince Maurice), and others who compared the instruments, convinced themselves of this fact, notwithstanding that his instrument was made of only coarse materials, and merely for the sake of experiment. That he has no doubt but that the contrivance, by improving the engine, might be brought to greater perfection, but that, besides, he believes and hopes to improve, in time, *the invention in itself*, so as to make it capable

of doing great service. That he apprehends that, in the meantime, other persons might imitate his invention, building on the foundations which he had laid, by the grace of God, with his *ingenuity, great labour, and intense study,* and by these means might frustrate him, and rob him of the fruits which he has a right to expect with great confidence of this invention; and, therefore, he prays their High Mightinesses to grant him a privilege (*octroi*), by which every one, *not possessing the said invention at present,* is prohibited from imitating this instrument, or even from selling or purchasing instruments made contrary to this privilege, without his express leave, and on a fine of a hundred florins on each instrument; and that this privilege is to last twenty years, or, instead of a privilege, to allow him such a remuneration as will be adequate to the utility and service likely to be derived from this invention.'

In the margin of the petition the following appointment is written:—'The petitioner is exhorted to make further investigations, to bring his invention to greater perfection; when his prayer for a privilege will be taken into consideration.

'*Actum,* 17 *October,* 1608.'

With the signature of 'Aersens,' the then Secretary of the States.

If we are disposed to give full credit to Adriaansz, whom, for brevity sake, we will call *Metiûs* in future, it appears that he began the researches which led him to the invention of the telescope as far back as 1606; that the invention was due to chance, and occurred while its author was trying other experiments; that he spent subsequently much time and labour upon it; but that in 1608, when he sent in his petition, his instrument was made of bad materials, and might be much improved. At the same time he readily admits that another person, *a spectacle-maker of Middelburg,* had offered before him a similar instrument to the States, which had been tried by Prince Maurice and other persons, and he gives us to understand that his instrument is equal to that of his competitor. Nothing is said which enables us to judge of the performance of either instrument.

Mr. Van Swinden examined the written Acts and Journals of the States-General of that time with great care. These

papers are kept at present among the state archives in the Hague. Under date of the 2d October, 1608, the following entry is made:—

'*Jovis*, 2 *October*, 1608.

'On the petition of *Hans Lippershey*, a native of Wesel, an inhabitant of Middelburg, spectacle-maker, inventor of an instrument for seeing at a distance, as was proved to the States, praying that the said instrument might be kept secret, and that a privilege for thirty years might be granted to him, by which everybody might be prohibited from imitating these instruments; or else to grant him an annual pension, in order to enable him to make these instruments for the utility of this country alone, without selling any to foreign kings or princes. It was resolved, that some of the Assembly do form a committee, which shall communicate with this petitioner about his said invention, and inquire of him whether it would not be possible to improve upon it, *so as to enable one to look through it with both eyes*; and further, to inquire what remuneration would satisfy him. And due report being made, it will be laid in deliberation, whether it is expedient to grant to the petitioner a remuneration or a privilege.'

From this document it appears who this inventor was, whom Metiús designates in his petition of the 17th of October, and whom he allows to have anticipated him in presenting a telescope to the States: it was the spectacle-maker of Middelburg, born at Wesel, and called Hans, *i. e.* John Lippershey. This man offers to keep his invention a secret; and he intimates a belief that it might be of service. This story offers also a ludicrous instance of the strange vexations to which ingenious men must often submit, from ignorant but official persons; this is—

'The insolence of office, and the spurns
That patient merit from the unworthy takes.'

Here comes Lippershey, tendering to the States an invention, which, in its further progress, is entirely to alter and to extend all our notions of the universe—an invention which bodes a complete revolution in navigation and astronomy, and the first thing which these wise men think of, is to lay the

inventor under the obligation of making a telescope through which *one could see with two eyes.*

Two days afterwards, the 4th of October, 1608, we find the following entry upon the Journals of the States:—

' *Sabathi*, 4 *October*, 1608.

' Resolved, that inclusive of the communication held the 2d instant with Hans Lippershey, a native of Wesel, inventor of the instrument to see at a distance, one person from each province will be named, to examine and to try the said instrument on the turret of the mansion of his Excellency (Prince Maurice), and to investigate whether it is likely to be of such utility as is generally believed; and, in such a case, to treat with the inventor, that he undertakes to make three such instruments of *rock-crystal* (*christael de roche*), for which he asks a thousand florins a-piece; that he moderates his charge, and promises never to transmit his invention to anybody.'

In this piece we have the counterpart of what happened to Galileo at Venice. Here we have the members of the States-General ascending the turret on Prince Maurice's house, to examine a distant object with the newly-invented spy-glass, as the Venetian senators mounted the steeple of St. Mark; and probably Lippershey was equally tired as the Italian philosopher, with showing off his instrument to persons requiring telescopes to make them see with two eyes.

The mention which, in this early stage of the invention, is made of rock or mountain crystal, appears very curious. It seems that, in this beginning, the difficulty of procuring glass fit for telescopes was equally as great as it is now, and *rock* crystal was frequently resorted to, in the construction of object-glasses. This appears, amongst others, from a passage in Hevelius, who, however, gives the preference to glass. At this present day the Parisian optician, Cauchoix, constructs telescopes of rock or mountain-crystal, which he calls *lunettes vitro-crystal lines;* but which, in my opinion, are inferior to glass telescopes of equal size. One consequence may be deduced from the circumstance of rock-crystal being used in the construction of these telescopes, which is, that this spectacle-maker must have been well skilled in his profession,

inasmuch as it is much more difficult to work and to select crystal than glass.

The 6th of October following, mention is made again in the Acts of the States, of the subject of telescopes :—

'*Lunæ*, 6 *October*, 1608.

'The Commissioners of the Provinces who have examined the instrument made by John Lippershey, spectacle-maker, and who have communicated with him, report that the instrument is likely to be of utility to the state, and that in consequence they offered to the inventor to make such an instrument of *rock-crystal* for the state, at the price of three hundred florins, payable immediately, and six hundred florins more when the instrument is completed and approved of. Resolved, to authorise these gentlemen, as is done by the present, to come to a final conclusion with Lippershey, about the making of the said instrument, and to limit him a time within which the instrument is to be completed and delivered in good order. And then the States are to deliberate whether a privilege or an annual pension is to be granted to the petitioner, under condition, that he will promise to make no such instruments, but with the consent of the States.'

Whilst these transactions were taking place with Lippershey, Metiús, the second competitor, handed in his petition the 17th of October. Having gone so far with Lippershey, the States were perhaps at a loss how to dispose of Metiús's claim. They contented themselves with giving him some empty words of encouragement, and some vague promises for the future. After this time nothing more was done by Metiús to attract public notice. He doggedly refused to show his telescopes to anybody, not even to Prince Maurice, and least of all to his brother, the Professor of Franeker. Perhaps Jacob Metiús was disgusted with the little encouragement he received, and it is not unnatural to suppose, that a man of his eccentric habits, having once failed in his object, could not make up his mind to make a second attempt.

The petition of Metiús appears, however, to have had some influence on the manner in which the petition of Lippershey was disposed of.

We next find the following notes on the Record Book of the States-General :—

' *Jovis,* 11*th December,* 1608.

' The petition is read of Hans Lippershey, spectacle-maker, inventor of a certain instrument for seeing distant objects: no resolution has been taken on it, but Messrs. Van Dordt, Magnús, and Vander Aa, are appointed to speak with the petitioner about the said invention.'

' *Lunæ,* 15*th December,* 1608.

' Messrs. Magnús and Vermanne report, in the absence of Messrs. Vander Aa and Boeles, that they examined the instrument invented by the spectacle-maker, Lippershey, to see at a distance *with two eyes*, and that they approved of it; in consequence of which it was proposed whether the privilege ought to be granted to the said Lippershey, of making alone the said instrument for a certain number of years, and to pay him the remaining six hundred florins which were promised him for the said instrument. Resolved, that whereas it appears *that many other persons have a knowledge of this new invention,* to see at a distance, it is expedient to refuse the prayer of the petitioner for an exclusive privilege, but that he will be commanded to make, within a certain time, two other instruments of his invention, for seeing with two eyes, for the same price; and checks are to be despatched to him for three hundred florins, and when the instruments are completed, of six hundred florins.'

Lippershey used no delay in making the instruments, thus setting an example which the most eminent in his profession are said not to have always followed. The next mention is made of Lippershey in February, 1609.

' *Veneris,* 13*th February,* 1609.

' Hans Lippershey delivered the two instruments for seeing at a distance, which he was ordered to make, and in consequence it has been resolved to despatch checks of the three hundred florins remaining of the nine hundred which were promised him for three of the said instruments.'

From these documents it appears that both Lippershey

and Metiús failed in their attempt of obtaining an exclusive privilege. But certainly the instruments of the former were liberally paid. Nine hundred florins, or 75*l.*, for an instrument such as it can be expected to have been at that time, is certainly a high price; and even at the present time a very respectable telescope could be obtained for that money. From this circumstance, we would be rather inclined to argue, that these instruments were not so roughly made as Italian authors, and those who follow them, are willing to persuade us. Our thrifty forefathers were too prudent and too economical to throw away considerable sums of the public money on things of bad manufacture and rough making.

Italian writers generally represent the Dutch telescopes as very imperfect. But how do these writers know this? Has Nelli, or any other, ever seen one of the telescopes of that time? If not, how can they judge of their performance? There is not the least necessity, in order to value the transcendent genius of a Galileo at its proper standard, to depreciate the merit of others; and we may admire Galileo without being unjust towards his contemporaries.

It is very remarkable that the absurd wish of the States to have an instrument which would enable them to see with two eyes, should have led to the invention of an instrument which has at present fallen into undeserved oblivion. It appears from the official documents, that Lippershey, indeed, gratified the wishes of the States, and that he produced an instrument with which they could see with two eyes. There can be little doubt but that this instrument was what was called afterwards *a binoculus.* The invention of this instrument is generally attributed to the Capucin friar, Rheita*, who describes it in one of the most singular books which ever were written. For terrestrial objects a well-arranged binoculus is perhaps the most pleasant telescope, but some dexterity is wanted to bring it to proper adjustment. It shows the objects considerably brighter and more distinct than a common telescope

* Oculus Enoch et Eliæ, sive Radius sidereo-mysticus planetarum, Antwerpiæ, 1645, fol. p. 338, 354. See also Dioptrique Oculaire par le Père Cherubin Le Gentil, Mémoires de l'Académie des Sciences, 1787. Smith's Optics, p. 974.

of equal power; and it has the great advantage of not straining and fatiguing the eye.

The readiness with which Lippershey furnished the States with the binoculus is a proof of considerable ingenuity, and must tend to do away with the notion that he was a low, ignorant mechanic, guided by mere chance.

The States, refusing to grant the privilege which Lippershey wished to obtain, give as a reason of their refusal that the invention was *known to many*. Of this we have evidence in Metiús's petition; but we may find some more, in a book from which one would little expect to draw scientific information.

Negotiations, which terminated in a twelve years' truce, were then pending in the Hague, between the States and Spain. The ministers of the King of France, Henry IV., were the celebrated President de Jeannin and Monsieur Bussi. The letters which Jeannin wrote on the subjects of these negotiations to the king and his ministers have been printed, and amongst them we find something relating to the history of the invention of telescopes *.

Thus on the 28th of December, 1608, a few days after the States had refused the privilege to Lippershey, Teannin and Bussi write to the king.

'The bearer, who returns to France, is a soldier of Sedan, who served some time in Prince Maurice's company †. He possesses several inventions for the war, and that form of glasses (the French has *lunettes*) which have recently been invented in this country by a spectacle-maker of Middleburg, by which one sees at a great distance. The States ordered the workman, who is the inventor of them, to make two for your majesty. We should not have required their favour, if the artist had been willing to make them at our own request; but he refused, saying, that he had express orders from the States, not to make them for anybody. We will send them to your majesty on the first opportunity; and notwithstanding this soldier makes them as well (*aússi-bien*) as the other, as appears

* Lettres et Négotiations du Président de Jeannin. Paris, fol. 1656.
† In the Prince's guards.

by the trials which he made, still the difficulty of making them is not great.'

The same day the President writes to the Minister Sully:

'The bearer of this letter is a soldier from Sedan, who belongs to the prince's company, and who is held very ingenious in many inventions and artifices of the war. He has also made, a few days ago, an engine (*un engin*), in imitation of that which has been made by a spectacle-maker of Middelburg, to see at a distance. He will show it to you, *and make you some for your sight.* I requested the first inventor to make me two, one for the king, and one for you; but the States prohibited him from making any but for themselves. They ordered some themselves to give them to me, that I may send them to you, which I will do the first day.'

The king's reply is very remarkable, being written about a year before that prince was murdered at the instigation of the Jesuitical faction. He writes thus the 8th of January, 1609:

'I shall see with pleasure the glasses which you mention in your letter, though at present I am more in want of such that can show me things near me, than of those which show distant objects.'

Having thus shown what are the respective claims of Metius and Lippershey, we must now consider those of a third pretender to the honour of the invention. This person was also a spectacle-maker of Middelburg, called Zacharias Tansz, and he has, more generally than Lippershey, been considered as the original inventor. The information of what we know about him must be wholly derived from Borel's book on the invention of telescopes. William Boreel, who appears to have been very anxious about this matter, being himself a native of Middelburg, had all the persons then living, and knowing something on the subject, examined before the magistrates in 1655. Their depositions are given in Borel's book; but the originals of these depositions have not been found in the records of the town of Middelburg, although a very diligent search was made for them. In these documents, the places and houses in which both Lippershey and Zacharias Tansz lived, are frequently mentioned. These houses have since been taken

down, and an open space now occupies the place where the telescope was invented.

Some of the witnesses, whose evidence is given in Borel's book, are in favour of Lippershey, and some in that of Zacharias. We must now carefully sift that evidence, and compare it with what Borel says on the subject, in a letter to Pierre Borel.

The first witness who occurs on the list is John Willems, a steward or beadle. He is seventy years of age, in 1655, and knew Lapprey personally when he made spectacles. Afterwards he made telescopes (*tubos longos*), which he did about fifty years, when Lapprey offered the first telescope to Prince Maurice, as he (the witness) heard at the time.

This witness brings the invention down to 1605, but he does not appear to have had a very clear recollection of the exact time of the invention.

The second witness is Edwold Kien. He is a messenger, aged sixty-seven; says that the man who made the telescopes was John Laprey, of Wesel; that he began making telescopes about 1610, and died in October 1619. He (the witness) married the daughter of this Laprey. Laprey offered to Prince Maurice and to the States some of his telescopes, for which he got a reward, and a privilege for three years. He adds, that the sign of the house where Laprey lived was *a telescope*.

From a comparison of dates, it is obvious that this witness is mistaken, and that Lippershey made telescopes, and offered them to the States long before 1610.

The third witness is a blacksmith of the name of Abraham Junius, aged, in 1655, seventy-seven. He says, that the name of the man who first made telescopes in this town was Hans, *i. e.* John, but that he did not observe the surname; that this man was commonly called John the spectacle-maker; that about forty-five or forty-six years ago this John made the first long telescopes (*conspillia longa*); that the witness knew him long ago, before he made spectacles, when he was a bricklayer; he assisted at the funeral of John; he knows, and heard very often that John made long tubes (*tubos longos*) and telescopes for the use of Prince Maurice.

This witness brings the invention to 1609 or 1610, and very little is to be concluded from his evidence.

The Capucin friar, Rheita*, attributes also the first invention to Lippershey, whom he calls Lippensum. This is certainly no great alteration of the original name, not greater than that which is made by the English author of the Life of Galileo, who chooses to translate Borel's name into Italian, and calls him *Borelli.* According to the version of Rheita, the invention dates from 1609, when Lippershey happened to place a convex before a concave, and discovered, by chance, that the weather-cock of a neighbouring church, and other objects, were magnified. He placed his glasses in a tube, and amused the visiters of his shop by showing them the weather-cock magnified, and larger than it could be seen with the unassisted eye. The Marquess of Spinola, happening to be at the Hague at the time, to negotiate about the truce, saw this new instrument, bought it, and gave it to the Archduke Albert of Austria, the Spanish Governor of Belgium.

In the mean time, persons of high station (*proceres*) heard of the circumstance, and that other similar instruments had been constructed by the maker. The inventor was forced to sell his instrument for a great price; but he was prohibited from making or selling any more of them. In this manner, says the worthy friar, this noble and capital invention would have remained in obscurity, and hidden perhaps for ever, if it had not been transferred, by the will of God, to the court of Brussels, and made known there.

The Capucin friar is mistaken in the dates, bringing the invention to 1609 instead of 1608. But, besides, the Marquess of Spinola was not at the Hague in 1609. He left that city the 30th of September, 1608, together with the other Spanish ministers. That he left the Hague a little before Lippershey presented his petition to the States; but the Marquess, residing at the Hague, certainly could not see an apparatus which a spectacle-maker had erected in his shop at Middelburg; but, at all events, there is a possibility that Spinola, residing at the Hague in September, 1608, heard of the invention, and produced a telescope for the Archduke.

* Oculus Enoch and Eliæ, p. 337.

(To be continued.)

Proceedings of the Royal Institution of Great Britain.

FRIDAY EVENING MEETINGS.

Jan. 21st.—The meetings for the season commenced this evening, and will be continued every Friday, except those of Passion and Easter weeks, until the 10th of June. The subject in the lecture-room, upon the present occasion, was given by Mr. Faraday, being, in fact, the developement and illustration of that peculiar class of optical deceptions which forms the object of the first article in the present Journal, p. 205. The effects were shown by large wheels cut out of pasteboard: those produced, by casting the shadows of the moving wheels upon a screen, were exceedingly well exhibited by means of the cone of rays from a magic-lantern. The appearances exhibited by reflection were also well shown; and, as some effects beyond those mentioned in the paper had been observed, and were explained, Mr. Faraday will add a note of them at the end of these proceedings.

In the library, Mr. Cuthbert showed the power of his beautiful microscope, by exhibiting some wheel animalculæ; and Mr. Varley also exhibited more of these animals, by means of excellent microscopes in his possession; the object was to give the members an opportunity of seeing the appearance of this curious creature, that they might the better understand the references made to it by Mr. Faraday, in pursuance of his subject.

Numerous presents of specimens of natural history, books, engravings, &c. &c. were laid upon the library-table. Mr. Pepys brought to the meeting a very beautiful piece of American glass casting; it was a small plate, the upper surface smooth, but the under surface covered by a beautiful design of scroll-work, &c. in very high relief, so that, as the plate stood upon a table, the reflection of light from it was of the most brilliant and metallic kind. The plate had been cast, the wheel had never touched it, yet the surface

looked as well almost as if cut; and the pattern was so rich and full, and of such a kind, as to preclude any imitation of it by cutting. Mr. Pepys also placed a beautiful spiral metallic thermometer, by Breguet, upon the table.

NOTE BY M. F.

In consequence of the necessity I was under of sending the paper referred to in the above proceedings to press (page 205) by a certain time, I was unable to pursue many of the beautiful combinations of form, colour, and appearance, to which the experiments led, especially as they promised only amusement and little more of instruction than the paper itself contained; but one or two varieties in the appearances, which have occurred to me since, are so striking, that I am glad of the opportunity of noticing them briefly in the same number with the paper. At page 218, I have described the singular appearance produced when the reflected image of a revolving cog-wheel, held before a glass, is observed through the cog-wheel itself. If, in such a wheel, a little nearer the centre, a series of regular apertures be cut, so as to represent cogs and their intervals, but the number different by 1.2.3, or any small quantity, from the number of the cogs, then, upon making the experiment as before, that series of cogs in the revolving wheel through which the eye looks will appear to stand still, but the other series will travel in the spectrum: upon changing the eye to the other series of apertures, then the quiescent part of the spectrum will move, and the moving part become quiescent. If two or three series more of such apertures be cut in the wheel, concentric one to another, but the number of intervals varying in each, then a great variety of changes are produced, as the eye looks through one part or another of the wheel. The series of cogs in the spectrum move with different velocities, or in opposite directions, changing with the slightest motion of the eye. Two or three persons looking through different parts of the wheel see appearances

entirely different; yet all these deceptive appearances result from a single reflection of a single wheel, moving in a constant direction and with uniform velocity.

By the application of colours and coloured foils, very curious effects occur, which are endless in their variety. As an illustration, let a wheel with a single series of cogs at the edge, and with intervals equal to the cogs, have a circle of colour applied between the cogs and the centre of the wheel; let the part below the cogs be green, and the part below the spaces red; the coloured circle will consist of green and red alternately. If this wheel be revolved before the glass, the green and red mingle, and the reflection observed in the ordinary way will exhibit one uniform colour; but if the reflection be observed from between or behind the cogs, the green and red immediately separate, and besides having the appearance of fixed cogs, there is also the appearance of fixed unmingled colours. If the interval be equal to only half a cog, and three colours be applied, the three colours may, after being mingled by rotation, be again developed, and it is easy in this way to separate many colours from each other. The experiment in illustration of Newton's theory of colour, by painting the head of a top and spinning it, is well known; by the means just described the experiment can be still further extended, and the colours separated one from another, even while the whole system remains in motion.

The combination of other forms than wheels by the apparatus described, page 208, produces very beautiful effects. The application of colours here also is so evident as to need no illustration. The variation of the proportion of the interval to the remaining pasteboard causes many curious appearances, especially when the shadows produced in sun-light are observed.

Since the printing of the paper, a friend has referred me to the article 'Animalcula,' in Brewster's Encyclopædia, where an opinion on the appearance of these creatures is given, nearly the same as that I have ventured. Speaking of the opinions of those who suppose them to be true revolutions, it is said, 'Yet notwith-

standing our respect for the skill and talents of such renowned naturalists, we cannot deny that we think the production of the vortex is more probably effected by the simple motion of the fibrilla—that it may ensue from their rapidly bending in regular or alternate succession, or by some analogous means.'

M. F.

ANALYSIS OF BOOKS.

Philosophical Transactions of the Royal Society of London, for the year 1830. Part II.

1. *Memoir on the occurrence of Iodine and Bromine in certain Mineral Waters of South Britain.* By Charles Daubeny, M.D., F.R.S., Professor of Chemistry in the University of Oxford. [Read May 6, 1830.]

THE author lays claim to being the first who announced to the public the existence of bromine in the mineral springs of England; a discovery similar to that which had been previously made by others in many analogous situations on the Continent. His reason for offering the present communication to the Royal Society is, that he has examined on the spot a great number of mineral springs, and endeavoured to obtain, wherever it was practicable, an approximation to the proportion which iodine and bromine bear to the other ingredients. He has also aimed at forming an estimate of their comparative frequency and abundance in the several rock formations; an object of considerable interest in geology, as tending to identify the products of the ancient seas in their most minute particulars with those of the present ocean. The results of his inquiries are given in the form of a table, in which the springs, whose waters he examined, are classified according to the geological position of the strata from which they issue, and of which the several columns exhibit the total amount of their saline ingredients, the nature and proportion of each ingredient, as ascertained by former chemists, or by the author himself; and lastly, where they contained either iodine or bromine, the proportions these substances bear to the quantities of water, and likewise to the chlorine also present in the same spring. He finds that the proportion of iodine to chlorine varies in every possible degree, and that even springs which are most strongly impregnated with common salt, are those in which he could not detect the smallest trace of iodine. The same remark, he observes, applies also to bromine; whence he concludes, that although those two principles may perhaps never be entirely absent where the muriates occur, yet their relative distribution is exceedingly unequal. The author conceives that these analyses will tend to throw some light on the connection between the chemical constitution of mineral waters and their medicinal virtues. Almost the only two brine springs, properly so called, which have acquired any reputation as medicinal agents, namely, that of Kreutznach in the Palatinate, and that of Ashby de la Zouche in Leicestershire, contained a much larger proportion than usual of bromine; a substance, the poisonous quality of which was ascertained by its discoverer, Balard. The

author conceives that these two recently-discovered principles exist in mineral waters, in combination with hydrogen, forming the hydriodic and hydrobromic acids, neutralized, in all probability, by magnesia, and constituting salts which are decomposable at a low temperature. He has no doubt that a sufficient supply of bromine might be procured from our English brine springs, should it ever happen that a demand for this new substance were to arise.

2. *Experiments to determine the difference in the Number of Vibrations made by an invariable Pendulum in the Royal Observatories of Greenwich and Altona.* By Captain Edward Sabine, of the Royal Artillery, Secretary to the Royal Society. [Read March 25, 1830.]

The invariable pendulum, No. 12, with which experiments recorded in this paper were made, was vibrated in the Royal Observatory of Greenwich in July, 1828; in the Royal Observatory at Altona in September and October of the same year; and again at the Royal Observatory at Greenwich in August, 1829. The mean of the results obtained at Greenwich in July, 1828, and in August, 1829, gives the rate of this pendulum at Greenwich, to be compared with its rate obtained at Altona. The details of all these series of observations are given in a tabulated form.

3. *Experiments to ascertain the correction for Variations of Temperature within the limits of the natural Temperature of this Climate, of the invariable Pendulum recently employed by British Observers.* By the same Author. [Read March 25, 1830.]

The correction for temperature which the author deduces as the general result of his investigation, is 0.44 of a vibration per diem for each degree of Fahrenheit between 30° and 60°. He considers this result as entitled to the greater confidence, from the favourable nature of the circumstances under which the inquiry was conducted; since the influence of natural temperature is more permanent and equable than that of temperatures artificially produced. He considers it as desirable, however, that means should be devised of extending experiments on this subject to a wider range of temperatures.

4. *On a new Register Pyrometer, for measuring the Expansions of Solids, and determining the higher degrees of Temperature upon the common thermometric scale.* By J. Frederic Daniell, Esq. F.R.S. [Read June 17, 1830.]

In the year 1821, the author published in the Journal of the Royal Institution, an account of a new pyrometer, and of some determinations of high temperatures, in connection with the scale of the mercurial thermometer, obtained by its means. The use of the instrument then described was, however, limited; and the author was subsequently led to the invention of a pyrometer of a more

universal application both to scientific researches, and to various purposes of art. He introduces the subject by an account of the attempt of M. Guyton de Morveau, to employ the expansions of platina for the admeasurement of high temperatures, and for connecting the indications of Wedgewood's pyrometer with the mercurial scale, and verifying its regularity. The experiments of that philosopher, on the contraction of porcelain in actual comparison with the platina pyrometer, were extended to no higher temperature than the melting point of antimony; but they are sufficient to establish the existence of a great error in Wedgewood's original estimation of his degrees up to that point. This he carries on by calculation, on the hypothesis of uniform progression of expansion, up to the melting point of iron; the construction of his instrument not admitting of its application to higher temperatures than a red heat, in which platina becomes soft and ductile.

Mr. Daniell shews, by an examination of M. Guyton's results, that he has failed in establishing the point he laboured to prove, namely, the regularity of the contraction of the clay pieces.

The pyrometer of the author consists of two distinct parts, the one designated the *Register;* the other the *Scale.*

The first is a square tube of black lead, eight inches long, cut out of a common crucible of the material, closed at one end, and having at the other a portion of about six tenths of an inch in length, cut away to the depth of half the diameter of the bore, so as to leave a shoulder near the end. A bar of any metal, six inches and a half long, is introduced into the cavity, resting against its solid end, and a cylindrical piece of porcelain, about one and a half inch long, which he calls the index, is placed upon the top of the bar, and projects beyond the open part of the tube, being confined in its place by a ring or strap of platina passing round it, and also round the end of the black lead bar, and made sufficiently tight by a small porcelain wedge inserted between them. When the instrument thus prepared is subjected to heat, the porcelain index will be forced up by the expansion of the metallic bar, to a certain distance, where it will remain when the bar retires from it, on cooling. The distance it has been moved from its original position, will be the measure of the difference of expansion of the metallic bar, and of an equal length of the black lead, in which it is contained. This cannot be influenced by any permanent contraction which the black lead may undergo by intense heat; because any such contraction will occur at the moment of the greatest expansion of the metal; and the index will still mark its point of furthest extension upon this contracted basis. It remains then to measure accurately the distance to which the index has been moved, by the application of the scale, which is a detached instrument constructed of two rules of brass, joined together at a right angle, the one fitting square upon two sides of the black lead bar, the other resting on its shoulder; with these are connected two arms, which, acting on the principle of proportional compasses, measure the distance of the

extremity of the index from the shoulder of the black lead bar. The spaces comprehended between the points of the shorter legs of the compasses, are magnified ten times by the longer legs, the angular motion being measured by a graduated arc furnished with a vernier, and capable of being easily read off to minutes.

The author next enters into a comparison of the results afforded by this instrument with those of former experimentalists, and especially with the accurate determination of the expansions of metals by Messrs. Dulong and Petit, with a view to shew the degree of confidence to which it is entitled. The close agreement in the results of a great number of experiments upon metals, which differ much in their expansions, is highly satisfactory in this respect. Differences having been found in the expansibility of different specimens of black lead, it becomes necessary to ascertain the expansions of each register for itself, by applying to it the heat of boiling mercury.

The author concludes with an account of some experiments which he made to determine the fusing points of different metals, referred to the common thermometric scale. The final results which he obtained were—for silver, 1873°; copper, 1996°; gold, 2016°; iron, 2786°.

A remarkable accordance is found between the results with platina and with iron, metals which differ widely in their expansions; conformably with the conclusion of MM. Dulong and Petit, the expansion of iron increases at higher temperatures in a greater ratio than that of the platina. The discrepancy between the temperatures derived from the observations with his first pyrometer and the present one he admits to be considerable, but believes they may be sufficiently accounted for by the differences in the circumstances of the experiments, without imputing inaccuracy to either instrument.

The author next attempted to ascertain the effects of the most intense heat which it was possible to produce in a furnace, and to measure the utmost limits of expansion in a platina bar; but various circumstances interfered with the success of these experiments, which afforded, however, many curious results as to changes of integration in platina by the effects of heat. The paper concludes with some observations on the practical advantages possessed by the present instrument*.

5. *On the Phenomena and Laws of Elliptic Polarization, as exhibited in the Action of Metals upon Light.* By David Brewster, LL.D., F.R.S. L. and E. [Read April 22, 1830.]

The action of metals upon light has always presented a remarkable, and hitherto inexplicable anomaly in the science of polarization. Malus, to whom this branch of optics owes its origin, had at first announced that metals exerted no polarizing influence on

* The register-pyrometer is made by Mr. Newman, 122, Regent Street.

light; but Dr. Brewster, by employing a different method of observation, ascertained that the light reflected from metallic surfaces was modified in such a manner as to exhibit, when transmitted through thin crystallized plates, the complementary colours of polarized light. He afterwards discovered the curious property possessed by silver and gold of dividing a polarized ray into complementary colours by successive reflexions. Mr. Biot, to whom the author communicated this discovery, pursued the inquiry to which it led, and arrived at the same conclusions as to the mode in which this class of phenomena should be explained. Subsequent researches, however, convinced the author that these generalizations had been too hastily formed, and the study of Fresnel's curious discoveries respecting circular polarization enabled him to advance still further in the inquiry; and he now presents to the Royal Society, in this paper, a complete analysis of the singular phenomena exhibited in the action of metals upon light.

The first section of the paper treats of the action of metals upon common light. A ray of common light reflected from a metallic surface, when analysed by a rhomb of calcareous spar, exhibits a defalcation of light in one of the images, as if a portion of the light was polarized in the plane of reflexion. This effect will be still more distinctly seen on examining the system of polarized rings formed round the axes of crystals by means of the light reflected from metals. If the light had suffered no modification by reflexion, or if the metal reflected in equal quantities the light polarized in opposite planes, the rings would not be visible at all. Whereas it is found that they are easily visible in the light reflected from all metals. They are most distinctly perceived at an incidence of about 74°, and become more and more faint as the incidence succeeds or falls short of that angle. They appear best defined in light reflected from galena, and from metallic lead, and with least distinctness in light reflected from silver and gold. On examining the effect of successive reflexion of the same ray by metallic surfaces, the author found that the quantity of light which each polarizes in the plane of reflexion increases with every reflexion; and that in several cases the whole incident pencil is completely polarized.

The action of metals upon polarized light forms the subject of the second section of this paper, in which he investigates the changes which polarized light undergoes accordingly as it is reflected at different angles of incidence, and in different azimuths of the plane of primitive polarization. The light experiences in these cases a physical change of a nature intermediate between that of completely polarized light, and light wholly unpolarized; neither does it possess the same characters as that which has passed through thin crystallized plates. Its constitution is exceedingly analogous to light which is circularly polarized; that is, which comports itself as if it revolved with a circular motion during its transmission through particular media. But in the case of circular

polarization, the ray has the same properties in all its sides, and the angles of reflexion at which it is restored to simple polarized light in different azimuths are all equal, like the radia of a circle described round the ray. In the case of metallic reflexions; the new phenomena discovered by Dr. Brewster may be designated by the term *elliptic polarization*, because the angles of reflexion at which this kind of light is restored to polarized light may be represented by the variable radius of an ellipse. In circular polarization the restored ray has its plane of polarization always inclined 45° to the plane of the second system of reflexion. In elliptic polarization the inclination of the plane of the restored pencil is always less than 45°. In the former case this plane continues by successive reflexions to oscillate on each side of the plane of reflexion, with a never varying amplitude $+45°$ to $-45°$. While in the latter case the same plane oscillates with an amplitude continually diminishing till it is brought to Zero in the plane of reflexion. In steel the polarization is highly elliptical, and the amplitude of the oscillations of the plane of restoration is quickly brought to Zero; but in silver, whose polarization approaches nearly to circular, the oscillations diminish very slowly in amplitude. The peculiar character of elliptic polarization shews itself also in another manner, in the variable position of the ellipses which regulate its angles of restoration upon steel. In the third section of his paper, the author treats of the complementary colours produced by successive reflexion from the polished surfaces of metals.

He concludes by observing, that although we do not understand the nature of the forces by which metals reflect the two oppositely polarized pencils, yet we are certain they do not act exactly in the same manner as the second surfaces of transparent bodies: when producing total reflexion setting out from a perpendicular incidence, the least refrangible rays begin to suffer the double reflexion sooner than the mean ray, and they sooner reach their maximum of elliptic polarization, thus exhibiting the inversion of the spectrum. The theory of circular polarization, as given by Fresnel, will, no doubt, embrace the phenomena of elliptic polarization, and when the nature of metallic action shall have been more thoroughly examined, we may expect to be able to trace the phenomena under consideration to their true source.

6. *Researches in Physical Astronomy*. By John William Lubbock, Esq., F.R.S. [Read April 29, 1830.]

The analytical expressions for the variations of the elliptic constants given by Laplace, in his *Méchanique Céleste*, are true only when the square and higher powers of the disturbing forces are neglected in the computation: and by proceeding on the supposition that all the planets move in circular orbits and in the same direction, he has demonstrated that the eccentricities and inclinations vary within small limits, and that the stability of the planetary system is always eventually preserved. But Mr. Lubbock shews, in the present paper,

that these conditions are not necessary to the stability of a system of bodies subject to the law of attraction which governs one system; and he gives expressions for the variations of the elliptic constants which are rigorously true, whatever power of the disturbing force be retained.

7. *On the Error in Standards of Linear Measure, arising from the Thickness of the Bar on which they are traced.* By Captain Henry Kater, V.P., and Treasurer of the Royal Society. [Read June 17, 1830.]

While engaged in the adjustment and verification of the copies of the Imperial Standard Yard destined for the Exchequer, Guildhall, Dublin, and Edinburgh, the author discovered a source of error arising from the thickness of the bar, upon the surface of which measures of linear dimensions are traced. A notice to that effect was published in the Philosophical Transactions for 1826; and the object of the present paper is to give an account of the experiments the author has since made on this subject, and to describe a scale which he has had constructed so as almost entirely to obviate the source of error thus introduced.

From the experiments detailed in the first part of the paper, the following conclusions are deduced. 1. That in a standard of linear measure traced upon the surface of a bar, an error arises from the thickness of the bar when it is placed upon a table, the surface of which is plane. 2. That this error in bars of the same material and of unequal thickness lies within certain limits as respects the thickness of the bar, and depends upon the extension of the surface of the bar which becomes convex and the compression of the bar which is concave. 3. That the error to which the same scale is liable from this cause is directly as the versed sine of the curvature of the surface upon which the scale is placed. 4. That the error very far exceeds that which would arise from the difference of length between the arc and its chord under similar circumstances, so much so that the sum of the errors from this cause in a bar one inch thick with a versed sine of not one thousandth of an inch is nearly one thousandth of an inch, whilst double the difference between the chord and the arc is not one fifty thousandth.

The author devised the following method of trying a surface supposed to be plane; namely, by applying to it in different directions a pianoforte wire, one 100th of an inch in diameter, which bears a considerable degree of tension without breaking, strung on a bow six feet long; a contrivance which, he states, may be applied to a great variety of useful purposes when a straight edge is required. He could detect the nature, and, in some degree, the extent of the irregularities of a surface by tapping with the fingers upon the wire whilst it was pressed by the weight of the bow upon the board. When it yielded no sound, the wire was, of course, in contact with the surface, which was in that case either convex or plane. When the wire yielded a sound the surface was concave; and some

idea might be formed of the extent by the acuteness or gravity of the sound produced, the edges of the concavity serving as bridges which limited the length of the string. So delicate is this test, that a concavity can be detected by this method, when the interval between the wire and the surface under examination is imperceptible to the eye.

The error in question, resulting from the extension and compression of the surfaces of the bar dependant upon its curvature is obviated in the following manner:—The neutral surface which suffers neither extension nor compression is shewn by the author to be at about one-third of the thickness of the bar from the surface which becomes convex. When the object is to have two points only on the bar, by cutting away one-half of the thickness of the bar at its ends, and placing the points upon the new surfaces, the error is reduced to the least possible quantity. But when a scale of inches is required, the nearest approximation to correct measurement is obtained by diminishing, as much as possible, the thickness of the bar, and by providing another bar on which it is to be supported, and on which it is allowed to slide freely in a dovetailed groove formed by two side plates of similar thickness, screwed to the surface of the bar, and to which it is to be fixed at its middle point by a single screw passing through it.

8. *On the Illumination of Light-houses.* By Lieut. Thomas Drummond, of the Royal Engineers.

The author, after briefly describing the different methods at present employed for illuminating light-houses, proceeds to detail what he considers an improvement upon those now in use. This consists in substituting for the Argand burners a small ball of lime, ignited by the combustion of oxygen and hydrogen.

From this small ball, only three-eighths of an inch in diameter, so brilliant a light is emitted, that it equals in quantity about thirteen Argand lamps, or 120 wax candles; while, in intensity or intrinsic brightness, it cannot be less than 260 times that of an Argand lamp. These remarkable results are deduced from a series of experiments made lately at the Trinity-house; and, having been repeated with every precaution, and by different individuals, there seems no reason to doubt their accuracy. In the best of our revolving lights, such as that of Beachy Head, there are no less than thirty reflectors, ten on each side. If, then, a single reflector, illuminated by a lime ball, be substituted for each of these ten, the effect of the three would be twenty-six times greater than that of the thirty. On account of the smaller divergence of the former it would be necessary to double their number, placing them in a hexagon instead of a triangle. In this case the expense is estimated at nearly the same. This method was tried lately at Purfleet in a temporary light-house, erected for the purpose of experiments by the corporation of the Trinity-house, and its superiority over all the other lights with which it was contrasted was fully ascertained and acknowledged.

On the evening of the 25th of May, when there was no moonlight, and the night dark, with occasional showers, the appearance of the light viewed from Blackwall, a distance of ten miles, was described as being very splendid. Distinct shadows were discernible, even on a dark brick wall, though no trace of such shadows could be perceived when the other lights, consisting of seven reflectors with Argand lamps, and the French lens, were directed on the same spot. Another striking and beautiful effect peculiar to this light was discernible when the reflector was turned, so as to be itself invisible to the spectator. A long stream of rays was seen issuing from the spot where the light was known to be placed, and illuminating the horizon to a great distance. As the reflector revolved, this immense luminous cone swept the horizon, and indicated the approach of the light long before it could itself be seen from the position of the reflector.

These singular effects must not, however, be understood as constant accompaniments of this light, for on a moonlight night, or when the weather is very hazy, they cease to appear.

9. *On the Electro-Magnetic properties of Metalliferous Veins in the Mines of Cornwall.* By Robert Ware Fox. [Read June 10, 1830.]

The author having been led, from theory, to entertain the belief that a connection existed between electric action in the interior of the earth, and the arrangement of metalliferous veins, and also the progressive increase of temperature in the strata of the earth as we descend from the surface, proceeded to the verification of this opinion by experiment. His first trial was unsuccessful, but in the second, he obtained decisive evidence of considerable electrical action in the mine of Huel Servel, in Cornwall. His apparatus consisted of small plates of sheet copper, nailed, or else wedged closely, against the wooden props stretched across the galleries. Between two of these plates of different stations, a communication was made, by means of copper wire, one twentieth of an inch in diameter, which included a galvanometer in its circuit. In some instances three hundred fathoms of copper wire was employed.

The intensity of the electric currents was found to differ considerably in different places; it was generally greater in proportion to the greater abundance of copper ore in the veins; and in some degree also to the depth of the station. Hence the discovery of the author seems likely to be of practical utility to the miner in discovering the relative quantity of ore in veins, and the directions in which it most abounds. The electricity thus perpetually in action in mines, does not appear to be influenced by the presence of the workmen and candles, or even by the explosion of gunpowder in blasting.

The author's experiments enable him to give a table of the relative powers of conducting galvanic electricity possessed by various metalliferous minerals. This power, he remarks, appears to bear no

obvious relation to any of the electrical or other physical properties of the metals themselves, when in a proper state, or to the proportions in which they exist in combination. He proceeds to point out various facts relative to the position of veins and the arrangement of their contents, which he thinks are irreconcilable with any of the hypotheses that have been devised to explain their origin.

He observes that ores which conduct electricity have generally some conducting substances interposed in the veins between them and the surface; a structure that appears to bear a striking analogy to the ordinary galvanic combinations. He is of opinion that the intensities both of heat and of electricity, and consequently of magnetism, increase in proportion to the depths of the strata under the surface of the earth; that they have an intimate connection with one another; and that the discovery of electrical currents in various, and frequently opposite directions, in different parts of the same mine, may, perhaps, hereafter afford a clue to explain the declination and variation of the magnetic needle.

10. *Sequel to a Paper on the tendency to Calculous Diseases, and on the Concretions to which such Diseases give rise.* By John Yelloly, M.D., F.R.S. [Read June 17, 1830.]

The author, in a paper published in the last volume of the Philosophical Transactions, gave the analysis of 328 calculi contained in the collection of the Norfolk and Norwich hospital; and has been since enabled to complete the analysis of the 335 remaining specimens which have now been divided. The results of the analysis are given in a tabular form, exhibiting in the order of their occurrence from the centre the consecutive deposits of the different materials of which the calculi are composed, according to the most prominent character of each material. The most remarkable circumstance brought to light in the course of this investigation, is the discovery of the presence of silex in one specimen composed principally of oxalate of lime, and weighing about five grains. The particles of silex were very minute, and were imbedded in, and diffused through the oxalate of lime. Three examples of a similar occurrence are quoted by the author.

The paper concludes with a few remarks on the statistical conclusions stated in his former communication. He thinks there is reason to believe that the average number of calculous disorders in Scotland has been much under-rated; on the other hand, the proneness to those complaints is very small in Ireland. A much larger proportion of calculous cases occurs in towns than in the country.

The Life of Sir Humphry Davy, Bart., LL.D., late President of the Royal Society, &c. &c. &c. By John Ayrton Paris, M.D., F.R.S., &c. &c. 4to. London, 1831.

THE history of science offers to our notice several remarkable epochs at which the human mind has seemed to receive an extraordinary impulse, when a train of circumstances has led to the development of some supereminent genius, who, soaring beyond the ken of his fellow men, by his happy discoveries in the regions of truth and nature, has traced out new roads to knowledge, tending to advance the progress of civilization whole ages in a few short years. Such were Bacon, Galileo, Kepler, Newton, Franklin, and Watt. A similar period has just elapsed in the first thirty years of the nineteenth century, and such a gifted being was Davy.

Dr. Paris has justly observed that—

'The extent of our obligations to a philosopher cannot be appreciated until time shall have shown the various important purposes to which his discoveries may administer. The names of Mayow and Hales might have been lost in the stream of discovery, had not the results of Priestley and Lavoisier shown the value and importance of their statical experiments on the chemical relations of air to other substances. The discoveries of Dr. Black on the subject of *latent* heat could never have obtained that celebrity they now enjoy, had not Mr. Watt availed himself of their application for the improvement of the steam-engine; and the views of Sir H. Davy respecting the true nature of chlorine become daily more important from the discovery of new elements of an analogous nature. In future ages, the metals of the alkalies and earths may admit of applications, and open new avenues of knowledge, of which at present we can form no idea; but it is obvious that, in the page of history, his name will gather fame in proportion as such discoveries unfold themselves.'

Humphry Davy was born at Penzance in Cornwall, on the 17th of December, 1778. His ancestors had long possessed a small estate at Varfell, in the Mounts Bay, to which his father, who had been apprenticed to a carver in wood, and exercised his art with considerable skill, at length succeeded. He was first placed at a preparatory school kept by a Mr. Bushell, who was so struck with the progress he made, that he urged his father to remove him to a superior school, and Dr. Paris has shown that in his early fondness for fiction, and in the power of creating imagery for the gratification of his fancy, Davy greatly resembled Sir Walter Scott. At an early age he was placed at the Grammar School of Penzance, under the Rev. J. C. Coryton, boarding with Mr. Tonkin, an eminent surgeon of that town. While at this school he wrote verses and ballads, and frequently amused his young companions with fireworks and thunder-powders of his own making, and other puerile exhibitions of the same class, which manifested his early passion for experiment. He was extremely fond of fishing, and of shooting when old enough to carry a gun, and made this last amusement subservient to his love of knowledge by forming a collection of rare

birds, which he stuffed with no ordinary skill. From Penzance he went to Truro, in 1793, and finished his education under Dr. Cardew, who did not discern in him the faculties by which he was afterwards distinguished. 'I discovered,' says Dr. Cardew, 'his taste for poetry, which I did not omit to encourage.' Davy's own opinion of the influence of his early school career is interesting.— 'After all,' he says, 'the way in which we are taught Latin and Greek does not much influence the important structure of our minds. I consider it fortunate that I was left much to myself as a child, and put upon no particular plan of study, and that I enjoyed much idleness at Mr. Coryton's school. I perhaps owe to these circumstances the little talents I have, and their peculiar application. What I am I have made myself. I say this without vanity, and in pure simplicity of heart.' His father died in 1794, and his mother (who had taken up her residence at Penzance, and entered into business as a milliner) apprenticed him to Mr. Borlace, a surgeon and apothecary, who afterwards practised as a physician in that town. Davy seems not to have had much predilection for this profession, and though he had long been engrossed with experimental philosophy, he now first manifested his decided turn for chemistry, the study of which he commenced with all the ardour of his temperament. He still continued to write verses, and several of his minor productions were printed in the Annual Anthology, edited by Southey and James Tobin, in 1799. Some of these Dr. Paris has reprinted. We know not whether it was upon the evidence of these effusions, or from the general character of Davy's writings, that it has been said, 'If Davy had not been the first chemist, he would have been the first poet of his age;' but Dr. Paris inquires, 'Where is the modern Esau who would exchange his Bakerian Lecture for a poem, though it should equal in design and execution the Paradise Lost?'

Davy's first original experiments in chemistry are said to have been made to ascertain the quality of the air contained in the bladders of sea-weed, in order to obtain results in support of a favourite theory of light; and to ascertain whether sea-vegetables might not be the preservers of the equilibrium of the atmosphere of the ocean; and he came to the conclusion that the marine cryptogamia were capable of decomposing water when assisted by the attraction of light for oxygen. His instruments of research were of the rudest description, made by himself out of the motley materials which chance threw in his way. Dr. Paris suggests, that from hence we may date his wonderful tact of manipulation, and that ability in suggesting expedients, and contriving apparatus, to meet and surmount difficulties in the unbeaten tracts of science, for which he was afterwards distinguished. At seventeen he had formed and promulgated an opinion adverse to the general belief in the existence of *caloric*, or the materiality of heat.

'No sooner,' says Dr. Paris, 'had he formed his opinion, than his eagle spirit urged him to put it to the test. Having procured a piece of

clock-work, so contrived as to be set to work in an exhausted receiver, he added two horizontal plates of brass; the upper one, carrying a small metallic cup, to be filled with ice, revolved in contact with the lower one. The whole machine, resting on a plate of ice, was covered by a glass receiver, and the air was exhausted by a syringe; [ingeniously modified for the purpose from an old glyster apparatus;] for as yet he had no air-pump, and, what is still more worthy of notice, had never seen one! The machine was now set in motion, when the ice in the small cup began to melt; whence he inferred that this effect could alone proceed from vibratory motion, since the whole apparatus was insulated from all accession of material heat, by the frozen mass below, and by the vacuum around it.'

The experiment was afterwards repeated under more favourable circumstances, and the results published in an Essay on Heat, Light, and the Combinations of Light; and it has been justly observed by Mr. Gilbert, that though it does not at all decide the important matter in dispute, but few young men remote from the society of persons conversant with science would be capable of devising anything so ingenious.

The introduction of Davy to Mr. Davies Gilbert about this time was perhaps one of the most influential circumstances in his life. Mr. Gilbert's attention was attracted to him, as he was carelessly swinging on the hatch or half-gate of Mr. Borlace's house, by the humorous contortions into which he threw his features; and being told he was fond of making chemical experiments, he spoke to him; soon discovered ample evidence of his singular genius, and after several interviews, offered him the use of his library, or any other assistance he might require in the pursuit of his studies, and gave him an invitation to his house at Tredrea, of which Davy frequently availed himself. The tumultuous delight which he expressed on seeing, for the first time, a quantity of chemical apparatus, and an air-pump, is described by Mr. Gilbert as surpassing all description.

Soon after, Davy's acquaintance commenced with Mr. Gregory Watt, who came to Penzance on account of his health, and lodged in the house of his mother.

'Davy sought to ingratiate himself with Mr. Watt by metaphysical discussions; but instead of admiration, he excited the disgust of his hearer. It was by mere accident that an allusion was made to chemistry, when Davy flippantly observed, that he would undertake to demolish the French theory in half an hour. He had touched the chord; the interest of Mr. Watt was excited; he conversed with Davy on his chemical pursuits, and was at once astonished and delighted at his sagacity —the barrier of ice was broken, and they became attached friends.'

Mr. Josiah and Mr. Thomas Wedgwood also spent a winter at Penzance; and Dr. Paris says he has reason to believe their friendship was of substantial benefit to Davy.

Upon the establishment of the 'Pneumatic Institution' at Bristol, for the purpose of investigating the medical power of gases, Dr. Beddoes required an assistant, and Mr. Gilbert recommended Davy. Dr. Beddoes was acquainted with his experiments upon

light and heat, which had produced a favourable impression, and after some little negotiation, he was engaged; his mother yielded to his wishes; and Mr. Borlace generously surrendered his indenture, indorsing upon it, that he freely gave it up 'on account of the singularly promising talents which Mr. Davy had displayed.' He, however, so offended his old friend, Mr. Tonkin, by this measure, that he revoked the legacy of his house, which he had previously bequeathed him, in contemplation of fixing him in his native town as a surgeon. Davy quitted Penzance for Bristol in high spirits, in October, 1798, before he had attained his twentieth year. His position was now extremely favourable to the development of his genius. He was constantly engaged in the prosecution of new experiments, in the conception of which he was greatly aided by Dr. Beddoes, and occasionally assisted by Mr. Clayfield, to whom he was indebted for the invention of a mercurial air-holder, by which he was enabled to collect, measure, and examine the various gases. He enjoyed at Bristol the advantage of intellectual society; among others with whom he was intimate, were Mr. Edgeworth, and James Tobin, the author of the Honey Moon. The present Lord Durham and his brother were then also resident in the house of Dr. Beddoes. With some of these eminent persons Davy contracted permanent friendships. Dr. Paris says, 'there was more than one avenue to his heart; and the philosopher, the poet, the physician, the philanthropist, and the sportsman, found each, upon different terms, a more or less ready access to its recesses; but the fisherman instantly caught his affections.' 'To be a fly-fisher was, in his opinion, to possess the capabilities of intellectual distinction, though circumstances might not have conspired to call them into action.' It has been asserted by those who knew him through life, 'that his extraordinary talents never at any period excited greater astonishment than during his residence at Bristol.'

At the commencement of 1799, Dr. Beddoes published a work under the title of 'Contributions to Physical and Medical Knowledge, principally from the West of England;' nearly one-half of the volume consists of essays by Davy: 'On Heat, Light, and the Combinations of Light;' 'On Phos-oxygen, or Oxygen and its Combinations;' and 'On the Theory of Respiration.'

'In his chapter on Light and its Combinations,' says his biographer, 'he indulges in speculations of the wildest nature, although it must be confessed that he has infused an interest into them which might be almost called dramatic. His first essay commences with an experiment in order to show that light is not, as Lavoisier supposed, a modification, or an effect of heat; but matter of a peculiar kind, *sui generis*, which, when moving through space, or in a state of projection, is capable of becoming the source of a numerous class of our sensations. With regard to caloric his opinion was, that it is not, like light, material; and he maintains the proposition by the same method of reasoning as that by which he attempts to establish the materiality of light, and which mathematicians have termed the *reductio ad absurdum*.'

Dr. Paris has given an outline of these extraordinary essays, to which we must refer the reader.

The letters of Davy at this period to his friend, Mr. Davies Gilbert, give an interesting account of his experimental pursuits. The accidental observation, that two pieces of bonnet cane rubbed together produced a faint light, led him to examine into the cause. On removing the epidermis, he found that no light was produced; and subjecting the epidermis to chemical analysis, it proved to have all the properties of silex: the similar appearance of the epidermis of reeds, corn-straw, and grasses, induced him to suppose that they likewise contained silex; by burning them carefully and analysing their ashes, he found they contained it in larger proportions than the canes, and that the straws and grasses contain sufficient potash to form glass with their flint. He says, 'A very pretty experiment may be made on these plants. If you take a straw of wheat, barley, or hay, and burn it, beginning at the top, and heating the ashes with the blue flame, you will obtain a perfect globule of hard glass fit for microscopic experiments.' It was at this period that he was led by the nature of his engagements at Bristol to commence his inquiries into the nature of nitrous oxide, and the results enabled him to give to the world the first satisfactory account of the combinations of oxygen and nitrogen. These he published in a distinct volume, in the year 1800, under the title of 'Researches, Chemical and Philosophical, chiefly concerning Nitrous Oxide and its Respiration.'

'The close philosophical reasoning,' (says Dr. Paris,)—'the patient and penetrating industry,—the candid submission to every intimation of experiment,—and the accuracy of manipulation, so remarkably displayed throughout this work,—have rarely been equalled, and perhaps never surpassed. What shall we say of that spirit, which led him to inspire nitrous gas at the hazard of filling his lungs with the vapour of *aqua fortis!* or what of that intrepid coolness, which enabled him to breathe a deadly gas [carburetted hydrogen], and to watch the advances of its chilling power in the ebbing pulsations at the wrist?'

Dr. Paris gives an amusing account of the effects which the breathing of nitrous oxide produced on several scientific and literary friends of Davy, and thinks that, though the fact is established, that the gas possesses an intoxicating quality, the enthusiasm of persons submitting to its operation has imparted a character of extravagance to its effects not quite consistent with truth. Davy had nearly fallen a victim to his temerity, in breathing three quarts of hydro-carbonate, mingled with nearly two quarts of atmospheric air. This daring experiment, Dr. Paris thinks, if the precautions it suggests be properly attended to, may become the means of preserving human life, and is also valuable, as affording support to physiological views, with which its author was probably not acquainted. It is important, inasmuch as it proves that, in cases of asphyxia, or suspended animation, there exists a period of danger

after the respiration has been restored, and the circulation re-established, at which death may take place, when we are the least prepared to expect it. In the 'Researches' no allusion is made to the theory or nomenclature of 'Essays on Heat and Light.' Soon after their publication, he says, in a communication to Mr. Nicholson, 'I beg to be considered as a sceptic with respect to my own particular theory of the combinations of light, and shall in future use the common nomenclature.' 'It is remarkable that in several passages of the "Researches" he advocates the theory of the atmospheric air being a *chemical compound* of oxygen and nitrogen; whereas in later years, he was among the first to insist upon its being simply a mechanical mixture of these gases.'

His health having suffered from close application and the deleterious nature of his experiments, he retired to his native place, where he soon recovered; and we find him in the vigorous pursuit of his experiments in October, 1800, when he first announces to his friend Mr. Gilbert, 'those new facts in voltaic electricity,' which may be said to have paved the way to his grand discoveries in that branch of science. He says—

'In pursuing experiments on galvanism during the last two months, I have met with unexpected and unhoped-for success. Some of the new facts on this subject promise to afford instruments capable of destroying the mysterious veil which nature has thrown over the operations and properties of etherial fluids. Galvanism I have found to be *a process purely chemical*, and to depend wholly on the oxidation of metallic surfaces, having different degrees of electric conducting power,' &c.

His 'Researches' excited general admiration in the philosophic world, which was increased by the circumstance of a work so replete with ingenious novelty and chemical discovery proceeding from the pen of so young a man; and the publication may be considered as the immediate cause of an event which proved in its result not less important in its influence on his future fortunes than it has been on the interests of science. The Royal Institution had been then recently established for the advancement of science and the useful arts, in the establishment and direction of which Count Rumford took an active part. The fame of the young philosopher naturally attracted his attraction. Mr. Underwood, a gentleman attached to science and devoted to the interests of the Institution, was among the first to urge the expediency of inviting him to London as a public lecturer, and the Count, having received full powers from the Managers to negotiate on the subject, communicated with Mr. Underwood, who referred him to Mr. James Thompson, Davy's intimate friend, who wrote to Davy, with an earnest recommendation that he should come to town and conclude the arrangement. Davy answered the letter in person, was introduced to the Managers, received in the most flattering manner, and engaged as Assistant Lecturer in Chemistry, Director of the Laboratory, and Assistant Editor of the Journals of the Institution. He arrived at the Institution, and entered

upon his functions on the 11th of March, 1801. The letter in which he announces the circumstance to Mr. Gilbert, contains the following passage:—

'Thus I am quickly to be transferred to London, whilst my sphere of action is considerably enlarged, and as much power as I could reasonably expect, or even wish for at my time of life, secured to me without the obligation of labouring at a profession. The Royal Institution will, I hope, be of some utility to society. It has undoubtedly the capability of becoming a great instrument of moral and intellectual improvement. Its funds are very great. It has attached to it the feelings of a great number of people of fashion and property, and consequently may be the means of employing, to useful purposes, money which would otherwise be squandered in luxury, and in the production of unnecessary labour. As for myself, I shall become attached to it full of hope, with the resolution of employing all my feeble powers towards promoting its true interests.'

It is said that the first impression produced on Count Rumford by Davy's personal appearance was highly unfavourable, but his first lecture removed every prejudice of this kind; they soon became friends, entertaining for each other the highest regard. He so greatly satisfied the Managers of the Institution, that on the 1st of June they passed a resolution—

'That Mr. H. Davy, Director of the Chemical Laboratory, and Assistant Lecturer in Chemistry, has, since he has been employed at the Institution, given satisfactory proofs of his talents as a lecturer. Resolved—That he be appointed, and in future denominated, Lecturer in Chemistry at the Royal Institution, instead of continuing to occupy the place of *Assistant* Lecturer, which he has hitherto filled.'

Dr. Garnett had been Professor of Natural Philosophy in the Royal Institution from its first establishment, and Davy had lived on terms of great intimacy with that amiable man, whose health had been long declining. He resigned his professorship on this account in July of this year, and was succeeded by the late Dr. Young, who was engaged as Professor of Natural Philosophy, Editor of the Journals, and Superintendent of the Establishment. With this eminent philosopher Davy associated with less ease and freedom. In November of this year he thus notifies another galvanic discovery:—

'I yesterday ascertained rather an important fact, namely, that a galvanic battery may be constructed without any metallic substance! By means of ten pieces of well-burnt charcoal, nitrous acid, and water arranged alternately in wine-glasses, I produced all the effects usually obtained from zinc, silver, and water.'

His introductory lecture, delivered on the 21st of January, 1802, was received by a crowded audience with universal applause. It contains a masterly view of the benefits to be derived from the various branches of science; and in referring to the great agency of chemistry in the improvement of society, he makes the following almost prophetic remarks:—

'Unless any great physical changes should take place upon the globe, the permanency of the arts and sciences is rendered certain, in consequence of the diffusion of knowledge by means of the invention of print-

ing; and by which those words, which are the immutable instruments of thought, are become the constant and widely-diffused nourishment of the mind, and the preservers of its health and energy.' 'Individuals, influenced by interested motives, or false views, may check for a time the progress of knowledge,—moral causes may produce a momentary slumber of the public spirit,—the adoption of wild and dangerous theories by ambitious or deluded men may throw a temporary opprobrium on literature; but the influence of true philosophy will never be despised, the germs of improvement are sown in minds even where they are not perceived; and, sooner or later, the spring-time of their growth must arrive. In reasoning concerning the future hopes of the human species, we may look forward with confidence to a state of society, in which the different orders and classes of men will contribute more effectually to the support of each other than they have hitherto done. This state indeed seems to be approaching fast; for, in consequence of the multiplication of the means of instruction, the man of science and the manufacturer are daily becoming more assimilated to each other. The artist, who formerly affected to despise scientific principles, because he was incapable of perceiving the advantages of them, is now so far enlightened as to favour the adoption of new processes in his art, whenever they are evidently connected with a diminution of labour; and the increase of projectors, even to too great an extent, demonstrates the enthusiasm of the public mind in its search after improvement.'

This lecture was printed at the request of a considerable portion of the Society.

'The sensation created by this first course of lectures at the Institution, and the enthusiastic admiration which they obtained,' Mr. Purkiss says, 'is at this period scarcely to be imagined—compliments, invitations, and presents were showered upon him in abundance from all quarters; his society was courted by all, and all appeared proud of his acquaintance.'

'It is admitted,' says his biographer, 'that his vanity was excited, and his ambition raised, by such extraordinary demonstrations of devotion; that the bloom of his simplicity was dulled by the breath of adulation, and that losing much of the native frankness which constituted the great charm of his character, he unfortunately assumed the garb and airs of a man of fashion; let us not wonder if, under such circumstances, the inappropriate robe should not always have fallen in graceful draperies.' It has been also urged, 'that the style of his lectures was far too florid and imaginative for communicating the plain lessons of truth; that he described objects of natural history by inappropriate imagery, and that violent conceits frequently usurped the place of philosophical definitions.'

Dr. Paris has well defended him from this latter censure, by reminding us of the class of persons to whom his lectures were addressed; and the writer of this abstract, then very young, well remembers the effective and impressive manner in which he led away his hearers and took their prisoned senses captive. Nothing can be more true than the remark, that 'the style which cannot be tolerated in a philosophical essay may, under peculiar circumstances, be not only admissible, but even expedient in a popular lecture.' In addition to these morning lectures he gave, at the same time, an evening course on galvanic phenomena. In May, 1802, he was appointed Professor of Chemistry to the Royal Institution. Davy seems himself to have been sensible that his audience required something more

than mere science to fix their attention. In giving his early friend, Mr. Gilbert, an account of his successful exertions, he says—'In lectures, the effect produced upon the mind is generally transitory; for the most part they amuse rather than instruct, and stimulate to inquiry rather than convey information.' In this letter he mentions the powerful galvanic battery which he had caused to be constructed for the laboratory of the Institution, consisting of 500 plates of five inches in diameter, and 40 plates of a foot in diameter, and that by means of it he had been enabled to burn inflammable substances, to fuse platina wire, &c., and to boil and decompound oil and water; and that he was then engaged in examining its agencies upon substances which had not as yet been decomposed. 'The elegance with which his experiments in the theatre were conducted was strangely contrasted to the slovenly style of his manipulations in the laboratory. So rapid were his movements, that he would carry on several unconnected experiments at one time, and while it was imagined that he was merely preparing for an experiment, he was actually obtaining the results.' 'With Davy,' adds Dr. Paris, 'rapidity was power.' Whatever diversity of opinion may have been entertained of Davy's style as a lecturer, his philosophical memoirs are so remarkable for clearness, simplicity of language, and freedom from technical expressions, that they have been proposed as models for all future chemists; and Mr. Brande, in a lecture delivered last year before the members of the Royal Institution, forcibly contrasted his style with that of another eminent foreign chemist on this ground. Davy himself, in his "Last Days of a Philosopher," has the following remarkable precept, which he supported by his example: "In detailing the results of experiments, and in giving them to the world, the chemical philosopher should adopt the simplest style and manner; he will avoid all ornaments as something injurious to the subject; and should bear in mind the saying of James I.,—that the tropes and metaphors of the speaker were like the brilliant wild flowers in a field of corn, very pretty, but which very much hurt the corn."'

The first series of the Journal of the Royal Institution was published in monthly numbers, and the price was fixed at one shilling, in the hope that it might be more generally diffused. It contained abridged accounts of what was going on in the scientific world, abroad and at home, and several very interesting original papers by Dr. Young and by Davy, who appears to have acted as joint editor. His original communications were—'An Account of a new Eudiometer;' 'Several Papers on Galvanic Phenomena;' 'On the Gallic Acid;' and 'On the Processes of Tanning;' 'An Account of a Method of Copying Paintings upon Glass, and making Profiles by the agency of Light upon Nitrate of Silver,' invented by Mr. T. Wedgwood, with observations by Davy; 'On the Collision of Flint and Steel in vacuo;' and some 'Observations upon the Motions of small Pieces of Acetate of Potash during their solution upon the surface of Water,' to which the late interesting observations of Mr. Brown is calculated to excite attention.

Davy's first communication to the Royal Society was 'An Account of some Galvanic Combinations, formed by an Arrangement of Single Metallic Plates and Fluids, analogous to the Galvanic Apparatus of M. Volta.' It was read in April, 1803, and in November of that year he was elected a fellow of that Society. He had been previously elected an honorary member of the Dublin Society. Shortly after his appointment to the Royal Institution, he had delivered a series of lectures on the art of tanning; and having, by a scientific examination of the subject, added many important facts, he now embodied them in a Memoir, which was published in the Philosophical Transactions for 1803, entitled, 'An Account of some Experiments and Observations on the Constituent Parts of certain Astringent Vegetables, and on their Operation in Tanning;' of which Dr. Paris has given an outline, and observes that 'it forms at this day the guide of the tanner; and those who previously carried on the process by a routine of operations of which they knew not the reasons, are now capable of modifying it without risk of spoiling the result.' In May, 1803, he gave a course of six lectures on Agricultural Chemistry, before the members of the Board of Agriculture, and was appointed chemical professor to that Board. This brought him into contact with the most eminent agriculturists and capitalists of the day, with many of whom he formed friendships which lasted through life. These discourses were published in the year 1813. His biographer may well say—

'We can scarcely picture to ourselves a being upon whom fortune ever showered more favours than upon Davy, during this golden period of his career. Independent in an honourable competence, the product of his genius and industry—resident in the centre of all scientific information and intelligence, every avenue of knowledge and every mode of observation open to his unwearied intellect—he must have experienced a satisfaction which few philosophers have ever before felt—the power of pursuing experimental research to any extent, and of commanding the immediate possession of all the means it might require, without the least regard either to cost or labour. What a contrast does this picture afford to that which has been too faithfully represented as the more usual fate of the philosopher and man of letters, and which exhibits little more than the unavailing struggles of genius against penury! . . . Not the least extraordinary point in the character of this great man was the facility with which he could cast aside the cares of study, and enter into the trifling amusements of society. In the morning, he was the sage interpreter of Nature's laws; in the evening, he sparkled in the galaxy of fashion. When not otherwise engaged, his custom was to play at billiards, frequent the theatre, or read the last new novel.'

Very shortly after Davy's arrival in London, he formed an intimate friendship with Mr. (afterwards Sir Thomas) Bernard, who allotted him a plot of ground, near his villa at Roehampton, for the purpose of making experiments in agricultural chemistry. Dr. Paris passes a well-deserved eulogy upon this most excellent person, whose 'life was one continued scheme of active benevolence;' and he merits a particular notice in these Memoirs, as being one of the principal founders and patrons of the Royal Institution. The primary object of the founders was the formation of an institution which

might teach the application of science to the advancement of the arts of life, advance the taste and science of the country, and improve the means of industry and domestic comfort among the poor. These benevolent designs were to be promoted by committees for the purpose, having for their object the advancement, by scientific investigation, of the arts of life, on which the subsistence of all, and the comfort of the great majority of mankind, absolutely depend. 'At this early period of its history,' says Dr. Paris, 'the Royal Institution presented a scene of the most animated bustle and exhilarating activity. It was "like a busy ant-hill in a calm sunshine."'

At the commencement of 1805, Davy enriched the cabinets of the Institution by a present of minerals, which were reported to be of the value of 100 guineas; and he was soon after, in addition to his professorship, appointed director of the laboratory; by which appointment, his annual income from the Institution was raised to four hundred guineas. At this period he delivered a series of lectures on Geology, and produced his paper, published in the Philosophical Transactions, 'Analytical Experiments on a Mineral Production from Derbyshire (Wavellite), consisting principally of Alumina and Water;' and soon after he communicated to the same body a paper 'On the Method of analyzing Stones containing a fixed Alkali, by means of the Boracic Acid,' which is said to have much advanced the art of mineral analysis. On the death of Dr. Gray, Davy was elected secretary of the Royal Society, at an extraordinary meeting on the 22d Jan. 1807, being at the same time elected a member of the council.

In Chapter VI. of his work, Dr. Paris enters upon that brilliant period in the life of Davy, 'at which he effected those grand discoveries in science, embracing the development of the laws of voltaic electricity, which will transmit his name to posterity;' prefixed we have a brief view of the history of galvanism, or voltaic electricity, divided into six grand epochs. Davy, in his Bakerian lecture of 1806, remarks—

'That the true origin of all that has been done in this department of philosophy was the accidental discovery, by Nicholson and Carlisle, of the decomposition of water by the pile of Volta, in April, 1800, which was immediately followed by that of the decomposition of certain metallic solutions, and by the observation of the separation of an alkali on the negative plates of the apparatus. Mr. Cruikshank, in pursuing these experiments, obtained many new and important results,—such as the decomposition of the *muriates of magnesia, soda, and ammonia;* and also observed the fact that the *alkaline* matter always appeared at the negative, and *acid* matter at the positive pole.'

'In September, 1800, Davy published his first paper on the subject of galvanic electricity, in Nicholson's Journal, which was followed by six others, in which he so far extended the original experiment of Nicholson and Carlisle, as to show that oxygen and hydrogen might be evolved from separate portions of water, though vegetable and even animal substances intervened; and conceiving that all decompositions might be *polar*, he electrised different compounds at the different extremities, and found that sulphur and metallic bodies appeared at the *negative* pole, and oxygen and azote at the *positive* pole, though the bodies furnishing them were separated from each other. Here was the dawn of the electro-

chemical theory. . . The Bakerian Lecture, read before the Royal Society in November, 1806, unfolded the mysteries of voltaic action; and, as far as theory goes, may be almost said to have perfected our knowledge of the chemical agencies of the pile.'

Of this celebrated paper, Dr. Paris has given an analysis, to which we must refer the reader; it embraces the discovery of the sources of the acid and alkaline matter eliminated from water by voltaic action—the nature of electrical decomposition and transfer—the relations between the electrical energies of bodies and the chemical affinities—a general development of the electro-chemical laws, and their application. He thus concludes what Dr. Paris justly styles one of the most masterly and powerful productions of scientific genius—

'Natural electricity has hitherto been little investigated, except in the case of its evident and powerful concentration in the atmosphere. Its slow and silent operations, in every part of the surface, will probably be found more immediately and importantly connected with the order and economy of nature; and investigations on this subject can hardly fail to enlighten our philosophical systems of the earth, and may possibly place new powers within our reach.'

Dr. Paris asserts that accident, which so mainly contributed to former discoveries in electricity, had no share in conducting Davy to the truth in this instance, but that he unfolded, with philosophic caution and unwearied perseverance, all the particular phenomena and details of his subject, and with the comprehensive grasp of genius caught the plan of the whole.

Buonaparte having founded a prize of three thousand francs (about £120), to be adjudged by the Institute, for the best experiment which should be made in each year on the galvanic fluid, and another of sixty thousand francs to the person who, by his experiments and discoveries, should advance the knowledge of electricity and galvanism as much as Franklin and Volta did—the first prize was awarded to Davy, about twelve months after the publication of his first Bakerian Lecture, for his discoveries announced in the Philosophical Transactions of 1807. When the bitter animosity which France and England mutually entertained towards each other at this period is recollected, the award was not more honourable to him who received the prize than to those who gave it.

In November, 1807, his second Bakerian Lecture was read, in which he announces the discovery of the metallic bases of the fixed alkalies—

'a discovery immediately arising from the application of voltaic electricity, directed in accordance with the electro-chemical laws he had developed. Thus having, in the first instance, ascended from particular phenomena to general principles, he now descended from those principles to the discovery of new phenomena; a method of investigation by which he may be said to have applied to his inductions the severest tests of truth, and to have produced a chain of evidence without having a single link deficient. Since the account given by Newton of his first discoveries in optics, it may be questioned whether so happy and successful an instance of philosophical induction has occurred.'

Dr. Paris, as before, gives a detailed account of the contents of this

lecture, which we regret we have not space to copy. In the lecture, Davy observes, that 'an historical detail of the progress of the investigation, of all the difficulties that occurred, of the manner in which they were overcome, and of all the manipulations employed, would far exceed his limits,' upon which Dr. Paris observes that, 'to the chemist, every circumstance connected with a subject of commanding importance is pregnant with interest;' and having, by permission of the managers of the Royal Institution, obtained leave to examine and make extracts from the Laboratory Register, he obtained the following interesting clue to 'the intellectual operations by which his mind ultimately arrived at the grand conclusion.' With these interesting MS. volumes we hope to make the reader acquainted in a future page of this Journal—in the meantime we shall follow Dr. Paris:—

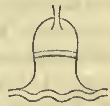
'It appears from this register, that Davy commenced his inquiries into the composition of potash on the 16th, and obtained his great result on the 19th of October, 1807. His first experiments, however, evidently did not suggest the truth; he does not appear to have suspected the nature of the alkaline base until his last experiment, when the truth flashed upon him in the full blaze of discovery. His first note, dated the 16th, leads us to infer that he acted on a solid piece of potash, under the surface of alcohol, and several other liquids in which the alkali was not soluble; and that he obtained gaseous matter which he called at the moment "*alkaligen gas*," and which he appears to have examined most closely, without arriving at any conclusion as to its nature. On the following day, he, for the first time, would seem to have developed potassium by electric action on potash, under oil of turpentine, for the note records the fact of "*the* globules giving out gas by water, which gas *burnt in contact with air;*" and then follows a query—"Does *it*" (the matter of the globules) "not form gaseous compounds with ether, alcohol, and the oils?" Here then he evidently imagined, that the matter of the globules, which he had never obtained from potash, except when acted upon under oil of turpentine, had formed gaseous compounds with the ether, alcohol, and oils, in his previous experiments, and given origin to that which he had termed "*alkaligen gas*." He then leaves the consideration of this gas, and attacks the unknown globules, which probably did not present any metallic appearance under the circumstances he saw them, for they must have been as minute as grains of sand. I rather think that he commenced his examination by introducing a globule of mercury, and uniting it with a globule of the unknown substance, for his note says—"Action of the substance on mercury, forms with it a solid amalgam, which soon loses its *alkaligen* in the air." And from the note which succeeds, he evidently considered this *alkaligen* (potassium) volatile, as he says, "it soon flies off on exposure to the air."

'October 19.—It is probable that, in consequence of the property which the unknown substance displayed of amalgamating with mercury, he devised his experiment of the 19th. He took a small glass tube, about the size and shape of a thimble, into which he fused a platinum wire, and passed it through the closed end. He then put a piece of pure potash into this tube, and fused it into a mass about the wire, so as entirely to defend it from the mercury afterwards to be used. When cold, the potash was solid, but containing moisture enough to give it a conducting power; he then filled the rest of the tube with mercury, and inverted it over the trough: the apparatus being thus arranged, he made the wire and the mercury alternately positive and negative;—and now, conceiving that I

have sufficiently explained his brief notes, the reader shall receive the result in his own words: on the same day he decomposed soda with somewhat different phenomena.'

Dr. Paris has given a fac-simile of the minute in Davy's handwriting of his successful experiment of October the 19th. It is highly interesting and characteristic, but should have been accompanied by the substance of it in print, for it is not every one who will be able to decipher it. It runs thus:—

'Oct. 19.—When potash was introduced into a tube having a platina wire attached to it, and fixed into the tube so as to be a conductor, *i. e.*, so as to contain just water enough, though solid, and inserted over mercury, when the platina was made negative, no gas was formed, and the mercury became oxydated, and a small quantity of the alkaligen was produced round the platina wire, as was evident from its quick inflammation by the action of water. When the mercury was made the negative, gas was developed in great quantities from the positive wire, and none from the negative mercury, and this gas proved to be pure OXYGENE.—CAPITAL EXPERIMENT, *proving the decomposition of* POTASH.'



Those who knew Davy will best conceive the enthusiasm with which this hasty record of his success was dashed off, and will recognise ευρηκα in his 'capital experiment!'

(To be continued.)

I. *Plantæ Asiaticæ Rariores; or Descriptions and Figures of a select number of unpublished East India Plants. By* N. Wallich, M.D. Vol. I. folio. London, 1830. Treuttell and Co.

II. *A numerical List of dried Specimens of Plants in the East India Company's Museum, collected under the superintendence of* N. Wallich, *of the Company's Botanic Garden at Calcutta.* Folio, pages 1—93, Nos. 1—3285; still publishing. (For private distribution only.)

IF we were to select one country in preference to another, as illustrative of the gigantic strides that have been taken by modern science, India and its vegetation should be our theme—India, which in its vast extent comprehends the climate of the equator and of the Pole, stretching from the classical mountains of Emodus on the north, to the ancient Taprobane, and the sultry islands of the Indian Archipelago on the south; and from the rose gardens of Amedabad, and the holy fountains and luxurious palaces of Cachmere on the west, to the frontiers of the celestial empire and the burning shores of Arracan, Pegu, and Martaban on the east; embracing regions of eternal snow among the craggy summits of the Himalaya and Nilgherry; parched plains, where the sun glares with his fiercest rays in Hindostan; and including all those gradations and diversity of climate which are the usual characteristics of an entire quarter of the globe, rather than of a country subject to the control of a single power, and distinguished by a single name;—a vegetation which seems, at first sight, to be in direct contradiction to

any known law that regulates the embellishment of the face of nature; where the orange and the lime, the shaddock, the pine-apple and the banana, grow almost side by side with the oak, the bramble, and the chesnut; which, in one district, consists of roses, elms, currants, raspberries, and wild flowers most similar to those of Europe; and in another, is so entirely tropical, that the trees are mangosteens and mangoes, and their inhabitants the parasitical loranthus, or the fantastic orchis, while the woods are of teak and sissoo, choked up by huge *lianes;* in which the sensible properties are so elaborated, that the very nettles become deadly, forest trees produce blindness, by mere contact with their juices, the poisons are of unheard-of virulence, and yet every sense is delighted by the fragrance of flowers of the most splendid colours, or by the rich flavour of the most luscious fruits.

The botany of this remarkable country has not failed to excite attention from the earliest periods. To say nothing of the Arabians, who first introduced ginger from Calicut to Spain—who described the pepper plant that climbs upon other trees, hiding its fruit beneath its leaves, lest the former be scorched up—who brought the sugar-cane from the banks of the Ganges; discovered the true camphor tree of Sumatra; distinguished the rhubarb that grows on the confines of China from the rheum of the Greeks; and made known the tamarind, the cotton plant, the tea tree, the nutmeg, and the cinnamon; and to pass by the now-forgotten names of Garcias ab Orta, Acosta, Linschoten, and Jacob Bont, there are two works that especially claim our attention.

In the middle of the seventeenth century, a Dutch Viceroy of Malabar, named Henry van Rheede tot Drakenstein, collected by means of Brahmins, missionaries, and others, a great store of drawings and descriptions of the more important plants of his government, which were subsequently published between the years 1676 and 1703, in twelve volumes, folio. Like the 'Flora Batava,' now publishing at the expense of the King of the Netherlands, the skill of the subordinate agents was by no means commensurate with the liberality of their princely employers, whose treasures were unfortunately lavished upon a work that was far from answering to the charges that were incurred in its publication. About seven hundred indifferent figures, accompanied by miserable descriptions, were the whole result of Van Rheede's patronage.

About the same time, the Flora of Insular India was investigated by George Everhard Rumf, a Dutch merchant and governor of Amboyna, whose collections were published in seven volumes folio, by John Burmann, between 1741 and 1751. Unlike Van Rheede, Rumf appears to have been a skilful botanist for his time, as well as a munificent patron; and hence both the figures and descriptions of the 'Herbarium Amboinense,' as his work is called, are of a character far superior to that of his predecessor. Like all drawings of natural history of the day, the figures are inaccurate in their details, but they are far from bad general representations; the

descriptions are written with care and minuteness, while the uses to which the plants are applied are explained in a manner that might serve as a model for a modern flora. These two works, with the Thesaurus Zeylanicus, of Burmann, compiled in 1737, from the materials collected by Paul Hermann, a Dutch Physician; the Flora Zeylanica, of Linnæus; and the Flora Indica, of Burmann, the younger, were the basis, till of late years, of our knowledge of Indian botany.

The whole of these publications did not, we believe, carry the flora of India beyond two thousand species, of which, those only that had been well figured could be said to be known to science, so defective were the modes of description formerly employed. Thus, after two thousand years that India had been constantly open to Europeans, or in three hundred years from the period that the Cape of Good Hope was first doubled by the Portuguese, the total number of species that the enterprize of naturalists and the wealth of their patrons, had accumulated in the boundless regions comprehended under the name of India, did not equal one half the flora of France. Modern botanists have done in a few years what their predecessors had failed to accomplish in centuries.

The principal cause of this progress is attributable to the powerful patronage of the English East India Company, who, from the period of their foundation of a botanic Garden at Calcutta, about the year 1785, have been the constant promoters of investigations of the natural history of their Indian possessions. Under these auspices collections of great extent were formed by several individuals in their service, particularly by Drs. Roxburgh, Russell, and Hamilton; while the addition of native draughtsmen to their botanical establishment laid the foundation of a series of drawings, unrivalled for extent and accuracy. A portion of these was published several years since, under the title of 'Plants of the Coast of Coromandel,' in three volumes folio, containing three hundred coloured figures; and vast quantities of dried specimens were deposited, by the Company's orders, in the hands of the Linnæan Society, and of the late Sir Joseph Banks; among whose unarranged collections, we understand, they still are to be found.

It was not, however, till the year 1815 that the powerful impulse was communicated to the prosecution of botanical researches in India, which has led to its present remarkable state. At that time a Danish gentleman, whose works stand at the head of this article, was appointed to the charge of the Calcutta garden; and from this period new vigour seems to have been infused into every department. The preparation of drawings in the garden was prosecuted with increased energy, and, under the new direction, with an accuracy and beauty which had never before been seen in India. The collection of living plants augmented rapidly, as was attested by large and constant exportations of seeds and plants, as presents from the Company, to public and private gardens in Europe. In this way great benefit has accrued to Great Britain; independently

of the vast numbers of hothouse plants of which every garden now enjoys the advantage, of late years those valuable trees and beautiful shrubs and flowers that, inhabiting the snowy mountains of Nipal, find a congenial climate in Great Britain, have begun to adorn our gardens. Dried specimens continued to be sent home, and in such abundance, that a general distribution was some years since entrusted by the Company to their officers in the museum of the India House, by which the botanists of this country extensively benefited. These collections had been chiefly formed by the personal visits of Dr. Wallich to various parts of our Indian possessions. The high lands of Nipal were traversed in 1820 and 1821; the islands of Penang and Sincapur were visited soon afterwards; the timber forests of Oude and Rohilcund were explored in 1825; and, finally, the kingdom of Ava, and the coasts of Tenasserim and Martaban, were the subject of personal investigation by this indefatigable naturalist in 1826 and 1827. Besides this, the Company's residents, plant-collectors, travellers, physicians, and others, all contributed, some (as the late Dr. Jack and Mr. Colebrooke) very extensively, towards the completion of the investigation. The result of these labours has been an accumulation of upwards of 8000 species, of which, if allowance be made for a deficiency in the insular species, near 7000 must have been discovered within the last forty years. A portion of these have been made known in the Flora Indica of Carey and Roxburgh; a further number have been published by several of the working botanists of the day, and fifty were figured by Dr. Wallich in his Tentamen Floræ Indiæ Illustratæ. Nothing, however, really worthy of the immense power that had been put in action to collect these materials, had been undertaken, when ill health rendered it advisable that the superintendent of the Botanic Garden, Calcutta, should visit England, and it was then determined that his collections should accompany him. In the true spirit of genuine science, collecting nothing for himself alone, and everything for the advantage of his favourite pursuit, the harvest of twenty years had been supposed to have been nearly exhausted by his numerous remittances to Europe; but when it was known that the mere remains of Dr. Wallich's gigantic herbarium occupied nearly forty huge chests, weighing almost thirty tons, and that countless thousands of duplicates still remained in his possession, the utmost anxiety was manifested to know in what way the East India Company would determine that they should be disposed of. That everything which the most exalted liberality could suggest might be anticipated was by no one doubted, nor has the public expectation been disappointed.

The same spirit that directed the formation of these collections presided over the councils of the company on their arrival. It was directed that the most select of the new species should be published from the drawings, and that the duplicate specimens should be divided 'among the principal public and private museums of Europe and America.' Of the two works that stand at the head of this article, the first is the result of the former part of the plan, and

the second of the latter. We shall offer a few remarks upon them respectively.

The first volume of the Plantæ Asiaticæ Rariores contains ninety-six species, represented in lithography, and coloured. Nearly all the drawings from which these figures have been taken were made in India by native artists attached to the Botanic Garden; a very small number has been prepared in England. Our limits preclude our quoting all the subjects that the volume comprehends; referring, therefore, the scientific botanist to the work itself, we select a few of the subjects that are likely to be most interesting to the general reader.

The two first plates illustrate the Amherstia nobilis, a Burmese tree, named in honour of the Countess and Lady Sarah Amherst, remarkable for bearing an elegant ash-like foliage, half hidden amidst which hang bunches of the most brilliant scarlet and yellow blossoms, each with its scarlet stalk nearly six inches in length. The Hindoos offer the flowers at the shrine of Buddha. For splendour of colouring and elegance of form, this plate is unrivalled. It is the high priest of the vegetable world, clothed in an investiture more splendid than that of the most gorgeous religion of mankind. Tab. 4, is Hibiscus Lindlei, a fine species, with large bright purple flowers, now cultivated in England. Tab. 9, Curcuma Roscoeana, is a plant with spikes of scarlet bracteæ six or seven inches long, unrivalled for beauty among the ginger tribe. Tab. 11 and 12 represent the Zit-si of the Burmese (Melanorhœa usitatissima), from which is obtained that poisonous but invaluable black varnish with which China and India toys and utensils are coated. From the account of this tree we extract the following:—

'The first time I met with this very interesting tree was at a small village below Prome, on the river Irawaddi, where a few had been planted; and on my return from Ava I found it again in abundance on the hills surrounding the first mentioned town; but in both instances the trees were without any fructification. In the Martaban province I had the satisfaction of seeing the trees in great numbers in March, 1827, on a small acclivity rising behind the town of Martaban. They were loaded with bunches of red, nearly ripe, fruit, but were not very large, few only exceeding thirty feet in height, with a short trunk measuring not more than four or five feet in circumference. The leaves had entirely fallen off, and strewed the ground in every direction. At Neynti, a village on the Attran river, behind the military station at Moalmeyn, I also observed a few trees; and lastly, on the Saluen river towards Kogun. Here they were of greater dimensions than those just mentioned; one of them being forty feet in height, with a stem twelve feet long, and eleven in girth at four feet above the ground. One of my assistants brought me fruit-bearing specimens from Tavoy on the Tenasserim coast. Before leaving Bengal I had an opportunity of identifying our tree with the majestic Kheu, or varnish tree of Munipur, a principality in Hindustan, bordering on the north-east frontier districts of Sillet and Tippera. Mr. George Swinton, Chief Secretary to the Bengal Government, (to whose kindness I am indebted for much valuable information concerning the produce of this and other useful trees of India,) obtained for me a supply of ripe

fruits from thence, which differed in no respect from those I had seen at Martaban. They vegetated speedily, and produced plants similar to those we already possessed. Captain F. Grant, who has a military command at Munipur, had the goodness to furnish the following particulars. The tree grows in great abundance at Kubbu, an extensive valley in the above mentioned principality, forming large forests in conjunction with the two staple timber-trees of continental India, the saul and teak (Shorea robusta and Tectona grandis), especially the former. Numbers of the gigantic woodoil-tree (Dipterocarpus) are also found in company with it. The size of it varies, but in general it attains very large dimensions. Captain Grant speaks of trees having clear stems of forty-two feet to the first branch, with a circumference near the ground of thirteen feet; and he mentions that they are known to attain a much greater size. All the individuals grow in the same manner, that is, they reach a great height before throwing out any branches. Our tree belongs to the deciduous class, shedding its leaves in November, and continuing naked until the month of May, during which period it produces its flowers and fruit. During the rainy season, which lasts for five months, from the middle of May until the end of October, it is in full foliage. Every part of it abounds in a thick and viscid greyish-brown fluid, which turns black soon after coming in contact with the external air. In the Edinburgh Journal of Science, vol. viii., pp. 96 and 100, there are two interesting articles, containing valuable information concerning the varnish produced by our tree, and its deleterious effects on the human frame. It is a curious fact, that, to my certain knowledge, the natives of the countries where the tree is indigenous never experience any injurious consequences from handling its juices: it is strangers only that are sometimes affected by it, especially Europeans. Both Mr. Swinton and myself have frequently exposed our hands to it without any serious injury. I have even ventured to taste it, both in its recent state and as it is exposed for sale at Rangoon, and have never been affected by it. It possesses very little pungency, and is entirely without smell. I know, however, of instances where it has produced extensive erysipetalous swellings attended with pain and fever, but not of long duration. Of this description was the effect it had on the late Mr. Carey, a son of the Rev. Dr. W. Carey, who resided several years in the Burman empire. Among the people who accompanied me to Ava, both Hindoos and Mahomedans, no accident happened, although they frequently touched the varnish, except in a slight degree to one of my assistants, whose hand swelled and continued painful during two days. Dr. Brewster informs me that, after resisting its effects for a long time, it at length attacked him in the wrist with such violence that the pain was almost intolerable. It was more acute than that of a severe burn, and the doctor was obliged to sleep several nights with his hand immersed in the coldest water. He considers it a very dangerous drug to handle. One of his servants was twice nearly killed by it.'

Tab. 22 and 23 are Dillenia scabrella and ornata, two noble trees, remarkable for the large rich golden yellow blossoms with which they are covered. Under Aphanochilus flavus, tab. 34, is the commencement of a scientific arrangement of Indian Labiatæ, by Mr. Bentham, a performance of great importance to science. Eria paniculata, tab. 36, Dendrobium formosum, tab. 39, and D. densiflorum, tab. 40, are magnificent air-plants, growing in damp forests on the lower mountains of India. Tab. 41, Aconitum ferox, is a frightful

poison called Visha, Vish, or Bikh, of which the following is some account:—

'This dreadful root, of which large quantities are annually imported, is equally fatal when taken into the stomach, or applied to wounds, and is in universal use for poisoning arrows; and there is too much reason to suspect, for the worst of purposes. Its importation would indeed seem to require the attention of the magistrates. The Gorkhalese pretend that it is one of their principal securities against invasion from the low countries; and that they would so infect all the waters on the route by which an enemy was advancing, as to occasion his certain destruction. In case of such an attempt, the invaders, no doubt, ought to be on their guard; but the country abounds so in springs that might be soon cleared, as to render such a means of defence totally ineffectual, were the enemy aware of the circumstance. This poisonous species is called *Bish* or *Bikh*, and *Haydaya Bish* or *Bikh;* nor am I certain whether the *Metha* ought to be referred to it, or to the foregoing kind. By referring to the experiments of Professor Orfila, in his General Toxicology, and of Mr. Brodie, in the Philosophical Transactions, it will be seen that the symptoms produced by the *Aconitum napellus* are very similar to those produced by the *Aconitum ferox.* Hence, then, it is most probable, that both species contain the same active principle; but the *A. ferox* must contain it in much greater quantity, as its effects are so much more powerful. Indeed the alcoholic extract of this root appears to be nearly equal in power to *Strychnine*, *Upas antiar*, *Upas tieuté*, and *Woorara* poisons. That it is equal in power to *Strychnine*, I can speak from numerous experiments which I have made with this latter; but with respect to the activity of the *Upas* and *Woorara* poisons, I can only speak from the experiments of Orfila, Brodie, and others.'

Ruellia gossypina, tab. 42, is a superb species, with leaves covered with dense wool, and deep sky-blue flowers growing in numerous panicles. Quercus spicata, tab. 46, is one of those gigantic oaks that give a kind of European air to the flora of Nipal; it bears its acorns in spikes a foot long. Mucuna macrocarpa, tab. 47, is a bean with flowers an inch and a-half, and pods a foot and a-half long: we are told that the beauty of the blossoms compensates for their hairs entering the skin and causing an intolerable burning. Justicia venusta, tab. 66, is a charming branched plant with elegant panicles of rich purple blossoms: it is now cultivated in England. Æscynanthus (Æschynanthus?) ramosissima, tab. 71, is a parasitical shrub, bearing umbels of orange and scarlet flowers, resembling our trumpet honeysuckle, but scentless. Argyreia festiva, tab. 76, is a gigantic bindweed, with stems as thick as the wrist. Pongamia atropurpurea, tab. 78, a vast tree bearing bunches of such inky purple flowers as to resemble nothing so much as a plant in mourning; and Bombax insigne, tab. 79 and 80, is a magnificent cotton tree with superb vivid red flowers, which fall from the trees in such profusion as to form a thick bed of living fire. Wightia gigantea, tab. 81, is one of those huge arborescent climbers, whose embrace is death, scrambling to the tops of the highest trees, and overwhelming them with the weight of their branches; and Oxyspora paniculata, tab. 88, is re-

presented as a common shrub in Nipal, producing large panicles of blood-red blossoms. Almost all the foregoing, surpassing as is the loveliness of some of them, are eclipsed by the Bignonia multijuga, tab. 95 and 96, a large forest tree found on the mountains of Sylhet, bearing immense woody panicles of flowers resembling those of the common Catalpa. And, finally, Begonia pedunculosa, tab. 97, is a lovely little herbaceous plant, which seems, from its stems and leaves, as if nature had intended them all for flowers.

We learn, from the preface, that this work appears under the immediate patronage of the East India Company; we congratulate the author that he has such patrons, and the Company that they have such a servant.

We have already stated that the second part of the Company's plan was that of distributing throughout the whole scientific world the immense collections which they had caused to be formed at such great charge to themselves, and such incredible personal exertion on the part of their civil servants. The whole of the details of executing this gigantic scheme were of course entrusted to Dr. Wallich, who judiciously adopted exactly the mode that would be most agreeable to a liberal-minded man. He invited all the botanists of Europe to co-operate with him in his enterprise, offering one tribe to this, and another to that person, taking care that in all cases the different families should be placed in the hands of those who were known by their published works to be best acquainted with them. What has been the effect? England has seen the learned men of Germany, Russia, and France, repairing to her shores to assist in this splendid project, and those of all nations enrolling themselves in the list of contributors to the noble enterprise of the British East India Company; she has beheld men of all parties, of all countries, concurring in the prosecution of it, and the great lords and princes of the land supporting it by their countenance and assistance.

May the example of the British East India Company, in regard to the collections of the Botanic Garden, Calcutta, be followed in all the public establishments of the United Kingdom, and in every department of their own! May the guardians of our national institutions direct a similar application to be made of the objects of science in their possession; and may our public functionaries destroy for ever, the pernicious practice of collections formed at the expense of the public purse, and by the authority of the British government, serving no other purpose than that of *exclusively* augmenting some single collection!

The second work at the head of this article is a numerical catalogue of the species that are thus distributed by the East India Company.

FOREIGN AND MISCELLANEOUS INTELLIGENCE.

§ I.—*MECHANICAL SCIENCE.*

1. On the Discharge of a Jet of Water under Water.—(*R. W. Fox, Esq.*)

The following letter is addressed to the Editors of the *Philosophical Magazine.*

'I am not aware that it has been before noticed, that a jet of water discharges the same quantity, in water, as in air, in a given time, without reference to the depth or the motion of the water, at least within certain limits. Thus when the experiment was tried with a head of water six feet high, the same orifice discharged equal quantities in equal times, in air, in still water, and in a rapid stream, moving at the rate of about six feet in a second; the jet having in one case been turned with the current, and in another against it: and when, by lengthening the tube, the aperture was submerged to the depth of fifteen feet, the effect was the same as at the surface, under the pressure of an equal column above it. These results have been obtained by my brother Alfred Fox and myself, and you may perhaps think them deserving a place in your Magazine, if they should appear to you to be new.

'We sometimes coloured the water, when the jet appeared to pass unbroken to a considerable distance under the water *.'

2. On preventing the Discharge of a Bullet from a Gun by the Finger.

At the sitting of the Helvetic Society of Natural Sciences of the 28th July last, a letter was read from Dr. Flachin of Yverdun, relative to an experiment before mentioned to the society, in which the ball was prevented from leaving the bottom of a musket when the gunpowder was fired, simply by putting the ramrod upon the ball, and the end of the finger upon the ramrod. He supposes the effect may be explained by the circumstance, that near the charge the ball has a very small velocity compared to that impressed upon it by the expansive force of the gases from the fired gunpowder, when exerted during the whole of the time in which it is passing along the barrel. It is well known that the effect thus accumulated is the reason why long pieces carry further than short ones, and why the breath of a man, which cannot exert a pressure of more than a quarter of an atmosphere, may, by means of a tube, throw a ball to the distance of sixty steps. The experiment above requires great care, especially as to the strength of the piece, which is very liable to burst in the performance of the experiment †.

* Vol. viii. p. 342.

† Bib. Univ., 1830, p. 447.

3. Clement's Experiment—easy mode of repeating it.

The very curious, and apparently paradoxical experiment, first described by Clement, in which air, gas, or steam, issuing with force from a hole in a flat surface, did not blow away a platter or other flat and extended body, but rather caused its adhesion, has been repeated since in a great variety of forms. M. Hachette contrived a simple little apparatus, by which every one was put in possession of the power of witnessing the effect: and such facilities are valuable, because they rapidly extend the knowledge of curious effects, and cause them to be still more extensively pursued and investigated. The experiment may be still further simplified in the following manner. When the fingers of the open hand are retained as close to each other as they can be, still there are certain slit-like intervals between them extending from joint to joint. Let the hand be held horizontally with the palm downwards, apply the lips to the interval between the second and third fingers nearest to their roots, and then blowing with force, a strong jet of air will of course issue from the aperture at the under side of the hand. Now, put a piece of paper or a card three or four inches square against that aperture, and again blow; it will be found that the paper will neither be blown away, nor fall by its own weight, but will be pressed upwards against the hand and the issuing current of air, so long as that current continues. The moment it ceases, the paper will fall away by its own gravity, in obedience to the ordinarily active laws of nature.—M. F.

4. Browne's moving Molecules.

Muncke, of Heidelberg, finds the following a simple and easy mode of showing the motions of particles;—triturate a piece of gamboge the size of a pin's head in a large drop of water on a glass plate; take as much of this solution as will hang on the head of a pin, dilute it again with a drop of water, and then bring under the microscope as much as amounts to half a millet-seed;—there are then observable in the fluid small brownish-yellow points, generally round (but also of other forms), of the size of a small grain of gunpowder, distant from one another from 0.20 to 1 line. These points are in perpetual motion, varying in velocity, so that they move through an apparent space of 1 line in from 0.5 to 2 or 4 seconds. If fine oil of almonds be employed in place of water, no motion of the particles takes place, but in spirit of wine it is so rapid as scarcely to be followed by the eye. This motion certainly bears some resemblance to that observed in infusory animals, but the latter show more of voluntary action. The idea of vitality is quite out of the question. On the contrary, the motions may be viewed as of a mechanical nature, caused by the unequal temperature of the strongly illuminated water, its evaporation, currents of air, heated currents, &c. If the diameter of a drop be 0.5 of a line, we obtain, by magnifying 500 times, an apparent mass of water of more than a foot and a-half

broad, with small particles swimming in it; and if we consider their motions magnified to an equal degree, the phenomenon ceases to be wonderful, without, however, losing anything of its interest *.

5. Exact Measure of a Degree.

Ten thousand rubles (upwards of 1500*l.*) a year have been granted by the Emperor of Russia for the continuation of the investigations undertaken to obtain the exact measure of a degree. This work, which, it is said, will last for ten years, is confided to the charge of M. Struve, of Dorpat. Two staff officers, natives of Finland, Messrs. Rosenius and Aberg, are already gone to their country for the purpose of discovering the mathematical points of union between Hochland and Tornea. M. Struve has projected a journey abroad, in furtherance of this great undertaking †.

6. Svanberg on the Temperature of the Planetary Space.

M. Fourier obtained, as one of the results of his important investigations of the temperature of the earth, &c., that the temperature of the planetary space was equal to —50° C., or (—58° F.), and arrived at the conclusion also, that the earth has arrived at its lowest degree of temperature, or that point below which it could not sink. M. Svanberg has arrived at a result so nearly the same, as to be very remarkable, especially considering that his mode of investigation was altogether of a different nature, and had for object atmospherical refraction. He wished to examine completely the problem of atmospherical refraction, and also the various hypotheses which have been put forth to determine its quantity. Some of these appeared sufficient for astronomical purposes, but having concluded their investigation, M. Svanberg wished to view the subject in a physical point of view, and then arose the difficulty of being able to determine, for each temperature observed at the surface of the earth, the law of the corresponding distribution of heat in the atmosphere, under the hypothetical condition of perfect equilibrium, and also the law according to which the temperature diminishes at different elevations above the level of the sea.

"In this examination, as in others (says M. Svanberg) where a greater or smaller number of natural phenomena are to be subjected to a mathematical formula, great inconvenience occurs, from the circumstance that an infinity of functions of various forms are capable of representing a finite number of observations, and that the real accuracy of the formula adopted can only be judged of by the accordance which it presents with those observations which have not served for the determination of its constants, by the number and the nature of the observations to which they may apply. Hence it

* Jameson's Journal, 1830.

† Bull. Géog. xiii. p. 306.

results, that one can never have a general rule by which to arrive directly at the point required, and that the work is obliged to be commenced with an hypothesis which, at a later period, is to be subjected to criticism from the observations. At the same time, the adoption of an hypothesis does not depend upon accident, but requires the most intimate knowledge of mathematical forms in their greatest extent, otherwise the progress would only be from error to error. One rule, however, should be observed—it is, to commence by trying those formulæ which require the determination of the smallest possible number of arbitrary constants.

'Guided by these considerations, and by the relation between light and heat so evident in the power which the solar rays possess of producing heat by their passage through bodies but little transparent, I commenced by supposing that the planetary space, with a perfect transparency, would undergo no change of temperature, neither by the effect of light nor radiant heat; and that therefore the elevation of temperature above that of the etherial regions can only commence at the limits of the atmospheres of the planets. A necessary result is, that the rate of change of temperature at a height infinitely above the surface of the earth is always proportional to the rate of the corresponding change in the capacity which the atmosphere possesses of absorbing light. Upon these considerations I expressed the temperature of the atmosphere by means of a formula, which applies to any height above the surface of the earth, and which contains only two arbitrary constants; one, which is also a function of the time, is always determined by the immediate observation of the corresponding temperature of the surface of the earth; and the other, which does not vary in relation to time, is the temperature of the planetary space.

'The numerical determination of these constants requires exact observations of the temperatures of isolated points, up to a considerable height above the earth's surface; but, unfortunately, these observations are so difficult, that at present I could take advantage of one only, that made by M. Gay Lussac, in his aërial voyage. It is very much to be desired that this observation should be repeated, especially in the neighbourhood of the equator, where atmospheric variations are small, and where, consequently, the influence of accidental circumstances are less to be feared. Nevertheless, the single observation of Gay Lussac has given me —49°.85 C., as the temperature of the planetary space, a number which only differs one-seventh of a degree from that obtained by M. Fourier, according to the laws of heat, radiating from the solid globe, supposed to have arrived at its state of fixed and invariable temperature.

'Without having much doubt as to the identity of light and heat, or as to the accuracy of our photometrical knowledge, I thought it would still be interesting to see the results which would be given by setting out from the data of Lambert, on the absorption of light, which, coming from the zenith, passes through the whole depth of the atmosphere: establishing my calculation on the supposition that

the differential of the augmentation of temperature is always proportional to the portion of light absorbed. In this way I found that the temperature of the planetary space was −50°.35 C., and I acknowledge the pleasure I felt at finding the remarkable accordance between these two results, and that of M. Fourier. The circumstance strengthens my opinion, that the formula I have given for the temperature of space deserves to be at least seriously examined. The immediate results which follow, are, that the temperature diminishes in a continually decreasing ratio as we ascend in the atmosphere, and that at a given height this ratio is greater as the temperature at the corresponding surface of the earth is higher.

'Although I had no intention of examining the formulæ relative to the determination of heights by barometrical observations, I have found, nevertheless, in the observations of M. Gay Lussac, that the influence of the results I have obtained becomes sensible, in the appreciation of heights equally great with that to which he rose, though it is not necessary to take note of it in the estimation of ordinary heights. The form of the function is to me very important, because from it I deduce the refractive power of the atmosphere in all the parts of the course pursued by light; and as I have already treated minutely the formula derived from it, for the definitive determination of the refractions themselves, so now I am in a condition to investigate the problem in question by the powers of mathematical investigation only, after having subjected it to the severest trials, by means of the physical observations which bear upon it *.'

7. On the Relation between the general Direction of the Stratification of the Earth and the Lines of equal Magnetic Intensity in the Northern Hemisphere.—(*M. L. A. Necker.*)

An interesting paper on this subject has been presented to the Geneva Society by M. Necker. Upon examining Captain Sabine's chart † of the curves of equal magnetic intensity, he was struck by the analogy of their direction with the form and position of the two great continents upon which they were traced. The northern extremities of these continents are somewhat symmetrical, and are placed at the extremities of a line, which, passing over the earth's pole, connects the two northern poles of magnetic intensity; the masses of which the continents of Asia and North America are formed, have an apparent tendency to extend themselves in the direction of the principal axis of the magnetic curves, and consequently on the same line, a tendency which is more manifest in America where the difference between the relative dimensions of the two axes of these curves is greatest. Finally, not only may the coasts of these continents be observed to have a general disposition to conform to the direction of

* Bib. Univ., 1830, p. 367. Berzelius' Report.

† Bib. Univ., 1829, p. 212, or Journal of Science, 1829, July—Sept., page 1.

the magnetic lines belonging to the pole situated on the one or other continent, but in many cases the direction is nearly parallel, and sometimes coincident with the direction of these curves. Thus in Asia, the coast from the north of the Persian Gulf is continued to Bombay, parallel to the curve ; the same is the case on the southern coast of China, which, at the south-east, turns to the north with the curve, and the latter follows exactly the direction of the long chain of islands Leou Kiou, Niphon, Jeso, Seghalien, which really form the eastern extremity of the ancient world.

In North America and elsewhere, M. Necker shows the same association of forms. He then refers to the well-known observation, that the geographical form of the earth is in direct relation to the elevation of the ground—that it is, in fact, derived from the lines of intersection made by the constant and uniform surface of the sea, conjointly with the irregular and undulating surface of the solid parts of the globe. Continents, islands, isthmus, &c., are parts bounded by these intersecting lines. It is equally well known now, and is receiving confirmation daily, that the elevation of the earth is determined by the stratification of the mineral masses which compose it, that is to say, that the assemblage of inclinations, or systems of inclination, which constitute the elevation, is in constant relation to the direction and inclination of the strata of the earth. It is to those chains of mountains, more or less elevated or extended, from which the inclined parts of a country descend, that the mineral strata also conform ; and we have reason to believe that the axes of each of those chains of mountains, or parallel set of strata, consist of unstratified masses of granitic or porphyritic rocks, the existence of which is connected with the laws which reign over the stratification of each region. All geologists generally admit that the direction of the strata on different sides of a chain of mountains is sensibly parallel to the direction of the chain itself.

From these considerations, it would appear, that if there is any analogy between the configuration of the northern continents and the direction of the curves of equal magnetic intensity, an analogy ought also to exist between the magnetic curves and the direction of the strata of which the earth is composed. M. Necker, therefore, proceeds to compare these curves with what is known of the stratification of the earth in the northern hemisphere, and finds a striking coincidence in direction. Thus the magnetic curve of 297 seconds traverses Scotland in a direction S.W. to N.E., which is precisely the direction he found the strata of that country to have when he personally examined them. The curve then passes to Christiana, in Norway, preserving the same direction ; and according to M. Buch, such is the direction of the strata at Christiana. It traverses Sweden, where, according to Hisinger, the N.E. direction of the strata still continues ; but on arriving at the Gulf of Bothnia, the curve changes and turns to the S.E. Here, and further on, correct observations of the direction of the strata are wanting, but the direction of the northern coast of Russia, Lapland from North Cape to the White Sea, which coast

is altogether rocky, and is prolonged from N.W. to S.E., is a circumstance in favour of the parallelism of the direction of the strata and the magnetic curve.

Other curves, examined in a similar way, offered similar analogies; and it may be observed generally, that, in western Europe, the magnetic curves of equal intensity, and the stratification, have both a north-eastern direction; whilst in eastern Europe they both have a north-westerly direction.

That there are anomalies M. Necker admits, and notes amongst them the Pyrenees in western Europe; the part of eastern Europe which, comprehending Styria, includes the mountainous portions to the west and south-west of Vienna and the north-west of Hungary; and the various groups, consisting of central masses of granite, clothed with mantles of stratified rocks.

The investigation is then carried on in North America, in Mexico, and in Asia, where, as far as the present state of geological knowledge will admit, the same general relations are observed. The longest chain of mountains in the world, namely, the Hymalaya, are found to coincide in direction perfectly with the magnetic curve.

Although the account of these analogies and directions is very incomplete, still M. Necker thinks it will be difficult to deny the existence of a relation between the stratification of the earth, the direction of the principal mountainous chains, the elevation of the continents, and the curves of equal magnetic intensity; and he thinks it must be a point of no small interest to the geologist, to observe the regular manner in which the mountains and mineral masses of the earth arrange themselves symmetrically about the two points of the northern hemisphere, which are nearly in the same places where Professor Hansteen and Captain Sabine have recognised the two poles of equal magnetic intensity*.

8. Force of Terrestrial Magnetism.

The following Table of the intensity of the earth's magnetic force at certain places on the continent, is from the recent observations of M. Quetelet, of Brussels, who was supplied with every facility of determining these intensities with accuracy:—

Place of Observation.	Duration of 100 oscillations of the Needle.	Mean horizontal intensity, that of Altona being 1.	Magnetic Inclination.	Total Intensity.
Berlin	391^s.13	1.0301	68°.42′	2.836
Gottingen ...	390 .72	1.0310	68 .39	2.832
Leipsic......	386 .72	1.0524	58 . 8.2	2.827
Dresden	382 .53	1.0756	67 .41.3	2.833
Brussels.....	392 .13	1.0245	68 .56.5	2.851
Frankfort....	385 .16	1.0614	67 .52	2.816

The places are arranged in the order of their latitudes, being

* Bull. Univ. 1830, p. 166.

comprised between 52° 32′ and 50° 7′. The instrument used was on the model of that belonging to M. Hansteen, and employed by Captain Sabine. The needles were small cylinders of steel, pointed at the extremities, and suspended in glass cases by silk filaments*.

9. An Acoustic Rainbow.

Professor Strehlke states that a sounding-plate, covered with a layer of water, may be employed to produce a rainbow in a chamber which admits the sun. On drawing the violin bow strongly, so as to produce the greatest possible intensity of tone, numerous drops of water fly perpendicularly and laterally upwards. The size of the drops is smaller as the tone is higher. The outer and inner rainbows are very beautifully seen in these ascending and descending drops, when the artificial shower is held opposite to the sun. When the eyes are close to the falling drops, each eye sees its appropriate rainbow, and four rainbows are perceived at the same time, particularly if the floor of the room is of a dark colour. The square plate on which Professor Strehlke made the experiment was of brass, nine inches in length, and half a line in thickness. The experiment succeeds best if, when a finger is placed under the middle of the plate, and both the angular points at one side are supported, the tone is produced at a point of the opposite side, a fourth of its length from one of its angles. An abundant shower of drops is thus obtained†.

10. Compression of Fluids.—(*Professor Oersted.*)

From a series of experiments on this subject, M. Oersted was led to the following results :—

i. The compressibility of fluids, up to the pressure of 70 atmospheres, is proportional to the pressure.

ii. Up to the pressure of 48 atmospheres, no perceptible degree of heat was developed in water.

iii. The compressibility of quicksilver does but very little exceed the millionth part of its volume for every atmosphere.

iv. The compressibility of sulphuric ether is three times as great as that of alcohol, twice that of sulphuret of carbon, and one and a half that of water.

v. Water which contains salts in solution is less compressible than pure water. At 32° F. pure water is by one-tenth more compressible than at 55° F.; at higher temperatures its compressibility also decreases, though not to such an extent as between 32° and 55°.

vi. The compressibility of glass is very small, much less than that of quicksilver.

Mr. Perkins found the compressibility of water more than double that resulting from M. Oersted's experiments; a difference which,

* Bull. Univ. 1830, p. 365.

† Poggendorff's Annal. 1830, No. 3.

according to M. Oersted, must be accounted for by the circumstance that, in Mr. Perkins's experiments, the compression was produced by percussion, the force of which cannot be calculated.

11. Peculiar Appearance of Saturn's Ring.

The second Number of Schweigger-Seidel's Jahrb., 1830, contains the report of M. Schwabe, of Dessau, on the appearance of Saturn's ring, which be repeatedly found at the eastern side to be more distant from the body of the planet than on the west side. He had convinced himself that the shadow of the planet had no influence on this appearance, which he had first discovered in 1827 with a $3\frac{1}{2}$ foot refractor, and found his observations confirmed in 1829 by a 6 foot refractor with 54 lines aperture. In the nights of the 21st of April and of the 3rd, 11th, and 20th of May, the inequality seemed to be at its maximum, and appeared less in the intermediate nights. These observations were also confirmed by those of Professor Harding.

---

§ II.—*CHEMICAL SCIENCE.*

1. Decomposition of Water by atmospheric Electricity.

M. Bonijol, conservator of the reading society of Geneva, has constructed many very delicate apparatus, by means of which water may be readily decomposed by the electricity of the ordinary machine, and also by atmospheric electricity. The electricity of the atmosphere is gathered by means of a very fine point fixed at the extremity of an insulated rod; the latter is connected with the apparatus, in which the water is to be decomposed, by a metallic wire, of which the diameter does not exceed half a millemeter ($\frac{1}{50}$th of an inch). In this way the decomposition of the water proceeds in a continuous and rapid manner, notwithstanding that the electricity of the atmosphere is not very strong. Stormy weather is quite sufficient for the purpose*.

2. Decomposition by ordinary Electricity.

M. Bonijol has also succeeded in decomposing *potash* and the chloride of silver, by placing them in a very narrow glass tube, and passing a series of electric sparks from the ordinary machine through them. The electricity was conducted into the tube by means of two metallic wires fixed into the ends. When a quick succession of electric sparks had taken place for about five or ten minutes, the

* Bib. Univ. 1830, p. 213.

tube containing chloride of silver was found to contain reduced silver: and when potassa had been submitted to the electric current, then the potassium was seen to take fire as it was produced*.

3. On the Decomposition of Metallic Salts by the Voltaic Pile, and on the state of Chlorides, Iodides, &c. in solution.

Whilst experimenting with a voltaic pile of thirty pairs of plates, M. Carlo Matteuci observed, that when the poles were plunged into solution of common salt, they both evolved gas ; but that when introduced into solution of sulphate of copper, although oxygen was evolved as before from the positive pole, hydrogen ceased to be disengaged at the negative pole, but metallic copper was there deposited. Using various other metallic solutions, he found that those of lead and silver, with some others, produced the same effect, *i. e.* evolved no hydrogen, but had the metals deposited in the metallic state, whilst others evolved gas at the negative pole, and had their bases deposited as oxides. Reasoning on the effect, he was induced to conclude, that in the cases in question, the hydrogen separated at the negative pole was employed in reducing the oxides of the metals; and hence its disappearance, and the deposition of the base in a metallic state. To assure himself of the truth of this view, he constructed a weak pile composed of only two elements, and incapable of decomposing a weak solution of salt. A solution of nitrate of silver is far more easily decomposed than water, as M. Becquerel has shown, and such a solution was readily decomposed by this weak pile of two elements; and at the same time it was observed, that the usual deposit of metallic silver did not occur, but an olive-coloured layer of oxide of silver was produced. It is, therefore, sufficiently proved, that the disengagement of hydrogen at the negative pole of the pile ceases, only because that element is employed in reducing the metallic oxides already separated from these acids by the action of the pile. It is a striking case of the powers of nascent hydrogen at common temperatures.

Having explained this appearance, M. Matteuci proceeded to decompose the chlorides and iodides by means of the pile, with the expectation of being able to deduce the nature of these compounds when dissolved in water. If it were possible to decompose these combinations by means of electric currents, incapable of decomposing water, one might then justly conclude that their composition was not changed by solution in that liquid. He, therefore, took a pile composed of two elements only, charged with water rendered slightly saline, and which had no power of decomposing water even a little acidulated. The platina conductors were then dipped in a solution of muriate of copper, and after some time, the negative conductor

* Bib. Univ. 1830, p. 213.

was covered with metallic copper, whilst the positive conductor evolved bubbles of gas. Having replaced the latter conductor by one of silver, it soon became covered with a yellow film gradually changing to violet, which was considered as chloride of silver. The experiment was repeated with the iodides of zinc and iron; the platina poles had scarcely touched the solutions before the iodine, with its distinctive colour, appeared at the positive pole, and the metals were reduced and deposited upon the negative pole.

'After these experiments it appears,' says M. Matteuci, 'that we may affirm with certainty, that these combinations, even when dissolved in water, do not change in their nature, and are not converted, as is often imagined, into muriates, hydriodates, &c. of the oxides of the metals present.'*

4. Voltaic Test of the State of Metals.

It is well known that Dr. Wollaston devised a beautiful little arrangement to ascertain the conducting power of certain crystals having metallic characters, and which ultimately proved to be titanium. If a plate of copper be in contact with a plate of zinc, and part of both plates be immersed in a dilute acid, the copper, by its electric condition, decomposes water and becomes covered with bubbles of hydrogen. If a piece of paper, or a card, be interposed where the two metals were in contact, the copper loses this power altogether, and no bubbles appear on it; but if a small hole be made in the paper or card, and a little piece of metallic matter put there, so as to touch at once both the zinc and copper, then the latter has its full power restored.

M. Macaire Prinsep has applied this test more generally; and he found, in the first place, that a metal was necessary to restore the effect—lead, bismuth, tin, &c. reproduced the bubbles; but sulphuret of arsenic, rutilite or oxide of titanium, grey cobalt ore, and the sulphurets of antimony, iron, tin, or lead, produced no effect. Portions of meteoric stone from Aigle and Barbotan, by producing bubbles, showed that they contained uncombined metal; and the method seemed competent to indicate, in all cases, whether the metals used were free, or in a combined condition.

As lead gave bubbles, but the sulphuret of lead none, experiments were made with lead, to which sulphur, in increasing proportions, had been added:—$\frac{1}{100}$, $\frac{1}{50}$, $\frac{1}{32}$, $\frac{1}{16}$, and $\frac{1}{12}$ of sulphur did not take away the property from lead; but when $\frac{1}{8}$ of sulphur was used, no bubbles appeared upon the copper. Then ascertaining the proportions in the definite sulphuret of lead, he found them to be exactly those which caused the evolution of bubbles to cease (86 lead and 14 sulphur.) The same effect occurred with the sulphuret of tin; and hence it was concluded that chemical combination in

* Bib. Univ. 1830, p. 138.

determinate proportions was necessary to prevent this electric decomposition, and that mixtures had no influence on the phenomena.

These results may be important to the mineralogist; and M. Macaire Prinsep, in illustration, concludes, that the grey cobalt ore of Lunaberg, which is composed of cobalt, arsenic, and sulphur, contains only sulphurets of the metals; that, on the contrary, the metals of aërolites, although sometimes found associated with sulphur, and always with silica, exist neither as sulphurets nor silicates, but in their metallic condition*.

5. Powerful Electro-magnet constructed by Professor Moll.

If a piece of iron rod be bent into the form of a horseshoe magnet, and coiled round with a copper wire, so that the latter may form a helix through which the voltaic current may be sent, the iron becomes for the time a powerful magnet.

Professor Moll has repeated this experiment upon an enormous scale. His galvanic apparatus was a copper cell, charged with water, mingled with $\frac{1}{60}$ of sulphuric and $\frac{1}{60}$ of nitric acid, into which was introduced a zinc plate exposing 11 square feet of surface to the acid. His magnet was made of a cylinder of soft English iron, 1 inch in diameter; when bent into form, the interval between the ends was $8\frac{1}{2}$ inches: the copper wire forming the spiral was $\frac{1}{8}$ of an inch in diameter, and made eighty-three convolutions; the weight of the whole was 5lbs. A connecting piece, of the usual form, made of soft iron, joined the two extremities of the horseshoe, and the ends of the spiral were dipped in mercury for ready voltaic communication. The horseshoe was hung in the usual manner of magnets.

In the first experiment this arrangement sustained, first, 50lbs., and afterwards, with care, 76lbs. by its magnetic attraction. When the suspended weight was small, it was found that the iron retained its magnetism for a time after the voltaic communication was broken. If, instead of merely breaking the direction, the electric poles were reversed, then the reversion of the magnetism took place with extraordinary rapidity. On effecting the change, the iron lost all power, the weight fell off, but, with the rapidity of lightning, was again attracted and sustained to an equal amount as at first.

The rapidity of this change is the more extraordinary, if compared with the slowness and difficulty of charging the poles of a magnet, of equal force, by the ordinary method. If, instead of a heavy weight, a light steel needle be in contact with the poles of the electro-magnet, then so rapid is the change that the needle never falls off, for the attractive force is destroyed and re-established before the gravity of the needle has time to remove it sensibly from its first position.

When the piece of soft iron connecting the poles is held by the hand during this change, the sensation is of the most extraordinary

* Bib. Univ. 1830, p. 146.

kind. Powerful attraction is first felt; this on a sudden fails, and the hand with the iron gives way, but the force is so instantly renewed, that the hand is violently drawn up again by an attraction as great as ever.

The moment the electric communication is completed, the iron is magnetised to a maximum, and bears its greatest charge. On increasing the voltaic apparatus in force, by adding to it another, exposing 6 square feet of zinc, so as to make 17 square feet of surface altogether, no increase of magnetic power was conferred upon the arrangement; nor by using a higher charge was any increase of power obtained, the maximum effect of the iron had been developed in the first instance. Whether the spiral were of copper or brass wire, made no difference. When it was iron wire, $\frac{3}{16}$ of an inch in diameter, and prevented from touching the curved soft iron by intervening silk, the weight taken up was rather higher, being 86lb.

A larger soft-iron horseshoe magnet was now made; the iron was $2\frac{1}{2}$ inches in diameter; the chord of its arc was $12\frac{1}{2}$ inches; the spiral was brass wire $\frac{1}{8}$ of an inch in diameter, and made 44 turns. The magnet weighed 26lbs. and its connecting piece 4lbs. With the voltaic apparatus of 11 square feet, this arrangement supported 139lbs.; this was raised to 154lbs. by using an iron spiral, with silk between it and the magnet. This is the maximum to which M. Moll has carried his experiments; but the force exerted is enormous, and at the same time instantaneous; and it is extraordinary to see an arrangement, which at one moment can support this weight, lose all its force merely by breaking or altering a distant contact and again have it as fully renewed.

On trying to heighten the power of an ordinary steel magnet, now capable of supporting 5lbs., but formerly much more, these means failed entirely: though left surrounded by the spiral for a long time, its force remained at 5lb. The powerful electro-magnets of soft iron just described have, however, every power of ordinary magnets in touching or affecting steel bars, or in strengthening and reversing the poles of ordinary magnets.

There is a magnet in the Teylerian Museum at Harlem, which supports 230lbs.; there are, perhaps, one or two other very powerful ones, but except these, the electro-magnet of Professor Moll is the most powerful of any known magnets, and yet is, probably, far short of what might be effected by similar means*.

6. Laws of Electrical Accumulation.

Mr. Harris, of Plymouth, has made an extensive series of experiments on the laws of the accumulation of ordinary electricity. The details of these experiments, with illustrative plates, are published in the Transactions of the Plymouth Institution, 1830. We have not space for more than the conclusions at which he arrives.

i. An electrical accumulation may be supposed to proceed by

* Bib. Univ., 1830, p. 19.

equal increments. A coated surface, charging in any degree short of saturation, receives equal quantities in equal times, all other things remaining the same. The quantity passing from the outer coating is always proportional to the quantity added to the inner.

ii. The quantity of matter accumulated may be estimated by the revolutions of the plate of the electrical machine, supposing it in a state of uniform excitation; or it may be measured by the explosions of a jar connected with the outer coatings. It is as the surface multiplied by the interval which the accumulation can pass: when the surface is constant, it is as the interval; when the interval is constant, it is as the surface. It is also as the surface multiplied by the square root of the free action or intensity: when the surface is constant, it is therefore as the square root of the attractive force.

iii. The interval which the accumulation can pass is directly proportional to the quantity of matter, and inversely proportional to the surface: it is as the quantity divided by the surface: if the matter and surface be either increased or decreased, in the same proportion the interval remains the same. If, as the matter be increased, the surface be decreased, the interval will be as the square of the quantity of matter.

iv. The force of the electrical attraction varies in the inverse ratio of the square of the distance between the points of contact of the opposed conductors, supposing the surfaces to be plane and parallel; or otherwise between two points which fall within the respective hemispheres at a distance equal to one-fifth of the radius, supposing the opposed surfaces to be spherical.

v. The free action or intensity is in a direct proportion to the square of the quantity of matter, and in an inverse proportion to the square of the surface: it is directly as the effect of an explosion on a metallic wire, all other things remaining the same. If the matter and the surface increase or decrease together, so in the same proportion the attractive force remains the same. If, as the matter be increased, the surface be decreased, the attractive force is as the fourth power of the quantity of matter.

vi. The effect of an electrical explosion on a metallic wire depends exclusively on the quantity of matter, and is not influenced by the intensity or free action. It is diminished by accumulating the matter on a divided surface: it is as the square of the quantity of matter: it is as the square of the interval which the accumulation can pass: it is directly as the attractive force of the free action, all other things remaining, in each case, the same: it is as the *momentum* with which the explosion pervades the metal*.

7. On the Emission of Light during the Compression of Gases.

When certain gases have been suddenly compressed, the evolution

* Page 97.

of light has been observed; at first this was supposed to be the case with all gases; but M. Soissy, of Lyons, stated, that it happened only with oxygen, air, and chlorine, a result which has been confirmed by M. Thenard. The latter philosopher, on reflecting that the pistons used had been greased, thought the light might perhaps be due to the formation of a little water, or muriatic acid, in these cases; and therefore repeated the experiments with pistons moistened only with water, and then found that no light was evolved.

He then made other experiments on the inflammation of various substances in compressed oxygen, chlorine, &c. We are constrained to omit the detail of these, but the following are the conclusions to the paper:—1. No gas becomes luminous of itself by pressure exerted in the ordinary manner in cylinders by pistons. 2. The highest pressure which can be given by the hand to gas in a tube of glass raises the temperature much above 400° F. Powders which are not decomposed at this temperature explode instantly in azote, hydrogen or carbonic acid gas, suddenly compressed. 3. Paper and wood inflame in oxygen suddenly compressed, and oiled paper inflames in the same manner in chlorine. 4. If the gases be compressed more forcibly and suddenly, they would doubtless attain a much higher temperature; but it is not probable that they would of themselves become luminous, except at very high temperatures*.

8. On Oxamide, a Substance which approximates to some Animal Bodies.—(*M. Dumas.*)

This substance is produced whenever oxalate of ammonia is distilled, and the name *oxamide*, or *oxalamide*, is given to it provisionally, as indicating that it is formed of oxalic acid and ammonia, and by particular treatment can reproduce these bodies. When acted upon by potash, it yields 36 per cent. of ammonia, though it contains none; by the same treatment it can produce 82 per cent. of oxalic acid, and yet includes none of that body. These curious properties associate oxamide with the phenomena which occur when animal substances are made to yield ammonia by the action of alkalies, and also with those new observations due to MM. Vauquelin and Gay Lussac, on the developement of oxalic acid, when organic matters are acted upon by potassa.

When oxalate of ammonia is distilled, it first loses water; the crystals become opaque; then, where close to the heat, fuse, boil, are decomposed, and disappear without any change occurring in the more distant parts of the mass. Ultimately, a little carbon remains, but nearly the whole has been volatilized. The water which has passed over into the receiver contains a flocculent substance; a thick deposit of a dull white matter also lines the neck of the retort; both these are oxamide. To isolate it, the whole is diffused in water, filtered, and washed, the peculiar substance remains in the filter. 100 parts of the oxalate of ammonia yield 4 or 5 of oxamide; the

* Ann. de Chimie, xliv., 181.

other products are ammonia, water, carbonate of ammonia, carbonic acid, oxide of carbon, and cyanogen.

Oxamide occurs in imperfectly crystallized plates, or as a granulated powder. When well washed and pulverized, it is a dirty white powder, looking like uric acid, having no taste or odour, and not affecting test papers. Heated carefully in an open tube, it volatilizes; heated in a retort, part sublimes, whilst part is decomposed, yielding cyanogen and a very bulky, light charcoal remains. It is scarcely soluble at common temperatures; a saturated solution at 212° F. deposits confused crystalline flocculi of the unaltered substance.

As oxamide is an azoted substance, the ratio of the azote and carbon to each other was first ascertained by combustion with oxide of copper in a glass tube. In this mode of analysis, M. Dumas points out the necessity of collecting the *whole* of the gas evolved, and ascertaining its composition. Portions of the gas often differ from each other; and if the composition of the whole be deduced from these portions, great errors may occur. In experiments on the oxamide, two volumes of carbonic acid were produced for each one of azote, so that the carbon and the azote are in the same proportion as in cyanogen; 100 parts of oxamide gave 26.95 carbon, and 31.67 azote.

When oxamide was heated with great excess of concentrated sulphuric acid, it yielded a mixture of carbonic acid and of carbonic oxide gases in exactly equal volumes; no cyanogen was formed; this is precisely what takes place with oxalic acid. When the sulphuric acid was diluted and saturated with potash, much ammonia was evolved, so that a sulphate of ammonia had been formed. In this way, therefore, oxamide is resolved into ammonia, carbonic oxide, and carbonic acid.

When oxamide was heated for some time with strong solution of potassa in great excess, much ammonia was disengaged. The potash, afterwards neutralized by nitric acid, was found to contain oxalate of potassa, so that potassa evolves oxalic acid and ammonia from oxamide, and those substances only.

These results created a suspicion, that oxamide was to oxalate of ammonia what pyrophosphoric acid is to the ordinary phosphoric acid. The substance, therefore, was compared to oxalate of ammonia, supposed to be dry, both by theory and experiment. The carbon is to the azote as 2 proportionals to 1 in both compounds; but 100 parts of oxamide contain 26.95 of carbon, and 31.67 of azote, whilst 100 parts of dry oxalate of ammonia contain only 22.6 of carbon, and 26.6 of azote. When 100 parts of oxamide were converted by potash and sulphuric acid into the elements of oxalate of ammonia, they gave products amounting to 120 parts, *i. e.,* 26.95 carbon, 31.67 azote, 54.70 oxygen, and 6.3 hydrogen$=$119.62. Now, the sulphuric acid and the potash could neither of them give carbon or nitrogen, but might communicate oxygen and hydrogen from the water present with them: withdrawing 19.62 of these elements in the

proportion to form water, there remains the following composition as nearly as may be:—

4 vols. carbon	27.08
2 — azote	32.02
2 — oxygen	36.36
4 — hydrogen	4.54
	100.

Oxamide may, therefore, be considered at pleasure as a compound of cyanogen and water; or as a compound of deutoxide of azote, and bicarburetted hydrogen; or as a compound of oxide of carbon and a hydruret of azote, different to ammonia. Whichever way it be viewed, if 2 volumes of vapour of water are added to it, dry oxalate of ammonia is produced; and it is in this way, apparently, that sulphuric acid and potassa act.

In conclusion M. Dulong remarks, that many animal matters, as albumen, gelatine, fibrine, &c., act with potassa as oxamide does. Uric acid approximates to it: hippuric acid also resembles it. All these bodies have properties in common with it so characteristic, that M. Dulong has been induced to commence an experimental comparison of them with this new substance *.

9. Preparation of Nitrogen.—(*Professor Emmett.*)

When zinc is dipped into fused nitrate of ammonia, it is instantly oxidized and dissolved, and nitrogen and ammoniacal gases are evolved. The zinc disappears with as much rapidity as when exposed to the strongest mineral acids; and, at the same time, so completely sustains the requisite temperature, that it becomes unnecessary to continue the application of heat after the action commences. The heat required is 280° or 300°, but a small piece of zinc soon elevates it to 540°. No nitrous or nitric oxide could be detected in the evolved gas, and therefore Professor Emmett recommends the operation as one well fitted to supply nitrogen gas.

A tubulated retort is to be partly filled with the nitrate of ammonia, and a cork fitted to the tubulature. Through this cork is to pass freely either a knitting-needle or an iron wire, holding, by means of a hook, the coil of zinc. As soon as the salt has entered into fusion, the knitting-needle must be pushed down far enough to place the zinc in contact with the nitrate. This arrangement is not only convenient but necessary; for if the zinc be thrown at once into the fused salt, the action will prove too violent and unmanageable; whereas, when contact is not constantly maintained, there is a strong tendency towards a vacuum in the retort, which would endanger its safety. By the process here recommended, there is no liability to accident, and the quantity of nitrogen may be easily

* Ann. de Chimie, xliv. 113.

regulated, by raising or lowering the zinc. Every grain of the metal furnishes nearly a cubic inch of the gas, while the ammonia, which also escapes, becomes wholly condensed as soon as it enters into the water of the pneumatic cistern *.

10. Action of mixed Nitrate and Muriate of Ammonia on Glass.

When equal parts of these salts are mixed and fused between two watch-glasses, the under glass becomes corroded nearly to one-half of its thickness, and the effect even extends to the cover. The heat of a spirit-lamp is quite sufficient for this purpose. Here, without water, or even perfect fusion, the alkali is entirely removed, and the silex left, forming a snow-white opaque substance, so soft as to admit of being cut through with the point of a needle or knife: green glass is not so easily affected, owing to its greater hardness and the absence of lead. The fused nitrate alone, if confined between watch-glasses, also produces slight corrosion, but the effect is so remarkable when the nitro-muriate is employed, that a person operating upon an unknown mineral, and ignorant of this property, would be induced to attribute the result to the presence of fluoric acid. Indeed, when we consider that the effect appears to depend upon the liberation of nitro-muriatic acid, or perhaps even to highly concentrated nitric acid alone, it does not seem improbable that similar cases have often occurred by the common mode of analysing; and this opinion is further strengthened by the fact, that some minerals, as the chondrodite, appear to have furnished fluoric acid to one operator and not to another †.

11. Pulverization of Phosphorus.—(*Casaseca.*)

If phosphorus be put with alcohol into a bottle, and shaken for some time, it may be obtained in powder of the utmost tenuity, which, when diffused through the alcohol, appears as if it consisted of a multitude of minute crystals.

12. Inflammation of Phosphorus by Charcoal.

Dr. Bache, of Philadelphia, states, that, at the temperature of 60° F., or upwards, carbon in the form of animal charcoal, or lampblack, causes the inflammation of a stick of phosphorus powdered with it; the effect takes place either in the open air, or in a close receiver of a moderate size ‡.

13. Preparation of Bi-Carbonate of Soda.

The following method of preparing this salt, in the large way, is described by Mr. F. R. Smith, of Philadelphia. The ordinary crys-

* Silliman's Journal, xviii. p. 259. † Ibid. xviii. p. 258.
‡ Silliman's Journal, xviii. p. 373.

tals of carbonate of soda are placed in a box made on purpose, and are surrounded by carbonic acid gas under pressure. The salt absorbs the gas, and, as the bi-carbonate requires but little water, much of that contained in the crystals of the original carbonate drip away in the form of a solution. When gas ceases to be absorbed, the salt is taken out, and dried at a moderate temperature.

Upon examination, after the absorption of gas has ceased, the portions of salt are found in their original form, but porous and friable, and the fracture without lustre. Each consists of an aggregation of crystalline grains as white as snow, and scarcely alkaline to the taste. In this way all the trouble of solution, evaporation, &c. involved by the ordinary process, is obviated. The production of gas should be continued for a sufficient time, and the subsequent drying of the salt should be at a moderate temperature, or else portions of carbonate may remain.

When a portion of salt thus prepared was washed with a little water, to remove any carbonate, then dried and analysed, it proved to be, not sesqui-carbonate, but true bi-carbonate. M. Boullay has repeated the process on a large scale, and obtained exactly similar results*.

14. Rock Salt in Armenia.

Armenia was incorporated with Russia in 1828, by the treaty of Tourkmantchaï, made with Persia. The salt is found in a mountain two leagues and a half from Nakchitchevane, situated on an extensive plain extending along the left bank of the Araxes. The mountain is seven leagues and a half in circumference, and, from the appearance of very ancient works, has evidently yielded salt for many ages. These remains consist of enormous horizontal galleries, supported by pillars of salt; and, according to the traditions of the people, many mines have been abandoned from the difficulties of working them, occasioned by the depth of the strata and frequent inundations. The Persian government, for the last fifteen years of its time, let them for a sum equal to 16,000 francs annually.

The salt is worked by gunpowder; the works are wrought by the inhabitants of a small neighbouring village, consisting of Armenians and Tartars, from three to twenty persons being required at a time. The Russian government has let the works, since March, 1829, for a year, for a sum equal to 16,000 francs †.

15. Preparation of Lithia.—(*Quesneville, fils.*)

One part of triphane is pulverised in water, mixed intimately with two parts of pulverised litharge, put into a crucible and heated to whiteness. In a quarter of an hour the whole is liquid; it is to be

* Journ. de Pharm. 1830, p. 118.

† Revue Ency. xlviii. p. 504.

poured out, finely pulverised, acted upon by nitric acid, and the undissolved silica separated. The lead is then to be precipitated by sulphuric acid, and the liquid evaporated to dryness, to drive off the nitric acid; the residue is to be dissolved in water, the alumina and metallic oxides precipitated by ammonia, the lime and magnesia by carbonate of ammonia, and the filtered liquid evaporated to dryness. The residue is to be strongly heated in a porcelain (not platina) crucible; what remains dissolved in water; the sulphuric acid precipitated by baryta water; and the new liquid, being filtered and evaporated, gives pure lithia.

M. Quesneville very strongly recommends the use of nitrate of lead in the analysis of alkaline minerals, according to M. Berthier's proposal*.

16. On the Submuriates of Iron, and other Subsalts. (*Mr. Phillips.*)

Whilst dissolving moist precipitated peroxide of iron in muriatic acid, Mr. Phillips observed that much more oxide was taken up than he had expected, and, after repeated additions, he obtained a very deep red-coloured solution, having little of the well-known chalybeate taste, and of the s. g. of 1.017; it was not decomposed by the addition of water, or by heat, unless evaporated to dryness: alkalies decomposed it. Ferroprussiate of potash gave a dark brown-green precipitate. When more oxide was added, the excess, or a portion of it, combined with the submuriate already formed, and the acid and oxide were totally precipitated, forming another but an insoluble submuriate. Even the addition of muriatic acid caused a partial decomposition of the soluble submuriate, and a precipitation occurred: this happens with no other binary salt.

Being analysed, the soluble submuriate gave 37 muriatic acid and 382 of peroxide of iron, equal to one atom of muriatic acid and 9½ of peroxide. Mr. Phillips is inclined to consider 1 : 10 as the true proportion.

Except the subacetate of lead, this is the only subsalt so largely soluble in water; probably, the only one which contains so small an atomic proportion of acid; the only one decomposed by addition of either acid or base: and the last mentioned point shows that there are two other submuriates of iron differing from this one, by insolubility in water.

Mr. Phillips has also analysed the submuriate of antimony, or powder of Algaroth. It consists of protoxide of antimony 92.45, muriatic acid 7.8; or 9 atoms and 1.

Subnitrate of bismuth was also analysed, and proved to consist of 81.92 oxide of bismuth, and 18.36 nitric acid; or 3 atoms and 1.

Submuriate, or magistery of bismuth, being analysed, gave 87 oxide of bismuth, and 13.6 muriatic acid; or 3 atoms and 1. Ac

* Journ. de Pharm. 1830, p. 1196.

cording to Dr. Thomson, the carbonate of bismuth is a triscarbonate, similar in constitution to the subnitrate and submuriate above.

Upon decomposing the subnitrate and submuriate of bismuth by alkali, they yield oxide of bismuth; that, in the first case, is always yellow, but in the second it varies much in colour, being frequently greyish-black and even deep bluish-black. The cause of these variations has not been discovered, nor even the circumstances which ensure a dark coloured preparation. It is not due to sulphuretted hydrogen or other impurities, nor to difference of composition. When the black oxide was heated on platina foil, it lost neither weight nor colour; but, being melted, it became yellow: the cause is probably, therefore, in some difference of aggregation; and may in that respect be analogous to the differences of colour, which can be induced, by various means, on chloride of silver *.

17. On the Reaction of Persalts of Iron and neutral Carbonates.

M. Soubeiran has experimentally investigated this action, and arrived at the following conclusions:—i. When salts of the peroxide of iron are decomposed by neutral carbonates, they yield a carbonate of the peroxide equally neutral: this carbonate is soon destroyed to produce a double salt, formed of the neutral alkaline sulphate and the subsulphate of iron: this new salt is also easily decomposed, and yields a new sulphate of iron heretofore unknown, and containing thrice as much base as the neutral salt: a feeble alkali in excess precipitates another subsalt, which chemists have not before noticed, and which is a true double salt, composed of the subsulphate of iron and hydrated oxide of iron. ii. That the aperient saffron of Mars is a hydrate of the peroxide of iron, containing 3 atoms of water mixed with variable and accidental quantities of sesqui-subcarbonate of iron, and sometimes neutral carbonate of iron †.

18. On the relative Action of diluted Sulphuric Acid and Zinc.—(*M. A. de la Rive.*)

Whilst engaged in experiments on the construction of the voltaic pile, M. de la Rive was struck more particularly with a fact which has often been observed by chemists, but has never received its proper explanation. If zinc, purified by distillation, be plunged into dilute sulphuric acid, it is scarcely attacked, especially at first; it produces but a small quantity of bubbles of hydrogen, and these succeed each other very slowly; but zinc of commerce, placed in the same circumstances, produces an enormous quantity of hydrogen, with an effervescence and vivacity well known to those who have prepared this gas.

* Phil. Mag. N. S. viii. p. 406.

† Journ. de Pharm. 1830, p. 535.

In examining the influential circumstances of this action, two appeared to have predominating power; the degree of dilution of the acid, and the state of the metal. These were estimated by the quantity of gas evolved from given surfaces of zinc in a given time; and a convenient little apparatus for that purpose was used, which allowed of the quick repetition of the experiments, and furnished accurate results as to the volumes of gas produced.

Six mixtures of acid and water were used. In the following table, the first column gives the number by which any mixture is distinguished in the future experiments, the second expresses the specific gravity, and the third the quantity of sulphuric acid per cent.

1	1.137	20.20
2	1.182	25.64
3	1.215	29.85
4	1.258	35.28
5	1.326	43.25
6	1.532	64.20

When the different kinds of zinc were immersed in these acids, it was with the exposure of certain measured and equal surfaces: thus, in the following table, pieces of zinc, each having 200 square millemetres of surface, were left in the respective acid, until each had evolved 300 cubic millemetres of hydrogen gas; and the time occupied, which constitutes the table, of course expresses inversely the facility with which the acid and zinc evolved gas.

Acid	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Zinc of commerce	0′.6″	0′.3″	0′.2″	0′.3″	0′.4″	0.′9″
Distilled zinc	3′.30″	1′.50″	0′.30″	0′.26″	0′.24″	1.′30″

These experiments were all made with liquid at the same temperature—*i. e.*, between 10° and 12° C., but the temperature rose, and the more the stronger the action; thus, with the acid No. 3 it rose about 5° C., or 9° F., in 15 minutes. Another fact to be noticed is, that the action was very slow in all at first, but afterwards rose slowly with the pure zinc, but rapidly with that of commerce: the latter generally attained its maximum action in 10 minutes, the former required several hours for that effect. It appears, also, that the acid No. 3 is that which acts most energetically upon ordinary zinc; the Nos. 2, 4, and 5 differ somewhat from it; Nos. 1 and 6 much. No. 3 contains 30 per cent. of sulphuric acid; and it may be said generally that, for the evolution of hydrogen most rapidly from ordinary zinc, the diluted acid should contain not less than 25, nor more than 50 per cent. of oil of vitriol.

The action of the acids on pure zinc, it may be observed, does not follow the same order as on ordinary zinc.

With regard to the cause of the difference between pure and ordinary zinc, it might at first be supposed to be due to a degree of openness or porosity in the latter, but it was found that each had the same

specific gravity, namely 7.2, and the differences were the same also, when each were reduced to filings.

Concluding, therefore, that it was more probably due to the presence of heterogeneous substances in the ordinary zinc, certain mixtures were made of pure zinc and other metals, and four alloys prepared;—the first, contained a tenth of iron filings, added when the distilled zinc was in fusion; the second, a tenth of tin; the third, a tenth of lead; and the fourth, a tenth of copper. These zincs were then tried as the former were, the same quantities of surface being exposed and of gas collected. The following are the results :—

Acid .	No. 1, 10° C.	No. 2, 10° C.	No. 3, 15° C.
Distilled zinc	3′.27″	1′.50″	0.30″
Tin zinc .	0′.24″	0′.12″	0.12″
Lead zinc .	0′.12″	0′.9″	0.10″
Copper zinc	0′.4″ to 6	0′.6″	0.3″ to 4
Iron zinc .	0′.4″	0.′3″	0.2″ to 1
Common zinc	0′.4″	0′.3″	0.2″ to 1.

Generally in these experiments the action was at first slow, and then increased more or less rapidly, according to the nature of the alloy, until it had obtained its maximum, which is the rate expressed usually by the time in the table; the copper zinc formed an exception —its action was most rapid at first, and gradually became slower, from the formation of a black crust of oxide, &c. upon it; this being removed, the rapidity of action was restored. The iron zinc, it may be observed, was acted upon as rapidly as the ordinary zinc of commerce.

The circumstances accompanying the phenomena in question are such as to induce a persuasion on the mind that the whole is due to electro-chemical action. The first circumstance is the powerful influence of a heterogeneous metal, mixed with pure zinc, to facilitate the decomposition of water and disengage hydrogen. The second is, that the diluted acid which is most powerful in exciting this action is that which is the best conductor of electricity. By a very careful set of experiments, made with the galvanometer, it was found that the acids 3 and 4, and especially 3, were much better conductors than any other of the mixtures. Former experiments had shown that concentrated sulphuric acid was a worse conductor than diluted; but now it was proved that acid, containing between 30 and 50 per cent. of oil of vitriol, was a better conductor than if either stronger or weaker; and it is precisely such acid which evolves hydrogen most rapidly from ordinary zinc.

As a further illustration of the influence of voltaic action on zinc dissolving in acid,—if a piece of distilled zinc be dissolved in the diluted acid, it requires a certain time to produce a certain quantity of gas; if a platina wire, immersed in the acid, be made to touch the zinc, it, of course, immediately gives out hydrogen; and the whole quantity of gas from the two metals, under these circumstances, is twice or thrice what it was before. If the platina wire

be rolled round the zinc, or if the latter be studded with pieces of platina, then the quantity of gas evolved principally from the platina is much more than from the zinc alone.

Now, the action upon the alloyed zinc appears to be quite analogous to the action upon the voltaic circle formed above by the zinc and platina. The small chemical action which takes place on pure zinc determines an electric current between each molecule of zinc, and the molecule of other metal in contact with it. These currents decompose the water which they traverse, according to the well-known laws of voltaic decomposition—evolving the hydrogen upon the heterogeneous molecule, which is negative in all the alloys and combinations mentioned, and carrying the oxygen to the zinc, which is positive, and, combining with it, it forms first an oxide and then a sulphate, which dissolves. This decomposition of water, and consequently the quantity of hydrogen evolved, will be greater as the minute currents of electricity are stronger, and these will be stronger as the acid increases in conducting power. Now it has been found experimentally that the acid mixture which conducts best, evolves most gas in a given time.

The decomposition of water should, in this mode of viewing the question, also increase with the difference between the oxidability of the zinc and the other metal. The iron zinc has, however, in these experiments, surpassed the copper zinc, although the two latter metals form a more powerful voltaic arrangement; but then two circumstances affect the result. The energy of the current depends much upon the facility with which it can pass from the negative metal to the fluid in contact; and it has been ascertained that this passage takes place to the acid from the iron, much more readily than from the copper. On the other hand, the copper zinc exerts always a stronger action at first than afterwards—stronger, indeed, sometimes than iron zinc; but then the intensity of its voltaic action causes decomposition of part of the zinc salt in solution, oxide is deposited upon the particles of copper, and, forming the crust before spoken of, diminishes very importantly its voltaic action. The same effect occurs with distilled zinc, furnished with platina wires.

From all these considerations, there appears to be no reason to doubt that the striking difference between pure zinc and zinc of commerce, when put into dilute sulphuric acid, is due to the presence of heterogeneous substances in the latter. By analysis, ordinary zinc is usually found to contain traces of copper, tin, lead, and rather more than a hundredth of iron; and, on extending the experiments with the zinc alloys, it was found that 2 per cent. of iron filings, added to distilled zinc, was sufficient to render it as active in acid as ordinary zinc.

The elevation of temperature resulting from the chemical action of the liquid upon these zincs, and which increases with the vivacity of the action, is very probably due to the heating power of these numerous electric currents. These currents are more powerful when most gas is disengaged; the heating power of the voltaic current is well

known, and, indeed, all circumstances accord in pointing out this as the principal source of the heat evolved.

A striking confirmation of the explanation now given of these exalted effects of common and alloyed zinc, is derived from an investigation of their power of forming voltaic combinations of more or less intensity. Being combined two and two, and examined by the galvanometer, it was found that the order was as follows: distilled zinc, lead zinc, tin zinc, iron zinc, zinc of commerce, and copper zinc: thus arranged, the most positive are first, or each is positive with those following it, negative with those preceding it. When combined into voltaic pairs with copper, distilled zinc, lead zinc, and tin zinc, were most powerful, then zinc of commerce, and iron zinc; copper zinc was last, and very inferior to the rest. It appears, therefore, that the kinds of zinc which exhibit least action in diluted sulphuric acid are those which form the most powerful voltaic combinations with such metals as copper, silver, platina, &c., and this might be expected: for the disengagement of hydrogen on the surfaces of the zincs does not arise from a direct chemical action, but from the action of the minute electric currents established between the molecules of the zinc and the heterogeneous metal present in it; whereas the current, sensible to the galvanometer, is produced by the direct action of the acid on the positive element of the pair of plates used. This *direct* action is stronger on the pure zinc than on the zinc mixed with less oxidable substances; and the less these heterogeneous substances are oxidable, the less positive should the zinc be *. This distinction will probably explain many apparent

* Is not the diminished power of the alloys in forming voltaic combination with more negative metals due rather to the circumstance of their finding the negative element ready for them, under more favourable circumstances, than that which, in the form of a copper plate or platina wire, is purposely added by the experimenter; than to any material diminution of the direct chemical action? The heterogeneous metal originally in the zinc forms a voltaic combination with it, having great extent of surface, because of its minute division, in excellent contact, and at the smallest possible distance; and therefore must divert the course of much of the electricity which in pure zinc finds its exit into the fluid only by the negative element purposely added.

We refer our readers to a similar effect to the above remarked by Messrs. Stodart and Faraday, in their paper on alloys of steel, and which they also referred to voltaic action. ' If two pieces, one of steel, and one steel alloyed with platina, be immersed in weak sulphuric acid, the alloy will be immediately acted upon with great rapidity, and the evolution of much gas, and will shortly be dissolved, whilst the steel will be scarcely at all affected. In this case it is hardly possible to compare the strength of the two actions. If the gas be collected from the alloy, and from the steel, for equal intervals of time, the first portion will surpass the second some hundreds of times. A very small quantity of platina alloyed with steel confers this property upon it; $\frac{1}{400}$ increased the action considerably; with $\frac{1}{200}$ and $\frac{1}{100}$ it was powerful; with 10 per cent. it acted, but not with much power; with 50 per cent. it was about equal to steel alone.' These alloys were very perfect; that which was most active in acids did not render a platina wire more negative than ordinary steel, and the cause, as was suggested at the time by Sir H. Davy, is referred to electrical action, the view taken being described at length in the paper in the Phil. Trans. for 1822, p. 262.

anomalies, and serves to show how difficult it is to judge of the true intensity of chemical action exerted upon a substance by liquids in contact with it *.

19. Crystallization of Bismuth.

M. Quesneville, *fils*, says, that by the following process, magnificent crystals of this metal may be obtained. Bismuth is to be fused in a crucible, fragments of nitre added from time to time, and the heat raised so as to decompose the nitre, and the whole mixed by agitation. Continuing the heat and the addition of nitre in this way for some hours, a time arrives when a little of the metal agitated in the air exhibits magnificent green and golden-yellow colours, which it retains when cold. If the metal displays only rose, violet, or indigo colours, and when cold is a white mass without colour, it is certain that good crystallization will not occur. When the metal is in right condition, it is to be poured into a ladle previously heated; and to prevent the surface cooling faster than the bottom, it should be covered, or a hot shovel held near it. The cooling should not be too slow, for then the metal crystallizes layer by layer, and offers no fine forms ; it is necessary that the cooling be rather sudden. When the upper crust has formed, it should be pierced by a hot coal, and not by percussion (which disturbs the crystals), and the remaining liquid metal decanted. In about half an hour longer the rest of the crust may be broken, and the interior will be found magnificently crystallized, the crystals being more beautiful as the above conditions have been more carefully followed †.

20. On discoloured Chloride of Silver.—(*M. Cavalier.*)

Chloride of silver blackened by sun-light is perfectly well known. M. Cavalier obtains it in a similar state by dissolving the recent chloride in ammonia, and passing chlorine gas into the solution; the usual decomposition of ammonia with elevation of temperature, evolution of azote, &c., takes place, and ultimately the liquid becomes turbid, and the chloride of silver appears first as a grey, and then, when the ammonia is entirely decomposed, as a violet precipitate.

This precipitate dissolves entirely in ammonia, and is precipitated in a perfectly white state by pure nitric acid. If 20 grains of it be decomposed by zinc in dilute sulphuric acid, it yields 15 grains of silver, exactly the quantity yielded by similar treatment from 20 grains of white chloride. Hence the difference of the chloride in these two states cannot be referred to difference of composition, but solely to some variation in molecular arrangement ‡.

* Bib. Univ. 1830, p. 391. † Jour. de Pharmacie, 1830, p. 554.
‡ Jour. de Pharmacie, 1830, p. 553.

21. Composition of Fulminating Gold.

M. Dumas has analysed the fulminating gold prepared by precipitating solution of chloride of gold by ammonia, the process adopted being that of burning it with oxide of copper. He found 100 parts to yield—

Metallic gold . .	73.00
Nitrogen . . .	9.88
Chlorine . . .	4.50
	87.38

by further experiment and reasoning, it was deduced that there were besides, 2.2 parts of hydrogen and 10.42 of oxygen. These elements are considered as being thus arranged:—gold 73; azote 5; ammonia 6; chlorine 4.5: water 11.5; and the proportions of the ultimate elements are given as 6 atoms of gold; 12 of azote; 2 of chlorine; 42 of hydrogen; and 9 of oxygen. It is finally viewed as a compound of 2 atoms of ammoniacal azoturet of gold, and 1 atom of ammoniacal subchloride of gold, with enough water to convert the azote into ammonia, and the gold into oxide of gold.

Oxide of gold digested in ammonia forms another fulminating compound. This compound analysed gave 2 atoms of gold; 4 of azote; 12 of hydrogen; 3 of oxygen *.

22. Whewell's written Nomenclature for Chemical Compounds.

Extract from Professor Whewell's Essay on Mineralogical Classification and Nomenclature:—

'Professor Whewell's mode of designating the combinations of chemical elements is different from that of Berzelius and of Beudant, but the alteration seems to be absolutely necessary. According to their method, the first combination of elements into binary compounds is indicated by writing the symbols together, without any connecting sign; as if they were algebraically multiplied: and the number of atoms of each element is denoted by figures, written as indices of powers generally are. Thus, $\ddot{C} + 2\ddot{c}$ they would represent by $\ddot{C}\ddot{c}^2$, and $3\ddot{C} + 2\dddot{S}$ by $\ddot{C}^3\dddot{S}^2$, &c. Now this notation is in the highest degree inconvenient, besides violating all symmetry and analogy.' For when the substance is indicated by $2\dddot{A}\dddot{S} + \ddot{C}^3\dddot{S}^2$, there is no longer any obvious identity with $2\dddot{A} + 3\ddot{C} + 4\dddot{S}$, which is the real result of the analysis.

* Ann. de Chimie, xliv. p. 167.

Substance.	Berzelius's Notation.	Whewell's Notation.
Phosphate of Lime	$\ddot{C}^3 \overset{:\cdot:}{P}^2$	$3\ddot{C} + 2\overset{:\cdot:}{P}$
Felspar . .	$\ddot{K}\dddot{S}^3 + 3\dddot{A}\dddot{S}^3$	$(\ddot{K} + 3\dddot{S}) + 3(\dddot{A} + 3\dddot{S})$
Alum	$\ddot{K}\dddot{S}^2 + 2\dddot{A}\dddot{S}^2 + 48\cdot Aq$	$2\cdot(\dddot{A} + 3\dddot{S}) + \ddot{K} + 2\dddot{S} + 48:Aq.$

Coefficients are, in all cases, used instead of indices.

23. Para-tartaric Acid.

M. Dulong read to the academy a letter from M. Berzelius, relative to numerous chemical compounds, which being similar in the nature and proportion of their elements, yet differ in property from each other. M. Berzelius had been particularly engaged with the acid found in tartar by M. Gay Lussac, which has been called Vosges acid (Thannic acid.) He shows that this acid, though differing from ordinary tartaric acid in many properties, has exactly the same composition.

Similar difference in properties without difference of composition is found in phosphoric and pyro-phosphoric acid; in stannic acid or deutoxide of tin, obtained from tin by nitric acid, or obtained from Libavius liquor by precipitation.

To associate and yet distinguish substances under these peculiar circumstances, M. Berzelius proposes to prefix the Greek term *para* to the name of that body which occurs most rarely, or which is obtained with the most difficulty, thus:—Phosphoric acid, and para-phosphoric acid; tartaric acid, and para-tartaric acid; stannic acid, and para-stannic acid, &c.*

24. Preparation of Piperin, by Mr. Clemson.

The pepper should be ground and digested in alcohol of specific gravity 0.832, or 0.817 at a smart distilling heat; an alembic, with its water bath, is at once convenient and economical; the whole should be agitated from time to time, and the fluid changed if necessary. I know of no better indication of the entire extraction of the piperin, than the want of taste in the marc or insoluble residue; although acridity (as has been represented) is by no means a property of piperin. The alcoholic solutions being united, should be reduced over a water bath. The distillation ended, there will be found in the bottom of the alembic a deposit composed of a great deal of piperin, and a black acrid resino-oleaginous substance; the separation of this latter compound from the piperin is difficult in the extreme; so much so, that I have seldom or never seen the preparation free from acridity, which not only destroys, but produces a contrary effect to that desired, when employed as a remedy. The greater part of this viscous oil may be separated by cold alcohol, piperin being much less soluble in

* Jour. de Pharmacie, 1830, p. 622.

this menstruum, when cold, than when warm, and much less than the oil. The latter portion may be entirely separated by the addition of a little lime to the warm solution of piperin with the oil, and leaving it to crystallize in the same vase; the piperin, when cold, may be separated at leisure: by re-dissolving the crystals thus procured, adding a little animal charcoal, and filtering when hot, a solution will be obtained, which, upon cooling, will afford crystals of a canary white, regular and free from acridity. Mr. Pontel has advised the use of caustic potash, and the effect is certainly very marked. The solution should be weak, for caustic potash has a tendency to alter the nature of the substance, and instead of procuring piperin, I once formed a compound that very much resembled soap, and all subsequent attempts to procure the substance in crystals failed; moreover, I have always observed, that those crystals obtained by the aid of potassa had more or less of a reddish tinge, and were very brittle. Piperin, when pure, crystallizes in right square prisms, occasionally presenting an anomaly, the crystals, particularly those obtained through the means of potassa, being hollow, or containing an interior decrement, the four vertical sides being entire, and showing the form of the crystal; they are insoluble in water, soluble in cold alcohol, and more so when warm; insoluble in acetic or other acids. Piperin has been employed latterly in Italy as a febrifuge*.

25. On Salicine by MM. Pelouze and Jules Gay Lussac.

Salicine, when pure, forms white crystalline prismatic needles. It has a bitter taste and somewhat of the odour of willow bark. One hundred parts of water dissolve 5.6 parts of salicine at 67° F.: at 212° F. it appears to dissolve in any proportion. It is equally soluble in alcohol, but ether and oil of turpentine take up no portion of it. Concentrated sulphuric acid gives it a fine red colour, like that of bichromate of potassa. Muriatic and nitric acids dissolve it without producing any colour. It is not precipitated from its solution by infusion of nut-galls, gelatine, neutral or subacetate of lead, alum, or emetic tartar. It does not saturate lime-water when boiled with it in excess: it does not dissolve oxide of lead: it fuses a little above 212° F., losing no water, and crystallizes upon cooling. If the heat be rather higher, it acquires a lemon-yellow colour, and becomes, when cold, brittle as resin.

Salicine, burnt by means of oxide of copper, yields a gas entirely absorbable by potash. The mean of two analyses gave the following as its composition:—

Carbon . .	55.491	= 2.028	proportions.
Hydrogen .	8.184	= 2.004	,,
Oxygen . .	36.325	= 1.000	,,

Its composition may, therefore, be represented by two volumes of olefiant gas, and one volume of oxygen †.

* Silliman's Jour. xviii. p. 253. † Ann. de Chimie, xliv. p. 220.

26. Preparation of Salicene.

The following is the process recommended for this purpose by M. Peschier. The bark of the willow is to be dried, crushed, boiled for one or two hours in water, and the liquid separated by a cloth and powerful pressure. Subacetate of lead is to be added as long as precipitation occurs; the whole filtered; the clear liquor boiled with enough carbonate of lime to decompose the excess of acetate of lead, saturate the acetic acid, and remove the colour. Being left to settle, the clear liquor is to be decanted, the deposit washed twice or thrice, the washing liquor added to the former, and the whole evaporated to the consistence of an extract. This extract, whilst hot, is to be put into bibulous paper, and pressed for some hours; after which it is to be digested in alcohol of s. g. 0.847, the fluid filtered and concentrated, when it will yield crystallized salicene, very white and pure.

Salicene thus obtained, when administered in doses, of from 15 to 18 grains, in the intervals of intermitting fevers, was found perfectly effectual in stopping their progress*.

27. New Kind of Indigo.

The *Registro Mercantil* of Manilla describes a new kind of indigo lately discovered in that island. This plant has been long known to the natives, especially in the provinces of Caramini and D'Albay; they gave it the name of *payanguit* or *avanguit*, and obtain a superb blue colour from it. In 1827 it attracted the attention of Père Mata, one of the members of the Economical Society of Samar. He made many experiments upon it, formed it into cakes, and dyed cotton, linen, and silk goods with it. The colour he obtained was so rich, and so equal to that of indigo, that he sent some of the cakes and the dyed fabrics to the Society, who directed other members residing in the same province to repeat Père Mata's experiments. All obtained most satisfactory results, and they sent many of the cakes, the leaves, and even the living plants, to Manilla. A committee of merchants and chemists was appointed to ascertain, by every kind of trial, whether the colouring matter was identical with that of indigo, and might be introduced as such into the market at the same price. The committee reported in the affirmative on these points, declaring that the *payanguit* had all the valuable properties of the plant to which it had been compared†.

28. Charring of Wood at Low Temperatures.

Mr. Phillips has described the following case of the slow decomposition of wood at low temperatures:—

Mr. Charles May, chemist, of Ampthill, has sent me some specimens of wood, converted into nearly perfect charcoal at a very low

* Ann. de Chim. xliv. p. 418. † Bib. Univ. 1830, p. 223.

but long-continued heat. The pieces, he informs me, are part of the bottom of a tub which held about 130 gallons, and which had been in use in his laboratory about three years and a half, and almost constantly worked for boiling a weak solution of common salt, generally with an open steam-pipe, and sometimes, though rarely, with a coil: the temperature was seldom higher than 216° or 220°, and the vessel was lined with tin, rolled into sheets, about the sixteenth of an inch thick, and nailed to the inside; the joints, however, were not so good as to prevent the liquid from getting between the metal and the wood. Mr. May states also that he had long since remarked, that on making extracts with steam of very moderate pressure, all the apparent effects of burning might be produced, but that he was not prepared to find so complete a carbonization of wood by steam: the vessel was made partly of fir and partly of ash, the former of which was most perfectly reduced to the state of charcoal*.

29. Change of Colour in the Wood of certain Trees.

M. Marcet has experimented upon this point, particularly with the wood of the alder, which, exposed to air, becomes red or brown. The change did not take place if, the instant the wood was cut, it was introduced into a perfect vacuum, or into gases containing no oxygen; but, on the contrary, being put into oxygen, the red colour became more vivid than in the air. If the wood, when cut, was plunged into water, it always reddened, whatever attempts were made to exclude oxygen. Some of the wood, which had acquired a yellow colour, communicated that colour to water, and the water, being evaporated, left a substance having every character of pure tannin. M. Marcet concludes, from his experiments, that the colouration of the alder wood is always due to a degree of oxygenation which the tannin undergoes immediately upon its exposure to the air or oxygen†.

30. Preservation of Blood.

Sugar refiners and others are often inconvenienced by the difficulty of obtaining blood at the time when it is required for use. M. Toursel has endeavoured, in part, to remove this difficulty, by proposing a method of preserving this agent for some time without injury. It consists in putting the blood into bottles or other vessels with very narrow mouths, and being careful to fill them up to the neck; a layer of oil, to the depth of at least half an inch, is then put upon it to cut off communication with the atmosphere, and the whole is left to itself. M. Toursel states that he has in this manner preserved blood, with all its physical and chemical qualities, from the 1st of December, 1827, to January, 1829‡.

* Phil. Mag. N.S. viii. p. 383. † Bib. Univ. 1830, p. 228.
‡ Journ. de Commerce.

31. Presence of Manganese in the Blood.—(*Professor Wurzer, of Marburg.*)

In some analyses of human blood, according to Engelhart's method, by liquid tests, Prof. W. was led to suspect that, besides the usual results, he had also obtained a small quantity of manganese : not being, however, quite sure of the correctness of his analyses, he was induced to repeat them in the following manner :—The blood, which had been obtained by venesection, on the day before the experiment, was ignited in an open crucible, the incinerated mass oxidized by nitre, and then diluted with water ; the residuum was dissolved in muriatic acid, and the iron precipitated from the solution by succinate of ammonia. As the precipitate contained also some phosphate of lime, it was again ignited, and then dissolved in muriatic acid : the phosphate of lime was separated from the solution by alcohol, the excess of the latter expelled by heat, and the iron precipitated by ammonia. By boiling the filtered liquid with carbonate of soda, the manganese was precipitated, and then dissolved in nitric acid and again ignited. In two grammes of the coal was found 0.108 ox. of iron, and 0.034 protox. of manganese*.

32. On two Ores of Tellurium from the Altai Mountains.—(*M. Rose.*)

During the journey through Russia and Siberia, which M. Rose, of Berlin, lately made in the company of MM. Humboldt and Ehrenberg, he found two ores of tellurium in the silver mines of Sawodinski, near those of Siranowski, at the river Buchtharma, and as this metal has hitherto been only found in the gold mines of Transylvania and in Norway, this discovery is of the greatest interest. We extract the description of tellurium-silver and tellurium-lead as it is given by M. Rose in *Poggendorf's Annalen.*

He first saw these two ores in the Museum of the town of Barnoul, near the river Ob ; besides numerous smaller pieces, there were two large blocks of about a cubic foot each, which, on account of their malleability and the large quantity of silver they contained, were considered to be silver-glass, from which they, however, were found to differ greatly. *Tellurium-silver* is of granular texture, not crystallized nor cleavable ; has much metallic lustre, and its colour is between that of lead and steel : it is malleable, though to a less degree than silver-glass ; and its specific gravity was found, by two different experiments, to be 8.565 and 8.412. The specimens which were examined by M. Rose were adhering to greenish-grey talc slate, and the ore was mixed with black blende, small quantities of sulphate of iron and of copper, and tellurium-lead.

When tellurium-silver was heated before the blowpipe on charcoal, it fused to a black mass, which, on cooling, became covered with

* Poggendorff's Ann. der Physik und Chemie.

numerous white points and ramifications of metallic silver. It fused also in open and closed vessels; and, when ignited in a retort, tinged the glass with which it was in contact of a yellowish colour: in the open tube it deposited a small quantity of white sublimate, part of which was volatilized by directing the flame upon it, the rest contracting into small globules.

It was found to dissolve in nitric acid, particularly when heated, but much less in nitro-muriatic acid, being soon covered by a crust of chloride of silver. If the solution in nitric acid was suffered to cool, small brilliant crystals were deposited, which consisted of the oxides of tellurium and silver, but in a different proportion from what they are in tellurium-silver, for, a short time after their formation, crystallized nitrate of silver was deposited.

M. Rose submitted the mineral to the following analysis:—It was dissolved in nitric acid, and after the silver had been precipitated by muriatic acid, the solution was filtered and evaporated; sufficient quantity of muriatic acid was now added until all nitric acid was decomposed, and no smell of chlorine could be perceived. The liquid was then diluted with water and heated, and on the addition of muriatic acid and sulphite of ammonia, a black precipitate was obtained, which consisted of metallic tellurium. The remaining fluid, being filtered, was again submitted to the action of sulphite of ammonia and muriatic acid, and this was repeated as long as a precipitate formed. A current of chlorine gas was then passed through the filtered liquid, in order to oxidize completely the small quantity of iron contained in it, and this was afterwards precipitated by ammonia.

By this process M. Rose obtained from 2.833 gramm. of the mineral, 2.348 gr. of chloride of silver, which contain 1.769 gr. of silver, 1.047 gr. of tellurium, and .010 gr. of oxide of iron. By a second analysis, 2.678 gr. of the mineral were found to consist of 1.669 of silver, 0.988 of tellurium, and .050 of iron.

According to the first analysis, tellurium-silver consists of

Silver . . .	62.42
Tellurium . .	36.96
Iron . . .	0.24

According to the second, of

Silver . . .	62.32
Tellurium . .	36.89
Iron . . .	0.50

And if tellurium-silver be considered as a compound of one atom of silver = 62.63, and one of tellurium = 37.37, the above results are nearly confirmed*.

The other mineral, *tellurium-lead*, is, like the former, not crystallized, but cleavable in three directions; the planes of cleavage are not quite even, but seem to be at right angles to one another. Its colour is tin white, almost like antimony, but a little more yellow;

* According to Berzelius, the atomic weight of silver is 1351,005, and that of tellurium 806,45; oxygen being 100.

it has much metallic lustre, is brittle, and of the hardness of fluor spar: spec. gr. = 8.159. It is mixed with small proportions of tellurium-silver, and before the blowpipe on charcoal, fuses to a small button, which gradually diminishes in size so as ultimately to exhibit a small globule of silver, surrounded by a ring of metallic hue, which seems to be formed by the volatilized and subsequently precipitated tellurium-lead. If the flame is directed upon it, it is completely volatilized, the flame becoming at the same time of a blue colour. It fuses also in a retort, and forms a small quantity of white sublimate, which, under the action of strong heat, contracts into small globules. If ignited in an open tube, it fuses and becomes surrounded by a ring of white drops, and at the lower portion of the tube a very dense white sublimate is deposited, which before the flame of the blowpipe contracts into small drops.

When powdered, it dissolved in nitric acid with the evolution of red vapours: the solution was diluted with water, and the silver contained in it precipitated by muriatic acid; the fluid was then filtered, and the lead precipitated by hydro-sulphuretted ammonia; after twenty-four hours the fluid was again filtered, the sulphuretted tellurium precipitated from it by adding muriatic acid, and the sulphur ultimately separated from the metal by dissolving the sulphuret in nitro-muriatic acid, which precipitated the sulphur.

As one analysis only could be made of the mineral, M. Rose refrains from giving any decided opinion on its composition at present; he is, however, inclined to consider it as a compound of 1.28 of silver, 60.35 of lead, and 38.37 of tellurium.

33. Berzelius' Method of preparing Urea.

The following process for obtaining urea is recommended by Berzelius in his recent work on Chemistry, vol. vi., p. 420, of the Swedish original.

Recent urine is to be evaporated, and as much of the residuum as possible taken up by alcohol. The alcoholic solution is to be evaporated, and the yellow substance remaining, dissolved in a small quantity of water, and digested with a little animal charcoal, until it is rendered quite colourless. The liquid is to be filtered, heated to 50° Centigr. (122° F.), and as much oxalic acid added as is soluble at that temperature; on cooling, the oxalate of urea is deposited in colourless crystals. If, instead of 122°, the fluid is heated to 212°, the solution of oxalic acid becomes of a brown colour and unpleasant smell; and the crystals, of oxalate of urea, are of a red or reddish-brown colour, but become colourless on adding a small quantity of animal charcoal. On applying gentle heat, so as slowly to evaporate the fluid, the deposition of crystals continues, and their quantity may be further increased by adding a new quantity of oxalic acid as soon as the fluid becomes thick and loses its sour taste. After this process, all the crystals are collected, washed with ice-cold water, and then again dissolved in boiling water with a little animal charcoal; when filtered

and cooled, the oxalate of urea is deposited, from the solution, in white crystals. These crystals are to be dissolved in boiling water, and powdered carbonate of lime added until litmus paper is no longer reddened by the fluid; the precipitate, which consists of oxalate of lime, is to be separated by the filter, and on evaporating the remaining fluid, a white salt-like mass will be obtained, which is urea, containing, however, in most cases, some oxalate of potash, soda, or ammonia. The two first of these salts are derived either from the oxalic acid or from the urine, when, to free the alcohol completely from water, some potash or soda has been dissolved by it; the oxalate of ammonia comes from the ammoniacal salts in the urine which, at the beginning of the process, were dissolved by the alcohol. On boiling the crystallized mass with concentrated alcohol, the oxalates are precipitated.

The oxalate of urea has a sour taste, and forms dendritical crystals, which, on being heated, melt and boil, giving out carbonate of ammonia and cyanic acid; the oxalic acid being decomposed into carbonic acid and carbonic oxide. They are very soluble in hot, but much less in cold water; at 60° F. 100 parts of water dissolve only 4.37 parts of the salt; if oxalic acid is added to the solution, part of the dissolved urea is precipitated. Alcohol dissolves but very little of it; 100 parts, of spec. grav. 0.833 and at 60° F., dissolve only 1.6 parts. According to Berzelius' analysis, it consists of 37.436 oxalic acid and 62.564 of urea, the oxygen in the latter being to that of the former as two to three. It does not contain any water of crystallization*.

34. On the Distillation of Nitric Acid.—(*By E. Mitscherlich.*)

During the decomposition of nitre by sulphuric acid, there are some circumstances regarding the combination of the acid with the potash of the nitre, which have hitherto been but little attended to. Of the three compounds of sulphuric acid and potash with which we are acquainted, the sulphate and bisulphate only require our consideration with respect to the above process, the former of which is sufficiently known; the bisulphate contains twice as much acid as the sulphate; and water, the oxygen of which is to that of the acid as one to six; this water is very fixed, and is not even evolved during the fusion of the salt at 392° F., but only when the salt itself is decomposed; a property which the latter has in common with the sulphate of the protoxide of iron, and some other salts. It would accordingly, perhaps, be better to consider the bisulphate of potash as a compound of the hydrate of sulphuric acid and the sulphate of potash: it consists of 58.80 sulphuric acid, 34.61 potash, and 6.59 water.

If equal parts of the nitrate and the bisulphate of potash are distilled with half a part of water, until the emission of red vapours begins, which is the case at about 418 F.°, the water in the receiver will be found to contain not more than 1½ per cent. acid of the nitrate em-

* From Poggendorf's Annal. der Phys. und Chemie.

ployed; and it accordingly is evident that the bisulphate and the nitrate commence only to act on each other at that temperature. On increasing the heat, the retort becomes filled with red vapours; oxygen is evolved and nitrous acid distils over, and is dissolved by the aqueous nitric acid in the receiver. The emission of red vapours continues when the retort is red hot, and it appears, consequently, that even at so high a temperature a large quantity of the nitrate is left undecomposed by the bisulphate.

If the quantity of sulphuric acid employed be just sufficient to produce the sulphate, the temperature required for the distillation of the acid does not exceed 302° F.; after half the quantity of the acid in the nitrate has been distilled over, the residue consists of bisulphate and nitrate of potash, which, on increasing the temperature, act on each other in the manner above described—viz., oxygen and nitrous vapours are evolved, and the liquid in the receiver is coloured by nitrous acid. The quantity of water employed in the process is quite indifferent, and influences only the strength of the distilled acid, which, previous to increasing the heat above 302° F., is perfectly colourless. According to this process, that is to say, where the quantity of sulphuric acid is 48.41 to 100 of the nitrate, the quantity of nitric acid produced does not exceed six-sevenths of that previously contained in the nitrate.

Nearly the same result is obtained by distilling 100 parts of nitre with 72.6 of sulphuric acid; but in this, as well as in the last process, a very great heat is required to decompose the last proportions of nitre, part of the acid of which will, moreover, also be found to be lost. But if, with 100 parts of nitre, 96.8 parts of acid are used, so that the bisulphate of potash is formed, the process will be found to be far more profitable, for none of the acid is lost; distillation takes place very easily, and at a heat not exceeding 248° to 257° F.; the nitric acid obtained is of 1.512 gravity, which, by distillation, may be increased to 1.54. The former, which is colourless, contains 86.17; the latter is rather yellowish, and holds 88.82 per cent. of acid.

If water is added to the acid of 1.522, the boiling point of the liquid gradually rises; and, on distillation, first concentrated and then weak acid will be found to pass over. This continues, however, only until the quantity of water amounts to 44 per cent. of the acid, the specific gravity of which is then 1.40, and the boiling degree between 248° and 249° F.; if the quantity of water is still increased, the boiling point falls, and the order of the distillation is, as it were, contrary to what it was observed before—viz., first weak and then strong acid is obtained. This likewise takes place during the distillation of nitric acid from nitre; for if, with 100 parts of nitre and 96.8 of sulphuric acid, the quantity of water is not equal to 44 per cent. of the acid formed, the first produce of distillation is strong, and the next diluted acid; if more water is employed, the contrary takes place.

It is, accordingly, most advantageous to use 100 parts of nitre, 96.8 of sulphuric acid, and about 40.45 of water, which will be sufficient, as

the nitrate of potash always contains some water, and the sulphuric acid is seldom so concentrated as to contain less than $18\frac{1}{2}$ per cent. The acid distils at 266° F., and its specific gravity is between 1.4 and 1.395. 28 lbs. of purified nitre, with $13\frac{9}{16}$ lbs. of sulphuric acid, of 1.85, yielded 34 lbs. of nitric acid, of 1.30 specific gravity; and the same quantity of nitre, with $27\frac{1}{8}$ lbs. of sulphuric acid, gave $37\frac{7}{8}$ lbs. of nitric acid, of 1.30*. Besides, the first process required almost twice as much fuel and much more time than the second.

In conclusion, M. Mitscherlich mentions some remarkable properties of nitric acid, of 1.522 specific gravity. Iron, tin, and several other metals, may be put into, and even boiled in it, without the least effect; whilst zinc is immediately oxidized and dissolved†.

35. On a peculiar Property of Alloys.—(*By F. Rudberg*‡.)

In a course of experiments which I lately made on the specific heat of lead and tin and some of their alloys, I observed a very remarkable proportion attending the specific heat of these alloys, and was particularly struck when, on repeating the experiments on other metals, I found the same proportion also to take place in them; so that it might, perhaps, be considered as a general property of alloys.

The following apparatus was used in the experiments:—A cubic vessel of thin iron plate, eight inches in height, was placed in another, of such dimensions that the sides of the outer were everywhere two inches distant from the inner vessel; the intermediate space was filled with snow. The larger vessel could be closed with a cover, the lower surface of which was blackened, and the upper covered with snow. In the middle of the central vessel, the inner surface of which was also blackened, there was a very thin cup of tin-plate on a ring of platina, suspended by platina wire from the sides of the inner vessel. A cover of tin-plate was made exactly to fit the opening of the cup, and had a central opening for a cork, through which the tube of a Centigrade thermometer passed§; so that, if the cover was placed on the cup, the bulb of the thermometer was nearly in its middle. The external surface of both the cup and cover were blackened.

The cup having been put on the ring was filled with the metal, or alloy, whilst in a state of fusion; the cover, with the thermometer, which had been previously heated, was placed on it, the external cover also put on, and the time which the mass required for cooling, carefully watched. By comparing the different lengths of time which the metal or alloy requires to cool, every ten degrees before and after its becoming solid, with those which mercury requires for the

* According to Thénard, from 100 parts of nitre and $66\frac{2}{3}$ of sulphuric acid 40.8 of very strong nitric acid, and from the same quantity of nitre with 144 parts of sulphuric acid, 81.6 parts of nitric acid of the same strength were obtained. These results appear to M. Mitscherlich to be erroneous.

† Poggendorff's Annalen.

‡ From the Kongl. Svensk. Vetensk. Acad. Handlingar, 1829.

§ All the temperatures in this paper are expressed in the Centigrade scale.

same ten degrees, the specific heat of the metal or alloy may be easily determined; for as all external circumstances and the difference of temperature are the same, the loss of heat which mercury experiences will be, to that of the alloy, in the ratio of the different lengths of time; and as, by the experiments of MM. Dulong and Petit, the specific heat of mercury has been ascertained both for high and low temperatures, that of the alloy may accordingly be calculated.

Whilst determining, in this manner, the specific heat of lead and tin, and several of their alloys, at different temperatures, I found, in general, the thermometer to be stationary at two points—one of which was the same for all alloys of the same nature, whilst the other varied according to the proportion of the two metals. I then examined the alloys of other metals, and obtained a similar result, as will be seen from the annexed table, which exhibits the observations on the alloys of lead and tin, tin and bismuth, and tin and zinc. The metals were combined in their simple atomic proportions, as is indicated by the number affixed to the initial of each metal.

The first table contains the observations on lead and tin; the former of which becomes solid at 325° C., the latter at 228° C. In the alloys, the thermometer was stationary at 187° C.; and the length of time, between 190° and 180°, is accordingly much more considerable than that of any of the preceding or following ten degrees. Besides this fixed point, (as it might perhaps be called, with reference to the other variable one,) there are other retardations of the thermometer, according to the proportion of the respective metals; as, in L^3 T, between 290° and 180° of 1′ 36″, in L^2 T between 280° and 270°, of 1′6″, &c. On increasing the quantity of tin, the variable was observed to approach the fixed point, and in the alloy L T^3 they coincided; in L T^4 the thermometer was stationary for a few moments when at 190°, or just below, and then suddenly fell to 187°. In L T^6 the retardation became again more distinct, being of 3′ 5″ between 210° and 200°; and in L T^{12}, of 4′ 23″, between 220° and 210°.

It seems then that there exist, for all alloys of lead and tin, (except for L T^3,) two points at which the thermometer is stationary—the one which is fixed at 187°, the other being variable, according to the proportion of the metals, but always at a higher temperature the more distant the mixture is from the combination L T^3. The length of time during which the thermometer is stationary on the fixed point also decreases in the same ratio, until it becomes = 0; when the metals are simple, the fixed point is then the same with that of their congelation.

The second table gives the result of the observations on the alloys of tin and bismuth; the fixed point is 143°, the variable point evidently coincides with the fixed one in T^3 B^2; and, according to the different proportions of the alloy, is near to, or distant from, the temperature at which the simple metals become solid.

The third table contains the experiments on the alloys of tin and

Lengths of time in which the Alloys of *Lead* and *Tin* cool to ten degrees of the Centigrade Thermometer.

	Degrees of Thermom. centigrade.	Lead.	L^3 T	L^2 T	L T	L T^2	L T^3	L T^4	L T^6	L T^{12}	Tin.	Degrees of Thermom. centigrade.	
	350°											350°	
	340											340	
Lead	330	12″										330	
becomes	320	(2′ 51″)	11″	11″ +	11″	10″+	11″	11″	11″	11″		320	
solid at	310	16″	13	13	13−	13	13+	13−	13	13−		310	
325°	300	15	14	14+	13+	14	14	14	14+	13+		300	
	290	15	15	15	16−	15	15+	15	15	15+	14″	290	
	280	16+	(1′ 36″)	17	17	17	18−	17+	17	17	16	280	
	270	17−	58″	(1′ 6″)	19−	17+	18	18	18	18	17+	270	
	260	17.5	45	1′ 0″	19+	19	19	20	19+	18+	19	260	
	250	19−	40	50″	21	20+	20	21	21	21−	20	250	
	240	20	36	44	(1′ 7″)	22	23−	22	22	22−	22	240	
	230	22	33	41	57″+	24	24+	24	24−	23+	23	230	Tin
	220	23+	33	39−	51	28	25	25	25+	24	(9′ 20″)	220	becomes
	210	25	33	37+	49	30	28	28	28	(4′ 23″)	33″	210	solid at
	200	27	34	38	48	(1′ 2″)	31	30	(3′ 5″)	2′ 34	33	200	228?
Fixed point	190	31	36	40	49	1′ 0″	35−	34	3′ 1″	1′ 56	35	190	
at 187°	180	34	(1′ 48″)	(3′ 0)	(5′ 48″)	(9′ 25″)	(11′ 29″)	(11′ 51″)	(6′ 26″)	(3′ 49″)	38	180	
	170		45″−	40″	40″	44″	50″	41″−	41″	41	40	170	
	160		42	42	40	44+	54	42	42	43	43	160	
	150		50	49−	52	53	1′ 5″	53	52	49	50−	150	
	140		1′ 12″	1′ 27″	1′ 33″	1′ 23″	1′ 11″	1′ 5″	59	56	52	140	
	130		1′ 19″	1′ 10″	1′ 2″	1′ 8″	1′ 17″	1′ 4″	1′ 3″	1′ 1″		130	

Alloys of Zinc and Tin.

Deg. of Ther. cent.	Z T	Z T^2	Z T^3	Z T^4	Z T^6	Z T^{12}	Deg. of Ther. cent.
350							350
340							340
330	16″	13″+	13″−				330
320	(1′ 16″)	15	13+	13″	14″	12″+	320
310	50″	16+	15	13	14	14−	310
300	48	17	17	15	14+	15	300
290	47	20−	18	16	17	16	290
280	48	(1′ 1″)	22−	18	18−	17+	280
270	46	49″	21	22−	18+	20−	270
260	45	49	22	20	20	19+	260
250	47−	50	(58″)	22−	24−	22−	250
240	47	51	51″+	25	26	23	240
230	50	52	52+	48″)	26+	24+	230
220	50+	53	52−	51″	28	27−	220
210	54	54	55	52	30	(4′ 41″)	210
200	(9′ 11″	(11 45″)	(12′ 40″)	(12′ 39″)	(13′ 15″)	(8′ 8″)	200
190	1′ 32″	1′ 9″	53″	50″	49′	41″	190
180	54″	47″	50	44	46+	40+	180
170	52	48−	49	48	49	43−	170
160	54	50−	50−	46−	51	45	160
150	1′ 0″	55	54	55	59	41	150
140	1′ 4″	58−	59	57+	1′ 2″	53+	140

Fixed point at 204°, Tin probably becomes solid much above 400°.

Bismuth becomes solid at 264°, Tin at 228°, Fixed point at 143°.

Alloys of Tin and Bismuth.

Deg. of Ther. cent.	T^4 B	T^2 B	T^3 B^2	T B	T^2 B^3	T B^2	Deg. of Ther. cent.
350°							350°
340							340
330	9.5″					10″+	330
320	11−	11″+		11″+		11−	320
310	12	11+	11″−	12		12−	310
300	14	12+	12	14−		13+	300
290	14	15	14.5	15+		15−	290
280	17	15.5	15.5	16	16″	16	280
270	18	16.5	16	17+	18−	16.5	270
260	18.5	18−	18−	18.5	18	17.5	260
250	20.5	19.5	19+	19.5	21	20−	250
240	22−	21.5	20	20.5	21	20.5	240
230	23+	22+	22	22.5	22+	22.5	230
220	25−	24	24−	24.5	25−	24	220
210	28	26	25.5	26.5	25+	27	210
200	29+	28	27.5	28+	29	28+	200
190	(2′ 18″)	32+	31	32−	31	(2′ 19″)	190
180	2′ 38″	33	33−	34	35	2′ 17″	180
170	2′ 18″	37	35	36	(2′ 20″)	2′ 5″	170
160	1′ 57″	(1′ 55″)	39	40	2′ 18″	1′ 50″	160
150	1′ 50″	2′ 37″+	44−	(1′ 19″)	2′ 11″	1′ 47″	150
140	(7′ 2″)	(13′ 50)	(18′ 5″)	(19′ 4″)	(12′ 17″)	(20′ 40″)	140
130	1′ 11″	59″	1′ 3″	1′ 14″	2′ 14″	1′ 41″	130
120	1′ 10″	1′ 7″+	1′ 6″	1′ 5″		1′ 0″	120

zinc; in Z T^6 the two stationary points coincide at 204°; the relation of the variable point corresponds with that of the other alloys.

For the combinations of lead and bismuth, I found the fixed point to be 129°, and the coincidence of the two stationary points to take place at L^3 B^4; in L B the variable point is at 146°, and in L B^2 at 143°, but this latter observation proved to be the result of a remarkable accident; viz., when the thermometer was examined, the ball was found to be compressed to such an extent as to raise the quicksilver in the tube by six degrees; this had, no doubt, been caused by the great expansion which bismuth undergoes when becoming solid, and which is such as generally to break the thermometer when immersed in fused bismuth, and left in it till it is completely solid.

In the alloys of zinc and bismuth the fixed point was found to be at 251°; the proportion, at which the depression of the thermometer is regular, could not be ascertained, but I conceive it constitutes an alloy, in which the relative quantity of zinc is very small.

It seems to follow from these combined observations, that whatever the proportion of the two metals may be which are fused together, an alloy is always produced, which is represented by a simple atomic ratio (and which might perhaps be properly called the chemical alloy); if the metals are combined in this proportion, the temperature of the mass regularly decreases till it arrives at the fixed point, which, under such circumstances, coincides with that at which the mass becomes solid, and which is generally lower than that at which either of the two simple metals solidify; if, on the contrary, one of the metals is in excess, the thermometer is rendered stationary at some point above that at which the chemical alloy becomes solid; for, as that portion of the metal which is in excess becomes solid before the chemical alloy, the latter derives from it the heat which becomes free by the congelation of the former. This must of course take place at a degree which will be the higher in the ratio of the quantity of the metal in excess. Within more or less time after the solidification of the metal in excess, the chemical alloy becomes also solid, and causes the thermometer to be stationary, in consequence of its latent heat becoming free: the latter is the fixed, the former the variable stationary point. The correctness of this view is also proved by the known fact, that if the mass in fusion is allowed to cool, solidification does not take place simultaneously, but it always, in more or less time, becomes of a mortar-like consistence; whilst, if the metals are fused in the proportion of the chemical alloy, the mixture will be found to become solid simultaneously, and almost in a moment.

It appears that there are also ternary chemical alloys as well as binary ones; of the alloys of lead, tin, and bismuth, for instance, one of the points at which the thermometer is fixed is 98°; for the ternary alloys seem to have two fixed points, neither of which, nor the proportion of the metals in the chemical ternary alloys, I have yet been able to ascertain*.

* Poggendorf's Ann. der Physik und Chemie, 1830, p. 240.

36. On the Combination of Chloride of Gold with the Chloride of Potassium and Sodium.—(*Berzelius.*)

The analysis of the oxide of gold and the other compounds of this metal has led to such various results, as to render it impossible to form any certain conclusion with regard to the proportion of their elements. The recent investigations of Berzelius on the subject are accordingly of much interest, especially as they, in some degree, tend to confirm his views on the atomic weight of gold. We give an extract from his paper in the Kongl. Vetensk. Handl. of 1829.

It is known that Pelletier was led, by the results of his inquiry into the composition of the iodide of gold, to consider the atomic weight of gold different from that which had been adopted by Berzelius; but that the views of the Swedish philosopher were strengthened by the subsequent researches of Javal, and particularly by his analysis of the compound of the chlorides of gold and of potassium, which he found to consist of 24.26 of chloride of potassium, 68.64 of chloride of gold, and 7.10 of water, the gold being accordingly united to twice as much chlorine as the potassium. Figuier, who soon after Javal examined the same subject, discovered the compound of the chloride of gold and of sodium, the proportion of which he found to be 14.1 of chloride of sodium, 69.0 of chloride of gold, and 16.6 of water, and the chloride of gold to contain accordingly nearly three times as much chlorine as that of sodium.

'Dr. Thomson, who,' says Berzelius in the above paper, 'has lately undertaken to determine the atomic weights more accurately than others, was also led to the examination of that of gold*, the oxide of which he found to consist of one atom of gold and three atoms of oxygen, and the chloride of one atom of the oxide and two of muriatic acid. The analysis of the oxide corresponds with my own; that of the chloride is evidently erroneous, as may be seen from the decomposition of the salt by heat, where chlorine and oxygen gas are not formed in the proportion of 4 to 1, as would necessarily result from Dr. Thomson's analysis. The compound of the chloride of gold and of sodium is, according to the same chemist, composed of 14.85 of chloride of sodium, 49.51 of gold, 17.82 of chlorine, and 17.82 of water, which is also erroneous; for if it were correct, the third part of the gold would be precipitated as an oxide, during the preparation of the compound from chloride of sodium and (what Dr. Thomson considers as a muriate) of the oxide of gold; but, according to his own experiments, such a precipitate is not formed. He concludes, however, that I am wrong in supposing that hydracids decompose oxidized bases, and that the muriate of gold gives a striking proof of the incorrectness of my ideas, as the oxide in this salt contains a third more oxygen than could be united to the hydrogen of the acid.

'After this rapid sketch of the previous labours on the subject, I come to my own analyses, which were made in the presence of Mr. Johnson, a pupil of Dr. Thomson's.

* Transact. of the Royal Soc. of Edinb., vol. xi., p. 23.

'*Chloride of Gold and Potassium* crystallizes in square prisms and rhombo-hedrons; is of an orange colour, and very efflorescent in a dry atmosphere; at 212° F. they part with their water of crystallization, but without losing any chlorine; two grammes of the salt were found to have lost 0.2125 gr. of water. On bringing the remainder in contact with hydrogen gas at a gentle heat, the gold was reduced, and 0.501 gr. of chlorine was found to have united with the hydrogen: of the remainder, 0.3505 gr. of chloride of potassium was dissolved, and .0936 gr. of gold was the residuum. If the compound be considered as composed of one equivalent of chloride of potassium (equal to one atom of chlorine and one of potassium), and one of chloride of gold (equal to one atom of gold and three of chlorine), the result of the analysis will be found pretty nearly to correspond with that of calculation:—

	By Analysis.	By Calculation.
Chloride of Potassium	17.522	17.566
Gold	46.800	46.827
Chlorine	25.050	25.014
Water	10.625	10.593

'*Chloride of Gold and Sodium* crystallizes in orange-red prisms, and cannot be freed from its water of crystallization without losing its chlorine. On reducing it by hydrogen gas, the residue of 100 parts consisted of 14.466 of chloride of sodium, and 49.51 of gold. In order to determine the proportion of chlorine in the compound, 3.026 grammes of the crystallized salt were mixed with 6 gr. of efflorescent carbonate of soda, and heated in a platina vessel, until the compound was decomposed and the gold reduced, which, when separated from the soluble salt, was found to weigh 1.4978 gr.: it formed, therefore, 49.497 per cent. of the compound. The solution was saturated with nitric acid, and nitrate of silver added to it; the precipitate was 4.3347 gr. of chloride of silver, which corresponds to 35.54 of chlorine in 100 parts of the salt. Of this chlorine 8.835 must accordingly belong to 14.466 of the chloride of sodium, and the remainder 26.501 to the gold; and if the compound be considered as consisting of one equivalent of chloride of gold (equal to one atom of gold and three atoms of chlorine), one equivalent of chloride of sodium (equal to one atom of chlorine and one of sodium), and four equivalents of water, the results of analysis and calculation will correspond perfectly.

	By Analysis.		By Calculation.	
Chlorine	8835	14.466		14.68
Sodium	5631			
Chlorine	26505	76.002	26.575	76.32
Gold	49497		49.745	
Water		9.532		9.00*'

* Poggendorf's Ann. der Physik und Chemie, 1830, p. 597.

37. On Magnesium.—(*Justus Liebig.*)

The Annales de Chimie, of March, 1830, contain a paper by M. Bussy, on magnesium, which he obtained by the action of chloride of magnesium on potassium; the properties of this metal appeared to M. Liebig to be so very extraordinary, that he was induced to make some experiments on it.

M. Bussy's method of obtaining the chloride consists in passing chlorine gas over a mixture of magnesia and charcoal, whilst in a state of ignition; it may, however, also be obtained by evaporating equal parts of the muriates of ammonia and magnesia, and heating the dry residuum in a platina vessel, until the muriate of ammonia is completely expelled, and the mass becomes fused. The remainder is chloride of magnesium, and if left to cool, forms white transparent leaf-like crystals.

In order to reduce the chloride of magnesium, from about 10 to 20 small globules of potassium are put into a glass tube, three or four lines in diameter, the chloride is placed over them, and heated over charcoal, until it begins to flow; the tube is then slightly inclined, so that the potassium runs through the chloride, which is thus reduced to magnesium with the evolution of light. If the mass, when cold, be treated with water, a large quantity of small metallic globules will be collected at the bottom of the vessel; they are of a silver-white colour, have much metallic lustre, and, though malleable, are very hard; neither cold nor hot water acts on them. If mixed with chloride of potassium, and heated in a crucible, they may be fused into one mass, and their point of fusion does not apparently exceed that of silver. The metal is dissolved by diluted acetic acid, as well as by sulphuric and nitric acids, with the evolution of hydrogen gas, and sulphurous and nitrous vapours: the solutions are found to contain no other oxide besides magnesia. When heated in atmospheric air or oxygen gas, the metal burns with the most vivid light; the vessel is covered with magnesia; and at the place where the metal was, a black spot remains, which seems to be silicium, as it was not destroyed by boiling hot acids. Sulphur did not seem to unite with the metal, when both were fused together. The solution in sulphuric acid yielded, on evaporation, crystals of sulphate of magnesia*.

38. On the Expansion of Bismuth and its Alloys during Congelation.—(*Professor Marx, of Brunswick.*)

Bismuth is known to be a very remarkable instance of apparent exception from the general rule, that fluids contract when becoming solid; and it corresponds with water in this respect also, that it communicates this property to other bodies, particularly metals, if it forms a certain proportion of the alloy. Where the maximum of

* Poggendorf's Annalen.

density lies, and in what degree the volume of the solid metal exceeds that of the fused, has, as far as we know, not yet been ascertained; but the former is probably very near the point of congelation; and of the latter, an approximate evaluation may, according to Professor Marx, be made in the following manner. If a quantity of bismuth be fused in an iron spoon or a glass tube, and then removed from the fire, the mass remains fluid for some time; it then congeals at the surface, but after the whole seems to be quite solid, all at once a large quantity of globular masses protrude from the surface, which are always proportional to the quantity of the metal employed, and may perhaps serve to determine the quantity of expansion; this was, according to several experiments of Professor Marx, found to be about $\frac{1}{53}$ of the weight of the whole, and consequently less than a third of the expansion of water. The force with which bismuth expands is so considerable, as to break glass tubes in which the fused metal is allowed to cool: thus, if a thermometer tube is plunged into fused bismuth, and then filled with it by sucking the metal up, it always breaks within a short time with a loud cracking, and in several directions, but mostly longitudinally, so as to form long parallel glass fibres. For the success of this experiment, it is, however, necessary to make the column of metal long enough, otherwise its longitudinal increase will cancel the expansion. The following were the alloys of bismuth, which Professor Marx examined:—

i. *Bismuth and Sodium.*—Four parts in volume of powdered bismuth, and one of sodium, were heated in an iron spoon. Long before the fusion of the bismuth, the sodium united with it, with the evolution of vivid light; the alloy was more fusible than bismuth, of a steel grey colour, and did not change by the contact of the air, until after some days, when its surface became covered with a black powder. If the alloy be fused, and then allowed to cool, the projections also formed, but to a much less degree than in pure bismuth; nor were the thermometer tubes burst, as in the above experiment. The alloy, with potassium, offered nearly the same results.

ii. *Bismuth and Arsenic.*—This alloy, consisting of three parts of the former, and one of the latter, did not seem to expand at all when becoming solid; on increasing the quantity of bismuth, the effects of expansion gradually became visible, and in the alloy B^{14} Ar^{1} were almost as great as in bismuth alone.

iii. *Bismuth and Antimony.*—It having been frequently remarked that antimony, like bismuth and water, expands on becoming solid, Professor Marx made several experiments in order to ascertain it, but without coming to any decided result; the alloy of both metals, in equal parts, exhibited the same phenomena as pure bismuth. This was also the case in the alloys B^{1} Ant.^{2}, and B^{1} and Ant.^{4}, though in a less degree.

iv. *Bismuth and Zinc.*—Zinc on becoming solid contracts so much, as to exhibit the contrary to what is observed in bismuth, the surface becomes depressed, and the wire in the thermometer tube often breaks into several pieces; the tube also bursts sometimes, but

always transversely, probably because it cannot follow the rapid contraction of the metal. Equal parts of zinc and bismuth fuse at a point below the fusion of bismuth; in cooling they separate, on account of the greater weight of the bismuth.

v. *Bismuth and Tin.*—Equal parts of both, present the same phenomena as pure bismuth.

vi. Equal parts of *Bismuth and Lead*, on the contrary, do not expand; and even if the quantity of bismuth is several times that of lead, there is but a slight increase in bulk. In B^8 L^1 the bismuth seems to have almost recovered its expanding force.

vii. *Bismuth, Tin, and Lead.*—The alloy B^2 T^1 L^1 is known for its great fusibility, the point of fusion being below 180° F. On becoming solid, the surface is rather depressed, and the mass seems accordingly to contract; and in most cases, however, the thermometer tubes burst longitudinally a long time after the mass has become solid. The tin seems, accordingly, under these circumstances, to overbalance the equalizing force of lead.

The following table shows the volume of this alloy, according to the experiments of M. Erman *.

Temperature.	Volume.	Temperature.	Volume.	Temperature.	Volume.
32 R.	1.007297	50 R.	0.992921	68 R.	0.996802
35	1.008364	53	0.992150	71	1.001057
38	1.007353	56	0.991337	74	1.008022
41	1.006390	59	0.992071	77	1.011576
44	1.001466	62	0.993640	80	1.017929
47	0.996196	65	0.994788		

The alloy B^2 L^1 T^1 did not exhibit the phenomena of expansion.

viii. *Bismuth and Copper.* If the quantity of bismuth is twice that of the copper, the expansion takes place a considerable time after congelation; but if the copper forms only the fifth part of the alloy, it is observed during, and immediately after, its becoming solid.

ix. *Bismuth and Mercury* does not seem to expand.

x. Equal parts of *Bismuth and Silver* do not increase in bulk; but if the bismuth is twice the quantity of silver, the expansion is very evident.

xi. *Phosphorus* could be made to unite with bismuth in small quantities only, and the expansive power of the metal was not altered by it; in the combination of *sulphur and bismuth*, however, the expansion seems to be considerably increased, almost to the fourth part of the mass. Professor Marx tried the combination of sulphur with several other metals, but without obtaining any similar result. This peculiarity of the mixture of bismuth with sulphur, and the known fact that fused sulphur at an increased heat becomes viscous, and then fluid again, led Professor Marx to make some experiments on sulphur alone, the result of which was, that contrary to bismuth

* 32° R. = 104° F. 80° R. = 212° F. 4° R. = 9° F.

and water, liquid sulphur contracts on becoming solid. He thought it, however, worth while to submit sulphur to another kind of examination, viz., by observing the different lengths of time which it requires to cool within certain limits, as he anticipated that in case the density varied according to the different degrees of liquidity, this would appear from the falling or rising of the thermometer. The sulphur was left to cool in the open air, the temperature of which was about 7½° R., and fell during the experiment 1½°. The time was observed during every five degrees by a very accurate watch, the minute of which was divided into 75 seconds. The following table gives the result of five series of experiments:—

150° R.	I.	II.	III.	IV.	V.
145	1′51″	1′38″		60″	67″
140	1′15″	1′4″		59	60
135	61	72		42	49
130	60	65		29	48
125	69	53		29	47
120	51	58		38	50
115	60	73	1′0″	56	45
110	57	69	70	65	55
105	69	69	73	61	47
100	63	71	72	67	49
95	70	74	1′1	74	1′12
90	1′2	1′7	1′3	1′4	1′3
85	67	1′14	1′9	1′4	1′1
Solid between	{86½°, 83°} 6′ 3″	{89°, 82½°} 15′ 3″	{87½°, 79°} 11′ 71″	{88⅘°, 80°}	{88¼°, 83°} 11′54″

The observations of I. and II. relate to sulphur which had not been melted before; those of III. were made on the sulphur of I., as were also the observations of V.; the sulphur of IV. was the same which had been used in II. From these experiments it would result—

1. That sulphur, after having been heated to 150° R., slowly expands, whilst cooling, to about 125°; the gradual decrease of the lengths of time appear, at least, to show that latent heat becomes free.

2. That from 125° downwards, there is a gradual contraction corresponding to the increase of the lengths of time.

3. That the degree at which sulphur becomes solid falls between 79° and 89°.

4. That during the congelation of sulphur, a greater quantity of heat becomes free than at the solidification of, perhaps, any other body*.

* Schweigger Seidel's Jahrbuch der Chemie und Physik.

§ 3. *NATURAL HISTORY.*

1. Formation of Hail.

M. de Perevoschtchikoff has endeavoured to support experimentally the objections made by Bellani against Volta's theory of hail, and to develope the influence of evaporation on the temperature of liquids. He used a thermometer with the tube bent, so that the ball was turned upwards, and the upper part of the ball was dished, so as to form a receptacle for fluid; in this way the temperature of any fluid evaporating from the part could be ascertained. From his experiments with water, he found that a prompt evaporation produced cold, even under the direct influence of the sun. From experiments with spirit of wine, he concluded that the temperature of an evaporating liquid could not rise, except when the evaporation was feeble; and he ultimately concludes, that the cause of the first formation of hail exists in a prompt evaporation of the vesicles constituting clouds. According to him, Volta lost sight of the principal cause of the cooling of clouds, and also of the concentric structure of hail-stones. The correct account of the phenomenon he conceives to be the following:—When the clouds consist of many thick strata, which gradually rise, they become an obstacle to the free distribution of the radiant heat from the earth, which being reflected back again, produces that suffocating sensation which usually precedes the storm. Above the clouds, however, the heavens are serene, and consequently radiation goes on freely from their upper surface. Hence the principal cause of the refrigeration upon which depends the formation of the nucleus of the hailstone. The specific gravity of these nuclei being too great to allow of their remaining suspended in the cloud, they fall; and traversing different strata of clouds, they become covered at each, by a fresh opaque coat of the liquid, congealed at their surface, the number of layers in the hailstone corresponding to the number of strata it has passed through. The hailstones, by concussion against each other, are supposed to have a rotatory motion given to them, tending to produce the spherical form. The author concludes that paragreles, or hailrods, are not only useless, but even dangerous*.

2. Georgia Meteor and Aerolite.

The following is a very circumstantial account of the descent of the stone which fell in May, 1829, at Forsyth, in Georgia, United States.

Between three and four o'clock on the 8th instant, on that day a small black cloud appeared south from Forsyth, from which two distinct explosions were heard, following in immediate succession, succeeded by a tremendous rumbling or whizzing noise passing through the air, which lasted, from the best account, from two to four minutes. This extraordinary noise was on the same evening accounted for by

* Bib. Univ. 1830, p. 410.

Mr. Sparks and Captain Postian, who happened to be near some negroes working in a field one mile south of this place, who discovered a large stone descending through the air, weighing, as was afterwards ascertained, thirty-six pounds. The stone was, in the course of the evening or very early the next morning, recovered from the spot where it fell. It had penetrated the earth two feet and a half. The outside wore the appearance as if it had been in a furnace; it was covered, about the thickness of a common knife-blade, with a black substance somewhat like lava that had been melted. On breaking the stone it had a strong sulphureous smell, and exhibited a metallic substance resembling silver. The stone, however, when broken, had a white appearance on the inside, with veins. By the application of steel it would produce fire. The facts, as related, can be supported by many individuals who heard the explosion and rumbling noise, and saw the stone*.

The following notice of the same event was given by Dr. Boykin, in June, 1830:—'No one can tell from what direction the meteor came. The first thing noticed was the report like that of a large piece of ordnance; some say the principal explosion was succeeded by a number of lesser ones in quick succession, similar to the explosions of a cracker; one has told me the secondary noise was only a reverberation. Very soon after the explosions some black people heard a whizzing noise, and on looking, saw a faint "*smoke*" descend to the ground, at which time they heard the noise produced by the fall of the stone: they ran to the spot, for they saw where it fell, and discovered the hole it had made in the ground, being more than two feet in a hard clay soil: the negroes, and others who went early to the spot, say they perceived a sulphureous smell. The stone weighed thirty-six pounds; it fell at a small angle with the horizon.

Dr. Silliman adds, that 'having received the specimens just as this number of the Journal is about being finished, I can add only the following notice. The colour of the interior of the stone is of a light ash-grey, and very uniform, except that it is sprinkled throughout with thousands of brilliant spots of metallic iron, having very nearly the colour and lustre of polished silver. The iron is rarely in points larger than a small pin's head, but the points are so numerous that nearly the whole of the powder of the stone is taken up by the magnet, even when it is in fine dust, and by a magnifier the little points of iron can even then be seen standing out from the magnet. It greatly resembles the Tennessee meteorite. It has the usual black crust on certain parts, and this, although resembling a semi-fused substance, exhibits bright metallic spots when a file is drawn across it. A similar black crust is seen pervading the stone in some places through its interior, and forming, where it is seen in a cross fracture, black lines or veins. The stone is full of semi-fused black points and ridges similar to the crust, and its entire mass seems half vitrified in points, so as to resemble an imperfect glass.'

The specific gravity, as ascertained by Mr. Shepard, is 3.37.†

* Elias Beall. † Silliman's Journal, xviii., p. 388.

3. On the Thermal Waters of Chaudes Aigues, in the Department du Cantal.—(*M. Chevalier.*)

The little village of Chaudes Aigues is situated to the south of St. Flour, on the border of a stream in a pleasant valley, surrounded by high mountains. Its mineral waters have long enjoyed some celebrity, but have fallen into medical disuse. At present establishments are forming for the reception of patients, and many circumstances combine to render the place agreeable and tempting, and so to favour the enterprise. The sources of the Par, which is the largest of all, yield 230 cubic metres and 4 decalitres every twenty-four hours; its temperature is at 80° C. (170° F.) It is this water which the inhabitants employ by means of ingeniously contrived conduits, which conduct it to the houses, to give warmth during the winter: in the summer they turn it away towards the river, that they may not be inconvenienced by its heat. This practice should be followed at other towns where there are sources of hot water, as at Plombières, Aix, &c. M. Berthier has calculated, that the water of the Par is equivalent, as a heating agent, to the wood which would be furnished by a forest of oaks 540 hectares (1334 acres) in area. The water of this spring is clear, limpid, and almost tasteless; it leaves a slight ochraceous film upon stones; it becomes spontaneously covered with a thin oily film, but may be retained a long time unaltered. It issues from massive sulphuret of iron, and its channels are obstructed by a deposit of the same substance.

The second spring is that of the mill of Ban. It flows over quartz, serving as the gangue for sulphuret of iron. This water is conducted to the hospital and several private houses, in the same manner, and for the same purpose, as the preceding water.

The third spring, that of the Grotto of the mill, is particular in this circumstance, that, though less hot than the others, it follows exactly the same changes of temperature. At its source it disengages carbonic acid mixed with oxygen and azote.

The Maison Felgère is in possession of four springs, one of which is at the temperature of 70° C. (158° F.) The water of the river, heated by all these streams, is said to be more favourable in exciting vegetation than other rivers.

These waters, besides being applied to heat apartments, are used also to cleanse wool, and M. Felgère has formed an establishment for the hatching of eggs, in imitation of that arranged by M. d'Arcet, at Vichey.

M. Chevalier has obtained, by chemical analysis, from 20 litres (1220 cubic inches) of the Par water:—i. A trace of hydrosulphuret of ammonia, which appears to be formed by the action of heat. ii. An organic animalized matter, which appears as flocculi, when the water is evaporated, and sometimes occurs united to carbonate of lime. iii. 18.86 grammes (291 grs.) of a light solid substance, more than half composed of subcarbonate of soda. These waters, by their heat

and purity, are very analogous to those of Plombières, a circumstance in favour of the formation of a similar establishment*.

4. Humboldt's Account of the Gold and Platina District of Russia.

The following account is part of a letter from M. Humboldt to M. Arago:—'We spent a month in visiting the gold mines of Borisovsk, the malachite mines of Goumeselevski and of Tagilsk, and the washings of gold and platinum. We were astonished at the *pepitas* (waterworn masses) of gold, from 2 to 3lbs., and even from 18 to 20lbs., found a few inches below the turf, where they had lain unknown for ages. The position and probable origin of these alluvia, mixed generally with fragments of greenstone, chlorite slate, and serpentine, was one of the principal objects of this journey. The gold annually procured from the washings amounts to 6000 kil. The discoveries beyond 59° and 60° latitude become very important. We possess the teeth of fossil elephants enveloped in these alluvia of auriferous sand. Their formation, consequent on local irruptions and on levellings, is, perhaps, even posterior to the destruction of the large animals. The amber and the lignites, which we discovered on the eastern side of the Ural, are decidedly more ancient. With the auriferous sand are found grains of cinnabar, native copper, ceylanites, garnets, little white zircons as brilliant as diamonds, anatase, alvite, &c. It is very remarkable, that in the middle and northern parts of the Ural, the platinum is found in abundance only on the western European side. The rich gold-washings of the Demidov family, at Nijneï-tagilsk, are on the Asiatic side, on the two acclivities of the Bartiraya, where the alluvium of Vilkni alone has already produced more than 2800lbs. of gold.

The platinum is found about a league to the east of the line of the separation of waters (which must not be confounded with the axis of the high summits), on the European side, near the course of the Oulka, at Sukoi Visnin, and at Martian. M. Schvetsov, who had the good fortune to study under Berthier, and whose learning and activity have been most useful during our travels in the Ural, discovered chromate of iron, containing grains of platinum, which an able chemist at Catherineburgh, M. Helm, has analyzed. The washings of platinum at Nijneï-Tagilsk are so rich, that 100 *puds* (about 400 lbs. Russian) of sand afford 30 (sometimes 50) *solotniks* of platinum, whilst the rich alluvia of gold at Vilkni, and other gold washings on the Asiatic side, do not give more than $1\frac{1}{2}$ to 2 solotniks in 100 puds of sand. In South America, a very low chain of the Cordilleras, that of Cali, also separates the auriferous and non-platiniferous sands of the eastern declivity (Popayan), from the sands of the isthmus of the Raspadura of Choco, which are very rich in platinum as well as gold. M. Bousingault may, perhaps, already have thrown a new

* Bib. Univ. 1830, p. 220.

light on this American formation, and his observations will derive some additional interest from those which we have made in this place. We possess pepitas of platinum, of many inches in length, in which M. Rose has discovered beautiful groups of crystals of the metal.

'As to the greenstone porphyry of Laya, in which M. Engelhardt has observed little grains of platinum, we have examined it on the spot with much care, but the only metallic grains which we have been able to detect in the rocks of Laya, and in the greenstone of Mount Belayr-Gora, have appeared to M. Rose to be sulphuret of iron; this phenomenon will be a subject for new research. The work of M. Engelhardt on the Ural seemed to us to be worthy of much praise. Osmium and iridium have also a particular locality, not amongst the rich platiniferous alluvia of Nijneï-Tagilsk, but near Belemboyevski and Kichtem. I insist upon the geognostical characters drawn from the metals which accompany the grains of platinum at Choco, Brazil, and in the Ural.'*

5. Parrot's Expedition to Ararat.

A scientific expedition set out from Dorpat some time since, under the direction of Dr. Parrot, charged with the examination of the country around Mount Ararat. After many fruitless attempts, Dr. Parrot arrived at the summit of Ararat, and measured the height of this celebrated mountain. He found it to be 16,200 feet in elevation, which makes it 1500 feet higher than Mont Blanc. Dr. Parrot caused a barometric levelling to be taken by M. Behaghel, one of his companions, of the whole route from Tiflis to Ararat, as well as of that which leads from this city, by Imerethi and Mingrelia, to the Kalch redoubt on the banks of the Black Sea; but his observations are not yet calculated. This traveller describes the western summit, which is the most elevated part of Ararat, as being a plain of about 150 paces in circumference; eastwards it communicates by a low plateau with the other summit, which is not so high; at about 1200 feet of elevation everything is covered with ice and snow. The instruments which Dr. Parrot had with him, consisted of a pendulum apparatus, a magnetic *inclinatorium* of ten inches, barometers, a surveying apparatus, &c. In point of astronomical instruments, the expedition was provided with a Reichenbach's theodolite of eight inches, an Arnold's chronometer and one of Maynie's, a Dollond telescope of three feet, and a Trongleton's sextant.

Dr. Parrot was accompanied, as we before mentioned, by MM. Behaghel, a mineralogist, Schiemann, a zoologist, and Hehn, a botanist—all three students in the university of Dorpat†.

6. Cuticular pores of Plants.

It is well known to botanists that the cuticle of most plants is furnished, especially on the leaves, with minute organs, the func-

* Edin. Geog. Journ. ii. 441.

† Ibid. iii. 38.

tion of which is a matter of conjecture, and the actual structure of which has given rise to much difference of opinion. These organs have received the names of pores, or glands, or stomata, according to the views of different observers; and while one class of botanists has considered them of unknown function and structure, others have contended that they are of the nature of pores, and that their office was, according to the one, to facilitate evaporation—to the others, to assist in the process of respiration. Their function is obviously of so obscure a nature, that no direct experiments are likely to demonstrate exactly what it is; but their structure is a point upon which observation may be expected to cast some light. Mr. Bauer long ago represented these organs in the wheat, as perforations opening into a minute subcutaneous cavity, and as destined to afford a direct passage into the interior of a plant for those minute fungi, whose ravages are so well known in the form of what the farmers call the mildew in corn. Other observers have, however, doubted whether the supposed perforations always existed; and Mr. Lindley, in his lectures in the University of London, has repeatedly expressed his difficulties upon the subject. The fact is, that they are so minute, the tissue of which they consist is so exceedingly transparent, and it is so difficult to examine them, except by the aid of transmitted light, that it is not, perhaps, possible to determine positively in all cases whether a perforation exists or not. Mr. Robert Brown has recently published some observations upon them, from which it is to be collected that, in the opinion of that distinguished observer, the stomata are rather of the nature of glands than of pores, and are undoubtedly in many cases imperforate—evidently having in their disc a membrane which is more or less transparent, sometimes opaque, or very rarely coloured. The existence of colouring matter in the stomata is the only circumstance that could have enabled an observer to prove their imperforate nature; for, in colourless membranes, such as those of Crinum, in which the stomata are particularly large, the best microscopes, employed under the most favourable circumstances, show nothing but an apparent orifice, closed up occasionally by the dilatation of two glandular bodies placed beneath it. Mr. Brown states, what was certainly a very unexpected fact, that these bodies will often, in proteaceous plants, by their figure and position, or magnitude, with respect to the meshes of the cuticle, determine the limits or even affinities of genera, or natural sections.

7. Smut in Corn.

This substance, which has been sometimes considered a mere organic disease, but more usually a parasitical plant, analogous to that which causes the mildew and the rust, and which has been described under the names of Reticularia segetum, Uredo segetum, and Uredo carbo, has been lately the subject of a particular inquiry on the part of M. Adolphe Brongniart, who thus describes the parts in which this malady is found, and who adopts the opinion that it

is caused by the ravages of a kind of fungus. 'The axis which supports the glumes and floral organs of grasses, is formed of elongated cellular tissue, the cellules of which are placed close together, without sensible intercellular passages, and of fibro-vascular bundles of false tracheæ or ducts, and spiral vessels; in the fleshy mass, of which the smut consists, no structure of this sort is visible, at whatever time it is examined; but, for examining it satisfactorily, I have taken the plant at the earliest period when the spike is capable of being examined. At this time the fleshy mass is found to consist entirely of an uniform tissue, containing uniform four-sided cavities, separated by partitions formed of one or two layers of very minute cellules. These cavities, which, in organization, resemble the regular lacunæ observable in the cells of aquatic plants, are filled by a compact homogeneous mass, composed of very small granules, perfectly spherical and uniform in size; they were slightly adherent to each other, and of a greenish colour in spikes but little developed—distinct, or simply clustered towards the centre of each mass, and of a pale nut colour, in spikes which were a little developed: finally, at a more advanced period, the cellular partitions disappear, the globules separate completely, and the whole mass is transformed into a cluster of powder, formed of very regular globules perfectly alike, black, and quite analogous to the reproductive bodies of other fungi.'

8. Structure of Leaves.

A memoir, by M. Adolphe Brongniart, upon the structure of leaves, and on their relation with the respiration of vegetables in air and water, has been read before the Academy of Sciences of Paris. The author states that the leaves of plants that live in the air have a totally different structure from those that are completely submerged, and that this difference in the structure of organs is in direct relation to the two principal functions of leaves, respiration and transpiration. In leaves exposed to air, the surface of the leaf is covered by an epidermis of uncertain thickness, formed of one or more layers of colourless cellules, closely packed together. This membrane is pierced with the pores usually known by the name of stomata. The doubts that have been entertained upon the existence of perforations in these stomata, M. Brongniart thinks he has removed, and that it is certain that in the centre of each stoma is an opening by which the outer air communicates with the parenchyma. This parenchyma is evidently the seat of respiration; for it is the part that changes colour in exercising this function, which becomes green by the absorption of the carbon of the carbonic acid of the atmosphere, and which is discoloured again in darkness by the combination of the carbon of its juices with the oxygen of the air. This parenchyma differs entirely from that of other organs by the numerous irregular cavities that it contains, which communicate with each other and the outer air by means of the openings of the

stomata. It is into these cavities in the cavernous parenchyma of aërial leaves that the atmospheric air penetrates when it is absorbed by the surface of the utricles of the parenchyma, that are distended with the fluids which seem to nourish the plant.

According to M. Brongniart, aquatic leaves, if submerged, differ, in being completely destitute of epidermis. It is not alone stomata that they want, as has long been known, but the epidermis also. There are none of the cavities that abound in the parenchyma of aërial leaves, but, on the contrary, the cellules of the tissue are compactly fastened together without any interstice, and the air dissolved in the water can only act on their outer surface. For this reason the proportion borne by this surface to the whole mass of the leaf is unusually great; the leaves, from want of epidermis, dry up quickly when exposed to the air, and can only exist in water or a very humid atmosphere.

Hence the author concludes that the epidermis is destined to protect aërial leaves against too rapid evaporation, and the stomata or pores of this epidermis become necessary to maintain a communication between the atmosphere and the parenchyma.

9. Crystals of Oxalade of Lime in Plants.

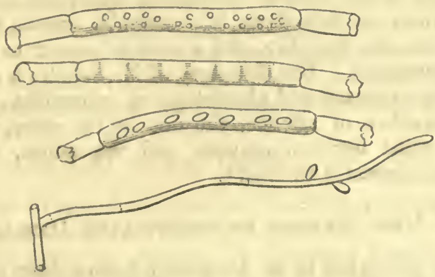
M. Turpin has discovered that the cellules of Cereus Peruvianus contain an immense quantity of crystals of oxalate of lime. He represents them as appearing to the naked eye like very fine glittering sand, and, under the microscope, as rectangular prisms, with tetraedral points and a square or parallelogrammic base: their size is variable; they are sometimes found collected in groups of three and four, but more commonly forming radiating spheroidal clusters, composed of crystals of various sizes. They existed in such abundance in some parts of the tissue as to form at least an 80th of the whole mass. The presence of such crystals in the tissue of plants has lately become well known to botanists, and are distinguished by the name of raphides. They may be found abundantly, in the form of needles, in the common Hyacinth, and in most succulent Monocotyledons, and in Phytolana decandria they give a kind of silvery appearance to the subcuticular tissue; but in no plants had they been previously seen so abundant or so large as in the plant which forms the subject of M. Turpin's memoir.

10. Growth of Vegetables.

There is no subject in vegetable physiology more obscure than the manner in which plants increase in size. While botanists are at issue as to such a point as the origin of the wood and the bark of dicotyledonous trees, it is scarcely to be expected that they should agree upon the mode in which development is effected. In truth, nothing whatever certain is known upon the subject.

Lately, M. Amici, the celebrated Modenese professor, has published some observations which he hopes may throw light on the inquiry. It is well known that, in the spring, the sap of the vine exudes copiously if the plant is ever so slightly wounded, and that the discharge which, in consequence of its limpidity, is fancifully called the 'tears' of the vine, becomes, after a short exposure to the air, of a rusty brown. M. Amici states that he found this substance, when examined under the microscope, to consist of long interlaced filaments, which were generally simple, but sometimes subdivided into two or three bifurcations. These filaments, or tubes, consisted of numerous joints separated by diaphragms, and, while some of the cells were filled only with air, others contained little moveable granules. Upon examining the vine sap in its limpid state, it was found to be entirely destitute of any trace of organization, but it was seen that the filamentous matter made its appearance upon being exposed to the sun for six hours, twelve hours after having been collected. One of these filaments was seen to multiply its original volume 24 times in the space of ten hours, and to have at the same time given birth to two young buds. Wishing to follow the development of this vegetation still further, the same object was left eleven hours longer upon the field of the microscope; at the end of this period it had grown from 0.2375 of a millimetre in length to 2.25, and had ramified and subdivided like a tree, and presenting joints formed at intervals by diaphragms; some of these joints contained very small granules, which circulated completely in the cavity of the cellules and of the tubes. This organization is obviously that of a Conferva; but M. Amici justly remarks that its constant existence in the tears of the vine makes it improbable that it should be of such a nature; and, at all events, the fact is one highly deserving the attention of physiologists.

Fig. 1.
2.
3.
4.



Figs. 1, 2, and 3 are magnified 1500 times; Fig. 4, 500 times. Fig. 1 is one variety of the filaments found in the red mucilage; it includes two joints formed by diaphragms; in the part between them are the small granules, which circulate round the whole included space as in the Chara. Fig. 2 is another variety of tube with various compartments or vacuities between the joints; these

may be false tracheæ. Fig. 3 is a third variety, which occurs where the vacuities are smaller; these may be the elements of porous tubes. Fig. 4 is the case, described as seen under the microscope; at first the lateral shoot extended like a bud only to the first mark; at the end of one hour it had reached the second; at the end of the second hour it had attained the third mark, and at the end of the third hour to the full extent figured, and had produced the two small buds or commencements. When the growth of the tube is seen under a high power, it appears as if it were a viscid substance pushed from within by an elastic fluid, which extends its length, but not its breadth. By degrees, molecules, or small grains appear, in the vacuities formed, and these circulate from one extremity of the canal to the other*.

11. On Circulation in Vegetables.

On the 27th of September, MM. Henri Cassini and Mirbel made a report upon the vegeto-anatomical and physiological observations presented by Dr. Schultz to the Academy of Sciences. It appears that a circulation takes place in vegetables, comparable, in some respects, to that in animals. In fact, when the vessels in a portion of stem, an inch or two long and two or three lines in width, are considered, assent cannot be refused to the idea, that a vital juice exists, and that it passes several times by the same path. But there is this remarkable difference between the circulation in vegetables and in animals of a high order, that in the latter there is one point in which terminate two vascular systems very distinct from each other, one carrying the blood to the extremities of the body, the other collecting it and conducting it to its source; whilst in vegetables we discover no special point of departure, nor any double vascular system. Vessels of the same nature form a net-work, of which the meshes are so many similar circulating apparatus communicating with each other, so that there is a common motion through them whilst the parts live together, and a motion proper to each so soon as they are separated. The discovery of M. Schultz is of the highest interest for the anatomy and physiology of vegetables; it enlightens these two branches of science, the one by the other, and it proves relations to exist between animals and vegetables, which before were not even suspected to exist †.

12. New Method of multiplying Dahlias.

Some dahlias belonging to M. Jacquemin having been injured by the wind in the first days of June, and some branches broken off, he placed them in the ground, in hopes of developing the flower. This did not take place; the vegetation languished, but the plants appeared good, and being carefully taken up, were found furnished

* Ann. de Sciences Nat. xxi. p. 92.
† Rev. Ency. xlvii. p. 784.

with tubercles. Hence a new means of multiplying these flowers, and the illustration of a curious physiological fact *.

13. Seat of the Sense of Taste.

The following general experiments and conclusions are from a work on the seat of this sense by MM. Guyot and Admyrauld. i. If the anterior extremity of the tongue be inclosed in a very soft, flexible case of parchment, so as to cover it completely, jelly, and in general all bodies, may be introduced into the mouth, and crushed between the teeth without any taste being distinguishable. The same effect is obtained also by retaining the tongue apart from the cheeks or teeth; sapid objects placed beyond its action give no sensation of taste. The tongue, therefore, is the essential organ of taste; the lips, palate, cheeks, and gums have no power of this kind.

ii. Nevertheless, if the tongue be entirely covered, and very sapid substances be swallowed, a little taste is perceived at the posterior part of the *velum palatinum*. If the palatal arch be covered with parchment, a sapid body produces its ordinary effect upon the tongue. If a little piece of extract of aloes be fixed upon the end of a rod, and passed over the palate and the roof of the mouth, it produces no other sensation than that of touching; but on the anterior and upper part of the soft palate there is a small portion of surface, not having definite limits, where the impression of sapid bodies is very sensible; the back part of the mouth does not partake in this property, so that this small portion of the palatal vault with the tongue forms the organ of taste.

iii. If the tongue be covered with parchment, pierced at the middle of its back surface, sapid bodies applied to the part produce no taste, until, being dissolved in the saliva, they gain access to the edge of the tongue. Extract of aloes passed over various parts of the tongue produce sapid impressions within a space of only one or two lines at the sides, three or four at the point, and within a curved space at the back. Hence this part of the tongue and the lateral portions are the especial organs of taste in deglutition; the portion of the soft palate already mentioned prolongs the sensation †.

14. Remarkable Cas¬ of the Re-union of a divided Part.

In the Quebec Hospital Reports we find the following case:—A man in chopping wood cut off the first phalanx of the middle finger. For two hours after the accident he remained occupied at home. Although the divided portion of his finger then appeared to be deprived of vitality, it was determined to follow the plan of Balfour of Edinburgh, and to attempt to re-unite the parts. The tip of the finger was fixed to the stump by adhesive plaster, and in three days union had taken place in two or three parts; and the extremity of

* Jour. de Pharm. 1830. p. 760. † Bib. Univ. 1830, p. 215.

the finger which had been divided had as much sensation as any other part of the body. The dressing was continued, and in three more days the re-union was complete *.

15. Singular Effect of Opium.

M. Cavalier states that he had used an enema, consisting of two ounces of mucilage and a grain and a half of opium. He was seized with nausea, but no vomiting; but having removed the cover of the night-lamp, the appearance of the light produced vomiting, and this increased whenever he submitted to the action of light. He endeavours to explain this curious phenomenon, but leaves it as obscure as he found it †.

16. Mechanical Powers of a Spider.

The following description of the capabilities and power of a small species of spider, supposed to be the *Aranea extensa*, is given by the Rev. Mr. Turner, in the Transactions of the Northumberland Natural History Society: it was shown to him by Mr. Mackreth—' On calling upon him (Mr. Mackreth) the next morning, he brought out a tumbler glass, which he had inverted on the table over a sprig of Laurustinus bush, on which he had observed a very small spider. Supposing that it might want air, he had slipped under the edge of the glass a small roll of paper. In less than three days, the little animal had filled the interior of the glass with minute, almost invisible threads, by means of which it had raised the sprig into the middle of the glass; and, not content with this, had raised also the coil of paper which by some accident had slipped from under the edge. After this, it laid, upon one of the upper leaves, a large ball of eggs, and having thus completed the ultimate object of its existence, it died, and fell into the meshes of its own web.

' How this little artist should have accomplished the Herculean task of raising a weight several hundred times greater than itself, and for what purpose it should have done this, are questions which may well deserve consideration.

' From a comparison of the individual inquestion with the very few figured by Donovan, it appears to be most like the *Aranea extensa*, vol. viii. p. 48; and as it is there said to be always found upon trees, and never upon the ground, this may be the reason why it has executed the arduous task of raising the branch, on which it was confined, to the upper part of the glass‡.'

17. White Bait.

Mr. Yarrell has made several attempts to preserve white bait alive, of which the following are the results:—

* Baltimore Adviser. Med. Jour. 1830, p. 370. † Med. Surg. Jour. v. p. 335.
‡ vol. i. p. 42.

'Several dozens of strong lively fish, four inches in length, were transferred with great care from the nets into large vessels (some of the vessels, to vary the experiment, being of earthenware, and others of wood and metal) filled with water taken from the Thames at the time of catching the fish. At the expiration of twenty minutes nearly the whole of them were dead, none survived longer than half an hour, and all fell to the bottom of the water. On examination, the air-bladders were found to be empty and collapsed. There was no cause of death apparent. About four dozen specimens were then placed in a coffin-shaped box, pierced with holes, which was towed slowly up the river after the fishing boat. This attempt also failed: all the fish were dead when the vessel had reached Greenwich. Mr. Yarrell was told by two white bait fishermen, that they had several times placed these fishes in the wells of their boat, but they invariably died when brought up the river. The fishermen believe a portion of sea-water to be absolutely necessary to the existence of the species, and all the circumstances attending this particular fishery appear to prove their opinion to be correct*.'

18. To restore the Elasticity of a Damaged Feather.

A feather when damaged by crumpling may be perfectly restored by the simple expedient of immersing it in hot water. The feather will thus completely recover its former elasticity, and look as well as it ever did. This fact was accidentally discovered by an amateur ornithologist of Manchester. Receiving, on one occasion, a case of South American birds, he found that the rarest specimen it contained was spoilt, from having had its tail rumpled in the packing. Whilst lamenting over this mishap, he let the bird fall from his hands into his coffee-cup; he now deemed it completely lost, but, to his agreeable surprise, he found, that after he had laid it by the fire to dry, the plumage of the tail became straight and unruffled, and a valuable specimen was added to his collection.

19. Ornithology.

At a late meeting of the committee of Science and Correspondence of the Zoological Society of London, Mr. Vigors, the secretary, called the attention of the committee to a gallinaceous group of America, which supplied in that continent the place of *Quails* in the Old World. Of this group, or the genus *Ortyx* of modern authors, which a few years back was known to ornithologists by two well-ascertained species only, he exhibited specimens of six species—viz., *Ort. Virginianus* and *Californicus*, which had been the earliest described, the former by Linnæus, the latter by Dr. Latham; *Ort. Capistratus*, a species lately figured, named in Sir W. Jardine's and

* Trans. Zool. Soc. Lond. p. 14.

Mr. Selby's 'Illustrations of Ornithology,' and *Ort. Douglasii*, *Montezumæ* and *Squamatus*, which had been described by himself in the Zoological Journal. In addition to these, he exhibited plates of three others, of which no specimens were to be obtained in London—*viz.*, *Ort. Macrourus*, figured by Sir W. Jardine and Mr. Selby; *Ort. Sonninii*, figured by M. Temminck, in the Planches Coloriées (No. 75); and the *Ort. Cristatus*, figured in the Planches Enluminées (No. 126) of M. Buffon. To these nine described species he added two others, apparently new to science, and which he characterized under the name of *Ort. Neoxenus* and *Affinis*, stating, at the same time, his doubts whether both might not be females or young males, of the imperfectly known species of *Ort. Sonninii* or *Cristatus*. Individuals of the four above-mentioned species, viz., *Ort. Virginianus*, *Californicus*, *Neoxenus*, and *Montezumæ*, had been exhibited in a living state in the garden of the Society, where specimens of the former three were still alive, having braved the severity of the last winter without artificial warmth. The *Ort. Virginianus* has bred in this country, and has even become naturalized in Suffolk*.

Indian Birds.—Mr. John Gould, A.L.S., has recently received from India a large collection of birds, of which he intends shortly to publish coloured illustrations. Among these are several species, apparently undescribed, from the Himalayan mountains. The forms of a large proportion of these birds are capable of being identified with those of Northern Europe, at the same time that many of the forms peculiar to Southern Asia and the Indian Archipelago, are found intermingled with those of the northern regions. Among the forms similar to the European are three species of *Jays*, which have been named *Garrulus Lanceolatus*, *Garr. Bispeculatus*, and *Garr. Striatus*. The two first of these exhibit a striking affinity to our well known British bird. The latter species deviates in general colour and markings from the European species. Although according in form, and in the former characters, they exhibit a manifest approach to the *Nutcrackers*, or genus *Nucifraga* of Buffon. A new species of this latter form, *Nucifraga Hemispila*, is also amongst this collection, thus adding a second species to a group hitherto supposed to be limited to one. The collection also contains two species of *Woodpecker*, which have been called *Picus Occipitalis* and *P. Squamatus*, and approach in size and colouring most closely to the European *Green Woodpecker*. There is also a species of *Hawfinch* (*Coccothraustes Icteroïdes*), according accurately with the characters of the northern species; and also a small owl (*Noctua Cuculoides*), nearly allied to the *Noctuæ Passerina* and *Tengmalini* of Europe.

Among the forms in this collection, which are peculiar to India, is a second species of the singular group, which contains the *Horned Pheasant*, or the *Meleagris Satyra* of Linnæus, and which has lately been separated by M. Cuvier, under the name *Tragopan:* it has been

* Proceedings of the Zoological Soc. Lond., p. 2.

named *Tragopan Hastingsii.* There is also a species of true *Pheasant* (*Phasianus albo Cristatus*), which seems to have been indicated by former writers from incomplete descriptions or drawings, but never to have been accurately characterized. A third species is likewise added from the collection to the group of *Enicurus* of M. Temminck, which has hitherto been considered limited in range to the Indian Archipelago (*Enicurus Maculatus*).*

The same collection also contains several species of humming birds; one of which, previously undescribed, has been called *Trochilus Loddigesii;* it approaches most nearly to the *Tro. Lalandii*, Vieill.†

Dr. Andrew Smith, of Cape Town, has informed the Zoological Society that he has discovered another species of the *Macroscelides*, as well as a new one of *Erinaceus*, and three species of the genus *Otis*, together with one of *Brachypteryx*, the descriptions of which he purposes to transmit very shortly.

20. Ichthyology.

Dr. Smith has transmitted to the Zoological Society a present of sixteen specimens of fishes, obtained in the neighbourhood of the Cape of Good Hope; amongst which are an undetermined species of *Dentex;* a fish allied to *Oblada*, Cuv., and apparently the type of a new genus; a new species of *Scomber*, Cuv.; an undescribed species of *Bagrus*, Cuv.; a species of *Scyllium*, Cuv., probably new to science; and a second species of the genus *Rhina*, Schn., which deviates from the type, by a slight production of the front of the head, and thus makes an approach to *Rhinobates*, Schn.‡

21. Influence of the Aurora Borealis on the Magnetic Needle.—(*A. T. Kupffer, of St. Petersburgh.*)

'During the night of the 5th of May, 1830,' says M. Kupffer, 'whilst I was engaged in observing the hourly variations of the magnetic deviation, I was surprised to see the needle oscillate greatly, and at the same time deviate considerably to the east. I immediately suspected that there was an aurora borealis, and was particularly gratified on finding my supposition confirmed; the phenomenon lasted till about two o'clock, when no visible trace of it was left. During the whole time, I carefully observed the needle, particularly as I knew that it would be also observed by my correspondents at Nicolajew, Kasan, Berlin, and Freiberg, the 5th of May being one of the days on which we had agreed to observe the hourly variations of the needle. The following table contains the observations at St. Petersburg, Nicolajew, and Kasan; those

* Trans. Zool. Soc. Lond., p. 7.

† Ibid., p. 12.

‡ Trans. Zool. Soc. Lond. p 11.

made at Berlin and Freiberg are not yet come to, but shall be given as soon as possible.

Time of Observ.	Variation of the Deviation.			Time of Observ.	Variation of the Deviation.			Time of Observ.	Variation of the Dev. in St. Petersburgh.
	St. Petersburgh.	Nicolajew.	Kasan.		St. Petersburgh.	Nicolajew.	Kasan.		
7^{h} 00′	42′ 00″	23^{m}3 0	13^{m}555	10^{h} 20′	22′ 30″	24^{m}00″	13^{m}195	12^{h} 38′	12′ 30″
20	42 00	30	585	40	23 00	06	275	40	7 00
40	42 00	26	585	11 00	(Oscillations.) 42 45	23 96	12^{m}825	45	Viol.
8 00	42 15	24	565	20	17 15	24 03	805	13 00	oscil.
20	42 00	30	605	40	9 00	16	545	5	10 30
40	40 15	31	575	12 00	4 45	23	475	20	30 00
9 00	32 30	59	645	8	16 00		—	40	16 15
20	32 00	63	575	20	20 32		525		
40	32 00	80	615	22	15 00				
10 00	26 15	77	435	27	18 15				

The numbers of this table give only the variations of the deviation, and are in no definite ratio to the absolute deviation. In the observations of St. Petersburgh and Nicolajew, an increase in the minutes and millimetres signifies a western deviation, and *vice versâ* in Kasan; 1 millimetre is equal to 14′ 3″.

It will appear from these combined observations, that the magnetic needle, in all three places, had a very irregular course at the same time; for, as in St. Petersburgh and Nicolajew, the oscillations began at about 9 o'clock, and in Kasan at 20 min. past 10, these times will be found nearly to agree; as, owing to the different longitude, 9 at St. Petersburgh corresponds with 9 and 7 minutes in Nicolajew, and with 10 and 16 minutes at Kasan.

The order of the variation will be seen in a more striking manner in the following table, in which the observations, which were made *at the same moment* (or nearly so), are placed in the same line, and the millimetre of the observations at Nicolajew and Kasan are reduced to arcs; besides, I have taken 42′ 00″ as the ordinary deviation at the three places, which may be done, as the observation refers only to the relative, and not to the absolute deviation. An increase in the numbers signifies an increase of the western deviation.

Time in St. Petersburgh.	Variation of the Deviation.			Time in St. Petersburgh.	Variation of the Deviation.		
	St. Petersburgh.	Nicolajew.	Kasan.		St. Petersburgh.	Nicolajew.	Kasan.
7^{h} 00′	42′ 00″	42′ 00″	42′ 00″	10^{h} 00″	26′ 15″	35′ 11″	30′ 24″
20	42 00	42 00	41 35	20	22 30	31 50	26 38
8 40	42 00	42 35	42 35	40	23 00	31 00	25 37
8 00	42 15	42 52	41 35	11 00	24 45	32 26	26 21
20	42 00	42 00	42 20	20	17 15	31 25	
40	40 15	41 52	39 32	40	9 00	29 32	
9 00	32 30	37 48	36 03	12 00	4 45	28 32	
20	32 30	37 13	37 13				
40	32 00	34 45	30 41				

From this table it will appear that the course of the needle was

very similar at the three places, but that the variations were far more considerable at St. Petersburgh than at Nicolajew and Kasan. The declination will be found to have been—

	At Petersburgh.	At Nicolajew.
at 9 o'clock . .	9′ 30″ . .	4′ 12″
10 ,, . .	15 45 . .	6 49
11 ,, . .	17 15 . .	9 34
12 ,, . .	37 15 . .	13 28
Total	1° 19′ 45	44′ 33″

and, as at St. Petersburgh, on the same day, between 7h. 40 m. A.M. and 2 h. 40 m. P.M., the declination amounted to 17′¼ towards the west, and at Nicolajew to 41′½, the above variation being nearly in the ratio of two to one, the extent of the irregularity will accordingly result.

A needle, which at St. Petersburgh performs 300 oscillations in 1108″, makes at Nicolajew the same number within 964″; the ratio of the squares is 1.32, and consequently much less than that of the variation observed on the 5th of May; the cause of the irregularity has accordingly acted more powerfully at St. Petersburgh than at Nicolajew.

The remark of M. von Humboldt, that irregularities of the above kind sometimes observe a sort of periodicity, being either followed or preceded by irregular oscillations at the same hour for some days, will be found confirmed by the following table of the progress of the needle at the three places of observation on the 4th of May.

Time of Observation.	At St. Petersburgh.	Variation of the Deviation at Nicolajew.	At Kasan.
8h 00′	39′ 30″	23m47″	13m.555
9 00	39 15	— 45	— .615
10 00	39 30	— 48	— .615
20	37 45	— 77	— .625
40	30 00	— 81	— .615
11 00	28 45	— 81	— .645
20	31 45	— 80	— .465
40	33 00	— 79	— .195
12 00	33 00	— 78	— .175
20	26 15	— 96	— .295
40	26 15	24 00	— .245
13 00	24 15	— 00	— .125
20	24 15	23 96	— .155
40	23 15	24 03	12 .900
14 00	21 45	— 10	— .905
20	22 00	— 02	— .925
40	20 15	— 04	— .925
15 00	28 00	— 05	— .765
20	32 45	23 84	— .890
40	37 45	— 76	— .890
16 00	35 30	— 60	— .825
17 00	39 45	— 57	13 .495
18 00	41 15	— 58	— .425

In a note to the above paper, M. Poggendorff states, that on the same day the needle also experienced great irregularities at Freiberg. On the 5th of May, at 7 o'clock 20 min. P.M. (8 h. 28′ of Petersburgh time,) the needle was 2′45″ eastward; from this time it moved in an easterly direction, so as to have reached, at midnight, 21′ 9″, when violent oscillations were observed, and the needle rapidly moved westward, so as to be at 12 h. 15′ A.M. 16′ 14″ west, and consequently 37′ 20″ more westward than a quarter of an hour before: this was the maximum of the westerly deviation—at 12 h. 17′ it had diminished to 12′ 0*.

22. Description of some Atmospheric Phenomena.—(*Professor Strehlke, of Danzig.*)

On the 29th of last March, 1830, I observed the phenomenon of coloured rings and parhelia, which, as I see from the papers, were at the same time also seen at other places. It appears, that for some time before and after the above date, the atmosphere was in a state peculiarly favourable to appearances of this kind, for, on the 20th of March, at 5 o'clock P.M., a large coloured areole had formed round the sun about 45° in diameter; the sky was covered with numerous parallel strata of clouds, which appeared to converge towards the sun, opposite to which they had another point of convergence. The colours of the ring were dull red internally, and bluish externally. On the 30th of March, at 10 o'clock P.M., a white halo of 45° had formed round the moon. On the 9th of April, at 6 o'clock P.M., the sun was surrounded by segments of an areole of 45° in diameter; on the 10th, at 11 o'clock A.M., it had an incomplete areole, from the uppermost portion of which a white circle subsequently formed, inclosing the zenith. But the most striking of all was the phenomenon observed on the 29th of March. On the 28th, the wind was northerly, and in the night it froze; on the 29th, the wind went through east to south. The following table exhibits the meteorological observations on these two days:—

					Thermometer.	Barometer at 0.46 above the sea.	
28th of March,	at	10	o'clock	P.M.	+ 2.0 R.	339‴.41	
29th	„	8	„	A.M.	1.1	339 .25	Cloudy towards east.
	„	10	„	„	4.3	339 .04	
	„	12	„	„	6.2	338 .66	Cloudy towards south—weak sunshine.
	„	2	„	P.M.	8.5	338 .22	
	„	4	„	„	9.0	338 .99	
	„	6	„	„	8.0	337 .87	Cloudy towards south.
	„	8	„	„	7.5	337 .63	
	„	10	„	„	8.0	337 .29	Clear.
30th	„	8	„	A.M.	7.0	335 .59	

At 3 o'clock 45 min. on either side of the sun, and at the distance of about 22° from it, there were two parhelia, the outer part of which was white, the inner red; the horizon was rather cloudy,

* Poggendorff Annalen der Physik und Chemie.

and the rest of the sky filled with nimbus. After a few minutes there appeared round the parhelia B and C (Fig. 1.), large segments of

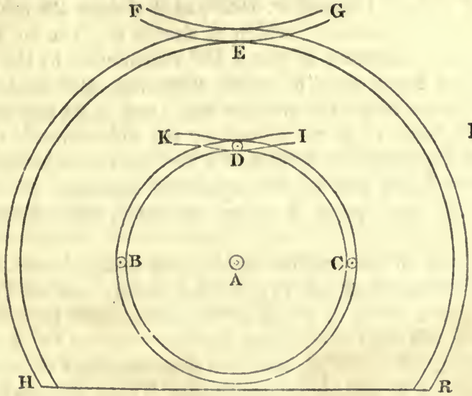


Fig. 1.

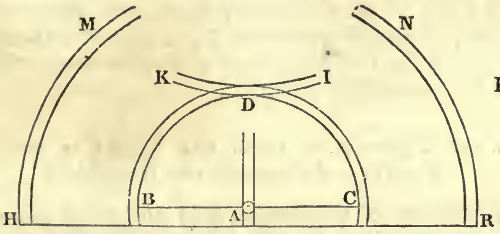


Fig. 2.

the circle B D C B, the inner part of which was red; towards C the circle was thinner, and not so complete as on the south side B; and at the uppermost part, D, the arch was not completely closed: at the same time a white sun was visible opposite to the real one at about 20° above the horizon. The clouds now gradually ascended towards the zenith, so as to leave but slight traces of the arch at B, and to efface C and the northern parhelion completely. The sunshine was very weak, and the clouds were of a brownish-red hue, when on a sudden the circle H E R appeared at 45° from the sun; it was red internally, green in the middle, and blue outside. The northern arch was less distinct than the southern, E H; at E also the segment of a circle round the zenith was visible of about 45°, and there the most vivid colours were seen, from purple to green and violet. At 4 o'clock, H E B had nearly disappeared; the segment F E I was still visible, and remained so till half past four, when all traces of the phenomenon had disappeared, and the sky was equally covered with a cloudy surface.

Two of my friends, who happened to be at a distance from me of some thousand paces towards the south, saw, besides the two parhelia B and C, a third in D, and through it there was a segment of about 22° parallel to the horizon; it was of red colour at the side directed

towards the real sun, and afterwards changed into an undulating line; the other appearances were those described above.

On the 1st of December, 1828, at 2 o'clock 25 min. P.M., I observed the phenomenon of which a sketch is given in Fig. 2. The sun A was, at a distance of about 45°, surrounded by the arches H M and R N, the inner parts of which were red, and the outer blue; at 22° from the sun there was another ring, and at its uppermost part an arch K D T, both of a red colour at the side towards the sun; besides these appearances, there was a vertical and a horizontal column of yellowish light, and of the apparent diameter of the sun. At 3 o'clock 35 min., when it began to snow, the phenomenon disappeared.

During one of the nights in August, 1828, I saw, at midnight, very beautiful coloured halos round the moon, and as I happened to pass over fields, some of which were covered with fog, and the others clear, I was surprised to find that the halos ceased to be visible whenever I was on a field which was free from fog, but always reappeared when I came on a field covered with fog. There were three, and sometimes four halos, at small distances from each other, and with very vivid colours, red being always at the outer circumference; opposite to the moon there were segments of a white circle. The sky was clear, and the atmosphere tranquil *.

23. On the Produce of Gold and Silver in the Russian Empire.—(*Alexander von Humboldt.*)

The yearly produce of the Russian gold and silver mines has lately been very variously stated; and as I am afraid that some of these statements may be attributed to me, I take an opportunity of giving the following numerical exposition of the fact.

According to official documents, the Russian mines yield annually about 22,000 marks of gold, and 77,000 of silver. In 1828 the produce of gold was 22,256 marks (318 puds, of which 115 were obtained from imperial, and 203 from private mines); of silver 76,498 marks (1093 puds); and of platina 6570 marks (94 puds); and the respective value was, of gold, 4,896,000 Russian dollars (700,000*l.* sterling); and of silver, 1,071,000 dollars (153,000*l.* sterling). The gold mines of the Ural yielded in—

1826	. . .	232 puds.
1827	. . .	282 ,,
1828	. . .	291 ,,

In the first six months of 1829 they gave 142 puds of gold (46 from imperial, and 96 from private mines), and 43 puds of platina.

The total produce of the Ural mines, from 1814 to 1828, is 1551 puds, of the value of about 3,413,000*l.* sterling; the last five years alone yielded 1247 puds.

* Poggendorff's Annalen.

The annual produce of gold in Europe and in Asiatic Russia amounts to 26,500 marks of gold, and 292,000 of silver, of which the Russian empire alone yields 22,200 marks of gold, and 76,500 of silver *.

24. On the Change which the Air in Eggs undergoes during Incubation.—(*Professor Dulk, of Kœnigsberg.*)

This philosopher has lately made some analyses of the air in the large end of the egg at different periods of incubation, and the following is the result of his inquiries.

Before incubation, the air contains considerably more oxygen than atmospheric air, the oxygen in the former being found, at two different experiments, 25.26 and 26.77; and that in the latter, on the day of the experiments, only about 21.0 †.

On the tenth day of incubation the air was found to contain 22.47 of oxygen, and 4.44 of carbonic acid; the absolute quantity of oxygen is accordingly nearly the same, but 4.44 of it has united with carbon.

On the twentieth day, the quantity of air in the egg was found to be nearly eight times as large as before incubation; the analysis gave, at four different experiments,—

Carbonic Acid.	Oxygen.
9.40	—
9.23	17.55
6.19	—
8.48	17.90

where the absolute quantity is the same as in the former experiments, but the quantity of carbonic acid is increased; that of the third experiment, being only 6.19 per cent., corresponds, however, in some degree, with the result of the other analyses, as the chicken had apparently died a considerable time before the experiment was made ‡.

* Poggendorff's Annalen, 1830. p. 273.

† Though this result is somewhat at variance with that obtained by M. Bischoff, according to whom the mean quantity of oxygen before incubation is only 23.475, the experiments of both philosophers agree, inasmuch as both show that the air of fresh eggs contains more oxygen than atmospheric air.

‡ From Schweigger-Seidel's Jahrb. der Chemie und Physik.

JOURNAL

OF

THE ROYAL INSTITUTION

OF

GREAT BRITAIN.

ON THE EMPLOYMENT OF NOTATION IN CHEMISTRY.

By the Rev. W. WHEWELL,

Professor of Mineralogy in the University of Cambridge.

THE greater part of English chemists appear to have been hitherto averse from the practice of using a technical and mathematical notation to express the chemical composition of bodies; while in France, Germany, and Sweden, such a notation is and has been for some time commonly employed. The disinclination of our countrymen to adopt this invention seems to arise from a belief that such an instrument is unnecessary, and from a perception of several anomalies and inconveniences in the system followed by foreigners. English chemists must judge for themselves whether they feel the want of such a contrivance; but I have no hesitation in saying, that in mineralogy it is utterly impossible to express clearly, or to reason upon, the chemical constitution of our substances, without the employment of some notation or other. Every one who makes the trial will find that, without a notation, his attempts to compare the composition of different minerals will be confused and fruitless, and that, by employing symbols, his reasonings may easily be made brief, clear, and systematic.

I have, therefore, endeavoured to remove the gross anomalies and defects with which the foreign notation is disfigured, and to reduce it, with as little change as possible, to mathematical symmetry and consistency. If this can be done, as I think it can, with no loss of simplicity and facility, I should

hope that the system so reformed might obtain general circulation; since the question undoubtedly is, or soon will be, not whether or no we shall employ notation in chemistry, but whether we shall use a bad and incongruous, or a consistent and regular notation.

I shall now endeavour to show the necessity and the advantages of a proper use of symbols in this science, and the inconveniences of those at present in use on the continent.

In many compounds of two ingredients there is no difficulty in expressing the composition clearly and simply, by means of the usual language of chemistry. We have carbonate, *bi*carbonate and *sesqui*carbonate of soda; where we may observe, however, that the possibility of expressing the latter compound in this form, depends upon the accident of there being a Latin term possessing the signification of *one and a half;* and that if we had *two atoms and a half* of acid, we should be at a loss how to devise a corresponding term. In the same manner we have sulphurets, *bi*sulphurets, *quadri*sulphurets; *prot*oxide, *deut*oxide, *trit*oxide; terms which sufficiently express the constitution of the compounds to which they refer.

But the usual nomenclature is, even in such cases, imperfect. The words hyposulphite, sulphite, sulphate, are defective, in not showing the relative quantity of oxygen in the acid; and, moreover, such terms are liable to become improper by the discovery of new compounds. The same may be said of such expressions as *per*oxide, *per*sulphuret.

Nor is this nomenclature capable of extending itself, in virtue of its own rules, in proportion as discovery extends. If new combinations of manganese and oxygen should hereafter be discovered, they must receive arbitrary, and probably anomalous designations. The oxide, deutoxide, peroxide, manganesious and manganesic acid, do not at all obviously refer to compounds, in which the proportions of oxygen are 1, $1\frac{1}{2}$, 2, 3, 4; and if we should find a combination in which the proportion of acid is $2\frac{1}{2}$ or $3\frac{1}{2}$, there is no denomination ready for it, nor would it be easy to find a good one. This applies equally to very many cases.

In other cases phrases are used, as the *sulphato-tricarbonate of lead* for instance, which, though capable of a right interpre-

tation, do not sufficiently interpret themselves; and even such can only be constructed for a few detached instances.

When the constitution is at all less simple than in the above examples, the expression to describe it becomes still more difficult to construct. If we have 3 atoms of lime and 4 of silica, there is no very compendious chemical name for the compound.

But if the usual phraseology be defective and inconvenient in compounds of two ingredients, it becomes unmanageable and almost impossible when there are more. Stilbite is *one atom of trisilicate of lime combined with four atoms of trisilicate of alumina, and six atoms of water.* Such a mode of description conveys no distinct impression, except by being considered as a mathematical form; and, if it be so considered, can be expressed much more simply and briefly by means of mathematical symbols.

Moreover, such a mode of description is, in some degree, hypothetical; for the direct and certain result of the analysis is only this: that the mineral just mentioned contains certain quantities of silica, alumina, lime, and water, respectively; and the process of connecting one certain portion of the silica with the lime, and another portion with the alumina, depends upon the assumption of the doctrine of atomic combinations. It is also, in some measure, arbitrary, even granting that doctrine; for there are generally, in such cases, more possible ways than one of making such combinations, though one way may often be simpler than the others. Thus, one atom of silicate of lime combined with one atom of bisilicate of alumina might just as well be considered to be one atom of bisilicate of lime with one of silicate of alumina; and as this choice of the mode in which the combination is to be taken is often a matter of great doubt, it is not desirable to adopt a notation which necessarily affirms one particular mode. The notation of Berzelius, however, does restrict us, in this manner, to one selected and frequently arbitrary view of the body's constitution.

It appears to me, that the objects of notation in such cases will be gained, and all the inconveniences avoided, if we adopt the most simple and natural method of representing chemical combinations by symbols. Let S represent the weight of an

atom of silica, A of alumina, C of lime, q of water. Then, $15\,S + 4\,A + C + 6\,q$ will represent a body which contains 15 atoms of silica, 4 of alumina, 1 of lime, and 6 of water. If 12 atoms of the silica go with the alumina, and 3 with the lime, the symbol may stand thus: $(12\,S + 4\,A) + (3\,S + C) + 6\,q$; or, what is the same thing, $4\,(3\,S + A) + (3\,S + C) + 6\,q$; in which form it is clearly seen that we have 3 atoms of S with 1 of A, also 3 of S with 1 of C, and that 4 of the former parcels are combined with 1 of the latter. The same analysis would also give other results, as $4\,(2\,S + A) + (7\,S + C) + 6\,q$, which is less simple, and so less probable than the former, as the representation of the chemical constitution of the body.

The expression $15\,S + 4\,A + C + 6q$, the immediate result of the analysis, may thus be put in various forms; and these forms are all identical, in virtue of the common rules of algebra or arithmetic. I can hardly conceive how any person, at all acquainted with mathematical symbols, can adopt any other mode of notation than this, inasmuch as no other can assist us in reasoning on the constitution of chemical compounds. Mr. Herschel long ago employed this mode of notation for such a purpose. In his paper on the hyposulphurous acid (*Edinb. Phil. Journ.*, 1819), he describes the decomposition of oxynitrate of silver by hyposulphite of lime. L represents lime; S, sulphur; *s*, silver; N, nitric acid; O, oxygen. He says, ‘we have, for the atoms present, before the decomposition,

$$\{L+2(S+O)\} + \{N+(s+O,\}$$

which afterwards groupe themselves thus:—

$$(L+N) + (S+s) + (S+3\,O);$$

that is, one atom nitrate of lime, one of sulphuret of silver, and one of free sulphuric acid.’ In the same manner, in the decomposition of carbonate of oxide of copper by sulphurous acid, *c* denoting copper, he obtains,

$$2(c+O) + 2(S+2O)$$
$$=\{(2c+O) + (S+2\,O)\} + (S+3\,O);$$

‘that is, two atoms of sulphurous acid disengage the carbonic acid from two of the carbonate, producing one atom of sul-

phite of protoxide, and generating one of sulphuric acid.' These seem to me very instructive examples of the use which may be made of such a method of notation.

Berzelius, however, has lent the weight of his great authority to a system which possesses none of these advantages, and which violates mathematical propriety so entirely, that it must always be disagreeable to see an example of it, for any person who has acquired the first rudiments of algebra; and this system has unfortunately been adopted into many excellent chemical and mineralogical works.

According to the system of which I thus complain, those combinations of elements which are supposed to be most intimate, are represented by writing the symbols of the ingredients in the way which in algebra denotes multiplication. Thus $A\,S$ is the silicate of alumina; and when there are several atoms of one ingredient, the number of these is indicated by the index of the corresponding letter: thus $A\,S^2$ is the bisilicate, $A\,S^{\frac{3}{2}}$, or $A^2\,S^3$, the sesquisilicate of alumina. And when these primary combinations are associated so as to form other compounds, the sign of addition, $+$, is used. Thus $A\,S^2 + C\,S^3$ is an atom of bisilicate of alumina, combined with an atom of trisilicate of lime. Also a multiplier is, if necessary, annexed to one of these members. Thus the former example, stilbite, (p. 439,) would, on this system, be $4\,A\,S^3 + C\,S^3 + 6q$. And, by such formulæ, minerals and other bodies are represented by Berzelius and many other writers who have followed him.

The insurmountable objection to such a notation is this: that it violates all mathematical consistency, and puts out of sight the identity of different ways of considering the same analysis. No one can, in the last formula, see any algebraical reason for supposing it the same with $4\,A\,S^2 + C\,S^7 + 6q$, which it undoubtedly is. No one can readily perceive at once that the direct result of analysis has in this case been $15\,S + 4A + C + 6q$; which is the fundamental fact. Is there any obvious connexion between $A\,S + C\,S^3$ and $A\,S^2 + C\,S^2$, which are mineralogically identical? Whereas $(A + S) + (C + 3\,S)$ and $(A + 2\,S) + (C + 2\,S)$ are manifestly the same quantity. If we adopt such a notation as this of Berzelius, it is almost entirely useless as an instrument of calcula-

tion, besides being singularly awkward in the eyes of every one at all acquainted with algebra.

The combinations of ingredients which make up compounds are clearly of the nature of *additions*, and never can have any analogy with the *multiplication* of the numbers expressing the components; they therefore ought by no means to be represented by that combination of symbols which denotes multiplication. I cannot account for the adoption of such a mode of representation any otherwise than by attributing it to the ambiguity of the words *factor* and *product*, which are sometimes used to express the ingredients producing a chemical compound by their addition, and the compound itself; but which in algebra properly refer to parts producing a number by multiplication. Whether or not this double meaning be the origin of the confusion, there can be no doubt of the exceeding impropriety, I might say absurdity, of such a kind of symbols.

It is much to be regretted that a system marked with such blemishes should have been promulgated by Berzelius, whose great knowledge and deserved eminence gave him more power, perhaps, than any other chemist possessed, to obtain a European circulation for the vehicle in which his speculations were conveyed.

I now proceed to the system which I propose to substitute for this. There can, I think, be no difficulty in its application. The letters being once fixed upon, which denote the ingredients to be represented, their combination is to be indicated by the sign of addition; and if they be distinguished into groups, these groups may be marked by brackets, as in Mr. Herschel's notation. These brackets may often be omitted without causing any confusion.

I will give an example or two to exhibit the manner in which a given analysis may be expressed in this notation; and also the manner in which, the symbol being given, we may see what ought to be the analysis.

We have the following analysis of Analcime by H. Rose:—

Silica . .	55.12	one atom $S = 16$
Alumina .	22.99	.... $A = 18$
Soda . .	13.53	.... $N = 32$
Water . .	8.27	.... $q = 9$
	99.91	

The soda appears to be the smallest of the ingredients. If we multiply all the numbers by 2.37, the soda will become 32, or one atom; and, as the proportions will not be altered, we shall have the corresponding numbers of atoms of the other constituents, by dividing by the weight of one atom of each. We have thus,

Silica . .	130.82	number of atoms =	8.18
Alumina .	54.49	....	3.02
Soda . .	32.09	....	1
Water . .	19.62	....	2.18

Since the numbers of atoms must be whole numbers, 8, 3, 1, 2 are the true results, and the formula is $8\,S + 3\,A + N + 2\,q$.

These elements may be variously grouped. The most probable arrangement appears to be $3(2\,S + A) + 2S + N + 2\,q$, as the atomic constitution of the analcime analyzed in this case.

I take, as another example, two analyses of Apophyllite; one by Vauquelin, and the other by Berzelius:—

	V.	B.	One atom.
Silica . .	51	52.13 ..	(16)
Lime . .	28	24.71 ..	(28)
Potassa (K)	4	5.27 ..	(48)
Water . .	17	16.20 ..	(9)
Fluoric Acid		.82 ..	(20)
	100	99.13	

The fluoric acid is probably accidental. If we suppose the potassa to be essential, we must multiply the parts of first analysis by 12, and those of the second by $\frac{48}{5.27}$, or 9.11, and then divide by the weights of one atom. We have thus,

V.		Atoms.	B.		Atoms.	
S	612	— 38	474.90	—	29.68	(30)
C	330	— 12	225.11	—	8.04	(8)
K	48	— 1	48	—	1	(1)
q	204	— 22 or 23	147.58	—	16.39	(16)

The second analysis would give $30\,S + 8\,C + K + 16\,q$, which might be grouped thus: $8(3\,S + C) + 6\,S + K + 16\,q$; and this is given by Berzelius as the constitution of apophyllite. But Vauquelin's gives us $38\,S + 12\,C + K + 22\,q$, which

deviates so far from the other, as to suggest strong doubts of the soundness of Berzelius's view. The latter analysis may be put in this form : $11(3\,S + C + 2\,q) + 5\,S + C + K$; and the former in this form : $8\,(3\,S + C + 2q) + 6\,S + K$: from which it appears that by much the largest portion of the mineral may be considered as $3\,S + C + 2q$, with an excess of S, and a small quantity of K. If we consider C and K as *isomorphous*, we may, by a rule which will be given immediately, write the formula thus, dividing by 9 in one case, and by 13 in the other (see p. 446.) :—

$$3\tfrac{1}{3}S + \tfrac{8}{9}C + \tfrac{1}{9}K + 1\tfrac{7}{9}q$$
$$2\tfrac{12}{13}S + \tfrac{12}{13}C + \tfrac{1}{13}K + 1\tfrac{9}{13}q :$$

neither of these differs much from $3\,S + C,\,K,\, + 2q$, $\tfrac{1}{9}C$ in one case, and $\tfrac{1}{13}C$ in the other, being replaced by a corresponding portion of K. Other analyses must be employed to determine whether this is the true formula for apophyllite.

It must, I think, be clear to the reader, that this reasoning could not have been conducted with any tolerable facility or clearness by employing the symbols of Berzelius. According to these, the grouping first mentioned would be $8\,C\,S^3 + K\,S^6 + 16q$; the next might be $12\,C\,S^3 + K\,S^2 + 22q$: the one in which K is omitted would be $C\,S^3 + 2q$; and the one last mentioned would be $(C,\,K)\,S^3 + 2q$. There is in these cases no trace in the symbols themselves of that relation by which their identity is to be tried, or of their dependence on two resembling analyses.

I will now exemplify the use of such formulæ by writing in this notation the results of the analyses of several minerals, which approach to apophyllite in their constitution. These minerals I will designate as follows :—(1) Apophyllite, (2) Heulandite, (3) Stilbite, (4) Harmotome, (5) Laumontite, (6) Analcime, (7) Scolezite, (8) Mesotype. I will also, in addition to the letters already introduced, use B for baryta (atom = 78).

$(1) = 30\,S + 8\,C + K + 16\,q = 8\,(3\,S + C) + 6\,S + K) + 16\,q.$
$(2) = 15\,S + 4\,A + C + 6\,q = 4\,(3\,S + A) + 3\,S + C) + 6\,q.$
$(3) = 12\,S + 3\,A + C + 6\,q = 3\,(3\,S + A) + (3\,S + C) + 6\,q.$
$(4) = 12\,S + 4\,A + B + 6\,q = 4\,(2\,S + A) + (4\,S + B) + 6\,q.$
$(5) = 10\,S + 4\,A + C + 6\,q = 4\,(2\,S + A) + (2\,S + C) + 6q.$

$(6) = 8\,S + 3\,A + N + 2q = 3\,(2\,S + A) + (2\,S + N) + 2q.$
$(7) = 6\,S + 3\,A + C + 3q = 3\,(S + A) + (3\,S + C) + 3q.$
$(8) = 6\,S + 3\,A + N + 2q = 3\,(S + A) + (3\,S + N) + 2q.$

The last column, containing the *grouping* of the ingredients, is for the most part according to the views of Berzelius.

If we now wish to compare the quantities of the ingredients in any case, we have only to introduce the atomic weights. Thus, for Heulandite, we have

$$15\,S + 4\,A + C + 6q$$
$$= 240 + 72 + 28 + 54 = 394.$$

There is no necessity to reduce these quantities of the ingredients to the supposition of 100 for the total, nor to do so with any given analysis; since the comparison of the analysis with the formula may be made with greater brevity, by avoiding such an intermediate step, and by trying directly the proportionality of the numbers in the experiment with those in the formula.

In order to compare the constitution of different species, as for instance those above mentioned, it would be convenient to reduce the most prominent term, which appears here to be one with S, to an identity in all, by multiplication or division; this process would give the following results:—

	S(16)	A(18)	C (28)	q (9)	Total.
$(1) = 30S + 8C + K + 16q$					
$2 \times (2) = 30S + 8A + 2C + 12q$	480	144	56	108	788
$2\frac{1}{2} \times (3) = 30S + 7\frac{1}{2}A + 2\frac{1}{2}C + 15q$	480	135	70	135	820
$2\frac{1}{2} \times (4) = 30S + 10A + 2\frac{1}{2}B + 15q$	480	180	(195,B)	135	990
$3 \times (5) = 30S + 12A + 3\,C + 18q$	480	216	84	162	942
$3\frac{3}{4} \times (6) = 30S + 11\frac{1}{4}A + 3\frac{3}{4}N + 7\frac{1}{2}q$	480	$202\frac{1}{2}$	(120,N)	$67\frac{1}{2}$	870
$5 \times (7) = 30S + 15A + 5\,C + 15q$	480	270	140	135	1025
$5 \times (8) = 30S + 15A + 5N + 10q$	480	270	160	90	1000

As (1) contains the ingredients with somewhat different relations, the results are not here given for that species.

In some cases it is found that different ingredients are *isomorphous*, or may replace each other without altering the form or species of the mineral. Thus we have Chabasite of the formula $8\,S + 3\,A + C + 6\,q$; and also of the formulæ $8\,S + 3\,A + K + 6\,q$, and $8\,S + 3\,A + N + 6\,q$; and a mixture of the two latter kinds, including both K and N. In such cases a part of the term corresponding to K appears to be potassa,

and a part soda, such portions of the two alkalis entering as to make up together a proportional ingredient. Thus we may have $\frac{3}{4}$K and $\frac{1}{4}$N; and the mineral would then be (9) $= 8\,S + 3\,A + \frac{3}{4}K + \frac{1}{4}N + 6\,q$. And $4 \times (9) = 32\,S + 12\,A + (3\,K + N) + 24q$: in which, instead of 4 K, we have 3 K + N; one atom of potassa being replaced by one atom of soda. In obtaining such formulæ from experimental analysis, the number of atoms of the two isomorphous ingredients must be added together, including their fractional parts, before we attempt to compare the atoms of different ingredients. And in the resulting formula, the symbols of the isomorphous ingredients may be written after each other with commas, thus,

$$(9)\ \text{Chabasite} = 8\,S + 3\,A + C, K, N, + 6\,q$$
$$= 3\,(2\,S + A) + (2\,S + C, K, N,) + 6\,q;$$

in which C, K, N, may be read C, *or* K, *or* N. And the composition of different varieties would be—

	S.	A.	Alk.	q.	Total.
$8\,S + 3\,A + C + 6\,q =$	128	54	28	54	264
$8\,S + 3\,A + P + 6\,q =$	128	54	32	54	278
$8\,S + 3\,A + N + 6\,q =$	128	54	48	54	284

$$8\,S + 3\,A + \tfrac{3}{4}K + \tfrac{1}{4}N + 6\,q = 128 + 54 + 36 + 8 + 54 = 280.$$

If such a notation as this were adopted, it would be highly desirable that there should be a general understanding among chemists and mineralogists with regard to the letters by which elements are to be designated; for we should then be able to use the formula without preface, with a great gain of brevity and clearness. The obvious method is to take the initial letter of the word for the symbol of the element, except when there are reasons to the contrary. Thus we have A, alumina; B, baryta; C, *calcia* (lime); G, glucina; L, lithia; M, magnesia; N, *natron*, or soda (for S is wanted to express silica); K, *kali*, or potassa; S, silica; Y, yttria; Z, zirconia; strontia may be represented by Sr. It may help the memory to observe that K, L, N, three letters near each other in the alphabet, represent the three alkaline earths. P is wanted for phosphorus; and K and N are thus used by Berzelius.

In these cases the earth or oxide of the base only occurs; the bases themselves, aluminium, barium, &c., not entering

into any common compounds, or into any that mineralogy is concerned with. But in the instances of the metals commonly so called, both the metal itself and its oxide or oxides occur in composition; and it would, therefore, be necessary to have symbols for both these kinds of ingredients. In cases where we have to reason concerning the quantity of oxygen, it would be necessary to have a symbol to represent that element. Thus, *fe* representing iron, and *o* oxygen, we should have *f e* + *o* for the protoxide; *f e* + $\frac{3}{2}$ *o* for the peroxide. But, in some very large classes of substances, the metals exist in the form of oxides only; and in others, they occur in the form of acids, as the molybdic, tungstic, chromic acids. It would, I think, be found more convenient to have expressions consisting of one term, than of two, in such instances. I would, therefore, represent the metals themselves by small letters, as *zi* for zinc, *mn* for manganese; and the oxides, which occur as bases, and which are analogous to the earths, by the same letters, beginning with a capital, as Z*i*, M*n*. The oxides of metals could not all be represented by single letters without confusion; and I would, therefore, in order to assist the memory by uniformity, represent each by *two* letters, taking the first letter in the name and some other prominent one. The Latin name should be used, for the sake of European communication. Thus, we should have *fe* (*ferrum*) iron, *sn* (*stannum*) tin, *cu* (*cuprum*) copper, *ag* (*argentum*) silver, *au* (*aurum*) gold. These are the symbols commonly used by those who follow Berzelius. The oxides would be F*e*, S*n*, C*u*, &c.

The bases of the acids might be represented by small letters, and the acids commonly occurring in minerals by these letters with accents. Thus we should have *s* sulphur, *s′* sulphuric acid; *c* carbon, *c′* carbonic acid; *p* phosphorus, *p′* phosphoric acid. In the same manner, *ar′* would represent arsenic acid, *mo′* molybdic, *tu′* tungstic, *cr′* chromic. The hydracids might be similarly represented, except, as before, where their constitution was required to be more expressly denoted; thus *fl′* would be hydrofluoric acid, *cl′* hydrochloric, *cl* being chlorine, &c.

Berzelius represents water (*aqua*) by *Aq*; for the sake of simplicity I have used *q*.

As examples of the use of this notation, I will take the minerals in which lead is an ingredient.

Carbonate of lead .	$Pb + 2c'$
Sulphate	$Pb + 2s'$
Arsenio-phosphate .	$Pb + p', ar'$
Molybdate . . .	$Pb + 2mo'$
Tungstate . . .	$Pb + 2tu'$
Chromate . . .	$Pb + cr'$
Murio-carbonate .	$Pb + 2c' + Pb + 2cl$ (Berzelius)
Sulphato-carbonate .	$Pb + 2c' + Pb + 2s'$
Sulphato-tricarbonate	$3(Pb + 2c') + Pb + 2s'$
Sulphuret	$Pb + 2s$

The preceding notation is intended principally for the purposes of mineralogy; in the calculations of chemistry it would be necessary to have some additional contrivances. Thus, it would be proper, as I have already observed, to indicate the mode in which both the oxides and the acids are formed from their bases, by the addition of definite portions of oxygen. For this purpose let o represent an atom of oxygen; also let h be an atom of hydrogen; and we shall have the following symbols:—

$fe + o =$ protoxide of iron (F*e*), $fe + \frac{3}{2} o =$ peroxide (F*es*.)

$mn + o =$ protoxide of manganese (M*n*), $mn + \frac{3}{2} o =$ deutoxide, (M*ns*) $mn + 2o =$ peroxide (M*nn*).

$mn + 3o =$ manganesious acid, $mn + 4o =$ manganesic.

$ar + \frac{3}{2} o =$ arsenious acid, $ar + \frac{5}{2} o =$ arsenic acid (ar')

$s + h =$ sulphuretted hydrogen, $s + 2o =$ sulphurous acid, $s + 3o =$ sulphuric (s').

$fl + h =$ hydrofluoric acid (fl') $cl + h =$ hydrochloric acid (cl').

It may occur as an objection to this system, that we have thus two ways of representing the same element, as $fe + o$ and F *e* for protoxide of iron, $s + 3o$ and s' for sulphuric acid, $fl + h$ and fl' for hydrofluoric acid. But it is to be observed, that the former symbol in each of these cases is the regular and systematic one, and the latter is merely an abbreviation, which may be employed for the sake of convenience, and which, I believe, in the cases of minerals it will generally be most simple to use. We may make such abbreviations for all the oxides

by repeating the second letter of the symbol for each additional atom of oxygen, and adding *s* (*semissis*) for half an atom. Thus M*n*, M*ns*, M*nn* are the protoxide, deutoxide, and peroxide of manganese.

In the notation of Berzelius, the atoms of oxygen are indicated by dots placed over the symbol of the base. Thus, $\ddot{fe}$, $\dddot{fe}$ are the protoxide and peroxide of iron, which he considers as having two and three atoms of oxygen respectively. This notation is compact and simple, but it is not consistent with algebraical rule, so far as the oxygen is concerned; and I conceive that, if this element be explicitly expressed, it ought to be done in the manner I have recommended above, $fe + 2\,o$, $fe + 3\,o$, &c.

The employment of different notations for the two purposes of expressing respectively mineralogical analysis, and the ulterior analysis with which chemistry is concerned, has been found so far convenient, that it has been introduced into general use by Berzelius and his followers. They have, however, embarrassed their method with rules and inventions, which very often make the relation between the two sets of symbols obscure and perplexed. Thus it is by no means, at first sight, obvious that the following pairs of symbols are identical, though the writers of whom I speak make them to be so:—

$$\ddot{\mathrm{M}}^3\,\dddot{\mathrm{Si}}^4 = M\,S\,i^2,\ \ \ddot{\mathrm{L}}\,\dddot{\mathrm{Si}}^4 = L\,S\,i^6,$$

$$\ddot{\mathrm{F}}^3\,\dddot{\mathrm{Si}} = f^2\,S\,i,\ \ \dddot{\mathrm{Fe}}^3\,\dddot{\mathrm{Si}} = F^3\,S\,i.$$

The number of atoms of oxygen which we must suppose to combine with the base in given substances is, in some cases, dependent on convention, and in others is not yet accurately determined. Hence, the view taken of many substances by Berzelius and by English chemists is different. Thus, he considers silica as a combination of 1 atom of base with 3 of oxygen, and writes it $\dddot{\mathrm{S}}i$. Most English chemists consider it as 1 atom of base and 1 of oxygen $= si + o$, for which we may write the abbreviation S.

I will now add the list of both the kinds of symbols which I have recommended, and which I hope the preceding observations have shown to be mathematically consistent and che-

mically useful. I have used the atomic composition adopted by Dr. Turner in his Chemistry.

ka = potassium	*ka* + *o* = K = Potassa.
na = sodium	*na* + *o* = N = Soda.
li = lithium	*li* + *o* = L = Lithia.
ba = barium	*ba* + *o* = B = Baryta.
sr = strontium	*sr* + *o* = Sr = Strontia.
ca = calcium	*ca* + *o* = C = Lime (calcia).
ma = magnesium	*ma* + *o* = M = Magnesia.
zi = zirconium	*zi* + *o* = Z = Zirconia.
gl = glucinum	*gl* + *o* = G = Glucina.
al = aluminium	*al* + *o* = A = Alumina.
si = silicium	*si* + *o* = S = Silica.
mn = manganese	*mn* + *o* = M*n* = Protoxide.
	mn + $\frac{3}{2}$ *o* = M*ns* = Deutoxide.
	mn + 2 *o* = M*nn* = Peroxide.
	mn + 3 *o* = M*n* = Manganesious Acid.
	mn + 4 *o* = *mn'* = Manganesic Acid.
fe = iron	*fe* + *o* = Fe = oxide.
	fe + $\frac{3}{2}$ *o* = F*es* = peroxide.
zi = zinc	*zi* + *o* = Z*i* = oxide.
cd = cadmium	*cd* + *o* = C*d* = oxide.
sn = tin	*sn* + *o* = S*n* = oxide.
	sn + 2 *o* = S*nn* = peroxide.
ce = cerium	*ce* + *o* = Ce = oxide.
	ce + $\frac{3}{2}$ *o* = C*es* = peroxide.
cb = cobalt	*cb* + *o* = C*b* = oxide.
	cb + $\frac{3}{2}$ *o* = C*bs* = peroxide.
ni = nickel	*ni* + *o* = N*i* = oxide.
	ni + $\frac{3}{2}$ *o* = N*is* = peroxide.
bi = bismuth	*bi* + *o* = B*i* = oxide.
ti = titanium	*ti* + *o* = T*i* = oxide.
cu = copper	*cu* + *o* = C*u* = oxide.
	cu + 2 *o* = C*uu* = peroxide.
ur = uranium	*ur* + *o* = U*r* = oxide.
	ur + 2 *o* = U*rr* = peroxide.
pb = lead	*pb* + *o* = P*b* = oxide.
	pb + $\frac{3}{2}$ *o* = P*bs* = deutoxide.
	pb + 2 *o* = P*bb* = peroxide.
hg = mercury	*hg* + *o* = H*g* = oxide.
	hg + 2 *o* = H*gg* = peroxide.

ag	= silver	*ag* + *o*	= Ag	= oxide.
au	= gold	*au* + *o*	= Au	= oxide.
pt	= platinum	*pt* + *o*	= Pt	= oxide.
pd	= palladium	*pd* + *o*	= Pd	= oxide.
ir	= iridium			
rh	= rhodium	*rh* + *o*	= R*h*	= oxide.
		rh + $\frac{3}{2}$ *o*	= R*hs*	= peroxide.
om	= osmium			
cr	= chromium	*cr* + *o*	= Cr	= oxide.
		cr + $\frac{5}{2}$ *o*	= *cr′*	= chromic acid.
mo	= molybdenum	*mo* + *o*	= Mo	= oxide.
		mo + 2 *o*	= Moo	= deutoxide.
		mo + 3 *o*	= *mo′*	= molybdic acid.
tu	= tungsten	*tu* + 2 *o*	= T*uu*	= oxide.
		tu + 3 *o*	= *tu′*	= tungstic acid.
cm	= columbium			
an	= antimony	*an* + *o*		= oxide.
		an + $\frac{3}{2}$ *o*		= deutoxide.
ar	= arsenic	*ar* + $\frac{3}{2}$ *o*	= *ar*\`	= arsenious acid.
		ar + $\frac{5}{2}$ *o*	= *ar′*	= arsenic.
p	= phosphorus	*p* + $\frac{3}{2}$ *o*	= *p*\`	= phosphorous acid.
		p + $\frac{5}{2}$ *o*	= *p′*	= phosphoric.
s	= sulphur	*s* + *o*		= hyposulphurous acid.
		s + 2 *o*	= *s*\`	= sulphurous.
		s + 3 *o*	= *s′*	= sulphuric.
se	= selenium	*se* + 2 *o*	= *se*\`	= selenious acid.
		se + 3 *o*	= *se′*	= selenic.
te	= tellurium	*te* + *o*		= oxide.
b	= boron	*b* + 2 *o*	= *b′*	= boracic acid.
c	= carbon	*c* + *o*	= *c*\`	= carbonic oxide.
		c + 2 *o*	= *c′*	= carbonic acid.
n	= nitrogen	*n* + *o*		= oxide.
		n + 2 *o*		= deutoxide.
		n + 3 *o*		= hyponitrous acid.
		n + 4 *o*	= *n*\`	= nitrous acid.
		n + 5 *o*	= *n′*	= nitric acid.
		n + 3 *h*	= A*m*	= ammonia.
fl	= fluorine	*fl* + *h*	= *fl′*	= hydrofluoric acid.
cl	= chlorine	*cl* + *h*	= *cl′*	= muriatic acid.
io	= iodine	*io* + *h*	= *io′*	= hydriodic acid.

In this table, the whole of the last vertical column of

symbols is to be looked upon as consisting of mere *abbreviations*, which are not indispensably necessary: they need not be used in chemistry, but they are generally convenient in expressing the constitution of minerals, as they make the formula shorter and more simple. In chemical investigations, it would generally be better to use the other or systematic symbols. In these it will be observed that none but small letters are used (capitals and accents being confined to the abbreviated symbols). All metallic elements are represented by *two* letters: the single letters used are, *b*, *c*, *h*, *n*, *o*, *p*, *s*. There is, I think, no part of the system in which any ambiguity can occur: thus S and *si* refer to silica and its base, s, $\grave{s}$, s' to sulphur and its acids: C, *ca*, lime and its base; c, $\grave{c}$, c', carbon and its acids: *cu*, C*u*, *cb*, C*b*, *cr*, *cr'*, &c. various metals and their combinations; N, *na*, soda and its base; n, $\grave{n}$, n', nitrogen and its combinations. The use, however, of the grave accent, as $\grave{s}$, $\grave{n}$, &c. for the sulphurous, nitrous, &c. acids, would be almost superfluous, as these do not occur in minerals.

As an exemplification of the above symbols, take the constitution of Alamandine and Melanite, according to Beudant—

$$\text{Alamandine} = (2\,\mathrm{S} + 3\,\mathrm{F}e) + 2\,(\mathrm{S} + \mathrm{A}).$$
$$= (2\,\overline{si + o} + 3\,\overline{fe + o}) + 2\,(si + o + al + o)$$
$$= 4\,si + 3\,fe + 2\,al + 9\,o.$$
$$\text{Melanite} = (2\,\mathrm{S} + 3\,\mathrm{C}) + 2\,(\mathrm{S} + \mathrm{F}es).$$
$$= (2\,\overline{si + o} + 3\,\overline{ca + o}) + 2\,(si + o + fe + \tfrac{3}{2}\,o)$$
$$= 4\,si + 3\,ca + 2\,fe + 10\,o.$$

The difference of these two expressions in the quantity of oxygen disappears according to the views of Berzelius, who considers S as $si + 3\,o$, C as $ca + 2\,o$, F*e* as $fe + 2\,o$, F*es* as $fe + 3\,o$, A as $al + 3\,o$. We have thus—

$$\text{Alamandine} = (2\,\overline{si + 3\,o} + 3\,\overline{fe + 2\,o}) + 2\,(si + 3\,o + al + 3\,o)$$
$$= 4\,si + 3\,fe + 2\,al + 24\,o.$$
$$\text{Melanite} = (2\,\overline{si + 3\,o} + 3\,\overline{ca + 2\,o}) + (si + 3\,o + fe + 3\,o$$
$$= 4\,si + 3\,ca + 2\,fe + 24\,o;$$

and the two expressions are now analogous.

The notation of Berzelius has already been widely diffused, and much valuable information has been embodied in it, espe-

cially on the subject of mineralogy; yet the objections to it are of the most weighty character. Its formulæ are merely unconnected records of inferences which are in some degree arbitrary; the analysis itself, the fundamental and certain fact from which the inferences are made, is not recorded in the symbol; and the connexion between different formulæ, the identity of which is a necessary and important circumstance, can be recognised only by an entire perversion of all algebraical rules. In the system which I have proposed, the fundamental analysis is the simplest shape of the formulæ; the various inferences from it are made by the most obvious changes, and the identity of these with the analysis, and with each other, is evident on the face of the notation. One method, by a misapplication of mathematical symbols, gives us a sign which can only record an opinion possibly false: the other represents simply what is certainly true, and enables us to reason from the fact to all its possible inferences, without considering anything except the notation itself. It will not long be possible to dispense with some such instrument in this country; and I should hope that what I have said may tend to induce our chemists to purify and improve the foreign system, before it is admitted to a familiarity and circulation among us, which may make the correction of its faults a task of great difficulty and inconvenience.

Trinity College, Cambridge,
March 15, 1831.

ON THE PLANT INTENDED BY THE SHAMROCK OF IRELAND.

By I. E. BICHENO, Esq., F.R.S., Sec. L. S., &c.

[Read at the Linnean Society.]

THE festival of St. Patrick has been so long recognised by those who traverse the streets of this great city, by the clover they see in the hats of the Irish, that any one who should entertain an opinion that this plant is not the original emblem of Ireland, will be thought to have no ground for

differing from the established belief; yet, I think I am in a situation to prove, by abundant testimony, that the *Trifolium repens* is not that shamrock of the Irish nation, nor any other clover, but that the wood-sorrel, the *Oxalis acetosella*, is the plant originally intended. As it is a point of some curiosity, I shall venture to lay the evidence before the Linnean Society. It would seem a condition, at least suitable, if not necessary, to a national emblem, that it should be something familiar to the people, and familiar, too, at the season when the national feast is celebrated. Thus, the Welsh have given the leek to St. David, being a favourite oleraceous herb, and almost the only green thing which is to be found in Wales at the season of his feast; the Scotch, on the other hand, whose feast of St. Andrew is in the autumn, have adopted the thistle (probably the *Carduus lanceolatus*), a plant most abundant at that period of the year. Our own patron, St. George, is a saint who has fallen so much to the leeward with us, that I do not derive any assistance from him; and I am not aware that his warlike temperament was ever represented by a plant or flower.

If the national emblem may be expected to be seasonable and familiar, the *Trifolium repens* is not a happy choice; for its leaves are scarcely expanded in the middle of March, and it produces its flowers in the summer,—its great merit in agriculture being to produce herbage during the droughts of summer and the autumnal months. Hence even in London, about which the earliest cultivation is found, we see in the hats of the *meri Hiberni* very starved specimens of the white, or Dutch clover, and sometimes the *Medicago lupulina*, and even chickweed and other plants substituted for it. But there is a still greater difficulty with regard to its being of common occurrence. None of the trefoils are naturally abundant in Ireland, but have become so by cultivation. The *Medicago* is pretty extensively sown; and the *Trifolium repens*, though now neglected by the farmer, has a wonderful propensity to diffuse itself in improved countries, and is by no means of frequent occurrence in wild uncultivated places. It is one of those plants which the Americans describe as *coming in* with cultivation. It is not a favourite, or rather there is a prejudice

against it, in America, yet it has completely naturalized itself in every dry pasture of the old states. We know that the trefoils are not of very ancient standing as cultivated plants in England; and that they were introduced into Ireland in the middle of the seventeenth century, of which a particular account is given in Master Hartlib's *Legacy of Husbandry*.

The term *Shamrock* seems a general appellation for the trefoils, or three-leaved plants. Gerard says the meadow trefoils are called in Ireland shamrocks; and I find the name so applied by other authors. The Irish names for *Trifolium repens* are seamar-oge, shamrog, and shamrock. 'This plant,' says Threlkeld, who printed the earliest Flora we have of the country, "is worn by the people in their hats upon the 17th day of March, yearly, which is called St. Patrick's day; it being a current tradition, that by this *three-leafed grass*, he emblematically set forth to them the mystery of the Holy Trinity. 'However that be, when they wet their *Seamar-oge*, they often commit excess in liquor, which is not a right keeping a day to the Lord, error generally leading to debauchery.'

The *Trifolium pratense* is called, in the statistic report of the county of Tyrone, the *horse shamrock*, evidently from its size. Threlkeld's and Keogh's Irish names (which are the best authority) for the *Oxalis acetosella* are so like those which are given to the *Trifolium repens*, both in spelling and sound, that they must be the same. Thus we have Threlkeld's names *Scumsog* and *Samsog;* while Keogh gives for the same plant *Samsogy* and *Shamsoge*.

In Gaelic the name *Seamrag* is applied by Lightfoot to the *Trifolium repens;* while, in the Gaelic Dictionary, published by the Gaelic Society, under the word *Seamrag*, many plants are mentioned to which this word is prefixed as a generic term, as *Seamrag chapuill*, purple clover; *Seamrag chré*, male speedwell; *Seamrag m'huire,* pimpernell. I conclude from this, that shamrock is a generic word common to the Gaelic and Irish languages, and, consequently, not limited to the *Trifolium repens*.

The poets, too, have made use of the word, as I find in a quotation made in the Gaelic Dictionary, from an ancient Gaelic poem in Smith's collection,

——Air an t seamrag's agus an neòinean
Santig aisling na h-oige a' m' choir.

The translation of which I find to be, 'On the shamrock, and amidst the daisies, when the dream of youth shall come unto me.' Now I would suggest, that either the word shamrock here employed is not the *Trifolium repens*, as is thought, and which its connexion with the daisy would lead me to infer; or, the poem is not so ancient as has been supposed; for the *Trifolium repens*, probably, nay almost certainly, was not common in Scotland before the middle of the seventeenth century.

In the early Irish authors we find the shamrock mentioned incidentally. I will take the liberty to quote a passage from Spenser's *View of the State of Ireland*, to prove that it was a plant eaten by the Irish, which is very unlikely to have been the case with any of the clovers. It is a description of the state of the poor Irish during the great Desmond war in Elizabeth's reign:—'Out of every corner of the woods and glynnes,' says he, 'they come creeping forth upon their hands, for their legs could not bear them, they looked like anatomies of death, they spoke like ghosts crying out of their graves, they did eat the dead carrions, happy where they could find them, yea, and one another soon after, insomuch as the very carcases they spared not to scrape out of their graves; and if they found a plot of watercresses or *shamrocks*, there they flocked as to a feast for the time, yet not able long to continue there withal, that in short space there were none left, and a most populous plentiful country suddenly left void of man and beast; yet sure in all that war there perished not many by the sword, but all by the extremity of famine, which they themselves had wrought.' That shamrocks were eaten, appears from various other authors, as in the following couplet from Wythe's *Abuses Stript and Whipt*, 8vo. Lond. 1613, p. 72, quoted in Brand's *Popular Antiquities*, by Ellis, p. 90:

And, for my clothing, in a mantle goe,
And feed on sham-roots, as the Irish doe.

So the author of the Irish *Hudibras*, printed 1689, says—

Shamrogs and watergrass he shows,
Which was both meat, and drink, and close.

And again, p. 31—

Thus hotly they pursued the scent,
Cramming their gorges as they went;
Until they cropt the very weed,
Where every day they used to feed.
Nees, when the shamrog he did spye,
Cries out, I have it in my eye,
Is vid me fait. And so he run,
To bring the presents to the nun.

My next quotation shall be from Fynnes Morrison, who went over to Ireland, in 1598, with the Lord-Deputy Mountjoy, to quell the Earl of Tyrone's rebellion; by which it will appear that the shamrock was not only eaten, but that it was a sour plant. It may also be inferred, as it was eaten after the winter stock of provisions, that it was a spring plant. 'Yea, the wilde Irish in time of greatest peace impute covetousnesse, and base birth to him, that hath any corn after Christmas, as if it were a point of nobility to consume all within those festival dayes. They willingly eate the hearbe *shamrocke*, being of a sharp taste, which, as they run and are chased to and fro, they snatch like beastes out of the ditches.'

If the shamrock should be proved, by a more diligent search into the old authorities, to have been a wood plant, this circumstance would materially corroborate the view I have taken, as the *Trifolium repens* is never found in such situations. The only authority for the fact, I have yet discovered, has been pointed out to me by my friend, Mr. E. T. Bennett, in the Irish *Hudibras*, where the plant is twice mentioned as being found in a wood:—

Within a wood, near to this place,
There grows a bunch of three-leaved grass,
Call'd by the boglanders sham rogues,
A present for the queen of shoges (spirits).

p. 23, and again at p. 30.

Nor is it difficult to account for the substitution of the one plant for the other. Cultivation, which brought in the trefoil, drove out the wood-sorrel. The latter, though now not common, was, doubtless, an abundant plant as long as the woods remained; but these being cut down, partly by the natives to supply their wants, and partly also by the government to prevent their enemies from taking refuge in them in the wars, the

commonest plant became the scarcest, and it was more easy to obtain that which was cultivated.

Upon the whole view of the case, I apprehend it can hardly be doubted, that the *Oxalis acetosella* is the original shamrock of Ireland. It possesses, in the first place, all the qualities to recommend it as appropriate for the national feast, and is even more beautifully three-leaved than the clover. It is abundant, and comes at the proper season, being one of the earliest plants, and pushing forth its delicate leaves and blossoms with the first spring. It was also eaten; while its flavour, too, answers exactly to the description of Morrison, which is a great point to assist in fixing its appropriation; and to the old Irish, who lived chiefly upon flesh, it must have been a most acceptable diet. It would be impossible to find any plant throughout the vegetable kingdom better entitled to become national; and I think it cannot be questioned, that St. Patrick, who is said by Gibbon to have been descended, and to have derived his name, from the patricians of Rome, exercised a good taste, worthy his noble birth, when he selected so beautiful an emblem for his favourite island.

ON THE EARLIEST EPOCH OF EGYPTIAN CHRONOLOGY.

[Being an Extract of a Letter from JAMES RENWICK, LL.D., Professor of Natural Experimental Philosophy in Colombia College, New York, to Captain EDWARD SABINE, R.A., F.R.S.]

ON a former occasion, I mentioned to you that I had undertaken the examination of the Hieroglyphic System of Young and Champollion, with a view to the discovery of the epoch whence the colonization of Egypt may date, along with the origin of its history. I am now enabled to transmit to you a more full exposition of the views I entertain on this interesting subject. These are, in this form, at your service, to make such use of them as you may think proper.

As you may recollect, I conceived that I had, by means of four distinct and independent methods, arrived at a close coincidence in the date, whence we are to date the earliest traditions of the Egyptians. These are as follows—viz.:

I. The principle upon which it is stated by ancient authors, that the commencement of the agricultural and astronomical

year of the Egyptians was determined; a principle that was only true at a remote period, and has since ceased to be applicable.

II. From the length assigned to the Sothic Cycle, at the end of which the beginning of the civil and astronomic years returned to the same day. A length which was given by the ancient authors is correct only between certain epochs, and was not true at those which were more remote, nor consistent at any time with the true length of the tropical year.

III. From the groupe of Zodaical stars assigned as the place of the sun, at the beginning of the agricultural year of the Egyptians, excluding all dates previous to his being in this groupe at the time of the rising of the Nile.

IV. From a version of a remarkable passage in Herodotus; a version in which I had the aid of my learned colleague, Professor Moore, and which I, at the time, believed had the merit of originality. You, however, inform me, that it has been construed in a similar manner by St. Martin, and this coincidence adds no small weight to the views I at that time supposed I entertained unsupported.

I. Before the introduction of calendars founded upon astronomical tables, it was the universal custom of the nations of antiquity to regulate their agricultural labours by the heliacal rising of remarkable stars. In the Egyptian climate, the whole of these were also determined by the phenomenon of the rising of the Nile. The country, which, without this happy provision of nature, would be absolutely desert, a mass of barren and inhospitable sand, and which suffers from famine when the inundation does not reach its mean height, is annually restored to cultivation by its means, and rendered one of the most fertile regions of the earth. The moisture with which the inundation charges the earth is long kept up by abundant dews, that the alternating excesses of solar and terrestrial radiation, during the day and night, give rise to; but, at the approach of the summer solstice, these naturally lessen, and, finally, cease altogether, vegetation loses its support, and the fertile fields assume the appearance of a sandy waste. Hence, the rising of the Nile is watched for, with the most intense expectation, not merely on the neighbouring shores of the river, but on the furthest frontier of the country, whence the joyful tidings are transmitted with all possible celerity. Hence, too, the astronomic pheno-

menon, supposed to coincide with the overflow of the Nile, became an observation of the greatest interest; and the star, in whose heliacal rising it consisted, passed into an object of worship.

Of this fact, and of its reason, many authorities may be adduced, even from the few authors that have come down to us, who have treated directly or incidentally of the affairs of Egypt. The star was the Dog Star, the Ἀστροκύων of the Greeks, the Sothis and Soth of the Egyptians; a star sacred to the goddess Isis, and probably worshipped itself under the form of Anubis—

Oppida tota Canem venerantur.—*Juvenal.*

From the rising of this star they were accustomed, according to Horus Apollo, to predict the occurrences of the following year. So also in *Cicero de Divinatione*, lib. i.:—"Eos accipimus ortum caniculæ diligenter quotannis solere servare, conjecturamque capere (ut scribit Ponteius Heraclides), salubris ne, an pestilens annus futurus sit." Likewise in *Porphyr. de Nymph.*, as quoted by Sir John Marsham:—"Ægyptiis principium anni, non Aquarius, ut apud Romanos, sed Cancer. Nam prope Cancrum est Sothis, quam Græci Canis-Sidus dicunt, Neomenia autem est ipsius Sothidis ortus, quæ generationis mundi ducit initium." By the testimony of Censorinus, we find that the epoch of Egyptian chronology was the coincidence of the first day of the vague year with the rising of Sirius; and we find, in Diodorus Siculus, a tradition of the priests, by which the rising of the Nile is connected with the appearance of Sirius. It is, however, useless to multiply citations to illustrate the admitted fact, that the heliacal rising of Sirius was considered as corresponding with the commencement of the inundation of the Nile.

It may then be concluded, that, at the time the wants and interests of the first settlers of Egypt led them to endeavour to connect the most interesting period of their seasons with astronomic phenomena, these two phenomena were so near to each other, that the appearance of the star might be taken as the sure prognostic of the rise of the river. This, however, is far from being the case at present; the inundation has already reached a considerable height before the star becomes visible, and the latter can no longer serve as an astronomic presage of an event that occurs subsequent to it.

The rising of the Nile is gradual, and is first to be remarked

at the higher parts of the stream, and hence it is unnecessary that the astronomic forerunner should be actually prior to the beginning of the inundation. This is more particularly the case in central and lower Egypt, where, even if the rising of Sirius did correspond with the first swell of the Nile on the frontiers of Nubia, some days must elapse before the connexion would be detected.

We may, therefore, consider that we are warranted in ascribing to a system, in which the two appearances, however dissimilar in cause, were considered as identical in point of time, an origin no farther distant than the period in which they were actually contemporaneous. The rise of the Nile, growing out of the tropical rains, follows in its law the tropical year, and recurs, on an average, on a fixed day of our present calendar. The heliacal rising of a star, on the other hand, is affected by the precession of the equinoxes, and, in consequence, recurs later every year than it did the preceding. But it is not governed by the sidereal year exactly; for, as the declination of stars alters, as well as their right ascension, the interval between the successive risings of the same star will not have a constant length corresponding to the last named period; but it will vary, being sometimes longer, and sometimes shorter; in respect to Sirius, this interval, as we ascertain from the calculations of Larcher, which have been confirmed by Biot, was, for from twenty to thirty centuries before the Christian era, exactly 365¼ days, being greater than the tropical, and less than the sidereal year. The difference, then, between the real length of the year marked by the star, and that determined by the rising of the Nile, will be the same as that known to exist between the Gregorian and Julian calendars, or three days in 400 years.

Now, the rising of the Nile below the cataracts, although usually referred to the solstice, actually occurs, at the isle of Philæ, at an average, on the 25th of June. This, therefore, is the earliest day to which we are warranted in referring the observation of the rising of Sirius, upon which the coincidence of the two phenomena is founded; while we are almost authorised to place it even later, as the star would otherwise have been seen at Thebes or This, before the increase of the Nile could have been perceptible.

The heliacal rising of Sirius is fixed by Censorinus as having happened, in the year 139 A.D., on the 20th of July; and the truth of this statement is amply confirmed by astronomic calculation. Between this date and the 25th of June there intervene twenty-four days, which is a difference that will take place between the Julian and Gregorian calendars in 3200 years. The observation cannot, therefore, be carried back farther than 3060 years before the Christian era; and if made by simple inspection of the river, instead of being referred to the marks upon a Nilometer, may have occurred 200 or 300 years later, particularly if made at This, instead of being observed at the frontiers of Nubia.

This is the first of the investigations by which I conceived myself warranted in restricting the earliest settlement of a colony in Egypt to about 2800 years before the Christian era.

II. The year of the Egyptians differed from that of any nation of antiquity whose records or traditions have come down to us. Herodotus informs us*, 'That the Egyptians were the first of men who invented the year, and divided it into twelve months; and this they found out by means of the stars. In this they seem to have acted more prudently than the Greeks;' for the latter 'intercalated every third year, but the Egyptians annually add five days to the twelve months of thirty days each.' Diodorus Siculus gives us another form of the year†: 'They say that they are the most ancient of nations; and that philosophy and astronomy were by them invented, the situation of their country assisting them to ascertain more clearly the rising and setting of the stars. The months and years are, however, arranged by them in a peculiar manner: for adapting their days, not to the motion of the moon, but to that of the sun, they attribute thirty days to each month, but after each twelfth month they intercalate five days and a quarter; and thus complete the circle of the year.'

This apparent discrepancy is explained by another ancient author‡:—'For they were desirous that the festivals of the gods should not be represented always at the same season, but they wished them to revolve through every period of the year;

* Lib. ii. cap. 4. † Lib. i.

‡ Geminus, as quoted by Marsham and Witsius.

that the same festival might at one time occur in summer, at another in winter; and again in spring, and in autumn. On this account they do not insert the quarter of a day, in order that the religious solemnities may retrograde.'

Thus we find the earliest authorities citing two different years, which are, as will be seen, reconciled by one of later date. The first of these was such that it circulated throughout the seasons, and is hence called 'vague;' the second was supposed to coincide with the course of the sun. A year of 365¼ days, however, does not coincide with the tropical year, nor had the true length of the latter been discovered by any people of remote antiquity. The close coincidence that the Egyptians attained to, cannot be considered as due to observations of the sun; for it is obvious, from various circumstances, that their astronomy did not go this length, but arose from the return of the heliacal rising of Sirius, which, as has been seen, during the flourishing periods of the Egyptian kingdom, occurred at exact intervals of 365¼ days. The same observation gave them originally a year of 365 days, for the discovery of which but few years would have been sufficient, and afterwards enabled them to ascertain its error.

How they reconciled these two species of years in their chronology we learn from Strabo:—'The Thebans, particularly the priests, are said to be astronomers and philosophers; it is their custom to reckon the days, not by the course of the moon, but by that of the sun. They annually add five days to their twelve months of thirty days each; but since a certain fraction of a day exceeds the complement of the whole year, they make a period of such a number of years, that the exceeding fractions may make up a year.'

Such a period would be equal to 1460 Julian or 1461 vague years, and it formed the famous Sothic period of the Egyptians, the Cynic cycle of the Greeks, and the Canicular of Latin authors. It took its origin when the first day of the vague Egyptian year coincided with the rising of Sirius, and closed when the same coincidence again occurred. This coincidence did occur, and the cycle terminated in the year 138 A.D. The origin is, therefore, to be found in the year 1322 before our era. Between these dates it was used as an ordinary mode of

computation, by which the vague year was reconciled to the changes of the seasons; this is evident, from a quotation from Manetho, taken by Marsham from Jamblicus, and from the ancient chronicle, quoted by Syncellus, in both of which the great period of 36,525 years, or twenty-five *generations* of the Cynic Cycle, is referred to. If used at all, it must have been employed as early as the commencement of this cycle, and of its use the contemporary testimony of Manetho is conclusive evidence.

The Egyptians, I think it is evident, had made but little progress in the astronomy of observation. The names and places of separate stars, and of several groupes, were known to them, and the year of 365¼ days. Much further they do not appear to have gone, as Ptolemy was compelled to have recourse to Chaldean records for the facts on which his work is founded. It appears, therefore, probable, that a cycle which actually formed a basis for the computation of a greater period, in which the year of Sirius, the vague year, and the lunar motions, again returned to the same epoch, must have been obtained by slow and long-continued experience, which was, therefore, long prior to the date of the origin of the cycle that terminated in A.D. 138. The happy superstition of the priests,—which led them to avoid restoring the vague year to that of Sirius, as soon as the difference was detected, but allowed their festivals to circulate through the different seasons,—enables us to proceed back to the first year of the previous cycle, when the first day of the year again coincided with the heliacal rising of that star. As the rising of Sirius marked the beginning of the agricultural year, to which the vague year was restored by a cycle, the first year of 365 days counted in Egypt must have fallen on the first year of some given Sothic period. It could not have fallen in 1322 B.C., for it was, at that time, used as a mode of computation; nor could it have fallen earlier than 2782 B.C., because prior to that period the cycle would not have been 1460 years.

That the cycle ending A.D. 138 was the only one actually used as a period in the formation of a calendar, we have strong additional evidence in a passage of Clemens Alexandrinus. Speaking of the exodus of the Israelites, he places

it, not in a given year of a former cycle, but in the 345th year before *the* Sothic period; precisely as we cite events of ancient history, as happening a given number of years before *the* Christian era. And, in conformity, the extract cited by Biot from a manuscript of Theon of Alexandria, in the Royal Library of Paris, takes, as the epoch of his calculations, the reign of Menophres, in which the cycle was renewed, or the year 1322 B.C.

Thus, then, I conceive, I am warranted in my conclusion, that the heliacal rising of Sirius was actually observed in Egypt in the year 2782 B.C.; and that, on the other hand, this was the first year of 365 days reckoned in that country.

To whom this mode of computation, so different from that of other nations, is due, is a matter of curious inquiry. Strabo informs us, in the sequel to the passage that we have already cited, that the Egyptians attributed their knowledge of the year to Mercury. In the first dynasty of Manetho, the second personage is Attothes; the Thoyth of the Egyptians, the Thoth of the Alexandrians, the Hermes of the Greeks. This Mercury, therefore, was the son of Menes, and the second king of Egypt. Cicero enumerates five Mercuries *. 'The fourth was the son of Nilus, whom the Egyptians consider it impiety to name; the fifth, whom the Phenicians worship, who is said to have slain Argus, and was for that reason appointed to rule over Egypt, and to have given the Egyptians laws and letters, him the Egyptians call Thoyth; and the first month of the year is by them called by the same name.' We find no other ascription of the introduction of the year to this celebrated personage; but the other benefits that he conferred on mankind are the subjects of frequent allusion. Sanconiatho directly names him as the inventor of letters †; the same is done by Philo ‡. 'I have heard,' says Socrates, in Plato's Phædo, 'at Naucratis in Egypt, that there was an ancient god, to whom the bird they call the Ibis was sacred; the name of this god is Theuth; he first invented numbers and the art of reckoning, geometry and astronomy, and the games of draughts and dice.'

* § Nat. Deor., lib. iii.

† Eusebii Prep. Evangel., lib. i. ‡ Ibidem.

And so in Diodorus Siculus*:—'He first distinguished the articulate sounds of language, and gave a name to many things before destitute of name; he invented letters; prescribed sacrifices and the worship of the gods; he first observed the course of the stars, and the nature and harmony of sounds.'

'Letters,' says Pliny†, 'I think always existed among the Assyrians; but others, as Gellius, think they were discovered among the Egyptians by Mercury; others again among the Syrians. Anticlides says that they were invented in Egypt by a person of the name of Meno, fifteen years before the time of Phoroneus, the most ancient king of Greece, and endeavours to prove it from monuments.' In the last passage we find attributed to Menes the father, what was due to the son.

This extract from Pliny derives considerable interest from modern discoveries. He states the discrepancy of opinion, by which it seems, on the one hand, that the Assyrians had always possessed letters, while, on the other, they were ascribed to the Egyptians as inventors. We now know that the writing of the Nile and Euphrates, if, perhaps, equally ancient, were founded upon totally distinct principles; the former being composed of the resemblance of physical objects, originally extremely numerous; the latter of but two arbitrary and simple symbols, made to express varieties of sound by their position in respect to the horizon. It has so happened that the former, happily simplified, seems to have extended its influence throughout the greater part of the world, while the latter, although obviously preferable, has sunk into such entire oblivion, that even to decypher it mocks the industry and patience of the most learned. From the Egyptians, the Hebrews and Phenicians obviously borrowed the principle on which their alphabets were founded; hence proceeded the Greek, the Roman, and the alphabets of modern Europe. Hence also, on the other hand, diverged the Arab, and all the characters of the present civilized nations of Asia, except the Chinese. Thus, then, if in days of ignorance and debasement, the elevation of the creature to honours due only to the Creator could be pal-

* Diodori, lib. i. † Plinii lib. vii.

liated by the magnitude of the benefits which he became the instrument of conferring upon our species, that superstition which raised Attothes to the rank of a deity appears to have the greatest justification.

Who, then, may it be inquired, was the father of this illustrious personage, his predecessor on the throne of Egypt? Syncellus, in his dynasties, confounds him with Misraim, the son of Ham, by whose name Egypt was called. Others, again, consider him identical with Ham himself. Neither of these names, however, has any similarity to that of Menes. It may, therefore, be proper to search, whether there be any name in the genealogies of scripture, to which we can refer that of Menes, or, as he is sometimes styled, Anamenes. This last form is found in that of Anamim, among the children of Misraim *. The identity, when the Greek termination is removed from the one, and the form of the Hebrew plural from the other, is complete. Menes, then, was the third in descent from Noah, and bore the same relation to the common progenitor with Nimrod, the son of Cush, the first who assumed royal authority in Asia; and, by our previous computation, it appears that his reign must have closed about 2782 B.C.: we may even place his death later; for some of the expressions in relation to the inventor of letters, would admit that he was not actually reigning at the time the discovery was made.

It remains that it should be shewn that this date is consistent with the chronology of scripture. In England, I am aware that the chronology of Usher is now exclusively adopted, and as this places the flood in the year 2348 B.C., all such remote antiquity is excluded. It is not, however, so in other countries; the French let another chronology run parallel with that of Usher in their best tables, and both appear to be admitted as of equal authority by the Catholic church, while, by the Greek, the latter alone is accounted authentic. Thus, then, the question is one that is so far open, that it may be examined without incurring the suspicion of wishing to interfere with matters of faith, or the received interpretation of scripture.

* Genesis x. 13.

These two methods of computation are founded, the one on the Masorete Hebrew text, the other on the version of the Septuagint. The ancient controversy on their respective authorities, so far as date is concerned, is well known; whichever of them has been altered to suit the opinion of its guardians has been changed with such skill, as to throw out of use all the usual methods of critical emendation, by the aid of the context. From this, however, may be accepted the name of Cainan, which is found twice in the Septuagint, and but once in the Hebrew, and who, not being found after the flood, in any version but that of the Septuagint, may be rejected from the list.

To compare these two chronologies, the following list may be made use of. The first name is placed opposite the number of years his birth dates after the flood; the remainder opposite to the age of the father, at the time the person named was born.

	HEBREW.	LXX.
Arphaxad	2	2
Salah	35	135
Heber	30	130
Peleg	34	134
Ren	30	130
Serug	32	132
Nahor	30	130
Terah	29	79
Nahor, elder brother of Abraham	70	70
	292	942

The difference between these two computations is 650 years; and, if that of the Septuagint be received, the flood, which, according to Usher, took place in 2358 B.C., is carried back to 2998 B.C.

This text is to be preferred for several reasons. The first of these is almost obvious from mere inspection. It consists in the fact, that the ages of the parents at which the children are born, fall off, according to the Hebrew, suddenly after the flood, and again increase in the case of Terah and Nahor; while, in the Septuagint, although lessened from what they were before the flood, they still keep up the semblance of that

gradual change, that we have a right to infer took place, in the growth and longevity of the human species, under the new circumstances in which they were placed.

The next reason is to be found in collateral evidence. Besides the Hebrew Text and the Greek translation, a nation, equally hostile to the Christian religion and Jewish people, has existed from the date of the captivity to the present time, and a small remnant still remains in their original seats in Palestine. This is the Samaritan, who, receiving no others of the sacred books but those that compose the Pentateuch, have, with scrupulous care, preserved ancient copies of them. The dates and genealogies of this text are identical with those of the Septuagint, if the postdiluvian Cainan be withdrawn.

Josephus was himself a Jewish priest of the highest class, and had access to the sacred writings in the very last year of their preservation in the temple. His chronology may, therefore, be considered as founded upon his best recollection of the numbers he had there seen. The sum of his numbers is 993, which exceeds the computation of the Samaritan and Septuagint; and therefore confirms their deviation from the Hebrew. It has been recently attempted to amend his text, by conjectural criticism, and make it correspond to the Samaritan. This, however, is unnecessary, for the present argument; it is sufficient to shew that he confirms the general truth of the longer computation, even if not identical with it in his numbers. A third reason might be found in the calling of Abraham, which, according to the computation of Usher, must have taken place during the life of Shem; for he lived 502 years after the flood, and the death of Terah, according to the Hebrew text, took place no more than 427 years after that important event. Although we might not presume to scrutinize the acts of infinite wisdom, still it may be permitted to state, that the necessity for a renewal of the promise could hardly have occurred, while there was a living witness of that made to Noah, upon the earth, and in the very family of him with whom the new covenant was to be made.

Another text of the Septuagint carries the flood back one hundred years further, or to 3098 B.C.

The former determination, of 2998 B.C., is, however, sufficient for our purpose. The family of Noah was speedily disunited, in consequence of the filial irreverence of Ham, and a curse was pronounced by the indignant parent on the end of the latter. Hence it requires no effort of reason to believe that Ham speedily sought the country that became his apanage. This is perfectly consistent with scripture, for Egypt took its name from Misraim, who was three generations prior to Peleg, in whose days the confusion of tongues took place. In this branch of the family, too, life was more speedily reduced to the present standard than in that of Shem, as is evident, from the astonishment with which the longevity of Jacob was regarded in Egypt.

If seventy years be allowed to a generation among the descendants of Ham, the third, whom we have held to be Athothes, would have been of the age of seventy-four in the year 2782, B.C., or 216 years after the flood; and of course competent to the highest exertion of his mental energies. This rapid decrease in longevity in Egypt is evident, from the dynasties of Manetho, the first king, having a reign of sixty-two years; the second Athothes, of fifty-seven; while the third falls off to thirty-one: this, if we allow one hundred to Misraim, or thirty less than to his cotemporary, Arphaxad, will correspond with seventy years to a generation, at a mean rate. Thus, then, the origin of the Egyptian year, in 2782 B.C., is fully consistent with the best supported text of scripture, and even agrees in a most remarkable, and I believe hitherto unnoticed, manner with its genealogies.

III. The third mode of determining the epoch of Egyptian chronology, is derived from a passage of Biot, in relation to the groupe of zodiacal stars, in which the sun is situated at the time of the heliacal rising of Sirius. This it may be as well to cite, particularly as it contains quotations that bear upon other parts of my argument.

Speaking of the work in the Greek language, which bears the name of Horus Apollo, he says *, 'The low antiquity of this

* Recherches sur plusieurs points de l'Astronomie Egyptienne. Paris, 1823, p. 203.

composition may be inferred from a passage when the true epoch of the author discovers itself.' 'The Egyptians,' says he, 'distinguish this phenomenon by the emblem of a lion; because, when the sun enters into the Lion, the swelling of the Nile becomes very considerable; and while it remains in this constellation (ζω δίῳ), the inundation often attains two-thirds of its total height.' Now, according to the testimony of all voyagers, from Herodotus to our own days, the Nile begins to swell below the last cataract, immediately after the summer solstice. Forty or fifty days elapse before it attains the half of its greatest height, and it does not reach its last limit of its increase until about one hundred days after the solstice. Consequently, at this first phase of the swelling of the Nile, which the passage cited marks as being already considerable, the solstice must already be past a considerable number of days; it would have been at a distance of thirty days, if, for instance, this place be supposed to correspond to one-third of the total height. Now, since, according to our author, the sun ought to be, then, in the commencement of the Lion, if we suppose he cites this emblem as a *sign*, that is to say, a twelfth part of Zodiac, it would be necessary that the solstice, falling thirty days earlier, should take place in a point of the ecliptic, that is, 30° more to the west, this carries it to the beginning of the sign Cancer: 'and as this disposition, which places the two equinoxes, and the two solstices at the commencement of the signs, was not generally adopted until after Hipparchus, it follows that the work which employs it as such, must have been written after the time of this astronomer.'

* * * * *

Having determined by this physical indication the low antiquity of the work of Horus Apollo, we shall examine hereafter what he says of the relations of Syrius with the Egyptian year; but I prefer, first, to discuss a passage of the Scholiast of Aratus, which seems much more proper to enlighten us on the real nature of these relations. This scholiast, who is believed to be Theon of Alexandria, expresses himself in the following manner *:—'The Etesian winds,' says he, 'invade

* Arat. Phen. Schol. on verse 153, ed. Lips. p. 45.

'the sea, when the Sun is the sign of the Lion; and among the 'Egyptians, the keys of the temples bear the figures of a lion, 'from which hang chains, to which a heart is attached. They 'have consecrated the whole of this constellation (ἄστρον) to the 'Sun. For then the Nile spreads beyond its banks, and the 'heliacal rising of the Dogstar takes place towards the twelfth 'hour*. They place at this instant the commencement of the 'year; and consider the Dogstar, as well as its rising, as con-'secrated to Isis.'

'The word employed by the author (ἄστρον) seems to indicate that he wishes to consider the Lion as a *constellation,* and not as a *sign;* but the heliacal rising of Sirius in Egypt, of which he makes a circumstance co-existing with the presence of the Sun in the Lion, finally confirms this sense, by showing that it is of the constellation that he speaks. In fact, the author of the Scholia lived towards the fourth century of the Christian era, and he cites the rising of Sirius as present, and taking place in his own time. Now, at this epoch, when Sirius rose heliacally in Egypt, which happened about twenty-seven days after the solstice, the Sun was no longer in Leo considered as a sign, but he had the same longitude with the stars of the head of the Lion. For, by an astronomical circumstance that has not hitherto been remarked, but of which I shall presently give the demonstration, from more than 3000 years before the Christian era, until more than 1000 years after that era, the Sun has *always* been in the same *constellation,* Leo, but in very different parts, at the time of the year in which the heliacal rising of Sirius takes place in Egypt.' At an epoch prior to thirty centuries before the Christian era, the Sun would have been in the groupe of Virgo at the time of the rising of Sirius; and hence the use of the hieroglyphic, or rather anaglyph, explained by Horus Apollo, could not have arisen at an earlier date, and the claims set up to a much more remote antiquity fall to the ground.

Biot also cites the passage of Porphyry, that has already been quoted; and in which, if we conceive that he has, as is probable, united the traditions of the ancient Egyptians with

* An hour before sun-rise.

the improved astronomy of his own day, there is strong corroboration of our views. The citation is therefore repeated.

'The Egyptians do not commence their year, like the Romans, with Aquarius, but with Cancer; for near Cancer appears the star Sothis, which the Greeks call the Dogstar; and the rising of the Dogstar is with them the renewal of the year, because this star rules over the epoch of the nativity of the world.'

This indication becomes still more precise; for, according to the calculation of Biot, from 2800 B.C. to 1000 A.D. the Sun has always been in the *sign* Cancer, at the period of the year at which Sirius rose heliacally in Egypt. And this did not, at the time Porphyry lived, take place when the Sun was in the first point of Cancer. Hence, when the Alexandrian school fixed the epoch of their year at the entrance of the Sun into Cancer, they must have referred to a circumstance that did not exist in their own day, but which had occurred 2800 years before the Christian era. Here, then, we again find an astronomical epoch, of a date closely coinciding with the two already determined.

IV. The passage in Herodotus is very remarkable, and its meaning has been much disputed. Some, from its appearing to involve an apparent absurdity, have been for rejecting it as a fable; while others have sought in it a hidden meaning, whence the date of the origin of the Egyptian monarchy may be deduced. The information of Herodotus was derived from the Egyptian priests, and he does not appear to have himself credited their statements; still, however, he is not content with detailing their communications simply, but adds comments of his own, which obscure the sense that the mystic expressions of the priests were intended to convey. Thus, in the earlier part of the passage, he informs us, that from Menes, the first mortal who reigned in Egypt, they counted three hundred and forty-one generations, and during this long series of generations a similar number of kings and priests. On this he founds a calculation that these reigns comprised the vast period of 11,340 years. But the calculation is obviously his own; and if it be admitted that, before the conquest of the shepherd kings, Egypt contained several kingdoms, as is most

probable, the number 341 does not appear excessive for the time that has been deduced from other considerations. He then goes on to state—'During this time, then, they said the Sun has four times risen out of his customary places; that both where he now sets he had there twice risen, and where he rises he had there twice set; that this had not produced any change in Egypt; that the productions of the earth and the inundations of the Nile had been the same; and that there had neither been more disease, nor a more considerable mortality.'

This change in the rising and setting of the Sun, without producing any change in the seasons or inundations of the Nile, is mysterious in appearance; but a reference to the nature of the Egyptian year will render it at once obvious. All that is to be remarked previously is, that the two clauses of the passage are in contradiction with each other, and that we, therefore, again see the double expression of the recital of the priests, and the comment of Herodotus. A change in the place of rising is attended with a corresponding one in that of setting, and, therefore, by the last clause, which is in detail, there are but two changes instead of four. The first, therefore, is, from its vagueness to be rejected in favour of the circumstantial account in the latter. Let us, then, see whether this last account of the change be consistent with astronomic phenomena. On the first day of the first vague year of the Cynic Cycle, marked by the heliacal rising of Sirius, the Sun was in the constellation of Leo, which was then *his accustomed habitation*, or ἦθος. At the end of 730 vague years, or at the beginning of the 731st of the cycle, the Sun would be in opposition to the stars of the constellation Leo, and would of course rise with that point of the celestial sphere which, on the same day of the vague year, at the commencement of the cycle, had set as he arose; and would set at that point of the celestial sphere which, at the former epoch, had risen at his setting. The change, therefore, spoken of by the Egyptian priests, would have occurred for the first time; at the end of 1460 Julian years, he would again be in the constellation Leo at the time of the rising of Sirius; but at the end of 730 vague years more, he would be in the position in

which he had been at the beginning of the 731st year of the previous cycle, and the same change would now be effected a second time. Within the space, then, of 2190 years the sun will have twice arisen, on a given day of the vague Egyptian year, where he had at first set, and twice set where he had before risen. Herodotus informs us, that the period of these changes was included between the reign of the first king, and that of Sethos, priest of Vulcan. The latter was rewarded by Sennacherib, the date of whose reign is well established at about 700 years B. C. Thus the most remote date that this passage will permit us to assign for the beginning of the reign of Menes is 2890 years B. C. It is, in addition, to be considered, that the year of 365 days was the invention of his successor, Attothes, and that every day by which the year fell short of that number of days will tend to reduce the length of this period; and hence the estimate deduced from the passage of Herodotus is easily reconciled with that of 2782 B.C., which has been deduced by other methods, as the close of the reign of Menes.

Neither of these modes of computation may, when standing by itself, be of any great value, but, when united, they struck me as furnishing a most convincing evidence, if not of the exact time of the origin of regal government in Egypt, at least of an antiquity, that however high it may be when compared with that of the nations whose authentic history has come down to us, is yet fully within the chronology of the sacred volume. Their close and remarkable coincidence was wholly unexpected by me, when I first took up the investigation, for it was hardly to be anticipated, that in the vague and scattered notices that have descended to us, of the origin and antiquity of that mysterious people, anything that would point out an exact chronological epoch could have been gleaned. I must say, that the results are still a matter of surprise even to myself: I cannot, however, avoid entertaining the hope that the singular coincidence thus obtained by four separate and distinct methods is not a matter of pure accident, but has really an important bearing upon the date of the settlement of Egypt, and thus upon the connexion of sacred and profane history, and the disputed chronology of those remote ages.

AN ACCOUNT OF A REMARKABLE INSTANCE OF ANOMALOUS STRUCTURE IN THE TRUNK OF AN EXOGENOUS TREE.

By JOHN LINDLEY, Esq., F.R.S., &c.
Professor of Botany in the University of London.

THE following case will, perhaps, be found to offer an interesting proof of the manner in which the wood is formed in the trunks of Dicotyledonous, or Exogenous Trees :—

In the year 1828, I was informed that a poplar-tree had been felled in a small court belonging to Mr. Nicol, near the Palace of St. James's, which exhibited the singular anomaly of *one tree growing within another;* at the same time, I received a specimen of a portion of the trunk of this supposed monster, which was sufficiently in accordance with this statement to justify the report, and to induce me to make further inquiries upon the subject. Upon proceeding to the place where the tree had grown, I fortunately found that the lower part still remained in the ground; and that this, with the fragment which had been sent me, and those which were still scattered about, contained nearly all the evidence that could be wished for of the structure of the tree before it was cut down. The principal specimen consisted of a shoot about four feet long, and an inch in diameter at the thickest part, having the distinct marks of the removal of a number of lateral shoots by a pruning-knife—the scars being as sharp and well defined as if the branches had been recently dissevered. No trace of bark was visible upon this specimen, except one small patch, half an inch in diameter at the lower end. The shoot was inclosed within the solid trunk of a poplar-tree, about thirty years old, of which it occupied the centre, but with which it had no organic connexion whatever, except at the two extremities, where it was continuous with the trunk itself. The wood within which it lay was applied closely to its surface, having, in the course of its formation, followed accurately every projection or impression upon the surface of the shoot; so that a cross section of the trunk would have exhibited no appearance whatever of this inclosed shoot, except by a circular line half an inch from the centre,

resembling one of those concentric zones characteristic of dicotyledonous trees.

The connexion of the lower extremity of this shoot with the trunk was a little below the ground line; the shoot itself was between four and five feet long; and throughout the whole of this space there was, as I have before stated, no organic connexion whatever between the surface of the shoot, and that of the wood which overlaid it.

The question which naturally arises out of the consideration of this specimen is, in what manner the shoot could possibly have been formed in the situation in which it was found. This enquiry is so closely connected with the formation of wood itself, that, before I attempt to offer any explanation of the specimen, it is necessary to examine in review the most important theories of the formation of wood in exogenous trees, which have, up to this time, been advanced by different physiologists. These may be divided into four classes: 1st, that the bark is produced by the wood; 2dly, that wood is produced by the bark; 3dly, that bark and wood reproduce themselves; and, 4thly, that neither the wood nor the bark produces the new matter which is deposited upon them, but that the latter owes its origin to the vegetation of the leaf-buds.

The first of these opinions has been attributed to the Rev. Stephen Hales, in whose very curious and useful work, called *Vegetable Statics*, such sentiments are said to be discoverable. I suspect, however, that there must be some mistake in this, as I have not succeeded in meeting with any passage in that work which can be said to indicate that such was the theory of the author. He says, indeed, vol. i., p. 334, that he 'agrees in opinion with Borelli, who, in his book, *De Motu Animalium*, part 2, ch. xiii., supposes the tender growing shoot to be distended like soft wax, by the expansion of the moisture of the spongy pith.' But it is not, perhaps, very important to inquire whether he did entertain the opinion ascribed to him, as, if he did, it has never been adopted by any succeeding writer, and appears to be totally unsupported by evidence.

The second opinion is that of Malpighi and Grew, the latter of whom, in his *Anatomy of Plants*, 2d edit., book i., p. 114, § 11, expresses himself thus: 'Every year the bark of a tree is

divided into two parts, and distributed two contrary ways: the outer part falleth off towards the skin, and at length becomes the skin itself: the inmost portion of the bark is annually distributed, and added to the wood.' This opinion has met with many supporters, and is, I believe, even at this day, not universally abandoned. Du Hamel, as is well known, and after him Mirbel, instituted the following experiment, in order to determine the truth of Grew's opinion. If, they reasoned, a metal plate is introduced between the bark and the wood in the early spring, before the growth of the new year has commenced, it ought to be covered by wood after a certain period, provided the opinion of Grew be well founded. A plate of silver was introduced, with the result that was anticipated. When examined, after the lapse of two or three years, it was found imbedded in the wood. At the same time that this experiment seemed to prove the accuracy of the opinion that wood was deposited by the bark, it also served to disprove the theory that the bark was produced by wood.

Satisfactory as the result of this experiment may appear to have been, physiologists have long been aware that it was liable to several objections; and hence has arisen the third hypothesis to which I have adverted, that the bark and the wood each reproduces itself; the substance out of which the annual addition is formed being supposed to be the viscid secretion found betweeen the bark and the wood, and known by the name of 'cambium.'

This view is that which is taken by many physiologists of the present day. But if we consider that the tissue of both the wood and the bark consists not only of cellular matter lying in all directions, but also of vessels and fibres running in lines parallel with the axis of development, and turning from their course, if any obstacle is opposed to them, just as a current of water when interrupted by stones or other obstructions, it seems difficult to reconcile such a state of organization with the idea of an induration of a mucous homogeneous deposit.

The fourth mode of understanding the origin of wood and bark—namely, that they are caused by the descent of matter sent down by the leaf-buds, is generally attributed to Darwin, but may be also traced to Hales, who justly enough observes

(i. 340), 'that it is not easy to conceive how additional ringlets of wood should be formed by a merely horizontal dilatation of the vessels; but rather by the shooting of the longitudinal fibres lengthways under the bark, as young fibrous shoots of roots do in the solid earth.' Whatever claim these authors may have to suggesting this idea, it is certain that it is chiefly known at the present day, in consequence of the writings of M. du Petit Thouars, and one or two others who have followed him. This doctrine leads to a curious view of the nature of plants in general; a subject upon which this is no place to enter fully, but of which a concise explanation is necessary, for the attainment of the end I have in view in this communication.

A plant is to be understood as a mass of individuals, each having its own peculiar system of life, growing together in a definite manner, and having a common organization, but nevertheless capable of vegetating independently, and not unfrequently separating spontaneously from each other. These individuals are buds, each of which is perfect in itself, and exactly the same as all the others of the same plant. They are combined by means of a fibro-cellular substance called bark, which is to be understood as being composed of the cellular integuments of as many individuals as the plant may have developed buds. As the act of vegetation consists in the development of a germinating body in two opposite directions, the one upwards, as stem, the other downwards, as root,—every bud, when it begins to grow, must be subject to this law, provided it is the independent being which it has been represented to be. And, in fact, if a bud is separated from the system to which it belongs, it does follow this law of development, as is well known to gardeners, from their practice of propagating plants by buds and eyes. Now, if buds, when in a state of combination, undergo the same kind of development as when isolated, as it is reasonable to suppose, it will be found that the fibrous and vascular tissue of the wood and bark, which always descends from the buds, is really their roots; and that, consequently, the concentric circles of the wood and bark of dicotyledonous trees are congeries of roots formed by the annual development of buds upon the surface of the plant. It is well known that, whatever the origin of the wood and bark may be,

their fibrous and vascular tissue is held together by a cellular substance which lies among them, assuming the form of plates radiating from the centre to the circumference, and called medullary rays. But it is apparent, from what has been already stated, that if the origin of the wood and bark is such as du Petit Thouars and his followers suppose, such an organic connexion between the outside and inside of a trunk is not indispensable to the formation of wood.

Let us now consider which of the three last theories best explains the structure of the specimen which is the subject of this communication. The central pruned axis was no doubt the original stem of the poplar, and was formed anterior to any of the superjacent wood; and the question is, how the latter originated, without some organic connexion with the centre. If we could suppose, with Grew or Malpighi, that bark produces wood, we might find an explanation of the phenomenon; but as this theory is open to such distinct disproval in other cases as to have been universally abandoned, a reference to it in this instance is inadmissible.

If we suppose that bark produces bark, and wood wood, we still are obliged to understand the existence of a continuity of tissue between the part producing and the part produced. Say that the cambium is the common matter, out of which the wood and bark are both formed; this substance is an exudation of the inner surface of the bark or the outer of the wood, and is incapable of distinct separation from either. So that if we suppose that this central axis was alive at the period when the wood was formed above it, it is difficult to understand in what way that organic connexion, which must have existed at the period of the new formation, was subsequently destroyed; and if we suppose the axis to have been dead at that time, there would be nothing left out of which the new wood could be formed. Hence, it seems impossible to avoid the conclusion, that the presence of a central axis, having no organic connexion whatever with the parts surrounding it, is incapable of explanation upon this theory.

But if we take the opinion of du Petit Thouars as the basis of an explanation of the structure of this specimen, none of the difficulties connected with the other hypotheses will be met

with. It is easy to conceive that, in any tree, almost any extent of living wood may be formed upon dead wood, in consequence of the action of buds, provided a proper medium exists in which the new matter can be formed; and that, while no cohesion takes place between living and dead matter, the usual cohesion may be renewed as soon as two deposits of living matter come again into contact. In this view the specimen now under consideration will, I think, be found at once a beautiful illustration of the theory of the formation of wood from buds, and an insuperable difficulty in the way of any other theories with which we are acquainted.

The explanation that might be given of this specimen would be as follows:—The poplar, when its principal shoot was four years old, was pruned, the whole of its lateral branches being removed. Between the period of being pruned and the next annual formation of wood, the whole plant died, with the exception of the terminal buds, (perhaps the bark,) and the root, with that part of the stem immediately above it. As soon as the terminal buds were called into action by the usual influence of a vernal atmosphere, they obeyed the ordinary laws of development, sending their roots downwards, under the bark, in the form of wood and liber. These roots did not perish, in consequence of there having been a sufficient quantity of moisture between the dead bark and wood to favour their descent; and the moment they came in contact with the living part of the stem at the ground-line, they united with it exactly in the same way as if no dead matter had intervened. Supposing the bark to have been alive, this descent would have been facilitated.

A communication once established in this manner between the upper and the lower living portions, the intermediate axis would be speedily inclosed within wood of its own nature, with which it could have no organic connexion, on account of its own previous death, and consequent incapacity of secreting the cambium or matter of cellular organization, by which alone this connexion is maintained. The absence of bark from the surface of the loose axis of this specimen was the necessary consequence of the mode of growth which I have supposed to take place; by which the whole of the bark, whether living or dead, must necessarily have been pushed

outwards. As to the little patch of bark which was found upon a small portion of the specimen, it may be presumed that at that point there had occurred a cohesion between the liber and alburnum, which the force of the fibres descending from the buds was not sufficient to overcome, and that, in consequence, such portion of the bark became incased.

Whatever opinion may be entertained of the foregoing explanation, it, I think, at least seems impossible to reconcile the structure of this specimen with the theory that bark produces bark, and wood wood; while, at the same time, it is entirely conformable to the opinion, that wood and bark are both the result of the development of the numerous systems of vegetation, of which every plant consists.

The accompanying wood-cut represents the specimen, much diminished, and may serve to convey a more exact idea of the subject to which the foregoing remarks apply.

ON THE FIRST INVENTION OF TELESCOPES, &c.

By Dr. G. MOLL, of Utrecht.

(Concluded from page 332.)

HAVING heard what was adduced on the side of Lippershey, we must now turn to the witnesses of Zacharias Tausz, or Taussen.

The first of these is the ambassador, Boreel himself, a man alike respectable for his rank, character, and abilities. He says, that in 1591, the year in which he (Boreel) was born, a spectacle-maker lived near his father's house at Middelburg; that this man's name was Hans, his wife's Maria, and that, besides two daughters, he had a son called Zacharias; that Boreel knew this Zacharias intimately, they having been playmates. This Hans, *i. e.* John, with his son Zacharias, as *Boreel often heard*, invented the first *microscope*, which was presented to Prince Maurice, and they obtained some reward. A similar *microscope* was afterwards offered by them to the Archduke Albert of Austria. When Boreel was ambassador in England in 1619, he saw that identical microscope there, in the possession of Cornelius Drebbel, of Alkmar, a man of much knowledge, and mathematician to King James, the Archduke having presented the microscope to Drebbel. This microscope of Zacharias was not, continues Boreel, as they are shown at present, with a short tube; but it was about eighteen inches long, and two inches in diameter, with a tube of gilt copper, resting on two sculptured dolphins; under it was a disc of ebony, on which the objects to be examined were placed*. But long after, in 1610, by dint of research, they (*i. e.* Hans and Zacharias) invented in Middelburg the long sidereal telescopes, with which we gaze at the moon, the planets, stars, and heavenly bodies, of which a specimen was given to Prince Maurice, who kept it secret, judging it useful in expeditions. However, as this admirable invention was rumoured about, and as curious men were talking about it in Holland and

* A stage.

elsewhere, a stranger came from Holland to Middelburg to inquire into this matter, and, asking for a spectacle-maker, he was shown by mistake into the shop of John Laprey. He spoke with him about the secret of the telescope. Laprey, being an ingenious man and a close observer, heard attentively what the stranger said, and thus, with laudable industry and care, became the second inventor of the long telescope, which he made to the satisfaction of the stranger. Therefore Laprey, who by his ingenuity discovered a thing which was not shown to him, deserves to be ranked as second inventor. He first sold telescopes, and made them generally known. Afterwards, Adrian Metius, Professor of Francker, and, later, Cornelius Drebbel, came to Middelburg in 1620, and bought each a telescope, not from Laprey, but from Zacharias Tausz.

From this evidence we may infer, that Hans, or John, and his son Zacharias, were actually the inventors of a compound microscope for opaque objects: the elegant ornaments of this instrument, and the general description which Boreel gives of it, make it probable that both Hans and Zacharias were men of ability. But with microscopes we have at present nothing to do. The point at issue is, whether either Hans or Zacharias, or any body else, actually made telescopes before the 2d of October, 1608; and since Boreel indicates 1610 as the epoch of the invention of Hans and Zacharias, the claim of Lippershey to priority remains unshaken, even by the evidence of Boreel.

The following witness is John, the son of Zacharias, and consequently grandson of this Hans, of whom Boreel has spoken. He says, in 1655, that he then was fifty-two years old; thus, at the period when Lippershey sent in his petition, *i. e.* in 1608, he was only five years old. He does not mention his grandfather, but says, that his father, Zacharias, was the first inventor of the telescopes; and that this happened, as he *had often heard*, in this town, in 1590; but the longest telescope made at that time did not exceed in length fifteen or sixteen inches. He affirms, that two such telescopes were *then* offered, one to Prince Maurice, the other to the Archduke Albert; and that telescopes of such length were in use till 1618. At that time, he, John, and his father, Zacharias, invented the construction and fabric of the longer telescopes, which are still

now used at night to look at the moon and stars. He further says that, in 1620, a man of the name of Metius came to Middelburg, and procured such a telescope, the construction of which he afterwards tried to imitate; and he adds, that Drebbel did the same.

This witness, fixing the epoch of the invention at 1590, speaks only from hearsay. Besides, he is in contradiction with Boreel, who states that the invention of the telescope by Hans and Zacharias was in 1610, at which time Boreel was nineteen, and this John Zacharias only seven years of age. John says nothing of the microscope, which Boreel actually saw and described. It is certainly possible that one of the Metii, perhaps the Professor, came to Middelburg in 1620, and bought a telescope. But this does not decide the question of priority, as we know, from incontrovertible authority, that Jacob Metius was in possession of the invention in 1608. What happened in 1620, when so many splendid discoveries were made by means of the telescope, is not of the least consequence, as far as concerns the first invention of the instrument.

There still remains another witness, whose evidence is very immaterial and of little importance. It is a woman called Sarah Goedard; she is a sister of Zacharias Jansz: she merely says, that it is forty-two or forty-four years ago since her brother invented the long telescopes in Middelburg. She often saw her brother at work making telescopes; but she cannot speak positively as to time.

This woman's evidence, who brings the invention to 1611 or 1613, cannot be of the slightest use in settling the question between Zacharias and Lippershey.

It was then the soldier of Sedan, who first brought the instrument to France; but his endeavours met with no great success in that country. It is most astonishing to find the French philosopher Peirese doubting the truth of the invention of telescopes as late as 1622, and ascribing it to Drebbel, a person wholly unconnected with it. In a letter to William Camden, he says, 'I should like to know what is true about the inventions of Cornelius Drubelsius Alkmariensis, who, as is said, has invented in your parts a globe representing ebb and flood, a covered boat going between two waters, and long

spy-glasses (*lunettes*), *with which a writing may be read at the distance of a league, which we do not easily believe here* *.'

And in another place † he says, 'We are told marvellous things here about the inventions of Cornelius Drubelsius Alkmariensis, who is in the service of the King of Great Britain, and who lives in a house near London; amongst others, a covered boat, which goes between two waters; a glass globe, which he makes to represent the tides, by a perpetual motion, regulated like the natural tide of the sea, *and of a spy-glass, which makes one read a writing at more than a league* (or a mile) *distance.* I beg you to write me a word about the truth of each of these inventions. We have here those small glasses (lunettes), by which insects and mites appear as large as flies, which is certainly admirable; but I should like to know what is true respecting these other inventions.'

It would appear that the invention was attributed by some persons to the soldier of Sedan, whose name appears to have been Crepi‡. He left, as we have seen, the Low Countries in December, 1608, and in May following, 1609, we find a Frenchman in Milan making telescopes. Sirturus § gives us the following account of this transaction :—

'A Frenchman hurried to Milan in May, 1609, who offered a telescope to the Count de Fuentes. He called himself a partner of the Dutch inventor. The Count gave the instrument to a silversmith, to have it included in a silver tube; it fell into the hands of Sirturus, who handled and examined it, and made a similar one (if his assertion is to be believed); but perceiving that much depended on the glass, he went to Venice to get some at the workmen.'

Simon Marius, who disputed the discovery of the satellites of Jupiter with Galileo, speaks of another Dutch telescope, which came into foreign parts at a very early stage of the invention. He says that, in 1608, at the autumnal Franckfort mass or fair (usually held in September), a certain General Fuchs de Bimbach, an amateur of mathematics, heard from a Dutch-

* Gul. Camdenii et ill. viror. ad Camden. Epistol. London, 1691. p. 333.
† Page 387.
‡ Borel de verotelescopii inventore, p. 19.
§ Sirturus de telescopio. Edit. Franckf. 1618. 4to. minori, p. 25.

man then at the fair, that an instrument had been invented which magnified objects and made them appear near. He wanted to procure one of these glasses, but the Dutchman asked too high a price; but being returned to Onoldsbach, Fuchs told the circumstance to Marius, adding that the instrument had two glasses, one convex and one concave, of which he even drew the figures. Marius adjusted glasses of this form, and convinced himself, to a certain point, of the possibility of the thing; but his object glass was too convex. He ordered some other glasses of the opticians of Nurenberg; but he could procure none that suited his purpose. The next summer, of 1609, Fuchs got a tolerable instrument from Holland, which he used with Marius in examining the heavens. About the beginning of 1610, Fuchs got two well-polished glasses from Venice, where they had been worked by T. B. Lanccius, recently returned from Holland.

If the account of Marius deserves credit, the person who brought the telescope to the Franckfort mass or fair, in September, 1608, did so a short time before Lippershey presented his petition to the States, which was done the 2d of October of that year. Fuchs, certainly with great reason, thought the Dutch telescopes high priced; we have seen Lippershey asking a thousand florins for one.

We are indebted to the English author of the *Life of Galileo* for an instance of another Dutch telescope being brought to Italy. Lorenzo Pignoria writes to Paolo Gualdo, from Padrea, the 31st of August, 1609, 'We have no news, except the return of his Serene Highness, and the re-election of the lecturers, among whom Signor Galileo has contrived to get 1000 florins for life, and it is said to be on account of an eye-glass, like the one which *was sent from Flanders to the Cardinal Borghese.* We have seen some here, and they succeed well.'

It will, after all, be very difficult to deny, that not only the rumour of the invention, but even some telescopes actually made, reached Italy from Holland, before Galileo ever made such an instrument. In May, 1609, there was a telescope in the hands of the Count de Fuentes. Another was in the possession of Cardinal Borghese; Lanccius, who came from Holland, is said to have made telescopic glasses at Venice;

Fuccarius distinctly says, that one Dutch telescope was brought to Venice, and *that Galileo saw it**. But such is our respect both for the genius and the character of Galileo, that his mere assertion that he never saw a telescope when he set about making one; that he did not know its construction, that his friend Jacob Badorere, by whom he got intelligence of the invention from France, did not give him any information of the manner in which it was made—his simple assertion of all this is taken by us as conclusive against any presumption.

Nelli, in his *Life of Galileo*, says, that the Florentine philosopher first heard of the invention in June, 1809. Galileo himself informs us, in a letter written in March, 1610, that he heard of the invention about ten months ago, which would fix the time of his first attempt to the month of May, 1609, the time when, we know from Sirturus, that a Frenchman brought the telescope to Milan.

Even after the very able manner in which the history of Galileo's discoveries have been recently given by an English author, it will not be superfluous to give Galileo's own account of the transaction.

In March, 1610 †, he wrote in the following manner:—' It is about ten months ago that it came to our ears, that a glass ‡ had been worked by a Belgian, by the help of which, visible objects, though at a great distance from the eye of the observer, may be seen distinctly. (In the Italian of the Saggiatore it is added, *ne piu aggiunto, no more was added,* or *this was all.*) And some experiments were related of the admirable effects of this instrument, which some believed, and others not. A few days afterwards the same was confirmed by letters of a noble Frenchman, Jacob de Badorere, from Paris; all which occasioned me to apply myself wholly to inquire into the cause of this, and to think on the means by which the invention of a similar instrument might be brought about; in which I succeeded in a short time, assisted by the doctrine of refraction: and I first procured a leaden tube, at the end of which I adapted spectacle glasses §, both plane on one side, the one convex on the other side, the second con-

* Kepleri epistolæ, No. 309, p. 493.
† Epist. 4. Td. Martii, 1610.
‡ Un occhiale, perspicillum.
§ Vitrea perspicilla.

cave. Bringing the eye near the concave glass, I saw the objects large, and near enough: they appeared three-times nearer, and nine times larger, than if seen with the naked eye.

'Afterwards I made another instrument, which made objects appear sixty times larger.

'Finally, sparing neither industry nor expense, I succeeded so far as to make an instrument of such excellence, as to make the objects seen through it, appear a thousand times larger, and more than thirty times nearer, than if seen with the natural power of the eye.'

Viviani, Galileo's favourite pupil and friend*, says, that in the month of April or May, 1609, it was rumoured in Venice, where Galileo then was, that a Dutchman presented to Count Maurus, of Nassau, a certain glass occhiale, with which distant objects appeared as if they were nearer, *nothing more was said* †. With this information only Galileo returned immediately to Padua, to try whether he could find out the construction of this instrument, in which he succeeded on the following night. The next day was employed in constructing the instrument, in the manner which he had imagined; and, notwithstanding the imperfection of the glasses which he procured, he saw the effects which he anticipated, and immediately gave notice of it to his friends in Venice. He constructed, after this, instruments of better quality; and six days later he took some of them to some elevated part of the city, and made the first senators of the republic observe distant objects, which they did with great admiration. Bringing constantly the instrument to greater perfection, he resolved finally, with his wonted liberality, to communicate his invention, and to make a free gift of it to the serene Prince and Doge Leonardo Donati, and to the Senate of Venice, presenting with the instrument a paper, in which he declares the construction, and the admirable use and results on *land and on sea*, which might be obtained from this invention. In consequence of this noble present, the serene republic, with generous demonstration of the 25th of August, 1609, wrote to Galileo, and a pension was granted him for life, with

* Viviani vita del Galileo, p. 69. † *Ne piu oltri fu detto.*

more than three times the salary, which it was the custom to give to a lecturer of mathematics.

Thus we perceive the Venetian Senators doing in August, 1609, the same thing which the members of the states-general had done in October, 1608, about ten months sooner. They ascended to high places for the purpose of gazing at distant objects. Both the Dutch and the Venetian magistrates nobly rewarded the invention, which was tendered to them. The Venetians rewarded Galileo as a philosopher should be rewarded, by an honourable station and independence. The Dutch treated Lippershey in the best way an artist can be treated; they gave a high price for his article, and made large orders for it. The date assigned by Viviani to these transactions, the 25th of August, 1609, agrees completely with what Lorenzo Pignoria wrote the 31st of August to Paolo Gualdo, and which letter was mentioned above.

It is exceedingly gratifying to observe, that Galileo almost immediately brought the telescope with a convex object-glass and a concave eye-glass, to all the perfection of which it is susceptible, without being achromatic. He observed with it all that could be seen by its means; he ascertained the power of his glasses with great ingenuity, and he indicates the difference between linear and superficial amplification with perfect accuracy. His German biographer, Tagemann, does not seem to have clearly understood this difference, for he appears to imagine that Galileo's telescope really had a power of a 1000 times, whereas it was only of about 32.

In a Galilean telescope the focal length of the object-glass cannot go beyond a certain extent, without narrowing the field too much. The eye-glass cannot be made very deep without making it too thin in the centre. Even at present, it would, perhaps, be difficult to make Galilean telescopes of greater power than 32, which is, indeed, that which Galileo obtained.

On the 7th of January, 1610, Galileo discovered three of the satellites of Jupiter; on the 13th, the four satellites were observed and recognized as satellites; but it is not my object to enter into that splendid strain of discoveries which illustrated the name of Galileo, and which lately have been so well described.

However perfect we allow the instruments of Galileo to have been, we see no reason to doubt that the satellites could be seen with the instruments made in Holland. The Italian authors certainly assure us that the Dutch telescopes were of an inferior description; but this assertion is wholly unsupported by proof. Indeed, we know nothing of these telescopes, except that they were long (*tubi longi*), and longer than sixteen inches *; and it is not unrational to suppose that, with this length, they were equal to Galileo's telescopes. Admitting the length of the telescope to have been sixteen inches, and the negative focus of the concave eye-glass half an inch, the power of the telescope was 32, or equal to that of Galileo. The Professor Adrian Metius †, brother of the co-inventor of the telescope, gives us some account of what could be seen with the telescopes then made in Holland. In a book printed in 1614, he says, 'During the day several planets are observed near the sun, which were unknown hitherto to all men, but which can only be seen with the glasses, which my brother, Jacob Adriaansz, invented six years ago (thus in 1608). These planets show themselves first in the eastern part of the sun, and from thence pass over the sun to the westward, in about ten days, as I observed several times, principally about sunrise and sunset.

'With these same tubes some erratic stars or planets are seen, which have their course round Jupiter; but of these nothing can be stated with certainty, unless my brother be pleased to publish his telescopes, by means of which many strange things will be brought to light, as well about the moon as elsewhere. Yea, the observations of the stars may then be made with much greater accuracy; because, by means of these telescopes, it will not only be possible to observe minutes, but even seconds.'

It does not appear from this quotation that the professor himself observed the satellites; nor does he even appear to be aware of their number. His brother Jacob, perhaps, gave him some incomplete information of the existence of the satellites.

* De vero telescopii inventore, p. 30.

† Adriani Metii, Institut. Astronom. et Geograph. Francq. 1614. Fondamentale en groudelyche ouderuysinge, ibid 1614. Adriani Metii tractatus de genuino usu utrusque globi, Francq. 1624. 4to.

But *he saw* the spots of the sun, which may be seen with instruments of a less power; and he labours under the erroneous notion, then common to many, that the spots were planets or satellites circulating round the sun.

But what the Professor says of *the accuracy* which the invention of the telescope is likely to insure to astronomical observations, is very remarkable. What does he mean by asserting, *that the observations on the stars will become accurate to a second?* Did the pupil of Tycho anticipate the application of the telescope to instruments of mensuration; to quadrants? I must own that it is difficult to take his distinct words in any other sense; and I am led to believe, that the idea of an invention, which did so much credit to Gascoigne, had occurred to Adrian Metius.

There is a passage in the English life of Galileo, which ought not to pass unnoticed. The anonymous author accuses William Boreel, to whom he chooses to give the Italian name of Borelli, of glaring partiality against Galileo. 'Borelli,' says this author, not 'satisfied with attributing the invention of telescopes to Zanssen, endeavours to secure for him and for his son the more solid reputation of having anticipated Galileo in the useful employment of the invention. He has, however, inserted in his collection a letter from John, the son of Zacharias, in which John, omitting all mention of his father, speaks of his own observations of the satellites of Jupiter, evidently seeking to insinuate that they were earlier than Galileo's; and in this sense the latter has been since quoted, although it appears from John's own deposition, preserved in the same collection, that, at the time of the discovery, he could be no older than six years. An oversight of this sort throws doubt on the whole of the pretended observations; and, indeed, the letter has much the air of being the production of a person imperfectly informed on the subject on which he writes, and probably was compiled to suit Borelli's purposes, which were to make Galileo's share in the invention appear as small as possible.'

I crave the liberty of replying to this passage, that if probabilities are to be introduced in the case, it seems extremely probable that the learned author of the Life of Galileo has

never read Borel's book with sufficient attention, and, as the book is scarce, he knows it perhaps only from quotations. There is *no letter* inserted in it from John, the son of Zacharias; but in answer to some queries either from Boreel or from Borel, he gives two memorials or notes of what a telescope of his making could show. In the first place, he mentions the appearances and dark places in the moon; and it is to be observed, that what he says of the appearance of the moon seen through his telescope, answers exactly to what one would expect of a good instrument. It plainly shows, says John, the moon to be a sphere, with distinct edges, and not a plane. The following is his statement of Jupiter's satellites: he often observed the planet which shows itself round, well defined, and spheric; near it he often saw two highly situated small stars, sometimes he saw three, and generally four of these small stars. As far as he could observe, they go perpetually in circles round Jupiter; but he adds, this I leave to astronomers to determine, for it is not, says he, my business to make astronomical observations, but to furnish astronomers with telescopes as good as I am able to make.

I challenge the author of the life of Galileo to point out the passage in Borel's book in which either Boreel, or John, the optician, exhibit the least intention of throwing Galileo's discoveries in the shade. But it may be permitted, I should think, to an optician, when asked by an ambassador at a foreign court, to state what the performance of his instruments is; and I believe that neither Mr. Dollond nor Mr. Tully could be justly accused of disparaging Sir William Herschel's merit, if they were to state that the Georgium Sidus is visible in their telescopes. John certainly says, in 1655, when he was fifty-two years of age, that he often saw four satellites with a telecope of his own making; but he never says that he saw the satellites before January 1610, the epoch of Galileo's discovery, nor does he even mention *when* he first saw them. He is, says he, no astronomer, but an optician; and when this optician states, in 1655, that he makes telescopes with which the satellites can be seen, it is difficult to understand how it can be inferred that he made this statement in order to deprive Galileo of the honour

of discovering the satellites in 1610. Thousands, certainly hundreds, saw the satellites in 1655; and why should not John, like other people? I, therefore, positively deny that any intention is shown in Borel's book, to depreciate the merits of Galileo; and, as far as Boreel is concerned, considering his character and station in life, it is absurd to say, *that his evident object* was to make Galileo's share in the invention as small as possible; but if Boreel really undervalued Galileo's merits, let the English author quote, and point out the place *where* he did so. I must offer another remark to this same anonymous author. I am quite prepared to believe that the telescopes made by Galileo's own hands were as perfect as art could make them at the time; but it is to be lamented, if the original telescope of Galileo still exists in Florence, that no Italian philosopher has favoured us with an account of its performance. We have, however, a sort of criterion of what could be done by it. The belts of Jupiter were, as far as I know, never seen by Galileo; they were observed, after his death or blindness, with instruments made by Evangelista Torricelli. But the author of the English life of Galileo asserts, as proof of the inferiority of the Dutch telescopes, that, in 1637, '*Gaertner, or, as he chose to call himself Hortensius*, wrote to Galileo, that no telescope could be procured in Holland, sufficiently good to show Jupiter's disk, well defined*.' Hortensius wanted more than could be accomplished in his time; and even now, telescopes of a certain size, which show Jupiter's disk well defined, are not of every day's occurrence. Does this author know many telescopes excepting those made by Mr. Dollond or by Mr. Tully, capable of showing Jupiter's disk, *well defined;* nay, does he know one single telescope, *not achromatic*, capable of answering the claim of Hortensius. The anonymous author favours his reader with a translation of Hortensius's name, which he pronounces to be *Gaertner*. He is mistaken, however: Gaertner certainly is the *German* of Hortensius; but he was *not* a German, and his name, in his mother tongue, was Van den Hore.

We find the celebrated Peirese, as late as 1622, doubting

* P. 25.

the invention of telescopes: in England, these instruments were known at a much earlier period. The celebrated English mathematician, Thomas Harriot *, actually observed the satellites of Jupiter, as early as the 10th of January, 1610, which is only *eleven days* later than Galileo's discovery. It is, indeed, astonishing that an *English author* should overlook this circumstance. Harriot also observed the spots in the sun for the first time on the 8th of December, 1610. They were first seen by Galileo in November of the same year. Harriot's telescopes had, it appears, powers of 10, 20, and 30. His observations run from the 16th of January, 1610, to 26th of February, 1612; he gives drawings of the configurations and computation of their revolutions. Now, it may be asked, from whence did Harriot get the telescope with which he observed the satellites only a few days later than Galileo? Certainly not from Italy; he either made it himself, or got it from Holland.

But a few months later we find another English astronomer furnished with a telescope. Sir Christopher Heydon writes, on the 6th of July, 1610, to the well-known William Camden: —'I have read Galileo, and, to be short, do concur with him in opinion; for his reasons are demonstrative; and of my own experience, with *one of your ordinary trunks*, I have told eleven stars in the Pleiads; whereas no age ever remembers above seven; and one of these, as Virgil testifieth, not always to be seen †.'

Telescopes were then, it appears, called *trunks*. Harriot, in his letters to Henry Percy, Earl of Northumberland, calls them *perspective cylinders*. It appears that the earl possessed many of them, and that he wanted some more. It is to be lamented that Harriot's papers and manuscripts are at present buried, in one of the libraries of the University of Oxford.

From all which has been said in this paper, the following facts may be established, as proved by authentic documents:—

* These observations, and other manuscripts of Harriot, were discovered, in 1784, by Baron de Zach, at Petworth, in Surrey, the seat of Lord Egremont. See Bode's Astronom. Tahrbuch, 1788, p. 155, Monatliche Correspondenz, t. viii. p. 144.

† Gulielmi Camdeni et illust. viror. ad G. Camden. Epistol. London 1691, p. 128.

That on the 2d of October, 1608, John, or Hans Lippershey, a native of Wezel, a spectacle-maker of Middelburg, in Zeeland, was actually in the possession of the invention of telescopes.

That, on the 17th of October, of the same year 1608, Jacob Adriaansz, sometimes called Metius of Alkmar, in Holland, also was in possession of the art of making telescopes, and that he actually made those instruments; but that either from disgust or some other reason he afterwards concealed his invention, and thus actually gave up every claim attached to the honour of it.

That there is little reason to believe that either Hans, or his son Zacharias Zansz, were also inventors of the telescope; but there is every probability that this Hans, or John, or his son Zacharias Zansz, invented a compound microscope about 1590.

That this Lippershey used rock or mountain crystal in the construction of telescopes, and that he is the inventor of the Cinoculus.

ON THE CONTRIVANCES OF SOME ANIMALS TO SECURE WARMTH.

By J. RENNIE, A.M., A.L.S.

Professor of Natural History, King's College, London.

THOSE who adopt the opinion, that the lower animals are actuated in their movements by reason, rather than by what is termed blind instinct, may find abundance of facts illustrative of their doctrine in the various modes employed by animals to keep themselves warm. But without involving ourselves deeply in the curious metaphysical controversy respecting instinct and reason, which seems to have little chance of being speedily decided, it may not be unprofitable to bring a few of the facts just alluded to under review. Some of these facts may be daily observed by anybody who will take the

trouble, though they seldom draw attention, or excite inquiry, and yet they may frequently give origin to the most interesting researches in natural history.

Without going further than the hearth-rug beside my chair, I may begin with the cat, which prefers that hearth-rug to any other corner of my study; and though she cannot be said to exhibit much contrivance in keeping herself warm, compared, at least, with her insidious cunning in taking her prey, she certainly shows most surprising knowledge and tact in discovering the best non-conductors of heat. Darwin would have considered this as unequivocal proof of knowledge derived from experience *; but as I cannot bring myself to give cats the credit of discovering, and then acting upon the philosophic principles of the distribution of caloric, I shall venture upon the inference from the fact, that they are not indigenous (contrary to the received opinions) to so cold a climate as Britain, and are impelled to search after warm places, in consequence of their great impatience of cold.

The feet of the cat, though they are thickly clothed with hair above, and padded with a soft cushion of thickened epidermis, intermediate between cartilage and tendon, on the soles, may be always observed to be cold to the touch when the animal has been exposed to a low temperature, as are the ears likewise; and, in such circumstances, it manifests its uncomfortable feelings by restlessly wandering about till it can find a warm corner. This very appetite (if it may be so called) for warmth, appears to me to be the chief cause which prevents our domestic cats from ever becoming wild; for, in every part of the country where there are woods, they might find abundant prey; and it is well known, that when cats once take to bird-catching in the woods, they never afterwards eat anything dead but with reluctance. I have had many opportunities of observing cats in this half wild state; but though they depended for food wholly upon what birds and mice they could catch, and were so wild as scarcely to permit themselves to be seen, much less approached, yet no instance ever came

* Zoonomia, i. s. 16, and Brown's Observ. p. 263.

to my knowledge of their having made their domicile in the woods, but uniformly sleeping and littering in the least frequented barns and other out-houses of farms. This I am inclined to attribute wholly to their finding such places warmer than any they could discover in the woods, and to the supply of mice they might find there when birds were less plentiful; for it could not well be traced to their attachment to man, whom they always fled from as fearfully as a fox would do.

A more particular instance of this once came under my observation. A cat, which had been long remarked as one of the wildest of those which frequented a barn on the borders of a wood in Ayrshire—so wild, indeed, as to be seldom seen —was, several times during a sharp frost, observed, with no little surprise, to pass and repass into the adjacent farm-house, which it had not for some years been known either to enter or approach. It might have been inferred that it was compelled by hunger, had not this been the best season for catching birds; but, in one of its stealthy visits, it was seen snugly coiled up beside a baby in the cradle, to the no small horror of the mother, who imagined, in accordance with the popular prejudice, that it had come to suck away the baby's breath. All I could say to persuade her of the impossibility of the cat doing this was of no avail, and orders were immediately given to every servant on the farm to kill the poor cat wherever she could be found. Her caution and agility, however, were long successful in saving her; and though the persecution she thus experienced rendered her, if possible, much wilder than before, yet she was not thereby deterred, not even after being wounded by a pitchfork, and her leg lamed by throwing a hatchet at her, from paying a daily visit to the baby in the cradle, because it was the warmest place within her knowledge; and, next to food, she considered warmth an indispensable of life. She persisted thus in venturing to the cradle, till she was at length intercepted and killed.

It is worth remarking, that this cat was a pale tabby, of small size, with a long slender tail tapering to a point; none of which circumstances agree with the common wild cat (*Felis catus*, Linn.) found in our mountain woods. The latter has

a short tail, which, when bent over the back, only reaches to the shoulder, while it is thick, or rather broad, and does not taper, but ends bluntly, as if a portion had been cut off.

M. Temminck, looking at these distinctions, and also at the great difference of size—the wild being a third larger than the domestic cat—is of opinion that they are decidedly different species; and he is disposed to consider a new species (*Felis maniculata*) recently sent from Nubia by M. Ruppel, as the original of the domestic cat *, which opinion would accord with the above remarks respecting its impatience of cold. Linnæus and Buffon seem to have been among the first to confound these two species, though the latter was aware of the remarkable difference in the length of their intestines; those in the wild cat being only thrice the length of the body, proving it to be purely carnivorous, while those of the domestic cat are much longer, being nine times the length of the body, proving it to be able to subsist on a portion of vegetable food, and, accordingly, we find that our cats are very fond of boiled greens, &c., which it is probable no wild cat would touch. That these changes are not caused by domestication, is proved by no such difference appearing in the intestines of the wild boar and the pig, and by domestic animals being always increased rather than diminished in size when compared with their known wild originals. To enter more minutely into this, however, would lead me too far from my immediate subject; but it may be worth mentioning, that the domestic cat is only of recent introduction in the higher northern latitudes, as in Sweden † and Norway ‡, while they are not yet introduced into Lapland §.

From the chinchilla (*Chinchilla lanigera*), being a native of Chili, it was inferred that, like the cat, it might be pleased to lie warm, and a piece of flannel was accordingly given to one in the collection of the Zoological Society; but, instead of lying upon it as a cat would have done, it always pulled it about, and dragged it to the outer division of its cage. It is

* Temminck, Mammalogie, No. iv. sp. 17.

† Linnæus, Fauna Suecica. ‡ Pontoppidan, Nat. Hist. Norw. ii. 18.

§ Zimmermann, Specilegia Zool. Geograph. p. 172.

to be recollected, however, that both its fur and skin are thick; while the skin of a cat is very thin and tender, which make it both susceptible of cold, and, as Pennant observed, terribly afraid of being beat.* The demoiselle heran (*Anthropoides Virgo*, VIEILLOT), which Buffon had from the coast of Guinea, was more attentive to its comfort than the little chinchilla; for he tells us, ' it had chosen for itself a room with a fire to shelter it during the night, and in winter (1778) it repaired every evening to the door, sounding for admission †.' This indicates more intelligence, instinct, or whatever it may be called, than occurs in an animal much wiser in appearance. A similar anecdote is related by M. Antoine of a lapwing (*Vanellus cristatus*, MEYER), which a clergyman kept in his garden. It lived chiefly upon insects; but as the winter drew on these failed, and necessity compelled the poor bird to approach the house, from which it had previously remained at a wary distance; and a servant, hearing its feeble cry, as if it were asking charity, opened for it the door of the back kitchen. It did not venture far at first, but it became daily more familiar and emboldened as the cold increased, till, at length, it actually entered the kitchen, though already occupied by a dog and a cat. By degrees it at length came to so good an understanding with these animals, that it entered regularly at night fall, and established itself at the chimney-corner, where it remained snugly beside them for the night; but as soon as the warmth of spring returned, it preferred roosting in the garden, though it resumed its place at the chimney-corner the ensuing winter. Instead of being afraid of its two old acquaintances, the dog and the cat, it now treated them as inferiors, and arrogated to itself the place which it had previously obtained by humble solicitation. This interesting pet was at last choked by a bone which it had incautiously swallowed‡.

The Barbary Ape (*Macacus sylvanus*, LACEPEDE), which, though a native of Africa, has established a colony on the rock of Gibraltar. Here it is occasionally so cold in winter, that

* British Zoology, vol. i. † Oiseaux, ART. *L'Oiseau Royal.*
‡ Antoine, Animaux Célébres, i. 70.

these poor apes are fain to huddle about any chance fire that may be lighted out of doors and left burning; but though they are seen sitting close to the dying embers, they have never been known to add a single chip of fuel to continue the fire *—a circumstance somewhat at variance, indeed, with the title of this paper, but not the less curious, as illustrative, by contrast, of animal manners.

Animals which lie torpid during winter are usually careful to provide a warm and well sheltered domicile for their long sleep, and it is not a little interesting to observe the proceedings of different species. The edible snail (*Helix pomatia*, LINN.), for example, found in the middle districts of England, but supposed to have been introduced from the Continent in the sixteenth century, forms, at the end of autumn, a very curious winter cell. When at liberty, it constructs this cell of earth, moss, and withered grass, by means of its muscular foot, enlarging the cavity by turning itself round, and forming the roof by carrying up portions of earth and moss†. But, according to Mr. Bell, 'it is not by the pressure of the foot and the turning round of the shell that this is principally effected. A large quantity of very viscid mucus is secreted on the under surface of the foot, to which a layer of earth or dead leaves adheres; this is turned on one side, and a fresh secretion being thrown out, the layer of earth mixed with mucus is left. The animal then takes another layer of earth on the bottom of the foot, turns it also to the part where he intends to form the wall of his habitation, and leaves it in the same manner, repeating the process until the cavity is sufficiently large, and thus making the sides smooth, even, and compact. In forming the dome or arch of the form, a similar method is used, the foot collecting on its under surface a quantity of earth, and the animal turning it upwards, leaves it by throwing out fresh mucus, and this is repeated until a perfect roof is formed‡.'

I brought a pair of these animals from the woods of Godesberg on the Rhine in 1829; and as they were kept under an inverted glass, with only a few leaves, it was amusing to see

* Scott, Intell. Philosophy, iv. 1.
† M. Gaspard in Majendie's Journ. de Physiologie, ii. 295.
‡ Zoological Journal, i. 94, note.

how solicitous they appeared to be to make the most of these in forming their cells. One of them made the side of the glass a part of the wall of its cell, against which it formed a sort of arch with what leaves chanced to be within its reach; but as it seemed to have no idea of bringing materials from a distance, the covering was thin and imperfect. The other attempted to establish itself in the middle of the area, apart from the sides of the glass; but it was less successful than its fellow, as it always deranged the portion of wall it had constructed by turning about in search of materials. It was curious to remark the different habits of two other species of the family (*Helix aspersa,* and *H. nemoralis*, Muller) confined under the same glass: the latter giving themselves no trouble about a covering, crept quietly up as high as they could get, and formed their calcareous lid (*operculum*) upon the bare glass; the second of the edible snails was at length reluctantly compelled to follow their example, after being foiled in all attempts to cover itself with a dome of leaves.

Our common hedgehog (*Erinaceus Europæus*) makes a similar preparation to the preceding for his winter's sleep, being frequently found so bewrapped in leaves as to have little resemblance to an animal. The hedgehog, however, has not, so far as I am aware, been ever observed in the act of forming this covering of leaves, though it is supposed to roll itself about till its spines take up a sufficient number, in the same way it is popularly believed (without proof) to do with apples. That it collects leaves for this purpose, and carries them to its den, has been repeatedly witnessed; and when domesticated, it will construct a barricado of leaves at the mouth of its den*. It would hence appear that the ancient Greeks erroneously undervalued the skill of the hedgehog, when, comparing it with the *polysophia* of the fox, they said it only knew the important art of defence†.

The hare, which remains active all winter, is somewhat less provident against cold, its close fur, particularly upon the feet, furnishing it with good protection; and yet the winter *form*, as it is called, or den of the hare, is a very snug little place. I had

* Gent. Mag. for June, 1782.

† Πολλ' οἶδ' ἀλώπηξ, ἀλλ' ἐχῖνος ἓν μέγα. Zenodotus ex Archiloch.

once occasion to cross the wild mountainous tract on the north-east boundary of Ayrshire, after a heavy fall of snow, which a subsequent frost had hardened on the surface into a crust sufficient to bear the foot without sinking. For several miles I did not see a living creature; and even the hardy raven, that might have fared sumptuously on the hapless sheep, many of which had fallen victims to the weather, seemed to have abandoned its summer haunts for the warmer vicinity perhaps of the sea-coast. On crossing a small holm by the side of a brook, the water of which I could hear running, though it was mantled over with snow and invisible, I was not a little startled,—alarmed, indeed, by a hare dashing through between my legs, and almost upsetting me, and I found I had actually stepped over her *form* before she was roused. The ancients had a notion that the hare sleeps with its eyes open*; and hence, Horus Apollo says, the Egyptians pictured a sleeping hare as the hieroglyphic of what was obvious.

'The Greeks,' says Gesner, 'had a common proverb (Lαγος καθευδον), "a sleeping hare," for a dissembler or counterfeit, because the hare sees when she sleeps; for this is an admirable and rare work of nature, that all the residue of her bodily parts take their rest, but the eye standeth continually sentinel †.' The hare in question, however, must have been in a profounder sleep than usual,—tempted, perhaps, by the supposed security of its retreat in this almost untrodden wilderness. Upon examining the *form*, I found it as neatly rounded as a bird's nest, and of considerable depth, the foundation being a thick tussock of withered rush (*Juncus maximus*), well lined with bent, not carried thither, it would appear, but grown upon the spot, and only beat down and arranged into a snug, circular, basket-like cavity, just sufficient to contain the little animal, when coiled up, to sleep. I could not ascertain whether it had been quite open above, or partly covered with bent and rushes, and curtained with snow; but I think the latter most probable, for had there been a speck of darker colour than the uniform white surface around me before I came to the spot, I

* Gesner, Hist. Anim., by Toplis, p. 208.
† Ibid., p. 209.

could scarcely have failed to observe it. If such a covering, however, had existed, it must have been destroyed at the exit of the hare.

White of Selborne, in describing the severe season of 1776, still remembered in popular chronology as the *Frosty Harvest*, says, 'the hares lay sullenly in their seats, and would not move till compelled by hunger; being conscious, poor animals, that the drifts and heaps treacherously betray their footsteps, and prove fatal *.' It is by no means unlikely that this was the case with the hare which I started; for I could perceive no foot-prints to or from her little nest; but if she did move out to forage, she must have gone at least a couple of miles to the nearest farmyard, at Whitehaugh, where she had every chance to be shot while tasting the rip of corn usually hung out about the hedges for this purpose; in which way, indeed, I had seen one killed the previous night at Waterhead farm. It may be true, as the older authors affirm, that hares never feed near home, 'either,' says Gesner, 'because they are delighted with foreign food, or else because they would exercise their legs in going; or else, by secret instinct of nature, to conceal their forms and lodging-places unknown †.'

The great naturalist of the middle ages, Albertus Magnus, says, that hares feed only in the night, because their heart and blood is cold; but evidently speaking, as was heretofore the custom, on mere conjecture; for the fact is well known, and one of the most extraordinary in the animal economy, though by no means as yet satisfactorily explained, that the interior heat of quadrupeds varies extremely little in the coldest and in the hottest climates. To the uneducated it appears no less erroneous to say, that the body is equally warm on a cold winter's morning as on the most sultry of the dog-days, as to affirm the sun is stationary, contrary to the apparent evidence of the senses, yet the one is as well ascertained as the other. For example, Captain Parry found, that when the air was from 3° to 32° at Winter Isle, lat. 66° 11′ N., the interior temperature of the foxes when killed was from 106¾° to 98° ‡; and at Ceylon, Dr. Davy found that the temperature of the

* Nat. Hist. of Selborne, lett. 106. † Gesner, as above, p. 209.
‡ Second Voyage, p. 157.

native inhabitants differed only about 1° or 2° from the ordinary standard in England*. At very high temperatures, however, there is a little more difference, as appears from the ingenious experiments made by MM. Delaroche and Berger, who exposed themselves to a heat of 228°, sixteen degrees above that of boiling water: they ascertained that, at such very high temperatures, there is an increase of seven or eight degrees of the centigrade thermometer †. The increase of cold, on the contrary, does not appear to influence the temperature of the body in a similar way, and hence we discover the cause why great cold proves less injurious and fatal to animals than might be *à priori* anticipated. White of Selborne, speaking of gipsies, says, ' these sturdy savages seem to pride themselves in braving the severity of the winter, and in living *sub dio* the whole year round. Last September was as wet a month as ever was known; and yet, during these deluges, did a young gipsy girl lie in the midst of one of our hop-gardens, on the cold ground, with nothing over her but a piece of a blanket extended on a few hazel-rods, bent hoop fashion, and stuck in the earth at each end, in circumstances too trying for a cow in the same condition: within this garden there was a large hop-kiln, into the chambers of which she might have retired, had she thought shelter an object worthy her attention ‡.' The half wild cats, mentioned above, were more attentive to their comforts than this young gipsy; since a neighbouring kiln for drying corn was their favourite resort when the fire was lit.

The law by which animal temperature is thus maintained at nearly the same degree on exposure to considerable heat or cold, though it is not easy to reconcile it to any of the received theories, supplies the only known reason why some of the smaller and seemingly tender animals outlive the rigours of our severest winters. The magpie (*Pica caudata*, Ray), though rather a hardy bird, has been found having recourse to what is often practised by smaller birds—several of them huddling together during the night to keep each other warm. A gentleman of intelligence and veracity informs me that he once

* Phil. Trans. for 1814, p. 600. † Jour. de Physique, lxxi. 289.
‡ Nat. Hist. of Selborne, lett. 67.

saw a number of these birds (probably a young family with their parents) on a tree on a fir plantation sitting so closely together that they all seemed to be rolled up into a single ball. Little is known of the roosting of these birds; but among smaller species the habit in question is not uncommon. Even during the day, in severe winter weather, I have observed a similar practice in the house-sparrow (*Passer domesticus*, Ray). On a chimney-top, which can be seen from my study window, I have often remarked the whole of a neighbouring colony of sparrows contest by the hour the warmest spot on the projecting brick ledge, which was in the middle. Here the sun shone strongest, the kitchen-fire below sent its most powerful influence, and here the middlemost bird was best sheltered from the frosty wind, which swept by its more unlucky companions that had been jostled to the two extremities of the row; but none remained long in quiet: for as soon as the cold air pinched them on the exposed side, off they popped to the middle, scolding and cackling most vociferously, and, as those who held the best places refused to give them up, the new comers got upon their backs and insinuated themselves between two of the obstinates, wedge-fashion, as you thrust a book into a crowded shelf. The middle places were thus successively contested, till hunger drove the whole colony to decamp in search of food.

I once witnessed, near Eltham, a similar contest for places among a family of the bottle-tit (*Parus caudatus*, Ray), whose proceedings I had been watching, while they flitted from spray to spray of a hawthorn hedge in search of the eggs of a coccus (*Coccus Cratægi?* Fabr.). The ground was covered with snow, and as evening approached, the little creatures, whose restless activity had no doubt tended to keep them warm, retreated from the open hedge to the shelter of a thick holly; 'the leading bird,' as Mr. Knapp correctly describes, 'uttering a shrill cry of *twĭt, twĭt, twĭt*, and away they all scuttled to be first, stopping for a second, and then away again *.' When they had all assembled, however, on an under bough of the holly, they began to crowd together, fidgeting and wedging

* Journal of a Naturalist, p. 164. Third edition.

themselves between one another, as the sparrows had done; but whether they intended to roost there, or were merely settling the order of precedence before retiring into some hole in the tree, I did not ascertain, for, in my eagerness to observe, I approached so near as to alarm them, and they all flew off to a distant field.

That the contest for places among the little bottle-tits was only previous to retreating into some more snug corner for the night, appears to me probable, from the known habits of their congeners, and also from what I daily observe among sparrows. Every evening before going into their roosting holes, the latter assemble on some adjacent tree or house-top, squabbling and shifting places for a considerable time, and then dropping off, one by one, according as they seem to have agreed upon the etiquette of precedence. Hardy as they certainly are, sparrows manifest great dislike to exposure during the night, and, accordingly, they may be observed taking advantage of every variety of shelter. They are most commonly seen, indeed, creeping under the eaves of houses, or the cornices of pillars, but they are equally fond of a hole in a hay-stack, of getting under the lee-side of a rook's nest on a lofty tree, or of popping into a sand-hole burrowed out for its nest by the bank swallow (*Hirundo riparia*, RAY).

But while I am disposed to give sparrows all due credit for their tact in discovering the warmest and best sheltered roosting places, I am convinced that White of Selborne attributes to them more intelligence than can be verified by facts. 'House-sparrows,' he says, 'build under eaves in the spring; as the weather becomes hotter, they get out for coolness, and nest in plum trees and apple trees *.' Dr. Darwin, on the other hand, imagined that the sparrows betake themselves to trees when they cannot find convenient holes, and, therefore, mentions as a singular circumstance, that 'in the trees before Mr. Levet's house in Lichfield, there are annually nests built by sparrows; a bird,' he adds, 'which usually builds under the tiles of houses or the thatch of barns †;' while M. Bonnet, taking a directly opposite view, says, 'Il l'établit *pour*, *l'ordi-*

* Letter 60. † Zoonomia, i. xvi. 13, 2.

naire, au sommet des arbres—et lorsqu'il bâtit son nid sous les tuiles ou sous les entablemens des édifices, il se dispense des frais de la calotte, qui serait, dans ce cas, très-superflue *.'

The truth seems to be, that the sparrow does not give itself much trouble in selecting its abode, depending on its industry and ingenuity for rendering, by means of a mass of hay and feathers, the bare branch of a tree as warm as a hole in a hay-stack or a burrow of the bank-swallow; and, accordingly, there may be observed, at least near London, about an equal number of sparrows nestling on trees, and under various sorts of shelter, and not at all, so far as I can perceive, influenced, as White would have it, by cold or warm weather.

Mr. Leonard Knapp, in conformity with the latter view, gives a very different account of the alleged intelligence of birds, in the instance both of the thrush family (*Merulidæ*, Vigors) and the sparrow. 'Birds,' he says, 'that build early in the spring seem to require warmth and shelter for their young; and the blackbird and thrush line their nests with a plaster of loam †, perfectly excluding by these cottage-like walls the keen icy gales of our opening year; yet, should accident bereave the parents of their first hopes, they will construct another, even when summer is far advanced, upon the model of their first erection, and with the same precautions against severe weather, when all necessity for such provision has ceased, and the usual temperature of the season rather requires coolness and a free circulation of air. The house-sparrow,' he adds, 'will commonly build four or five times in the year, and in a variety of situations, under the warm eaves of our houses and our sheds, the branch of the clustered fir, or the thick tall hedge that bounds our garden, &c.; in all which places, and without the least consideration of site ‡ or season, it will collect a great mass of straw and hay, and gather a profusion of feathers from the poultry yard to line its nest. This cradle for its young, whether under our tiles in

* Contemplation de la Nature, ch. 28, note 6.

† This is a mistake: the thrush never uses loam, but forms her plaster of horse or cow-dung and fibres of rotten wood cemented with saliva, as I have proved by examining numerous specimens of the nests.

‡ This is at variance with the above extract from Bonnet, as well as with my observation.

March or July, when the parent bird is panting in the common heat of the atmosphere, has the same provisions made to afford warmth to the brood *.'

So true is this of the thrush and the blackbird (*Merula vulgaris*, Ray), that in the early spring they seem not even to take the usual precautions for concealment, as I have often seen these nests in leafless bushes: a thrush's I particularly recollect observing near Blarney-Castle, in Ireland, about the end of March, when the winds were almost as biting and cold as in January, placed in the naked fork of a young oak. In accordance with their usual instinct, the mother-bird was so afraid of exposing her eggs to the cold wind, that she suffered me almost to touch her before she would stir from her place. This family of birds, however, though so careful to provide shelter and warmth for their eggs and young, shew no wisdom in procuring the same comforts for themselves during winter, as they usually roost along with red-wings and chaffinches in the open hedges, where they are often frozen to death in severe weather †, or are captured by bat-fowlers. The starling (*Sturnus vulgaris*, Linn.) exhibits more care for itself, by roosting in the holes of trees, in the towers of churches, or under the tiles of an old house, like the sparrow, and frequently among the thick tops of reeds, in marshes. Yet will they sometimes suffer from frost even there; and one winter's day in 1822, after a very keen frost in the night, when I was searching for lichens on the trees in Copenhagen-fields, I found a cock starling lying in a hole, frozen to death. It was in very fine condition, and more perfect in plumage than I ever saw this species; but it did not appear, upon the closest examination, to have received any shot or other injury to cause its death besides the effects of the frost.

It may be remarked, that, like the sparrows and other birds which roost in holes, the starlings huddle closely together, contending for places, a circumstance, indeed, recorded by Pliny. 'As touching sterlings,' says he, 'it is the property of the whole kind of them to flie by troups, and in their flight to gather round into a ring or ball, whiles every one of them hath

* Journal of a Naturalist, p. 167. Third edition.

† White's Selborne, letter 150.

a desire to be in the middest *,' corresponding exactly with what I have above mentioned of the sparrows and bottle-tits. It is not a little interesting thus to verify facts which were observed by the ancients; and Mr. Knapp has done so in the instance of the starling now under consideration. 'There is something,' he remarks, 'singularly curious and mysterious in the conduct of these birds previous to their nightly retirement, by the variety and intricacy of the evolutions they execute at that time. They will form themselves, perhaps, into a triangle, then shoot into a long, pear-shaped figure, expand like a sheet, wheel into a ball, as Pliny observes, each individual striving to get into the centre, &c., with a promptitude more like parade movements than the actions of birds †.'

In the instance of the redbreast, the hedge-sparrow (*Accentor modularis*, Bechstein), and the wren (*Anorthura communis*, Mihi), one can scarcely imagine how any of the species survive the winter, were it no more than the difficulty of procuring food. Selby, indeed, has observed wrens to perish in severe winters, particularly when accompanied with great falls of snow. 'Under these circumstances,' he says, 'they retire for shelter into holes of walls, and to the eaves of corn and haystacks; and I have frequently found the bodies of several together in old nests, which they had entered for additional warmth and protection during severe storms ‡.'

My friend, Allan Cunningham, tells me that he once found several wrens in the hole of a wall, rolled up into a sort of ball, for the purpose, no doubt, of keeping one another warm during the night; and though such circumstances are only observed by rare accident, I think it very likely to be nothing uncommon among such small birds as have little power of generating or retaining heat in cold weather. This very circumstance, indeed, was observed by the older naturalists. Speaking of wrens, the learned author of the *Physicæ Curiosæ* says, they crowd into a cave during winter to increase their heat by companionship: 'Multi uno specu in hyeme conduntur, ut parvus in tam minutis corporibus *calor*

* Natural Historie, by P. Holland, p. 284. Ed. 1634.
† Jour. of a Naturalist, p. 195.
‡ Illustrations of Brit. Ornith. i. 197.

societate augeatur *.' The value of this author's testimony, however, may be estimated by his adding, that when wrens are put upon a spit to roast, it turns of its own accord; a fact which he professes to have himself witnessed, in company with the celebrated Kircher, at Rome, they being commanded to try the experiment by a certain eminent cardinal, who furnished the bird, and a hazel rod for a spit. At first they despaired of success; but just as Kircher, who had lost all patience, was going away, the spit (*mirabile dictu!*) began to turn slowly† ! ! Those who keep wrens in cages, usually furnish them with a box, lined and covered with cloth, having a hole for entrance, where they may roost warmly during the night ‡. Yet even in keen frost the wren does not seem, in the day-time, to care much for cold, since I have, in such cases, frequently heard it singing as merrily as if it had been enjoying the sunshine of summer; contrary to the remark of White, that wrens do not sing in frosty weather §.

It is in a similar way that the cold is braved by the tiny harvest mouse (*Mus messorius*, PENNANT), the least of our British quadrupeds, which only weighs about an eighth of an ounce, and measures two inches and a half, exclusive of the tail. This little creature does not appear to become torpid like some of the same order of animals; but a party of them assemble at the close of autumn, dig a deep burrow to contain their colony, and, collecting a competent stock of provisions for their common use, they crowd together, as we have seen some species of birds do, to economize their animal heat by sharing it in common. We are not sufficiently acquainted with the winter habits of several others of our little quadrupeds—such as the water-shrew (*Sorex fodiens*)—to say how they pass the winter, though it is not improbable they adopt some similar method to the one just mentioned, as they are not known to become torpid. The only notice we possess of the water-shrew is one by an ingenious living observer, Mr. Dovaston, of Shrewsbury, who saw one in April burying itself under some leaves, at the bottom of a pool. White records an instance of the water rat (*Mus aquaticus*, MERRET), as having a winter cell

* Phys. Curiosæ, p. 1249.
† Idem, ib.
‡ Syme, Brit. Song Birds, p. 159.
§ Selborne, let. 60.

in a dry chalky field, far from water, artificially formed of grass and leaves, and containing above a gallon of potatoes regularly stowed; but whether this is done by its congeners remains to be discovered.

The retreat of the dormouse (*Myoxis avellanarius*, Fleming) is better known. These little creatures provide a store of nuts and grain, and retire to holes at the bottom of hedges and trees, where they commonly lie torpid in cold weather, but in mild winters remain awake and feed on their stores, in the same manner as the squirrel. In the winter I have found great numbers of their nests, about four or five feet from the ground, in hedges and hazel copses, and I imagined it possible that some individuals might winter there almost as snugly as under ground; for they are constructed with a great quantity of dry grass leaves, well wound together, with no perceptible opening; but among at least fifty of those which I have examined, I found no inhabitants, and, therefore, conclude that they are only built to protect the young during our colder summer nights, and placed high in the bushes, to be somewhat out of the reach of cats and weasels.

Having recently had occasion to investigate the structure of various nests with some minuteness, I have been led to adopt the opinion, that the arched coping or dome,—so remarkable in several small birds for ingenious and beautiful workmanship, —is designed to preserve their animal heat from being dissipated during the process of incubation—an opinion which appears corroborated by our native birds that thus cover in their nests at top being all very small. Among these are the common wren, the wood wren (*Sylvia sibilatrix*, Bechstein), the hay bird (*S. trochilus*), the chiff-chaff (*S. hipolaïs*), the gold-crested wren, the bottle-tit (*Parus caudatus*, Ray), and the dipper (*Cinclus aquaticus*, Bechstein). There are other birds, no doubt, a little larger than these, such as the blackcap and the babillard (*Curruca garrula*, Brisson), which do not build domed nests; but it is worthy of remark that the latter usually lay much fewer eggs,—the babillard seldom more than four, and the blackcap four or five; while the gold-crested wren lays from seven to ten, the bottle-tit from nine to twelve, and common wren from eight to (some say) fourteen

and even twenty. It will follow, of course, that, in order to hatch so large a number, these little birds require all their animal heat to be concentrated and preserved from being dissipated. The dipper, indeed, lays but five or six eggs, and weighs from six to eight times more than any of our other dome-builders; but it is to be recollected that its being a water bird, and building near water, it may have more occasion to use 'all appliances' to concentrate its heat. Such are some of the circumstances which occur to me corroborative of the opinion.

On the other hand, it may be alleged, that there are more birds in tropical countries which cover their nests with domes, than among our European natives*. I would account for this, however, on the principle of procuring shade, in the same way as our sailors put up an awning on deck in tropical latitudes; for birds, constantly sitting on their eggs during incubation, must in these countries be frequently exposed to the rays of a vertical sun, which could scarcely fail to prove injurious. I am well aware that it is the received opinion, these covered nests are designed to protect the eggs and young from snakes; but this mistaken notion has been adopted, without taking into account the natural habits of the accused snakes—the smaller ones of which would more readily pry into a nest with a narrow hole for a side entrance, than into an uncovered nest. In the instance of a domed nest of the hay-bird (*Sylvia trochilus*), with which I was acquainted last summer, I found a large snake (*Coluber natrix*) lying close by it, and the eggs untouched; but as it had just swallowed a large frog, which it disgorged upon being caught, it might, probably, have no appetite at this time for the hay-bird's eggs.

The same anxiety to secure warmth by preventing the dissipation of animal heat evidently actuates the wild animals which pass the winter where snow is either permanent or partial. Some of these, such as the marmot (*Arctomys Marmota, A. Bobac*, &c.), lie several weeks or months torpid in cells, previously prepared with no little care. This preparation of a winter abode has always excited admiration; and

* Prince Maximilian's Travels in Brazil, p. 105

hence, as is usual in such cases, it has been exaggerated by the fancies of inaccurate observers. 'Their wit and understanding,' says Gesner, 'is to be admired; for, like beavers, one of them falleth on the back, and the residue load his belly with the carriage, and when they have laid upon him sufficient, he girteth it fast by taking his tail in his mouth, and so the residue draw him into the cave;'—'but I cannot,' he well adds, 'affirm certainly, whether this be truth or falsehood; for there is no reason that leadeth thereunto, but that some of them have been found bald on the back*.' I should not have alluded to this evident fable here, were it not that it is still met with in recent scientific publications, gravely stated as an ascertained fact; and M. Beauplan goes so far as to imagine that he has seen a party trailing one of their loaded companions by the tail, taking care not to overset him †. This feat, however, seems to be outdone by the legend lately given as authentic of the marmot's skill in haymaking. 'They bite off the grass,' it is said, 'turn it, and dry it in the sun ‡;' stories too absurd for the almost indiscriminating credulity of either Linnæus or Buffon, both of whom reject them.

But even several animals which do not become torpid, and provide no hay-lined cell as a snug retreat from the cold, contrive to prevent the dissipation of their animal heat by retreating under the snow itself, taking advantage of the covering furnished by Providence for the protection of vegetables. The latter is beautifully illustrated, as it appears to me, by what occurs in the cultivation of Alpine plants in our gardens, many of which, such as auriculas, some saxifrages, &c., are not unfrequently destroyed or rendered unhealthy by our winters, whilst they flourish amidst their native snow; wholly, it is probable, because, in the Alps, where they are growing wild, they are throughout the winter covered with a complete coating of snow, which, from not being a rapid conductor of heat, is instrumental in the earth's not parting quickly with its warmth, in the same manner as woollen garments prevent the escape of heat from the body; this protects them through the

* Hist. Animals, by Toplis, p. 407.
† Descript. Ukraine.
‡ London and Medical Gazette for 1828, and Mag. of Nat. Hist. i. 377.

cold season. Whereas, in our climate, these plants are exposed alternately to the severe influence of frost (unprotected by the covering of snow), and to long continued rains. Even during the winter months our plants frequently commence growing before the spring arrives, and thus are rendered more obnoxious to the succeeding frosts; and, in addition, the chief strength of the plants (which should be reserved for the great effort to be made in the spring) is exhausted before its due season, whilst, in the Alps, they lie entirely dormant until the sun at once melts the snow, and calls them into life and blossom. Gardeners, accordingly, in the cultivation of the finer sorts of auriculas, &c., have to imitate, as far as possible, their native climate, by protecting them in a frame or shed both from the severe frosts and wet.

Amongst the animals which take advantage of the non-conducting property of snow, the white grous, or ptarmigan (*Lagopus vulgaris*, Flem.), may be mentioned, which will burrow under the drifted wreaths, picking up a scanty subsistence among the herbage and seeds of heath for many weeks. This, indeed, may be considered one of its destined and regular habits *; and it no doubt feels as comfortable while it is protected from the keen frosty gales of the mountain by its snowy canopy, as does the partridge of the low country when skulking for a similar purpose under the lee side of a hedge; but there are two other native species of grous, the black cock (*Tetrao Tetrix*), and the moor fowl (*Lagopus Scoticus*, Flem.); the latter peculiar to Britain, which only resort to the same expedient when forced by accident. The common shelter of both of these is the higher and more bushy clumps of heath (*Calluna vulgaris*, Hooker); but when these, as occasionally happens in most winters, become covered with snow, the grous find it convenient to remain under cover rather than venture abroad where they have less chance of meeting with food and shelter.

It appears to arise from some instinctive presentiment of the same kind, that sheep, during a snow-storm, always flee to the nearest shelter, though this is certain to end in their destruc-

* See Olaus Magnus, Hist. Septentrion. xix. 33, for an interesting account of the mode of hunting these birds.

tion, if the snow fall deep and lie long; it therefore becomes one of the most painful tasks of the shepherd in such circumstances to keep his sheep steadily in the very brunt of the blast. This I was at least told by an old shepherd whom I encountered at night-fall the end of December, 1808, in a wild mountainous pass, near Douglas, on the borders of Lanarkshire, who was actually engaged in thus guarding his flock in as heavy a fall of snow as I recollect ever witnessing. The Ettrick Shepherd, in an intensely interesting narrative, entitled 'Snow Storms,' in his *Shepherd's Calendar*, does not allude to this propensity in sheep; though it may be inferred that they had acted upon it, from his having found a number buried under the snow by the side of a high bank, under which, no doubt, they had fled for shelter, at the onset of the storm. Though sheep, from their mode of life, ought to be hardy, they exhibit an anxiety for procuring shelter well worthy of remark. It is mentioned by Lord Kames*, that the ewe, several weeks before yeaning, selects some sheltered spot where she may drop her lamb with the most comfort and security; and Hogg, in the volume just referred to, gives an instance in which a ewe travelled to a great distance to the spot where she had been accustomed to drop her lambs; but what was still more remarkable, a ewe, the offspring of this ewe, though removed to a distance when a few days old, returned to the same spot to drop her first lamb†.

The care taken by insects for warmth is shewn by the early appearance of some species. Although few insects are seen during cold weather, yet on fine days some are always stirring; but it is much less wonderful to see the larger butterflies (*Vanessa Urticæ, Gonepteryx Rhamni*, &c.) braving the cold, inasmuch as their bodies and wings are warmly clothed with down and feathers, than some of the more delicate moths (*Tortricidæ, Tineadæ*) which appear to be far less comfortably clothed. The common hive-bees, when tempted by a glimpse of sunshine to leave their hive, frequently perish of cold before they can effect their return, though they also have a tolerably thick coat of hair for their defence. This early appearance of bees, however, as well as of some butterflies, may be considered

* Gentleman Farmer, p. 45.

† Shepherd's Calendar, Chapter on Sheep.

as accidental rather than according to the usual order of things; but there are several insects whose regular time of appearance is fixed by nature in the first months of the year, probably for the purpose of supplying a scanty meal to such of the soft-billed birds as are permanent residents, the berries on which they have in part subsisted being now useless or exhausted. Amongst these we may reckon the small egger-moth (*Eriogaster lanestris*), which is disclosed towards the end of February, having lain from the preceding July in a pupa case similar to plaster of Paris in consistence and appearance. The moth itself is but of middle size, and is pretty closely covered, particularly on the body, with hair. Its inconspicuous chocolate-brown colour might furnish the advocates for concealment in respect of colour with a very good illustration.

The little gnat (*Trichocera hiemalis*, MEIGEN), which may be seen in troops during winter weaving eccentric dances in the air even when the ground is covered with snow, flies for shelter, as I have frequently found, to the hollow stems of umbelliferous plants and similar places near its usual haunts. A much smaller and more delicate fly, which has not a little puzzled systematic naturalists to class (*Aleyrodes Chelidonii*, LATREILLE), preserves itself from the cold in a similar manner. This species is so small, that it would not cover the area of a pin's head, and its snow white wings, as well as its elegant form, might entitle it to the appellation of the mite-butterfly; yet so well does this tiny creature know how to avoid cold, that, after the severe winter of 1829-30, I found three of them sporting about in March in Shooter's-hill Wood, as lively as if no frost had occurred.

During the previous frost in that season, I opened two nests of the yellow ant (*Formica flava*), in which the inhabitants were by no means torpid or inactive, although not so lively as in summer; but these nests had been carefully constructed in a peculiarly warm situation, being both in the trunks of old willows, rendered quite spongy by dry-rot, and facing the south-west, where they had the benefit of every glimpse of sunshine. Ants, indeed, exhibit the most extraordinary tact in attending to variations of temperature, so much so, that they might, in a glazed formicary, constructed upon Huber's plan, be made to

serve the purpose of a thermometer. Sir Edward King, an excellent naturalist, who lived in the time of King Charles II., seems to have been the first to discover this peculiarity:—'I have observed,' says he, 'in summer, that in the morning they bring up those of their young, called ants-eggs (*cocoons*), towards the top of the bank, so that you may, from ten o'clock till five or six in the afternoon, find them near the top, for the most part on the south side. But towards seven or eight at night, if it be cool, or likely to rain, you may dig a foot deep before you can find them*.' Ants, during winter, unquestionably manifest more intelligence, instinct, or whatever it may be termed, than bees; for the hive-bee will rashly venture abroad on the occurrence of a mild day, or even of a few hours' warm sunshine, when the ground is covered with snow; but I have never observed ants, either in the colonies naturally established, nor in the artificial formicaries that I have kept, tempted to venture abroad before the return of spring. The result is, that the bees (foolish in this instance, though wise in so many others) frequently perish from their rashness; while the ants are snug in their cells. This is the more surprising, that in the instance of swarming bees appear to be uniformly regulated by the temperature of the weather, and will not leave the original colony when the air is below a certain degree.

While I was concluding this paragraph, I was, by accident, furnished with an example of the contrivances in question in a well known insect—the flea (*Pulex irritans*), which chanced to leap upon my paper, and, as I took care not to disturb it, I observed it attempting to dig a burrow with its beak. To this task, I have no doubt, it was sufficiently equal; but after working into the paper, so as to make a perceptible hole, it abandoned the spot, as if it did not like the material. After skipping about for some time, it settled on the green cloth cover of my desk, where it again made an attempt to burrow; and I remarked that, in this case, contrary to its mode of working on the paper, it threw itself on its back, pushing the wool upwards with its feet, and downwards with its shoulders,

* Phil. Trans. No. xxiii. p. 425-7.

till it wedged itself into the nap quite out of sight, intending of course to lie snug and warm till hunger should prompt it upon a foraging excursion. I am well aware that observations like this have drawn forth the ridicule of witlings, who have represented naturalists as little better than children or idiots; but, if the Great Creator did not think it beneath him to adapt, with wonderful skill, the structure of a flea to its mode of life, it can never be a trifling study to observe and admire such instances of his providential wisdom.

Many other illustrations of the attention of animals to secure warmth crowd upon my recollection; but, as this paper may already be deemed too tedious, I shall, for the present, forbear to go into further detail.

Lee, Kent, March 7th, 1831.

ON THE AURORA BOREALIS OF THE 7th OF JANUARY, 1831.

By Dr. MOLL, of Utrecht.

FOR many years the beautiful phenomenon of the Aurora Borealis has been of very rare occurrence in this country; so much so, indeed, that I do not recollect having seen it more than once, and that was in 1828, and even then it was in England. During the time of the late Professor Van Swinden's residence in the University of Franeker, between 1766 and 1784, particularly in 1769, 1772, 1773, and 1777, it was very frequently witnessed by that diligent and accurate observer, and his observations are well known to the scientific world. Since that period, it scarcely ever shone in all its splendour; and now and then only its existence in more northern regions has been announced to us by some faint coruscations near the boreal part of the horizon.

On the 7th of January last a beautiful exhibition of this phenomenon was witnessed here between 6 and 10 P.M., the effect of which was particularly striking. The sky was very clear and transparent; the stars were remarkably bright; Cassiopéa nearly in the zenith; Orion ascending in all its glory towards the meridian; Procyon standing in the

east, whilst Lyra and the Swan descended towards the horizon in the N.W. The air was very calm: after some days of thaw the weather had become frosty. The thermometer, during the time of the phenomenon, ranging between 26° and 24° Fahr. The little breeze there was blew from the S.E.

From the S.W. to the N.E. a bright arch of whitish light extended itself through the firmament, its width being about 10° or 12°. This luminous arch passed through the zenith, a little to the northward of the Pleïades. Its light was of a white colour, and uniform throughout. Shortly after, a second similar arch sprang up to the north side of the first. From the S.W. to the N.E. a column of light arose in an oblique direction; a similar one formed in the zenith; these three columns joining together, and thus a double arch of unparalleled beauty illuminated the heavens, whose continual coruscations formed a most extraordinary spectacle. To the south of this arch, in that part of the sky where Orion then was, and somewhat lower than γ and α of that constellation, and near the Eagle and Dolphin, the firmament was of a dark blue; and Orion, glittering on this dark ground, shone in beautiful contrast with the vivid light of the luminous arch. The appearance of this arch or luminous belt lasted only a few minutes: it began first to fade in that part of the air whence it arose in the beginning. In the N.W. the air was illuminated as if by the crepuscule of a summer's night.

Being then in the country, I hastened to an open field, where the view of the horizon in the north was not impeded by buildings. There, in the north, that luminous *circular arch*, which is so frequently mentioned by writers on the Aurora Borealis, was splendidly visible. I would rather call it a *segment of a circle*, of which the horizon was the chord. The bright star α Lyra was nearly in the middle of this segment, and it extended as far northward as the tail of Ursa Major. Under this luminous arch the sky was somewhat blacker; but I did not observe under it that dark part which frequently occurs in descriptions of the Aurora Borealis. From out of this luminous arch, as if from its centre, rays of tremulous white light were incessantly springing up in all directions; of these

rays have been often (not unjustly) compared to the sticks of a fan. Sometimes these rays ascended nearly as high as the zenith, then disappeared, and were succeeded by others. The space between the columns was frequently of a beautiful rose-colour.

In about half an hour the flame-like rays ceased to rise from the luminous segment in the N.W.; but the segment still continued to shine with a softer light.

At about nine o'clock the beautiful appearance called by authors on the Aurora, *the Pavilion*, was displayed. From the zenith, large and bright streams of flame-like light descended towards the S.W., N.E., N. and N.W. in splendid succession; and the view they afforded was sublime and magnificent beyond description. The N.W. part of the firmament was now covered with coruscations of glowing red light, continually varying in appearance. The brown heath on which I walked was so illuminated as to make objects appear perfectly distinct, even at some distance.

During the vivid and sudden changes of these luminous flashes, a single mass of light, like a cloud, arose from the N.E. towards the zenith, passing in quick motion very near the Pleïades, and disappearing in the S.E. This orb of light, through which the stars were visible, was round and globulous in the fore-part, and terminated in a flaming, tapering tail. Its appearance was short and very striking. It was a glorious illustration of the truth of Lucan's description:—

> Ignota viderunt obscuræ sidera noctes;
> Ardentemque polum flammis; cœloque volantes
> Obliquas per inane faces.

The phenomenon disappeared gradually; the Pavilion lasted but a short time: at about ten o'clock the luminous arch alone remained visible in the N.W.; this continued during several hours, till the wonted darkness of night was entirely restored.

The next day the weather was thawing and snowy, the wind S.E. The sky, however, seemed in the night following somewhat brighter in the N.W.

The following is the abstract of the barometric and thermometric observations some days before and after the Aurora Borealis.

OBSERVATORY, UTRECHT.

	Hour. H.	M.	Barom. Inch.	Therm.	Weather.	Winds.
1831, Jan. 5.—	2	33	29.807	31.3	Foggy.	Calm.
6.—	6	8	30.083	32.6	Thawy.	N.N.E.
7.—	1	34	30.500	29.3	Very clear.	N. afterwards S.E.
8.—	2	48	30.400	29.9	Hail and Snow.	S.S.W.
9.—	2	42	29.963	36.8	Cloudy.	W.S.W

On the 3d of March, 1821, when a shock of earthquake was felt at Dover and the neighbouring places, nothing in the atmospheric state indicated in this country that anything extraordinary was happening at so short a distance. At about 4 P.M. the barometer stood at 29° 520′, the thermometer at 47° 3′. The air was dark and overcast; it blew a strong gale from the W.S.W.

OBSERVATIONS ON THE AURORA BOREALIS OF THE 7th OF JANUARY, THE 11th OF JANUARY, AND THE 7th OF MARCH, 1831.

By the Hon. CHARLES HARRIS.

IN consequence of the account of the Aurora Borealis of the 7th of January, given by Mr. Christie in the last Number of this Journal, we have been favoured, by the Hon. Mr. Harris, with the following extracts from his Meteorological Journal, kept at Heron Court.

Friday, January 7th, 1831.—Magnificent Aurora Borealis at night. It first appeared about 5^h 30^m P.M., in the shape of a white cloud in the north, 10° in depth and 55° high, extending from west to east, much denser towards its extremities, where it was edged off with prismatic tints of red and green. It seemed coming over from N., and a narrow band of it passed over as far as about 20° on the south of the zenith. In the north it soon became traversed with bright columns, with here and there a hazy patch of flame colour, especially in the N.W. It was most beautiful about nine P.M., when the cloud came nearly over head, being driven, as it were, by the wind from the north. Its eastern extremity, which was very dense and luminous, drove by as far as E.S.E. Suddenly, however, it streamed back again, in bright streaks, towards due north, look-

ing as if the fiery cloud had burst. In the midst of these streaks appeared bright patches, especially N.E., of the most brilliant flame colour. At one time, these united and formed a most beautiful track of pale crimson from N.W. to N.E., the flame colour in some places occurring outside the white cloud against the clear sky. The northern edge of the cloud gradually became dense and ragged, while the sky in the north, to the height of twenty degrees, was of the most inky blackness. Excepting the bright prismatic patches of crimson, faint green, and sometimes yellow, it had all the appearance of a spent snow-squall strongly lighted by the moon. By ten P.M. the light gradually withdrew from the zenith, and settled very like a bank of cloud, extending from west to east, about 10° in depth, the sky beneath, for 20°, being perfectly black. In the brightest parts of the cloud the stars were seen, but faintly; and, indeed, Ursa Major, at one time, was scarcely visible. The thermometer at the time 24°, barometer 30.60.

Tuesday 11*th.*—Aurora Borealis visible at eight. About 8^h P.M. it appeared in the form of two horizontal luminous bands of cloud, about 20° high, and extending from N.W. to N.E., with a distinct dark space intervening. About 8^h 30^m they were rendered more confused by a hazy white light streaming up through them from the north, giving the luminous strata the appearance which geologists call 'a fault.' The eastern edge of the light was very clearly defined: indeed it had the appearance of a strong light streaming through a half-closed aperture. Barometer 30.03, thermometer about 30°. At nine P.M. the night became cloudy, and no observation could be made.

Monday, March 7*th.*—Aurora Borealis very fine at night. I observed it first about 8^h 40^m P.M. Its lower edge then formed a very regular arch from N.W. by W. to N.E.; the greatest altitude of which was in N.N.W., about 18°. A large star (Deneb in Cygnus about N. by W.?) was 2° above its lower edge. The light suddenly seemed to become concentrated in the N.E., where the lower edge became irregular and approached the horizon like the folds of a curtain; at the same time three or four streams of vertical white light shot up in the N.E., N.N.E., and N.: these almost immediately faded away;

the Aurora became as before, the light becoming insensibly fainter from the lower edge to about 45°, where it was no longer perceived. After the coruscations in the N.E., the main body of light seemed to move rapidly to the westward, the whole phenomenon, however, becoming much brighter. The lower edge again became ragged, and approached the horizon; and in the W.N.W. brilliant columns of light, tinged with pale flame colour, shot up to the altitude of 50°. From this time to about 9^h 15^m the Aurora was in its greatest beauty; a large body of light appearing constantly travelling from N.W. to N., and *vice versâ.* Whenever coruscations were about to be thrown up, the lower edge of the cloud became like the base of a thunder-shower, the ragged points being at times brilliantly luminous: from these points the columns of light shot up with great intensity. About 9^h 15^m a most splendid coruscation shot up from due N. It appeared to extend down to the horizon, and shot up to the height of about 50°, the column itself being 4° wide. The upper extremity, and, indeed, the greater part of the sky from N. to W.N.W., at the height of about 55°, was of a beautiful pale flame colour. Beautiful columns of light were at the same time shooting up from the N.W. and N.N.W., and faint nebulæ of light were visible about 10° to 15° N. of the zenith; the Aurora then again faded into a large luminous track from N.W. to N.E., and its lower edge but ill defined: the flame-colour remained for some time on the upper edge. Small cirostrati in the N. and N.E. were thrown out in the most striking relief; and at 9^h 15^m Deneb was, for a few seconds, but faintly visible. The light against the north side of a house was as strong as that of the moon at the quarter. At 10^h P.M. the Aurora was very faint; its lower edge about 7° from the horizon, from which the light gradually faded off, and was faintly perceptible at 45°. The sky beneath, however, was of that inky blackness so peculiar to this phenomenon. Barometer, 9 P.M., 29.82, thermometer 40.5. About 4 P.M., in the N.E. and N.W., a thin brown haze was visible, a very unusual circumstance, with a westerly wind and in unsettled weather; the haze split off at times in horizontal tiers, and it was impossible to discover whether it was haze or distant cirrus; indeed,

it was this that induced me to look out; and I have little doubt, from a similar circumstance mentioned by Parry in his first Tour, that this was the *daylight* appearance of the Aurora.

ON THE HEIGHT ABOVE THE SURFACE OF THE EARTH OF A LUMINOUS ARCH OF THE AURORA BOREALIS, ON THE 7th OF JANUARY, 1831.

By S. H. CHRISTIE, Esq., M.A., F.R.S., &c.

THE height of the Aurora Borealis above the surface of the earth has been so variously estimated, that any observavations which determine limits to the height of a particular phenomenon become interesting, although these limits may not be extremely close. The most permanent of the phenomena are the luminous arches, and these are therefore the best adapted for determining the height; but even these appear to be by no means stationary: and as more than one are sometimes visible, it may, in many cases, be doubtful, whether simultaneous observations, made by distant observers, refer to the same arch. The Aurora of the 7th of January last made its first appearance in this neighbourhood, in the south-east, and in a few minutes afterwards, a single well-defined arch, and which was visible but for a short time, was formed across the southern meridian. If then the commencement of the Aurora happened to be observed, in the form of an arch, at a considerable distance to the south of the place where I observed the altitude of the arch, there could be scarcely any doubt of the identity of the luminous band forming these arches. It appears from Mr. Harris's account of the commencement of the phenomenon, that he observed an arch, at Heron Court, to be elevated 55° above the northern horizon, at the same time that I observed one, at Blackheath, to pass over the planet Mars, then not far from the meridian, and about 46° above the southern horizon. Mr. Harris observed nothing to the south of the zenith until some time after the first appearance of the arch: I observed no arch towards the north for a considerable time afterwards; and as we observed at as nearly

as possible the same time, $5^{h}\ 30^{m}$ P. M. in both cases, there can, I think, be no doubt that our observations were made on the same luminous band in the same position. Having, through the kindness of Mr. Faraday, been favoured with Mr. Harris's interesting observations on the Aurora, I have computed the height of this arch above the earth's surface, from these observations and my own; and although there may be some doubts with respect to the absolute height, as determined from these, in consequence of the unfavourable relative positions of the two places of observation, yet, as the limits which they determine are very different from the height most recently assigned to similar phenomena, I do not hesitate to publish the results.

Assuming that the arch was formed by a band of light, of great extent, in a line nearly at right angles to the meridian, and parallel to the earth's surface, it is evident that, although different portions of this band were actually observed, the absolute height will be determined from the observed altitudes of the highest points, and the arc between the parallels to this band on the earth's surface, in the same manner as if the observations had been made on the same portion, from places in a plane at right angles to the arch.

Let then α, β be the angles of elevation of the same point in the arch, at two places A and B, in a vertical plane passing through that point; γ the arc on the earth's surface between A and B; θ the angle contained by two lines drawn from the point in the arch, the one to A, the other to the centre of the earth: then if $\delta = \pi - (\alpha + \beta + \gamma)$, we shall have

$$\cot \theta = \frac{\cos \beta}{\cos \alpha \sin \delta} + \cot \delta;$$

or if β is the greater of the two angles α, β, and $\frac{\cos \beta}{\cos \alpha} = \cos \psi$,

$$\cot \theta = \frac{2 \cos \frac{1}{2} (\delta + \psi) \cos \frac{1}{2} (\delta - \psi)}{\sin \delta}.$$

If r is the radius of the earth, and h the height of the arch above its surface,

$$h = r \left(\frac{\cos \alpha}{\sin \theta} - 1 \right).$$

As nearly as I could judge, circumstanced as I was at the time, the highest part of the arch passed over the planet Mars, then a few degrees from the meridian, and Mr. Harris's observations likewise indicate that the arch was nearly at right angles to the meridian. Assuming then, in the first instance, that this band of light was perpendicular to the meridian, the difference of latitude of Blackheath and Heron Court will be the arc between the two places. The latitude of Heron Court is nearly 50° 47′ N., and that of my place of observation 51° 28′ N., so that $\gamma = 0° 41'$.

Mr. Harris observing towards the north, and my observation being towards the south, the highest point of the arch in the one case would be nearly the lowest point in the other. The altitude of the highest point, according to Mr. Harris's observation, was 55°, and the depth of the arch 10°; so that the altitude of the middle point was 50° from the northern horizon. The altitude of Mars was, at the time I observed, 46° nearly, and the middle point of the arch about 1° 30′ below the planet; so that the altitude of this point was about 44° 30′ from the southern horizon. We have then from these observations $\alpha = 44° 30'$, $\beta = 50°$, $\gamma = 0° 41'$. Substituting these values, and 3960 miles for r, we have

$$\theta = 45° 8', \text{ and } h = 25{\cdot}7 \text{ miles.}$$

This determination of the height of the luminous band above the earth's surface is on the supposition that it was perpendicular to the meridian: if we suppose that the highest part of the arch was accurately in the same vertical plane as Mars, whose azimuth was then about 13° east, or that the band made an angle of 77° with the meridian, the value of h will be considerably diminished. In this case, we must, instead of the difference of latitude, take for γ the arc which would be a perpendicular from Blackheath on the arc passing through Heron Court, and making an angle of 77° with the meridian of Blackheath. The longitude of Heron Court being 1° 50′ W., this arc will be about 23½′. Substituting this value of γ, we shall have $h = 14{\cdot}86$ miles.

There is another supposition, with regard to the direction of the luminous band forming the arch, which would still further reduce its height, as determined from these observations. It has

been supposed that the Aurora Borealis has generally a reference to the *magnetic* meridian, and that the luminous arches are perpendicular to it: indeed, those which I subsequently observed on the 7th of January, generally appeared to have their summits in that direction. According to this supposition, these arches would consist of luminous bands, making an angle, at this place, of about 65° 30′ with the meridian. If this was the case with the arch which I observed to pass over Mars, the value of γ will be reduced to 7′ 40″, and the value of h to 4·9 miles. I do not, however, consider that I made any very sensible error in the situation which I assigned to the summit of this arch, and consequently the observations will not warrant the adoption of such a value of h. I ought, notwithstanding, to notice, that the altitudes of an arch seen later in the evening, would assign nearly this value to h. Mr. Harris states that 'at 10 P. M. the light gradually withdrew from the zenith and settled very like a bank of cloud, extending from west to east, about 10° in depth, the sky beneath for 20° being perfectly black.' The altitude of the lower edge was therefore 20°. On referring to my observations on the Aurora, it will be seen that 'at $9^h\ 50^m$ P. M. three concentric arches were distinctly visible, their highest points being still in the magnetic meridian.' As Mr. Harris mentions but one arch at very nearly the same time, we may infer, that it was the highest of these arches whose lower edge he saw at an elevation of 20°. I did not, at the time, make any particular observation on the altitudes of these arches, but, from recollection of their relative positions, I consider that the altitude of the lower edge of the highest was at least 50°, which, I must remark, is the altitude I assigned to it previous to making any calculations on its absolute height. If this altitude be correct, and this was the same luminous band which Mr. Harris observed, its height above the surface of the earth must have been about 4·7 miles. I do not, however, lay much stress upon the coincidence of this result with the former, as there must be great uncertainty respecting the observations from which it is deduced.

As undoubtedly the summit of the first luminous arch was not to the west of the meridian, we must conclude, from the observations, that it was certainly not more than twenty-five

or twenty-six miles above the surface of the earth, and that it was probably not at a less distance than fourteen or fifteen miles. These are wide limits, but we can scarcely expect, unless under very favourable circumstances, to determine within much narrower limits the region of such variable phenomena. Had the two places of observation, in the present instance been nearly on the same meridian, any error that might have been made in determining the summit of the arch, would not have produced so sensible a difference in the result, and it is to be hoped that simultaneous observations may at future opportunities be made on similar phenomena, by observers in better relative positions than Mr. Harris and myself, and more favourably circumstanced for observation than I was at the time. The points principally to be determined are the altitude and azimuth of the summit of the same arch, at the same instant, by two observers at a considerable distance from each other, in a direction nearly perpendicular to the arch.

The limits which I have thus determined for the height of this luminous arch are much below those which have, most recently, been assigned to a similar phenomenon. From a number of observations, made at very distant places, on a remarkable Aurora on the 29th of March, 1826, Mr. Dalton concludes that a luminous arch seen on that occasion could not be less than one hundred miles above the surface of the earth *. He likewise determines the height of an Aurora, observed at Newton Stewart, Keswick, and Gosport, on the 17th of October, 1819, to have been one hundred miles; and infers, from the observations made at Kendal and Manchester on one seen on the 27th of December, 1827, that its height was nearly the same. In conclusion he says, ' I am induced to believe that these luminous arches of the Aurora, which occasionally appear stretching from east to west, are all of the same height, about one hundred miles.' The following extracts from Lieutenant Hood's Journal at Cumberland House, however, show that the observations made by himself and Sir John Franklin, in North America, indicate a very inferior elevation to the Aurora :—

* Phil. Trans., 1828.

'On the 2d of April (1820) the altitude of a brilliant beam was 10° 0′ 0″, at $10^h\ 1^m\ 0^s$ P.M.; at Cumberland House, fifty-five miles S. S. W., it was not visible. As the trees at the latter station rose about 5° above the horizon, it may be estimated that the beam was not more than seven miles from the earth, and twenty-seven from Cumberland.

'On the 6th of April, the Aurora was for some hours in the zenith at that place (Cumberland House), forming a confused mass of flashes and beams; and in lat. 53° 22′ 48″ N., long. 103° 7′ 17″ W., it appeared in the form of an arch, stationary about 9° high, and bearing N. by E. It was, therefore, seven miles from the earth.

'On the 7th of April the Aurora was again in the zenith before 10 P.M. at Cumberland House; and in lat. 53° 36′ 40″ N., and long. 102° 31′ 41″ W., the altitude of the highest of two concentric arches at 9 P.M., was 9°; at $9^h\ 30^m$ it was 11° 30′, and at $10^h\ 0^m\ 0^s$, 15°, its centre always bearing N. by E. During this time it was between six and seven miles from the earth.

'On the 13th of November the Aurora was seen between the clouds and the earth by Mr. Franklin (Sir John) and Dr. Richardson.

'On the 13th of March (1821) I saw an Aurora, which was emanating in wreaths from the N. W., pass over the lower surface of a stratum of white cloud. The Aurora passed at the altitude of 70°, and therefore could not have been more than two miles from the earth, supposing that the elevation of the clouds was 2½ miles.

'On the 27th of April, $10^h\ 30^m$ P.M., a single column of Aurora rose in the north, and traversed the zenith towards the south; another column appearing N. E. by E., and taking a parallel direction. It passed the western horizon in ten minutes, and was followed by the other, which became brighter as it approached the zenith. I am now convinced they were borne away by the wind, because the columns preserved exactly their distance from each other during their evolution; and some detached wreaths, projected from them, retained the same relative situations of all their parts, which never happens

when the Aurora is carried through the air by its own direct motion. The wind was E. by N., a strong gale, and the temperature of the air 9°*.'

The circumstance here noticed by Lieutenant Hood, of the columns having been borne away by the wind, clearly indicates that they could not have been at a very great elevation. Mr. Harris notices a similar appearance in the Aurora of the 7th of January, and although I did not remark the circumstance, yet the appearance of the detached masses of light, shortly after the first luminous arch had disappeared, was precisely that of low thin clouds illuminated by the moon; indeed I found it difficult to persuade myself that they were not clouds at no great elevation, and only concluded they were not so, from the circumstance, that the moon would not rise for many hours.

From the observations of the Aurora of the 29th of March, 1826, it appears that the luminous band was at right angles to the magnetic meridian, and Mr. Dalton considers that this is the general direction of the arches. I have before stated that, if we suppose this to have been the case with the arch which first appeared in the Aurora of the 7th of January, its height will be reduced to about five miles; so that the result would differ more widely, than in any other case, from what he assigns as the general height of such arches.

Upon the whole, I think we must conclude, that, although these luminous bands may sometimes be at the great height of a hundred miles, yet at others they are at heights varying from five to fourteen or fifteen miles above the surface of the earth.

Royal Military Academy, 9th April, 1831.

* Captain Franklin's Journey (First) to the American Shores of the Polar Sea, Appendix, No. 2.

ON ELATERIUM; AND A NEW PRINCIPLE OBTAINED FROM IT BY ANALYSIS.

By HENRY HENNELL, F.R.S., M.R.I.
Chemical Operator, Apothecaries' Hall.

A FEW weeks since, at a meeting of the members of the Royal Institution, Mr. Brande mentioned a crystalline vegetable principle, which I had recently obtained from elaterium, and which it was supposed might prove to be the active principle of that drug. From experiments since made, I am induced to believe that not to be the case; but as the substance itself has not hitherto been described, and as, in the analysis of elaterium which the discovery of this substance led to, results have been obtained, differing from previously published accounts, a statement of that analysis may perhaps be interesting to medical readers.

Elaterium is procured from the wild cucumber, by slicing and gently pressing the ripe fruit; a juice is obtained, which in a few minutes deposits a greenish-grey fecula, which, when carefully dried, is light and pulverulent.

A tincture made by digesting this substance in alcohol, exhibited, after spontaneous evaporation, evident appearances of crytallization: this induced me to enter into a more careful examination of elaterium. One hundred grains of good elaterium were digested in repeated portions of alcohol, of specific gravity of 0.820, until it ceased to give out colour or taste. The tinctures were mixed, and, after distilling off the greater part of the alcohol, the remainder was left to evaporate spontaneously. Crystals were obtained, mixed with a quantity of green coloured matter; this latter, I found, might be readily removed by sulphuric ether, in which it quickly dissolves, while the crystals are very sparingly soluble in that fluid. The whole mass was washed with two ounces of sulphuric ether (specific gravity 0.750), in three portions, which removed the colouring matter, leaving numerous white distinct crystals. These were dried at a temperature of 212°, and weighed forty grains. The etherial solution was evaporated by a gentle heat; for I had found, by a previous experiment, that the temperature of 212°, if continued for any length of time, destroyed the beautiful green colour which characterizes the substance dissolved by

the ether. The resulting green extract weighed twenty-one grains. It had the characters of a resin.

The residue, not soluble in alcohol, weighed nearly forty grains; boiled in distilled water, six grains were dissolved. This solution was nearly without taste or colour. Alcohol produced in it a white precipitate; solution of iodine, one of a deep blue colour; and from the quantity of this blue precipitate, I should judge the six grains to have been principally starch.

Of the residue, insoluble in water and alcohol, after being dried, twenty-five grains were burnt in a platinum crucible; it burnt like woody fibre, leaving five grains of earthy residue. I ascertained that two ounces of cold ether dissolve about four grains of the crystals: taking this, therefore, into account, from one hundred grains of elaterium, forty-four grains of the crystallizable substance, seventeen grains of green resin, six grains of starch, twenty-seven grains of woody fibre, and seven grains of earthy matter, had been obtained.

The increase of weight here, I attribute to moisture remaining in the crystals and resin, but principally in the latter, which I was fearful of injuring by too much heat.

The crystals are soluble in about five times their weight of cold, and twice their weight of hot alcohol, from which they again deposit in acicular tufts, very sparingly soluble in ether, and nearly insoluble in water, and in dilute acids. Solutions of acetate of lead, nitrate of silver, and sulphate of iron, were not precipitated by the addition of a few drops of the alcoholic solution; the crystals fuse at a temperature between 300° and 400°, and in the flame of a spirit-lamp burn, throwing off a great quantity of carbon. So far as I have observed, these crystals do not form neutral compounds with the acids; they may perhaps be considered as crystalline bitter principle. What (if any) are the medical properties of these crystals, I am not at present prepared to state. Analysis by oxide of copper gave, as the ultimate elements of these crystals,

Carbon	17
Hydrogen	11
Oxygen	18

The green resin already mentioned, possesses all the properties of elaterium in a concentrated form. A tincture, made in the proportion of three and a half grains to an ounce, by

measure, of alcohol, has been administered in two cases of dropsy, in St. Bartholomew's Hospital, by my friend Mr. Blackmore. To a woman, aged forty, labouring under ascites, ten minims were given without much effect; five or six hours after the first dose, twenty minims more were administered, which, in half an hour, produced nausea and sickness, and in the space of a few hours twelve copious and watery evacuations. The secretion of urine was considerably increased. The other case was that of a strong young man, also labouring under dropsy; twenty minims of the tincture were given, which produced nausea and copious watery evacuations. The next day, ten minims more were given, with nearly the same results; and on the third day the purging had not entirely ceased. The secretion of urine did not appear to be increased. The quantity of resin administered to each patient was less than a quarter of a grain. Dr. Paris, in the second volume of his 'Pharmacologia,' gives an analysis of elaterium, and describes the green resin as the active principle, to which he gives the name of 'elatine'; but the crystalline body appears to have escaped his observation entirely, probably from his having operated on very small quantities.

CONTRIBUTIONS TO THE PHYSIOLOGY OF VISION.

No. II.

On the Insensibility of the Retina to feebly-illuminated Objects, when continuously presented.—In accounting for the beautiful and extraordinary phenomenon described at page 111 of this volume, I have admitted as an established fact, that weak impressions on the retina become obliterated when rendered continuous. To confirm the explanation there offered, it will be satisfactory to adduce a few instances equally illustrative of the rule, which is only one case of a more general law, confirmed by numerous observations, viz., that *all* luminous objects, when continuously presented to the same points of the retina, become invisible; and that the rapidity of their disappearance is in proportion to the feebleness of the light emitted by or reflected from the object. Astronomers are well aware that, on looking intently at a star through a telescope, it will sometimes completely disappear, and again become visible on

changing the position of the eye, so that its image may fall on another part of the retina; stars also, which, from the feebleness of the light they emit, are ordinarily invisible, may be made apparent by the same means; and if the statement of Majendie and Desmoulins be correct, stars may be thus made to appear in full daylight*.

Dr. Brewster has described several analogous cases of the disappearance of visible objects †; and the following experiments of the late Benedict Prévost of Geneva (published posthumously in the 'Mem. de la Soc. de Phys. et d'Hist. Nat.,' t. iii. 2d part) will afford an additional instance, and will at the same time prove to us that the employment of very simple means may greatly assist our powers of observation with regard to a variety of optical phenomena, in which the feebleness of the objects might otherwise be deemed an obstacle to successful inquiry.

On an appearance of Decomposition of White Light, by the Motion of the Body which reflects it.—In a chamber sufficiently dark, into which a ray of the sun penetrates, move a rectangular piece of white card, about two inches in breadth, backwards and forwards, as if you would cut this ray nearly perpendicularly to its axis.

At the moment the white card traverses this axis, the eye which regards it evidently receives from this object a white light, as if the card remained stationary at this place. But it happens, however, that the disc, illuminated by the ray, the section of which it represents, appears coloured; it is white only in the centre. The very small white space which surrounds the centre, changes to violet, deepening as it recedes. The violet spot is surrounded by a zone of a deep indigo colour, very distinct and well defined, and exactly resembling the colour of the heart's-ease (*viola tricolor*). Around this indigo zone is a zone of greenish-yellow, equally well defined; then, surrounding it externally, a red tint. If the observer be very attentive, and seize the most favourable moments and situations, it will be seen that the white ray reflected by the disc has been decomposed, as it would have been by the prism, into seven principal colours, arranged nearly in the same order.

* During day-time, in an unclouded sky, the light of the stars, which is but one sixty-fourth of the luminous splendour of the atmosphere, is insensible to our eyes. In general, any body projected and immoveable on a plane with which it has this same degree of luminous intensity, is invisible. But if by a displacement, either of the body upon this plane, or of the image of a star in a telescope, it is made to pass over a certain arch, repeated on the retina by the displacement of the focus of its rays, this body or the image of the star becomes visible.—(Majendie et Desmoulins, Anatomie des Systêmes Nerveux des Animaux à vertèbres. Tom. ii. p. 670.)

† Edinburgh Journal of Science, No. IV.

If a red or pink card be substituted for the white card, the decomposition of the ray appears still more distinctly.

If, on the contrary, a blue tinted card be employed, this decomposition is less distinct than with the white.

Besides, all these colours are subject to vary according to different circumstances, such as the velocity of the motion, the obliquity of the axis to the card which cuts it, the distance from the section to the origin, or to the base of the luminous ray, the different shades or tints of the card, the intensity of the light, &c. But there is always an apparent decomposition.

With a yellow card, a circular areola, of a more brilliant yellow than that of the card itself, is seen externally, when there is no motion.

With a black card there is no coloration, unless a nebulous shade in the centre may be so considered. Besides, it is probable that this shade arises from the black of the card being far from perfect. A card covered with black velvet did not present the slightest appearance of decomposition.

The phenomenon is observed if the card passes the ray but once; this proves that it is independent of the fatigue of the eye.

Neither does it depend immediately on the agitation or motion of the card, but doubtlessly only on some effect of this motion, most probably because the illuminated space strikes the eye during a short time only: for if the card be so large that the illuminated space always remains upon it, and that, notwithstanding the motion, the eye continues always to see it, it appears white, as if it were at rest, and there is no appearance of the decomposition of the light.

In the preceding experiments the coloured rings evidently result from the diffraction of the light at the edges of the aperture which admits the ray; the colours thus produced, being of very feeble intensity, become almost immediately invisible when constantly presented to the same part of the retina; but the intermittent action of the luminous object produces an analogous effect to the shifting of its place on the retina in the previously mentioned experiments. The explanation of these phenomena given by Prévost himself, is founded on Cuvier's theory, which supposes that the visual sensation is occasioned by the chemical action of material light on the nervous substance of the retina; and that each colour, having a different affinity for this substance, requires a different time to exert its energy upon it; but admitting for a moment this totally unsupported hypothesis, the attempted explanation does not accord with the facts of the case.

There is another case of coloration which, I believe, has not yet been noticed, and which admits of a similar explanation. If a sheet of paper, with black characters, either printed or written, be moved rapidly backwards and forwards at the ordinary distance of distinct vision, the lines described by the motion will appear accompanied by very evident colours, the green and red obviously predominating. The experiment succeeds better if the lines are far apart, and perpendicular to the direction of the motion; and is still more perfect if a printed word be fixed at the extremity of a vibrating wire, (as mentioned in the description of the Kaleidophone, in the Journal of Science, N.S., No. xxiii. p. 344). This experiment indicates that there is a faint production of colours at the limits of light and darkness.

From all the known facts, it may be inferred that luminous impressions, continued on the same part of the retina, are evanescent in proportion to their feebleness; and that there are two means by which weak objects may be rendered continuously visible: 1st, by shifting their positions on the retina, and 2ndly, by causing them to act intermittently on the same points of the retina.

Though these are obvious inferences from the collected observations above stated, some of the facts separately presented might appear to admit of other explanations. Thus Majendie and Desmoulins concluded, from the circumstances noticed by them, 'that the sum of a certain number of impressions on different points of the retina in a given time may render a body visible, which would not be so were the interval of the impressions greater, or their number not sufficient.' This explanation would answer only for a limited number of the facts now brought together, and would exclude the experiments of Prévost, where the image is periodically presented to the same part of the retina.

There are various other optical, or rather visual phenomena which equally manifest the truth of the inferences above drawn; but as they are complicated with other circumstances foreign to the present purpose, viz., illustrating the explanation formerly given of the vascular figure of the retina, the consideration of them will be deferred to a future occasion.

C. W.

ON THE RIPPLE-MARKS AND TRACKS OF CERTAIN ANIMALS IN THE FOREST MARBLE.

By G. POULETT SCROPE, Esq., F.R.S., F.G.S., &c.

THE surface of the great elevated oolite range north of Bath is occupied throughout a very considerable area, by highly fissile limestone beds, belonging to the forest marble, and a prolongation of the Stonesfield slate, which are here likewise in general use for roofing buildings. Residing in the centre of this district, I have had frequent opportunities of observing, in a great number of neighbouring quarries, the tendency of this rock to exhibit a *wavy* or *wrinkled* surface, so completely identical in all its varieties with the rippled markings of the sea-sands left dry by the ebbing of the tide upon some of our coasts, as to leave no room for doubting that it was produced precisely in the same manner, at the period of the deposition of the beds.

This configuration, though it has not yet perhaps attracted sufficient attention, (suggesting as it does several very interesting questions, and tending to confirm many important geological views,) has been remarked by others as well as myself, and in other localities. But I have also lately discovered other appearances on the rippled surface of these beds, of a novel character, to which it may be worth while to call the attention of those geologists who have time and opportunity for the further examination of this and similar marine formations.

I have observed the ripple-mark in a vast number of quarries, scattered pretty thickly over a broad band of country, stretching along the eastern slope of the great oolite range from *Bradford* in Wilts, to *Tetbury* in Gloucestershire. I have little doubt that it will be found elsewhere along the continuation of the same beds.

It is repeated throughout a series of strata of considerable thickness; and is continuous, not only over slabs of the largest size which the quarry-men uncover at once, (I have seen one twenty-five feet long entirely covered with these wrinkles,) but apparently extends throughout a very much greater area, to be

measured, perhaps, in miles; the corresponding beds in neighbouring quarries being found to have the same configuration.

It is to be seen chiefly in the *very fissile* laminæ, but not unfrequently on the surface of slabs eight or ten inches in thickness. It affects indifferently those which contain a large proportion of clay, those which are highly calcareous and crystalline, and others in which sand and oolitic grains, or minute fragments of shells predominate. The *only* circumstance, as it appears to me, which the ripple marked beds possess in common, is *their separation from the neighbouring strata, by more or less thin seams of clay*, moulded on the irregular surface below, and by which the preservation of that surface, in complete integrity, exactly as it was figured by the waves of the ocean, at an incalculable distance of time, seems to be simply and naturally accounted for.

The ripple-marks are always on the *upper* face of the bed; but where the seams of clay are very thin, the alternating limestone laminæ have taken the impression of the uneven surface on which they were deposited, and thus present an imperfect ripple on their *lower* face also. In this case, however, the undulations of the upper and lower surfaces do not correspond, but often cross and run counter to one another: occasionally, too, a double system of wrinkles may be seen on the same surface; the undulatory movements of the water, by which they were produced, having shifted their direction (perhaps through a change of wind) during the period of the deposition.

I am not acquainted with any published explanation of the cause of the rippled surface which, at low tide, may be seen extending over many square miles of sand or mud along the Devonshire, Lancashire, and many other of our flat and shallow shores. That it is disturbed and renewed again, partially or entirely, by every fresh tide, is known to all who have remarked the constant changes which it undergoes, and the obliteration of all marks made in the sand at one low tide, before the next ebb. There can be little doubt that it is produced by the oscillatory motion of the lower stratum of water in contact with the sandy or muddy bottom, as communicated to it from the

superficial waves. It is easily imitated by agitating to and fro a vessel of water, with a flat bottom, on which sand has been strewed.

To what depth superficial undulations affect water is a problem yet unsolved, though the general opinion is, that they do not ever extend beyond thirty or forty feet. The most violent movements of the surface water must be neutralized by its inertia, and their lateral extension as they are propagated downwards. But they will probably reach considerable depths before they die away entirely, and will then, I conceive, subside in precisely the sort of gentle and minute oscillations fitted to produce the wrinkles in question in mud or sand, either in the act of subsiding to the bottom, or stirred up there by the commencement or increase of the agitation.

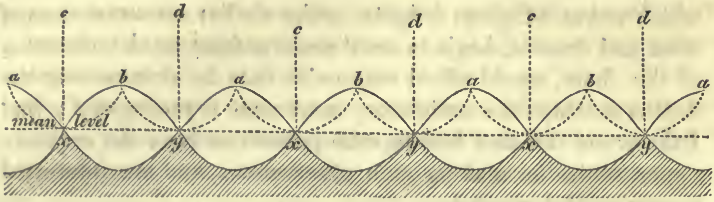
There is an observation which, it strikes me, might perhaps be applied to determine the depth to which the superficial undulations of water are propagated; namely, by ascertaining the depth of the water at the spot where the waves, approaching a shore or shoal, *begin to swell* above their average height in deep water, and to take the line of direction of the shoal or the coast, instead of that which the wind impressed them with originally. Supposing the British Channel wrinkled by waves driven before a powerful westerly wind; these waves, which in mid-channel have their long axes directed due north and south, will, as they near the coast on either side, but particularly the shoaling coasts of Devon and Dorset, gradually take the line of the shore, upon which each wave *breaks* at length in a direction nearly exactly parallel with all its sweeps. This alteration of their original direction is, no doubt, impressed on them by their reaction from the bottom, the resistance of which retards the waves as soon as they come in contact with it, and gradually compels them to assume its sweep. The reaction of each oscillation from the bottom also causes it to rise by the rebound higher at the surface, and hence the *swell* of each wave as it nears the shore, and its beautiful curve and fall at last, owing to its superficial movement outstripping that of the lower part, which is retarded by the friction of the bottom.

A series of careful observations on the depth of the water at

which waves of given heights, under similar circumstances of wind and current, begin to swell and conform to the direction of the shore, would afford reasonable data for determining the depths to which the undulatory movement is propagated; and I throw out this as a hint to such persons as have the requisite opportunities for making such observations, and are interested in solving this question.

A wave is clearly a parcel of water heaped up by the concussion between any external impulse and the resistance of its own inertia. If the impulse is single, as when a stone is thrown into still water, the disturbance subsides through a succession of oscillations, like those of a pendulum, till the equilibrium is restored. The wave formed in the parcel of water first affected, as it descends, communicates the impulse laterally to the next parcel, which consequently rises, and falling again transmits the impulse to a third, and so on, until the original impulse is lost by expansion. If the impulse is continued, as by the prolonged action of *wind* on the surface of water, the waves maintain their full force, or rather increase gradually, so long as the fresh impulse received during each oscillation is greater than that lost by lateral or vertical dispersion. Hence, when a wind begins to act on a calm surface of water, the waves, at first small, gradually *wax* higher and broader, and no doubt progress downwards in the same ratio; and what sailors call '*a swell*' gets up, after a wind has blown on the sea for a certain time. This swell continues, owing to the vast *momentum* acquired by the agitated waters, long after the wind which produced it has gone down or shifted, and gradually subsides as it gradually commenced. It is often vulgarly supposed that there is a real movement of the water in the direction of the waves, and indeed the eye has some difficulty in detecting that this is not the case. On the contrary, waves caused by wind frequently move with great rapidity in the *opposite* direction to that in which the body of the water is carried by tides or currents.

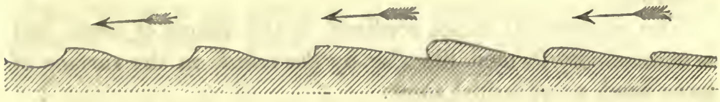
The long axes of waves are of course transverse to the impelling force. In the annexed diagram, each of the waves alternately rises and falls.



As the wave *a* falls, and that marked *b* rises, the particles of water that lie between *a* and *x* (the axis of oscillation) move laterally towards *b*; so that when *b* is at its utmost elevation, the particles that were at *x* are now at *b*, those that were at *b* at *y*, &c., while those that were at *a* are at *x*, those that were at *y* at *a*. As *b* falls and *a* rises, the particles return again, those that were at *b* to *x*, those that were at *x* moving to *a*. Thus there is a continual oscillation of particles of water along the arcs *axb*, &c. in the direction of the dotted lines. In the case of superficial waves, floating objects will appear to vibrate backwards and forwards between *a* and *x*, and *x* and *b*. But these oscillations are not merely superficial: they are propagated to greater or less depths in vertical columns, or rather wedges, whose axes of vibration are represented by the dotted lines *cx*, *dy*, &c. In the oscillations of the lower stratum of water, the sand, fuci, or other objects that are held in suspension near the bottom, or are easily moved along it, will also be seen to vibrate *across* the axes of oscillation. The shaded part of the diagram shews the simplest form of sand-ripple formed by this vibratory action of the water in contact with the bottom, the ridges of the wrinkles corresponding to the axes of oscillation, or intervals between the contiguous oscillations; there the water is comparatively still, and the sand consequently either deposits itself or remains undisturbed, while the intervening hollows are scooped out by the motion of the water.

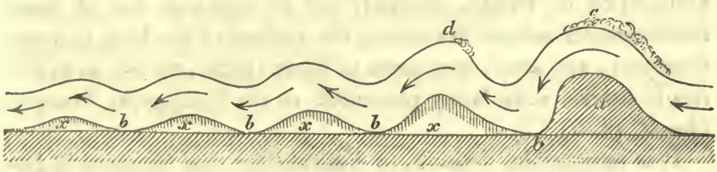
If the alternate vibrations are not equal in force, but the movement is more powerful in one direction than in another, as will be the case with waves breaking on a shore, or those formed in a stream of running water, the ridge will be produced on one side of the ripple, that *towards* which the strongest oscillations move, as in the diagram below; so that each ripple will have one steep and one gentle slope. Where

the motion of the water drifts forward the grains of sand, &c., as in a flowing tide or a river current, the ripples will advance by a sort of rolling motion one over the other. These circumstances produce the compound forms of ripple-marks, sections of which are to be seen in the diagrams annexed.



The waves observable on running water are occasioned by the resistance either of the sides or bottom against which it impinges, and will be greater in proportion to the roughness of these resisting surfaces, under similar circumstances of velocity and volume. The impulse which produces the waves in running water is its gravity urging it down an inclined plane ; the elasticity of the fluid causing it, after yielding to this impulse, to rebound from the bottom or sides against which it strikes ; and the rebounding wave will be higher and larger in proportion to the force of the impulse. Shallow water, flowing with a certain velocity, thus moves in a series of bounds or ripples, whose direction and size are determined by the nature of the sides and bottom of the stream, and which remain constant in the same spot as long as these circumstances and the velocity and volume of the current remains the same.

Thus, in the diagram below, representing a stream of running water, which, meeting with a large stone *a*, is flung upwards in a constant wave *c*, which is broken at the top if the obstacle is considerable, and falling downwards excavates a hollow at *b*, immediately behind the obstacle. From thence the water rebounds upwards in the form of the wave *d*, which also usually has a tendency to break, and falling thence, continues its course in a series of waves, gradually diminishing in height until the impulse to a vertical oscillation, originally com-



municated by the resistance *a*, has exhausted itself. The points *b*, *b*, in the diagram, are those where the water impinges on the bottom. The spaces between them, *x*, *x*, are comparatively at rest, and here, therefore, sand and drift arranges itself, producing the ridges or ripple-mark observable on the bed of a stream.

The ripple-mark is not confined to the effects of *water*, but is occasionally formed likewise by the *wind* on the surface of loose sand, or of drifted snow.

To return to the ripple-beds of forest marble. I had often admired the sharpness and beauty of their wrinkles, appearing as if freshly moulded by the receding tide, and carrying the observer back at once to the moment when the sea broke upon the then narrow shores of this infant island. I had often remarked upon their surface sinuous traces, like the track of some animal burrowing in the sand, but failed in satisfying myself to what they were owing (fig. 1, pl. 5). Having, in the summer of last year, visited the sandstone quarries of Corncockle Muir, in Dumfriesshire, where several impressions of the footsteps of tortoises have been found, it occurred to me to examine minutely the surfaces of these oolite beds, which, particularly when rippled, are as smooth and fresh in appearance as when first formed, and likely therefore to have retained any impressions originally made upon them of similar foot-tracks.

I had not long looked for such before I found a very great abundance of tracks, certainly not of tortoises, but of some much smaller animal, traversing the surface of the beds in every direction; and some specimens of these tracks, as well as of the ripple-marks, were lately presented to the Geological Society. (fig. 2, pl. 5).

It is impossible, I think, to hesitate for one moment in believing them to be the foot-marks of some small and active animal moving on the soft surface of the sand and mud immediately after its deposition; and it is difficult to suppose that surface not to have been *left dry* at the time by the receding tide, since so small an animal as this must have been, could hardly have had sufficient weight to make such deep marks below, or at any depth below the surface of the water; nor is it easy in that case to believe, that they would, if made, have

escaped obliteration. I throw out, merely as a hint, the idea that the rippled surfaces were left dry and became partially consolidated under the influence of the air and sun, and that the seams of clay which cover them were the *first deposit* of the rising tide, bearing before it the mud washed into the sea *at low tide* from the mouth of some neighbouring river. The alternate laminæ of limestone will, in this case, have been deposited during the temporary stillness of the water *at high tide.*

I shall not make an attempt at determining to what *genus* or even *class* of animals these tracks are to be referred; whether *marine*, *terrestrial*, or *amphibious*. It will be observed, that the foot-marks vary considerably in size, but are uniformly found in double lines, parallel to each other, and each shewing two indentations, as if formed by sharp claws, with occasional traces of a third claw. In the most perfect specimens (fig. 2, pl. 5) there is also a third line of tracks, midway between the other two, as if produced by the tail or the stomach of the animal touching the ground at each bound; and where the animal passed over the sharp ridges of the wrinkles, they are flattened and brushed down, as if by a moving power of a considerable force. Thus a ridge between *b* and *d* (fig. 2, pl. 5) has been flattened, and there is a hollow at *c*, on the steep side of the ridge, which may have been produced by the animal slipping down, or climbing up the acclivity.

I leave it entirely to more experienced zoologists than myself to determine, or even to guess at the animal to which we must refer these remarkable tracks.

The long and sinuous traces to which I previously referred (fig. 1, pl. 5) are to me of equally doubtful origin. I should be inclined to suppose them the produce of some annulose or molluscous animals burrowing in the sand, were it not for their great analogy to some very slightly different marks in the same beds, which, from their feathery appendages, seem to be broken portions of the long tentacula of some species of encrinite.

The further examination and comparison of specimens will probably enable us to clear up these doubtful points.

I shall content myself with remarking on the number of interesting memoranda here brought together, often within the compass of a single slab, of the remote time when the waves of

the ocean were beating against a line of coast now in the centre of our island. We see a succession of sedimentary deposits, consisting of clayey and calcareous mud, wrinkled on the surface by the waves, exactly after the manner of the sandy shores of our actual coasts; and, like them, having this surface strewed with small fragments of the shells then existing, of corals, encrinites, spines of echinus, and crustacea; blackened, occasionally, with carbonized vegetable remains, and intersected by the fresh tracks of some animal which has been actively pursuing its prey or disporting upon it. Layer upon layer was deposited, with these characters, for a considerable thickness, till a change took place; not, however, suddenly, but by a gradual increase in the quantities of sand, clay, or calcareous matter, till the deposit of the forest marble was succeeded by the thicker beds of sand and gritstone, of clay, and then of rubbly limestone or cornbrash.

If the ripple-marks and foot-tracks are allowed to attest the local coast line of the emerged lands at the date of their formation, it will become very doubtful whether these beds were ever *in those situations* covered by the *newer*, or, as we call them, the superior deposits. If they were, there must have occurred a considerable *subsidence* in the interval; but until the ripple-marks, &c. are found at a great depth below existing marine strata, it will be allowable to doubt such alternations of *submersion and emersion*, and to believe that we have in these beds the *last* deposits of the sea in that locality, and that the coast line since that period has been progressively shifting eastwards, by the gradual elevation of the island, and the annexation of new littoral deposits.

At all events, it appears to me, that the further examination of these marks may prove highly useful, by throwing light on some of the most interesting, and, at this moment, most agitated questions in geology, namely, the probable outline of the elevated or emerged lands at the date of the several marine formations—the problems as to their alternate subsidences and elevations—and the analogy, in every point of view, of the oceanic deposits of early date with those which are forming at present on the existing shores, or at the bottom of the sea.

March 2, 1831.

Proceedings of the Royal Institution of Great Britain.

FRIDAY EVENING MEETINGS, 1831.

Jan. 28th.—On the determination of the ages of rocks, of supposed igneous origin, by Mr. Ainsworth.—Mr. Ainsworth's object was, by a mineralogical and physical examination of those rocks known to be of igneous origin, together with the circumstances of association in which they are found, and those in which they differ from neighbouring rocks, to form such associations of indications as would lead to a comparative estimate of the ages of these rocks, and in many cases, by consequence, throw light on the rocks with which they were grouped. His discussions were purely geological, and he drew for his data from his personal observations of Plutonic rocks, both at home and abroad, and from the descriptions of others who have studied this part of science.

Amongst the things on the library tables were a small collection of minerals from South America, presented by Mr. Bolleart, formerly chemical assistant in the laboratory. Samples of improved porcelain ware, for chemical uses, recently manufactured by Messrs. Wedgwood, and two large cakes of British silver, from the lead mines of the Duke of Devonshire, covered upon their upper surfaces with those tortuous and tubular configurations which result from the evolution of oxygen from the metal at the moment of its solidification. See page 627, ' Miscellaneous Intelligence.'

Feb. 4th.—Mr. Brande *on the relation of the vegetable alkalies to the common alkalies, and to certain proximate principles of vegetables.*—After adverting to the generic characters of the alkalies and to their importance as chemical agents, Mr. Brande proceeded to remark that, before the discovery of the composition of the fixed alkalies, various speculations, hypotheses and theories had been adopted respecting their probable constitution, the most prevalent being that they contained nitrogen, a notion derived from the existence of that element in ammonia. Experiments had, however, actually been made to shew that they were (as was afterwards proved) metallic oxides; and this view of their nature was founded upon the well-known fact that by far the greater number of the salifiable bases, that is, of bodies neutralizing and forming definite saline compounds with the acids, included a metal and oxygen. Hence, therefore, analogy had led to a right conclusion, but experiment long failed in its verification; and had it not been for the invention and application of a new power, as it were, in chemistry, we might still have remained ignorant of their real nature. The knowledge of the nature of the fixed alkalies led, by more successful

analogy and experiment than the former, to the detection of metallic bases united to oxygen in the alkaline earths, and to the discovery of the bases of the other earths and of boracic acid. These are happy instances of observation, guided by analogy, leading to experiment, and analogy verified by experiment, establishing new scientific truths. The nature of the fixed alkalies having been thus ascertained, and it having been demonstrated that they consisted of metals united to oxygen, analogy, emboldened by previous success, ventured to suggest a similar composition for the volatile alkali or ammonia: this singular body acts upon vegetable colours, and neutralizes the acids in the same way as the other alkalies, but then ammonia had been satisfactorily resolved into hydrogen and nitrogen ; *its* nature, therefore, forms more intricate considerations, for, if susceptible of metallisation, its metallic bases must, in all probability, be constituted of those gases or their bases ; and if so, the nature of hydrogen and of nitrogen would be more or less included in the discovery, and perhaps even that of the metals themselves. Such were the analogies that led to the experiment called the metallisation of ammonia, upon which a theory has been founded, under the idea that the singular appearances attendant upon that experiment do really result from the union of metallic matter with the mercury ; but in the first place, the supposed metal has never been separated or insulated ; and in the next, there are strong grounds for believing that the metallisation is a delusion, and that the effects depend upon a mechanical alteration in the arrangement of the particles of the mercury, and not upon its combination with an evanescent metal, an opinion strongly corroborated by Mr. Daniell's experiments, of which he has given an account in the first number of this Journal.

After shewing the supposed metallisation of ammonia by electrising mercury in contact with its aqueous solution, and by the action of an alloy of potassium and mercury upon moistened muriate of ammonia, and comparing these appearances with those produced by the action of spongy platinum and mercury upon dilute acetic acid, Mr. Brande made some remarks upon the apparent necessity of the presence of hydrogen in the production of the phenomena, and proceeded to observe that, whatever opinion might be entertained respecting the cause of these appearances, they had opened an avenue to some new speculations upon the discovery of these very singular proximate principles in vegetables, which, under the name of alkalies or alkaloids, constitute a definite series of salifiable compounds: in regard to these the question had arisen, what would be the effect of rendering mercury negatively electrical in contact with them? The experiments were then shown, of which an account is given in the last number of the present Journal, after which some observations were offered respecting the ultimate constitution of these alkalies, and the analogies which connect them with other vegetable principles bearing many resemblances to them, but not salifiable. The existence of nitrogen in the salifiable bases was especially noticed as a leading peculiarity and connecting link

between them and ammonia; it exists in them in ternary combination with hydrogen and carbon; oxygen was found in most of them, but is apparently absent in cinchonia. In consequence, however, of the imperfection of their ultimate analysis, no general conclusions could be satisfactorily drawn respecting their atomic constitution; some peculiar form, however, of hydrocarbon appeared essential to their saturating power in respect to acids, and is, perhaps, connected with their high equivalent numbers. The properties of salicine and of a new crystallisable principle from elaterium were then shown, and the absence of nitrogen in those compounds pointed out: its presence was, however, shown in narcotine and in caffeine, bodies possessing many of the characters of the former, and yet not salifiable.

A magnificent collection of volcanic specimens, perfect in its kind, collected from Vesuvius under the superintendence of Monticelli, and presented to the Institution by William Pole, Esq., M.R.I., was laid upon the library tables.

Several instances of thick-rolled lead, perforated by the larvæ of insects, were also exhibited.

Feb. 11*th*, 1831.—Mr. Harris on the power of various substances to intercept magnetic action.—The recent discoveries in this department of science go far to prove that every known substance is in a greater or lesser degree open to magnetic excitation; but it had not yet been shown that non-ferruginous masses could screen or stop out the action; on the contrary, from the few experiments hitherto tried, it was rather to be inferred that such masses were devoid of this screening power.* In the course of an extensive inquiry, however, by Mr. Harris, lately communicated to the Royal Society, it was observed, that though a single plate of iron of about the tenth of an inch in thickness, could effectually arrest the action of a revolving magnet on a disc of copper, yet it had not the same effect on a disc of iron. In the latter case it was found requisite to multiply the intervening mass very considerably. Hence, it seemed reasonable to infer that a screening power might actually be obtained by other substances not containing iron, but which were susceptible of magnetic change, provided such substances were employed in large masses: such was found to be the case. The apparatus employed by Mr. Harris, and described by him, consisted of a magnetic disc, delicately balanced by means of a ring of lead upon a fine centre, and which was set rotating without sensible vibration, at the rate of 600 revolutions in a minute, by means of a train of wheels and a long silk line rapidly run off from its circumference. When the disc was free of the silk and wheels, it was carefully covered by a closed cylinder of glass, having a flat surface above and a light disc of tinned iron moveable on a delicate point placed immediately over

* See the interesting researches of Mr. Herschel, Mr. Babbage, and others, detailed in the Philosophical Transactions.

it; this last was also covered with a glass receiver, and was sustained on a plate of glass at about four inches distant from the revolving magnet. When the iron began to rotate, a large mass of copper, of about a foot square, and three inches in thickness, was interposed; the copper being placed on a convenient carriage, moveable on a rail-way, so as to admit of being interposed very easily without deranging the subject of experiment. The result was, that the motion of the iron became soon sensibly diminished, and at last ceased altogether, on withdrawing the intervening copper, the motion of the iron commenced, and this could be repeated at pleasure. Similar effects were evident with four heavy masses of zinc in blocks, each about an inch thick: Mr. Harris had also, he observed, obtained the same result with a dense mass of silver, of about three inches thick. He, therefore, concludes that this screening power is common to every substance in any degree susceptible of magnetic excitation, and is probably in the direct ratio of its energy, as estimated by observing its influence in fettering the vibrations of an oscillating magnetic bar. To exemplify a similar screening influence to that just mentioned, by means of distilled water at 32° Fahrenheit's scale, or a little below, Mr. Harris believes it would be requisite to obtain a slight action on the disc of iron at about thirty feet distance, so as to interpose nearly that thickness of ice. Mr. Harris accompanied his observations by occasional experimental illustrations; he seemed very carefully to distinguish between the magnetic state, which amounts to a case of permanent polarity, and that induced or transient state which vanishes when the exciting cause is removed, to which he considered the immediate effects now in question might be referred; for, whilst the intervening mass undergoes magnetic change by induction, it at the same time neutralises, in a greater or lesser degree, the power of the exciting magnet on a third substance.

In the library, Mr. Harris illustrated, by a few experiments, the operation of two instruments, which he had recently invented for investigating the laws of magnetic forces. First, a magnetimeter for general purposes. A small cylindrical mass of iron, or otherwise a magnet, is balanced by means of a hydrometric counterpoise from the horizontal diameter of a delicate wheel, moveable about an axis on friction wheels. This wheel is furnished with an index formed of a light straw, duly adjusted, so as to indicate divisions on a graduated arc, when an attractive or repulsive force is made to operate on the suspended iron or magnet. By means of a light frame of brass, and an adjusting screw, a magnet or a mass of iron can be brought to act under many varying conditions, as to distance, position, &c. on the suspended body, and the force due to the latter simultaneously observed. Second, an instrument for measuring magnetic intensity by means of an oscillating bar. The bar is suspended through a long tube of glass, by means of a filament of silk, and vibrates under a graduated ring of paper with its poles quite

free, and without being directly opposed to any substance capable of acting on it; the extremities of the bar are furnished with two light indexes of gold wire, which indicate the arc of vibration on the ring above, and the whole is enclosed in a good pneumatic void, on a pump plate of fine slate. The entire instrument is made up of non-metallic bodies, with the exception of the exhausting tubes, and which are away from the influence of the vibrating magnet. The magnetic bar is drawn aside, and liberated at any given angle from its meridian, by means of a double stop of light brass wire, attached at right angles to a vertical rod, moveable in an air-tight collar, through the middle of the pump plate; and which, being inconsiderable as to mass near the magnetic centre of the bar, and otherwise at a considerable distance when the bar is set free, do not operate in disturbing the result of the experiment. By means of this double stop, the bar may be brought completely to a state of rest; so that when set free it vibrates steadily and without a swinging motion.

In applying this instrument to determine the magnetic energies of non-ferruginous bodies, Mr. Harris observed, that it was impossible to deduce the comparative values of these energies, from the mere number of oscillations made in a given arc, under the influence of these substances, since the result is compounded of the retarding force of the given body under examination, and that retarding force by which the bar would be brought to rest, when oscillating in free space. To get the former force, he divides the number of oscillations made in a given arc in free space, by the number of oscillations performed in the same arc, whilst under the influence of the given substance, and subtracts one from the quotient, by which he considers that we shall obtain, in every case, a fair value of the force we seek to determine; for as the time of performing a given number of vibrations is not caused to sensibly vary in this species of action, we cannot resort to the common law of pendulums, and take the square of the number of oscillations performed in a given time as a measure of the force.*

There was also on the library table, by Mr. Harris, a beating

* Mr. Harris has deduced his formula in the following manner:—

Let $r =$ the retarding force in free space,

$\mathrm{R} =$ the retarding force of the given substance,

Then $r + \mathrm{R}$ will be the whole force in action.

Let $a =$ the number of vibrations in a given arc in free space,

And $b =$ the number of vibrations in the same arc whilst under the influence of the given substance,

Then, since these oscillations may be supposed in the inverse ratio of the retarding forces—we have

$r : r + \mathrm{R} :: b : a,$

Hence $r a = b\,(r + \mathrm{R}) = b\,r + b\,\mathrm{R}$. That is,

$\mathrm{R} = \frac{r a - b r}{b} = r\left(\frac{a}{b} - 1\right)$ but as r is constant in every case, or may be taken as unity, we have

$\frac{a}{b} - 1$ for the value of the force in action.

pendulum, which he proposes to employ in cases where the audible beat of a pendulum is required for a short time only. This instrument is extremely simple in its construction, and will continue to beat, after a slight impulse communicated to it, for about ten or twelve minutes.

Two double-headed planaria from Dr. Johnson; Mr. Parker's æro-fountain lamp; finely crystallized glass, &c., &c., &c. were also upon the table.

Feb. 18*th.*—Mr. Faraday, on Oxalamede.—It was the object of the speaker this evening to give an account of that substance, so curious in a theoretical point of view, discovered by M. Dumas, and described at page 382 of this volume. The relation of oxalic acid, ammonia, and oxalamede, to each other, was shown experimentally; but as the matter was the same with our former account, that will suffice for a description of the subject of the evening.

Upon the library tables were placed by Mr. Johnson a very handsome chain of palladium, made for the Emperor Nicholas; a large piece of native platina, with crystals of the same metal in the hollows and depressions; a clock, with a peculiar maintaining power; a surveying quadrant by Col. Bainbrigge, so constructed that, by looking through the centre of the index glass, all parallax was avoided, and an angle even of 170° readily taken, &c. &c.

February 25*th.*—Mr. Cowper, on recent improvements in paper-making.—The improvement of any article in general use is frequently of more importance than at first sight would appear. In a low state of civilization men are satisfied with an article so long as it just answers the purpose, but in a more advanced state of society improvement gratifies the eye of taste and even influences the judgment and feelings. With respect to paper, curious and influential distinctions have arisen in this way, which, however, sink into insignificance when compared with *physical* advantages: as for instance *legibility*, as in the case of printed books; for those that are most legible will be most read. Mr. Babbage deemed this point of so much importance, that he had his table of logarithms printed on a variety of tinted paper, to ascertain which was most legible, most agreeable, or least fatiguing to the eye, and, as he is an accurate observer, the result is interesting:—*Green* paper is the least fatiguing *colour*, but, by losing its *distinctness*, required more effort to read the figures; while *white* paper, although more fatiguing as a colour, renders the printing so distinct, that the eye catches the figures (or words) with the least effort.

It is economy of production which must introduce an improved article into general use. It is comparatively of little importance to have beautiful books unless they are cheap, and this desirable object has been attained by the improvements in paper-making and printing.

The process of bleaching has mainly contributed to the improvement in the quality of paper, while the paper machine, the principal object this evening, has secured economy of production.

The machine in general use for making paper was introduced by Messrs. Fourdrinier; it was first suggested by M. Didot, (not the celebrated printer), but his plans were too crude for practice, and the machine received its perfection from Mr. Donkin. It consists of an endless web of wove wire, about thirty feet long in the web, or 'wire,' as it is called, and joined together after the manner of a jack-towel; the endless wire runs over a number of rollers, placed horizontally, so as to present a level surface about fifteen feet long: as the wire moves, a quantity of pulp is allowed to flow upon it from a chest, or vat, at one end of the wire; this chest is furnished with a broad and level spout extending across the wire; the spout has an apron or slip of leather nailed to it, so that the leather apron lies upon the wire and prevents the pulp from running off, while the flexibility of the leather allows the wire to move under it without injury. A shaking or jogging motion is given to one end of the wire to produce the felting of the floating fibres, and the water continues to drain from the pulp till it reaches the further end of the wire: here the water is more completely pressed out by rollers; the web of paper may then be considered in form; it is, however, too weak to support its own weight, and is passed over an endless cloth, in order to expose it to the air, to soak out still more moisture, and to make the texture firmer by passing it between other pressing rollers; it is then passed over large cylinders filled with steam, which effectually dries it, and the web of paper is finally wound round a reel, which will thus sometimes contain a single sheet of paper three-fourths of a mile long.

Beautiful and striking as this machine is, it is yet exceeded by the machine invented by Mr. Dickinson. Instead of the endless web of wire thirty feet in length, he employs a perforated brass cylinder, about twenty inches diameter, covered with the woven wire. The cylinder revolves in a vat of pulp, in which it is so far immersed as to leave about one foot of the surface of the cylinder above the surface of the pulp. As the cylinder revolves, the water flows through the wire into the interior of the cylinder, whence it is abstracted by a syphon passing through its hollow axis, and the pulp continues to accumulate upon the whole immersed surface of the cylinder. At that part of the cylinder which is not immersed in the pulp, the action of an air pump is most ingeniously applied; a pipe from an air pump is introduced through the hollow axis of the cylinder, and terminates in a pan or trough fitted close by 'packing' to the interior of the unimmersed part of the cylinder; this air trough maintains its position while the cylinder revolves over it, and the instant the unformed paper comes over the air-trough, the water passes through and the paper is set: it is subsequently passed between pressing rollers, and dried by steam, and reeled up as already described.

It has now to be cut into sheets for use. In the ordinary way this is done by cutting through the reel or coil of paper in the direction of its axis, then laying it flat on the table, a block of wood of the required size is pressed down upon the paper, and a cutting-plough carried round the edge of the block. This method occasions a great loss of shavings, as it is obvious that the outside of the coil must be larger in circumference than the inside. This loss is now obviated by a machine invented by Mr. Cowper; in this machine the web of paper is cut longitudinally by circular knives, and in a transverse direction by a serrated knife, resembling a row of pen-knife points.

Notwithstanding all the care taken to keep little knots and straws out of the pulp, some will escape, and consequently every sheet has to pass through the hands of women, who, with a sharp knife, scratch out any lumps they find; but even this defect seems likely to be removed by the recent invention of Mr. Ibbotson, who has devised a strainer of a peculiar construction, and which is found to answer. It consists of two brass plates cut into very long angular teeth, extending across the box or sieve of which they form the bottom; when the plates are put together, the teeth of one plate fills and completely closes the spaces of the other; on withdrawing them a little series of long narrow openings or slits are formed. Now it is found that the fibres of pulp will flow through such an opening, although they will not flow through small square holes, while the knots are as effectually stopped by the long slit as they would be by the square holes.

It is gratifying to observe the extensive results of the improvements in paper and printing. Whether we turn to the departments of morals, of sciences, or of the imagination, books are now published to an extent far greater than at any former period. An expensive work on political economy never reaches the poor misguided labourer who destroys machinery; but when 'The Results of Machinery' is published, containing two hundred pages for *one shilling*, in three months 25,000 are in the hands of the very people who ought to have them. Who would not have supposed that almost all the readers of Sir Walter Scott's admirable tales had been already supplied; and yet as soon as a cheap edition is brought out, the publishers themselves are astonished at the demand, which obliges them to print more than 1000 volumes every day for three years, i. e. more than 1,000,000 volumes.

A series of very fine coloured wax anatomical models of healthy structures, and also of large anatomical drawings and engravings, were placed upon the tables and appended to the walls of the library, by Mr. Schloss, of Southampton Buildings.

March 4th.—Dr. Edmund Clarke on Vesuvius and Pompeii.—Dr. Clarke gave an account of the present state of this extraordinary mountain and town, drawn from his personal observations, and

illustrated by numerous drawings, models, and specimens, domestic, mineralogical, and botanical. He reasoned upon the account of the eruption of 79 A.D. given by Pliny, explaining some parts and correcting others, by means of the superior knowledge now possessed of the history of the mountains, and the natural causes brought into action by and connected with it.

In the library, amongst many presents, models of useful apparatus, &c. were some Kandyan productions, brought by Captain Chapman from Ceylon; the most interesting of which were native drawings relating to the revolt and punishment of Pilime Talarvie in 1812, and the barbarous treatment of the wife and children of Ehelypole. It was through the influence of these and similar pictorial appeals to the minds of the people, that the island at last came into the possession of the British.

March 11*th.*—Mr. Ainger gave an account of the machinery employed by Mr. Mordan in the manufacture of common pencils, the leads for the ever-pointed pencils, and the Bramah's pens, the latter machine being the invention of the late Mr. Bramah. Mr. Mordan's own machinery, which is of the most beautiful, perfect, and effective kind, was set up in the lecture-room in working order, and all the operations of pencil and pen-making performed. Although the beauty and power of the machine could be judged of by its effects and its principles, and easily understood by the assistance of verbal explanation, it would be impossible, by writing, to give an intelligible account, unless it were also a long one, and accompanied by numerous drawings.

An improved mountain barometer was exhibited in the library by Mr. Robinson of Devonshire Street, the column of which was divisible into two portions when not in use. The fragility of the tube of a barometer, its inconvenient length, and the necessity of carrying it in an inverted position, expose it to more frequent accidents than perhaps any other instrument employed by the scientific traveller. The objects of this contrivance are to reduce the length of the barometer, when not in use, to one half of the usual length, and to render the position in which it is conveyed indifferent, and thus make it capable of safe and convenient transport. The one portion of the instrument is a glass tube, of half the length of a barometer tube—in this tube the mercury is boiled: this tube is cemented into a steel cistern, the tube projecting into the cistern nearly two-thirds the length of the cistern; this part forms, when in use, the upper portion of the mercurial column. The other portion is a glass syphon tube; on the end of the longer leg of which is cemented a steel screw, which screws into the cistern, forming an air-tight joint: the cistern is not quite filled with mercury, the air occupying that space, being the agent employed to cause the mercury to descend into the syphon. If now the syphon be screwed to the cistern, and the instrument be put in an erect

position, the air will pass to the upper part of the cistern, and there its elastic pressure on the surface of the mercury being the same as that of the atmosphere in the syphon tube, it will cause the mercury to descend in that tube; and the length of the mercurial column, equal in weight to the atmospheric pressure, being thus completed, the mercury will descend in the upper tube, and rise in the shorter leg of the syphon; if the instrument be inverted, the mercury will return into the cistern, into which a stopper is screwed when it is not in use. Each portion of the instrument is enclosed in a brass tube, with a scale and vernier to read each end of the mercurial column.

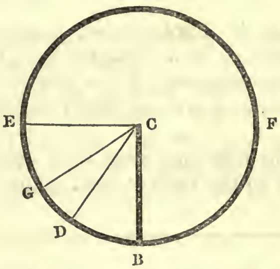
A portable transit instrument, also made by Robinson, was placed upon the table by Captain Grover.

March 18*th.*—Mr. Ritchie delivered a lecture this evening on elasticity in general, particularly the elasticity of torsion in threads of glass, with the application of this property to delicate physical researches. If a portion of air be compressed into a smaller bulk, it endeavours to regain its former state with a force directly proportional to the force which has been employed to compress it, or inversely proportional to the bulk into which it has been compressed. This power, resulting in all probability from the repulsive energy of the molecules of heat with which it is combined, is termed its elasticity, the only kind of elastic force possessed by aëriform substances. If a solid body be compressed into a smaller bulk, it also endeavours to regain its former state with a force proportional to that with which it was compressed, provided the molecules of the body have not undergone a permanent displacement. This is called the elasticity of compression. If a rod or wire be stretched, it will, within certain limits, also endeavour to regain its former length with a force equal to that with which it has been stretched, and directly proportional to the increments of length which it has received. This is called its elasticity of tension. When a rod is bent, the atoms on one side have suffered compression, and on the other side extension; hence both these forces will act in restoring the rod to its former state, with a force proportional to the degree of flexure which it has undergone. If one end of a wire of iron, brass, steel, &c. be fixed, and the other twisted round, the wire will endeavour to return to its former state with a force directly proportional to the number of degrees through which it has been twisted, provided the atoms have not suffered a permanent displacement; or, in other words, provided the wire has not taken *a set.* Coulomb was the first person who investigated the nature of torsion belonging to metallic wires, and employed this property in a beautiful manner in his torsion balance. The celebrated Cavendish also employed it to determine the attraction of leaden balls, and thence the attraction, and consequently the specific gravity, of the earth itself; so that, to use the words of a

French philosopher, he may be said to have weighed the earth in this delicate balance.

But of all substances glass is the most perfectly elastic, and from the facility with which it can be drawn into threads of any degree of fineness, Mr. Ritchie prefers it to wires in all kinds of torsion balances. He showed the application of this property to a torsion electrometer, to a magnetometer, galvanometer, and torsion balance; but as the most important of these applications are already described in the Transactions of the Royal Society, and at page 29 of the present volume, we shall refer the reader to these works for more complete information.

Towards the end of the lecture, Mr. Ritchie applied the force of torsion to demonstrate experimentally the two following propositions: 1st. If a magnetic needle or pendulum be deflected from its state of rest, the force with which it endeavours to return to its former position is proportional to the *sine* of the arc or angle of deflection.



Let E B F be a vertical circle, and let C B be a small wooden pendulum, turning freely on an axis C.

Let one end of a glass thread six or eight feet long be attached to the axes of the pendulum, and the other end fixed to a torsion key, as in the torsion balance. Turn round the key, and observe the degrees of torsion which the thread has undergone in raising the pendulum to different heights, and it will be found that these degrees are directly proportional to the *sines* of the arcs D K, G B, E B, through which the pendulum has been raised. If, for example, it requires 300° of torsion to raise the pendulum through an arc D B of 30°, it will require 600° of torsion to raise it to 90, or to bring it to a horizontal position, the sine of 30°, being half the sine of 90°.

The second proposition, to which the property was applied, is the following: If a pendulum be made to oscillate, the forces which cause it to oscillate are inversely as the squares of the number of oscillations performed in the same time. This proposition was experimentally demonstrated by ascertaining the relative strength of two threads of glass, and then suspending them from a fixed point, and attaching a small horizontal pendulum to the lower end, which was then turned round, and allowed to vibrate by the elastic force of the glass threads. The squares of the oscillations performed in the same time being inversely as the elastic forces of the threads employed.

In the library, were a specimen of the platycercus unicula, or

ground parrot of the Australian islands, from the Zoological Society; a pump lamp without mechanism; self-acting syphon; self-registering thermometer; and other apparatus by M. Bourdon. Parts of Mr. Gould's century of birds, with the originals; casts in bronze, models in wax of fruit, &c. &c.

March 25*th*.—Mr. Faraday on Light and Phosphorescence.—The object of the speaker was to put before the members an account of the experiments recently made in the laboratory by Mr. Pearsall, chemical assistant, on the communication of the power of phosphorescence by heat to those bodies which had been deprived of it, and even to those which had never possessed it. These experiments are already described in this volume, pp. 77, 267; but the results were shown at the evening meeting; and in order that their bearings on the portion of knowledge regarding light, already in possession of men of science, might be understood, Mr. Faraday gave a brief view of the theories of light, and the facts which at present, imperfectly understood, seemed for that very reason to be half-opened doors to new knowledge.

On the library-table, amongst other things, were a series of samples of New Zealand flax (*Phormium Tenax*) in different stages of manufacture. It is now worked largely into twine, rope, and cables, and is exceedingly strong and durable.

The meetings were then adjourned over the 1st and the 8th of April to the 15th of that month.

Proceedings of the Academy of Sciences in Paris.

Astronomical Science, &c.

Aurora Borealis.—The following observations, communicated by M. Arago to the Académie des Sciences on the 10th of January, may be useful in order to determine the real height of the Aurora Borealis of the 7th of January, by comparing them with the observations simultaneously made in other places. M. Arago was induced, at an early part of the evening, to anticipate the appearance of an Aurora Borealis. In his observations on the variations of the needle, he perceived that, instead of stopping as usual at a quarter past one, it continued to advance until five in the afternoon. At this time the declination was 12′ 40″ greater than usual. The Aurora was soon visible towards the magnetic north. At ten minutes past six, the declination had diminished 43′ 8″ since five o'clock: at a quarter past six, it had diminished 48′ 37″; at eighteen minutes past six, 50′ 58″. It then began to increase gradually until a quarter past seven, when it attained its maximum. After a few moments' repose, the northern point of the needle resumed its march towards the east; the minimum of its declination was at

half-past eight. It appears that, by comparing the declination at this hour with that observed at five o'clock, the horizontal needle had been affected by the Aurora to the extent of 1° 16′ 39″. The instruments with which these observations were made are of such exactness, as to render it certain that the errors are not more than five seconds at the utmost. The effect of the Aurora on the needle of inclination was not less decided; but the variations of the latter had no connection with, or analogy to, those of the needle of declination. Thus sometimes the inclination increased while the declination diminished; and at others both increased or decreased together; and several times one of the needles was almost stationary at the moment of the greatest variations being observed in the other. On the 7th of January, the least inclination was at ten minutes past two in the afternoon, and the greatest at thirty-five minutes past seven. The total variation was twenty-one minutes, while at this season the diurnal variation scarcely exceeds one minute. At the moment when the Aurora was at its height, the atmospheric electrometer of the Observatory did not give the least sign of electricity.

Tides in the Atmosphere.—At the meeting of the 31st of January, M. Murphy communicated to the Academy a variety of observations tending to prove that there exists an analogy between the lunar influence on the tides and the atmospheric temperature. This analogy is most apparent at the equinoxes and solstices. M. Murphy stated that during the last winter the lowest degree of temperature, both in London and Paris, was in each period of frost the day or day but one after one of the lunar quarters.

BOTANY.

Kelkoa.—On the 31st of January, a report was read to the Académie des Sciences respecting the *kelkoa* or *planera*, a tree growing on the coasts of the Caspian and Black seas. This tree, which was erroneously distinguished in France by the name of Siberian elm, received the name of *planera* from Gmelin, who so called it in memory of Planer, Professor of Botany at Erfurth. The report states, that the wood of this tree being hard, elastic, and not easily injured by damp or worms, may be advantageously used by carpenters and cabinet-makers; while the luxuriance of its foliage, which is not liable to injury from caterpillars, who reject it as food, renders it a desirable substitute for the elm in avenues. It has also another advantage over the elm, in not being subject to the cankers by which the trunk of the latter is so frequently destroyed. Seeds may readily be procured from Tiflis, or the *planera* may be grafted on the elm, which is the easiest mode of propagating it.

Maturation of Fruit.—21st of February. A report was presented to the Academy on several memoirs relating to the phenomena observable in the ripening of fruits. Various opinions, in

some respects contradictory, were given by the different writers, which it is, therefore, unnecessary to detail. The only certain results appear to be the following. In every stage of the progress of the fruit towards maturity, carbonic acid is constantly produced. The mode in which this acid is produced is explained in three different ways by the different writers, all of which, however, are the result rather of conjecture than of experiment. The progress of the fruit towards maturity is thus described. First, the sap is converted into a viscous liquid, (*cambium*,) which circulates under the rind. When this liquid becomes abundant, it allows part of its water to escape, which evaporates and is converted into gum: it passes through the peduncle to the ovary, where it forms the pericarpium. In its course, it modifies itself by appropriating part of the oxygen of the water of which it is composed, and thence result the various acids, as citric acid, &c. As the fruit enlarges, the pellicle becoming thin and transparent allows the light and heat to act with more effect; then commence the phenomena of ripening, properly so called. The acids have a reaction on the *cambium* which circulates in the fruit, and, by the aid of heat, transform it into a sweet or syrupy substance. These acids soon disappear in their turn, being subjected to a species of saturation by the gelatine. When these phenomena are accomplished the maturity is perfect. Several of the experiments made, in order to ascertain the mode in which the saccharine matter is produced by the reaction of the acids on the gum or gelatinous part of the fruit, are very curious, and merit particular attention.

1. If the jelly of apples be acted on by a solution of a vegetable acid in water, in a short time, (if a proper temperature be preserved,) a saccharine matter, analogous to that of grapes, is obtained.

2. The gum of peas, placed in the machine autoclavi *, with a certain quantity of oxalic acid, and in a temperature of 257° F. is converted into saccharine matter.

3. Ordinary fecula, heated in the same manner, passes first into a state resembling externally gum arabic, but differing from it, inasmuch as when acted on by nitric acid it does not generate mucic acid.

4. If this gum of fecula be added to the juice of unripe grapes, and heated, the liquor becomes sweet. A similar effect is produced if, after having saturated it with chalk and filtered it, tartaric acid be dissolved in it and the solution boiled. We hence see why fruits become sweeter by dressing, and grape-juice by evaporation, because part of the jelly is converted into saccharine matter.

Circulation in Plants.—An interesting conversation took place in the Institute, on the 21st March, between MM. Cassini, Arago, and Humboldt, on the subject of a letter written to the Academy by M. Dutrochet, in which he attempted to prove that the supposed circulation discovered by M. Schultz in the celandine (*ficus elastica*) and other plants with milky juice, was a mere optical deception, occasioned by a trepidation of the molecules, similar to that which

* An improvement of Papin's Digester.

is observed in the capillary blood-vessels of dead animals; this trepidation of the molecules in the blood-vessels is not perceptible under a diffused light, but becomes visible when the vessels are exposed to the action of the direct solar rays. The united testimony, however, of the three academicians above mentioned was in opposition to M. Dutrochet's opinion, which they supposed to have originated in his having only tried the experiments with the celandine, in which the circulation is, in fact, not visible, except with direct light, whereas, in the *ficus elastica*, *alisma plantago*, and others, it is clearly to be distinguished by diffused light.

This subject was resumed on the 28th of March, when M. Mirbel read a letter from M. Amici, (well known for his improvements on the microscope,) detailing some experiments which he had recently made on the leaves of celandine, and which had induced him to attribute the movement of the juice, not to a system of circulation, but merely to the effect of the heat either of the lamp, or even, in some cases, of the human hand alone. He conceives that the caloric, acting on the gaseous molecules interspersed among the solid or liquid molecules, occasions those molecules to dilate and displace each other, thus forming a constant movement, which, however real, he does not admit to be circulation, as it is not dependent on a vital principle; or he supposes that the heat acts by means of a thermo-electric current passing through the juices; or, thirdly, by means of the air passing from the trachea by the anastomosis existing between the two orders of vessels, and thus impelling the juice before it. But in order to remove any doubt as to the effect being produced by the heat, and not by the light, M. Amici subjected the leaves to the action of a hot iron, and perceived, by the aid of a reflecting mirror, that the heat invariably determined a motion of the fluid in an opposite direction, changing from left to right, and *vice versâ*, as the position of the iron was altered. M. Amici thence concludes that celandine may be made useful as a thermoscope. M. Mirbel, in remarking on this letter, observed that, although some doubt may exist as to the reality of the circulation in celandine, there can be none as to the *ficus elastica*, where there is evidently not mere *trepidation* but *translation* visible by a diffused light; and that it is not determined by the influence of caloric, as supposed by M. Amici with respect to celandine, is evident from the fact of the liquid passing in circulation under the point exposed to the action of the direct solar rays, where the heat is consequently greatest, and where, the action being vertical, the supposed repellent power of the caloric ought to be neutralized, and the liquid remain stationary in that point, and to be thence repelled in opposite directions as from a centre, which is not the case, the movement being uniformly in the same direction in each vessel. M. Cassini also remarked that, if M. Amici were right in his opinion, the phenomenon which he (M. Cassini) had observed of two vessels containing the juice, and placed close together, exhibiting a circulation in opposite directions, would be impossible. M. Dutrochet also sent a second letter, in which he stated that he

had confirmed his opinion of the non-existence of circulation in celandine, by placing a small quantity of the yellow juice in a glass tube, taking care that the space occupied by it not being beyond the field of vision of the microscope, he could see both extremities on exposing this liquid to the action of the direct rays. The appearance of a rapid current flowing along the tube was produced; but the liquid did not, in fact, change its place in the slightest degree. This molecular *trepidation* was no longer visible when a diffused light only was employed. It does not, however, appear that M. Dutrochet has extended his experiments beyond the celandine. It appears, however, certain that the circulation cannot be asserted to exist in plants with milky juice, (*à suc laiteux*,) as a class, but that it is a phenomenon existing partially and arbitrarily, which must be traced and examined in each individual case.

Chemistry.

New Metal Vanadium.—At the meeting of the Académie des Sciences, on the 7th February, M. Dulong read a letter from M. Berzelius, announcing the discovery of a new simple substance by M. Sefstrom, director of the mines of Fahlun, in Dalecarlia. M. Sefstrom having occasion to examine an iron remarkable for its softness, observed the presence of a body which appeared new to him, and which he succeeded in separating, but in too small a quantity to determine its properties. He afterwards observed that cast-iron contained much more of it than wrought iron, which induced him to suppose that he should find still more in the scoria, in which he was not deceived, as he obtained it in considerable quantities. It appears to be a new metal, to which he has given provisionally the name of *Vanadium*, derived from an ancient deity of the Scandinavians.

At the meeting of the Academy on the 28th February, M. de Humboldt exhibited a specimen of this new metal. He stated that the same metal had been discovered in Mexico, by M. del Rio, in a brown lead ore found in the district of Zimampas. M. del Rio gave it the name of *Erythronium*, but was afterwards induced to suppose that it was not a simple substance, but merely an impure chrome. Since the discovery of M. Sefstrom, however, the brown lead ore of Zimampas has been again analysed, and a simple substance, precisely similar to that found in the iron by M. Sefstrom, obtained from it. See page 625 of Miscellanea.

Magnesium.—21st February. A report was made to the Academy on the mode adopted by M. Bussy, for obtaining magnesium in a metallic state, which is, by decomposing chloride of magnesium by means of potassium. Magnesium is a brilliant metal, of a silvery whiteness, perfectly ductile and malleable, fusible at a comparatively low temperature, and, like zinc, capable of sublimation at a temperature very little higher than that of its fusibility, and condensing under the form of small globules. It does not decompose

water at the ordinary temperature ; it oxidises at a high temperature, and is converted into magnesia ; slowly when it is in rather large pieces, but when it is in fine dust, it burns with great splendour, throwing out sparks like iron in oxygen.

Perchloric acid.—March 14. M. Serullas communicated to the Academy the result of an experiment which he had just made on perchloric acid. He stated that he had observed that this acid, when combined with several of the vegetable alkalies, formed acid salts in a perfect state of crystallization, which induced him to endeavour to obtain perchloric acid in a concrete form, in order to strengthen his theory of the general tendency of concrete acids to generate acid salts. He had already tried the experiment with potash, but had obtained only a neutral salt.

He now distilled the perchloric acid with about two or three times its weight of concentrated sulphuric acid. This, when in a state of ebullition, occasioned the separation of chlorine, which gave a yellow colour to the liquid, and at the same time of perchloric acid, which was received in a tube and surrounded with ice; part of this acid was in a liquid, and part in a crystallized. In this state it does not contain any sulphuric acid, the temperature not being sufficiently high to admit of its distillation ; when exposed to the air, white and very thick vapours are emitted ; when poured into water, each drop produces a sound similar to that of red-hot iron dipped into the liquid. The solid portion crystallizes in transparent prisms, and the liquid part, when exposed to the air in a watch-glass, is rapidly volatilized, acquiring solidity, until it totally disappears. M. Serullas had not ascertained whether it was entirely free from water.

Geology.

Elevation of the Morea.—In a paper communicated to the Académie des Sciences, on the 31st January, containing a series of geological observations made by M. Boblaye, in the Morea and in Egina, it is stated that there are positive proofs of the whole soil having risen considerably, not in a gradual or continuous manner, but by sudden starts, so that the grounds abandoned by the sea are marked out in steps or layers in irregular gradation.

Humboldt's Map of Heights.—On the 21st March, M. de Humboldt exhibited to the Academy a map, which he calls 'Esquisse Hypsométrique des rauds des montagnes et des ramifications de la Cordillère des Andes depuis Cap Horn jusqu'à l'Isthme de Panama et à la chaine littorale de Venezuela.' It extends from 62 to 84 degrees of west lóngitude, (Mer. Paris,) and from 21 degrees south to 11 north latitude. In a verbal explanation which he gave of it, he observed that notwithstanding the numerous men of science who had traversed the Isthmus, no positive information had been obtained respecting the heights of the mountains which continue

the chain of the Andes at that spot, until two Colombian engineers, MM. Lloyd and Falmarck, made a geometrical survey, by order of General Bolivar. It results from their labours that the chain is so much lowered here as not to exceed ninety-five toises in height, which perfectly coincides with the opinion formerly expressed by M. de Humboldt, that, judging from the vegetable productions on the summit, the height must be between five and six hundred feet. M. de Humboldt then entered into an elaborate geological dissertation on the raising of the chain of the Andes, and its ramifications, and clusters in the form of feeders (*filons*); he described the ridges or sills which, stretching across plains, unite the apparently insulated mountains of Lake Parimee with the Andes of Timana, and the chain of Brazil with the mountains of Cochabambo. The pretended chain of mountains which has been represented as uniting the Oural and the Altai, in the north of Asia, is in fact only a ridge serving as a line of division between the waters which fall into the Obi, and those which flow to the lake Azal.

Medical Science.

Twisting of the Arteries.—On the 31st January, M. Amussat presented to the Academy four individuals on whom his principle of twisting (*torsion*) of the arteries after amputation had been successfully tried. In no case had any secondary hemorrhage occurred. He stated the advantages of this system over that of the ligature to be, that it could be carried into effect by one operator without assistance, that it is never followed by consecutive hemorrhages, and allows the immediate re-union of the parts in the full force of the term. The system has been successfully adopted by MM. Waust and Anciause, at Liege; Friecke and Schreuder, at Hamburgh; and Dieffembach and Rust, at Berlin.

Lithotrity.—On the 24th January, Dr. Civiale communicated to the Academy a report of the cases of lithotrity recently placed under his observation. He attributes the failure of the process, in most cases in which failure has occurred, to the imperfection of the instrument employed. By his method of treatment he state that lithotrity has been successfully resorted to in 163 cases, 152 of which were operated on by himself.

Galvanic Application.—On the 7th February, Dr. Andrieux announced to the Academy that he had invented an apparatus, by means of which the action of galvanism on patients can be so graduated as to allow it to be applied daily either in the same degree or with a gradual increase of intensity. He attributes the small advantages hitherto derived from the application of galvanism in medicine, to the fact of the apparatus not having been so disposed as to allow of comparative results being obtained.

New Remedy.—At the same meeting, M. Jumeret Perrault, of Neufchatel, announced that he had discovered in the mountains a plant which affords a sovereign remedy in phthisical and pulmonary complaints. He offers to supply it at fifteen sous (sevenpence halfpenny) per pacquet. From the description it appears to be a species of *asplenium*.

Action of Oil of Turpentine, Opium, &c. on the Nervous System.—At the same meeting M. Flourens detailed the results of a series of experiments which he had made on the action of the essential oil of turpentine, opium, and alcohol, when applied to different parts of the brain. The operation was performed on rabbits, the cranium and dura mater having been previously removed; in all the experiments he took care to renew the substances as soon as they disappeared from the surface of the organ by absorption or evaporation. The oil of turpentine applied to the lobes of the brain, in a certain time produced an agitation, with occasional intervals of repose. Sometimes the animal leaped forward, and at others turned round in a spiral direction. During the paroxysms the animal appeared in a state of furious madness, and neither saw nor heard, but in the intervals of repose the faculties of sight and hearing appeared uninjured. On applying the oil of turpentine to the cerebellum, a strong tendency to run and leap was observed. The effects of alcohol were similar, but less violent, never producing the circumgyration. Liquid opium applied to the lobes of the brain in a very short time produced an insensibility or torpor, which continued even when the animal was pinched. A tension of the anterior limbs sometimes occurred to such a degree as to push the body back, so as to turn it over on its back. Opium, when applied to the cerebellum, produced an overthrow of the equilibrium, so that the animal could only move by dragging itself along on its abdomen. Having remarked that the involuntary movement produced by the oil of turpentine tended to carry the animal forward, while that produced by the opium carried it backwards, M. Flourens applied both together, and found that, to a certain extent, these contrary effects neutralized each other. The effects produced by the opium appeared to resemble those produced by the successive removal of different parts of the brain, while those of the oil of turpentine and alcohol were analogous to what might be supposed to result from an over repletion *(hypertrophe)* of the different parts of the brain.

Acupuncturation of the Arteries.—At the meeting of the 14th of February, M. Velpeau suggested the acupuncture of arteries as a means which might generally be advantageously substituted for ligature.

Affections of the Vocal Organs.—On the 7th of March M. Majendie made a very favourable report on a memoir presented by

Dr. Bennati, physician to the Italian Opera, on the diseases of the uvula to which singers, orators, and others accustomed to great exertion of the vocal organs, are subject. Dr. Bennati details a number of instances in his own experience which prove the inexpediency of excision in cases of relaxation or prolongation of the uvula, and recommends cauterisation with nitrate of silver as an almost infallible remedy. The doctor has also invented a new species of *portocaustique*, by means of which the front, back, and lower parts of the uvula are at once subjected to the action of the caustic.

Application of Galvanism.—At the same meeting a very elaborate paper on the application of galvanism to medicine was read by Dr. Fabré Palaprat. He details a number of experiments tending to prove the analogy, if not identity, of the electric with the vital principle. The result of his observations is, that galvanism may be usefully employed as a medical agent in the following diseases: nervous affections in general; chronic diseases of the abdominal organs, when not resulting from an organic injury; hypochondria, nervous asthma, head-ache, and some cases of paralysis. M. Fabré Palaprat also states that he had found the union of acupuncture with galvanisation highly advantageous in producing slight instantaneous irritations of the skin, and also for introducing various medicaments deep into the body, by means of the acupunctural conductors acted on by galvanism.

Use of Salicine.—March 14th, two instances of the successful application of *salicine* or willow-bark in cases of intermittent fever, were communicated to the Academy by Dr. Ferrand de Missol. The first was that of an infant of twenty-five months old, who was suffering from *odaxistique*, or teething fever. Eight grains of salicine were administered in two doses: after the first, the child, who for many days had refused nourishment, showed a disposition for food; two days afterwards twelve grains were given in three doses, and afterwards continued for four days in doses of one grain each. At the end of that time the child was perfectly recovered. This case gives the doctor occasion to remark, that where the fever is essentially *odaxistique*, the character and symptoms which constitute it are principally *nervous*. The second case was that of a young man, aged seventeen, suffering under a strong intermittent fever: an emetic was first administered, which relieved the pains in the stomach and head, but did not otherwise diminish the fever; five days afterwards twelve grains of salicine were administered, the shiverings ceased, but there was a paroxysm of fever at six o'clock; the next day eighteen grains were given, the paroxysm was at seven; for six days the dose was gradually increased to forty grains, and every day the paroxysm took place at longer intervals; on the seventh day there was none, the dose was then dimi-

nished to twenty grains; and in three days more the patient was cured.

Cholera Morbus.—An incalculable number of letters, memoirs, and documents of every description, have been piled on the table of the Academy relative to the cholera morbus; but as they generally consist of speculative theories, and merely controversial discussions, it would be idle to lay them before our readers. An exception, however, to this rule exists in a paper forwarded by Dr. Jahinichen, who, as a member of the council appointed to examine the progress of the disease, had personally observed the majority of cases in Moscow, and whose talent renders his opinions valuable. The conclusions at which he has arrived are the following:—1. The cholera morbus is not a pestilential disease. 2. It is not either directly or indirectly contagious. 3. A germ or *miasma* of cholera emanating from the diseased person exists in the atmosphere surrounding him. 4. These emanations may be sufficient to originate disease, even when only proceeding from a single person, if the malady be violent, but will always be so in a hospital. 5. But a particular predisposition (arising generally from the greater or less irregularity in the mode of living) is necessary in each individual, to produce the developement of this *miasma* of cholera. The proportion in which this predisposition exists in a population has not been ascertained with sufficient certainty to establish a general rule; at Moscow it was about three in every hundred. 6. The propagation of the cholera is in accordance with the usual laws of epidemic diseases. 7. There is every reason to believe that pulmonary absorption is the only method by which the *miasma* is introduced into the human body. There is, therefore, no contagion, in the strict meaning of the word, but rather a species of penetration. 8. The *miasma* appears to have a peculiar affinity with the aqueous vapours in the atmosphere, and to be equally volatile. Dr. Jahinichen then adds, that he obtained, from the condensation of these vapours in rooms containing a number of patients, a substance entirely resembling that obtained by Moscati at Florence, and suggests that a close observation of the hygrometrical and barometrical variations of the atmosphere may throw light on the geographical march of the disease. He also thinks it probable that the *miasma* inherent in the aqueous vapours may rise in the atmosphere, and, being transported by a current of air to other countries, be inhaled by the inhabitants of those countries, and thus originate the disease. Should this conjecture be well founded, all quarantine and other precautionary measures must be useless, unless respiration could be suspended; and there is reason to fear that the ravages of the disorder in the western parts of Europe may be more extensive than in Russia, in consequence of the prejudices existing against hospitals, which, by keeping the patient at home, will render each house a separate source from which the fatal

miasma may emanate. We should mention that these opinions of Dr. Jahinichen have been warmly attacked by M. Moreau de Johnes, and other advocates of the contagious properties of the disease, but their arguments are rather theoretical than founded on specific facts.

NATURAL PHILOSOPHY.

Electrical relations of bodies to heat.—At the meeting of the Académie des Sciences, held on the 17th of January, M. Becqueril read a memoir entitled 'Theoretical considerations on the changes operated in the electrical state of bodies, by the action of heat, contact, friction, and different chemical actions, and on the modifications which are occasionally produced in the arrangement of the constituent parts of those bodies.' The object of this memoir is to explain some of the causes which in process of time effect a change in many of the substances forming the superficial stratum of the globe. After referring to Laplace's theory of the igneous origin of the earth, he observes, that the diminution of the temperature must have successively produced great change in the combination of the elements of which the bulk of the earth is composed, in the constitution and pressure of the atmosphere, &c. He proposes to trace the origin of all these phenomena, and to investigate their causes and physical laws, and commences by some general considerations on certain properties of matter; after which he examines the effects of heat on the electric fluid of metallic substances considered separately and in contact, and the state of the atoms in the various combinations. By means of a very simple apparatus, he demonstrates that heat does not possess any influence over free electricity; but on the contrary acts very decidedly on the natural fluid. He has observed, that the heat which separates the molecules of bodies, produces on the natural fluid an effect analogous to that obtained by the cleavage of regularly crystallized substances, viz. the diminution of the reciprocal action of the two electricities. He then enumerates a variety of experiments, which authorize the conclusion, that the two electricities are raised by heat to a higher degree in bodies which are negatively than in those which are positively electric. This fact explains the reason of the oxides of the negatively electric metals being more easily decomposed by heat than those of other metals. He then, after having given a detailed account of the various phenomena relating to the influence of heat in exciting the electric power in metals, enters into the question of the development of electricity by contact. Volta, in attacking Galvani's theory on muscular contractions, conceived the idea that they were owing to the electricity emanating from the contact of two heterogeneous substances: according to his theory, two substances always become in a state of contrary electricity by mutual contact, leaving out of consideration any modifications produced on the surfaces in contact. M. Becqueril then noticed the theory,

advanced by M. Delarive in opposition to Volta, that the action of the contact was only the result of the difference of the chemical actions of the air and water, and of external agents, on each of the two bodies; and stated, that though he had at first been staggered by it, the consideration that the electric fluid acts as a moving power in producing combinations induced him to retain his original opinion. In order to show the nature of this action in its full extent, he pursued his experiments on mineral substances which are electric conductors, and so little susceptible of atmospheric action, that their constitution sustained no change from being exposed for ages to the inclemency of the seasons. He details his experiments on platina, peroxide of manganese, magnetic oxides of iron, and gold; from which it appeared that the peroxide of manganese was, as might be expected from its high degree of oxydisation, negative in its contact with all the other bodies. He next examines the causes of the thermo-electric action in closed circles composed either of one or of two different metals, and states, from all his experiments it appears that these phenomena are owing to the difference of the thermo-electric powers of the metals. From some observations made on the relation between the thermo-electric faculties, and the capacity for heat in various metals, it appears that those metals which are most negatively electric have the least specific heat. The memoir concludes with an exposé of the electric properties of atoms. M. Becqueril examines the theory of M. Ampère, and also that of M. Bégneul, who, from the experiments he had made, concluded that the atoms in combination were merely small electric piles, the reciprocal and continuous action of which constitute what we call molecular attraction; but M. Becqueril considers the question, whether the action of particles of bodies on each other is entirely produced by electric action or by some unknown power, as still undecided.

ZOOLOGY.

Sturgeon.—On the 24th January M. Cuvier made a very favourable report to the Academy on a work by Messrs. Brandt and Ratzeburg, of Berlin, entitled the Monography of Sturgeon, in which the genus is divided into fourteen species, which are described with great minuteness, and in a manner calculated to be of great advantage to zoologists.

Teleo-saurus.—On the 21st February M. Geoffroy de St. Hilaire presented his two memoirs on the animal, the fossil remains of which were discovered in Normandy in the years 1828, 1829, and 1830, which has been erroneously designated as the fossil crocodile of Caen. He now names it the genus *teleo-saurus.* These memoirs describe, at great length, the difference between this animal and the crocodile: the scales have no centre ridge, but are placed one over the other like those of fish, whence it is supposed that this animal was more

essentially aquatic than the crocodile. The whole of the animal has been now found, with the exception of the anterior feet and part of the posterior feet. The whole *plastron* of the back of the *teleo-saurus* is not composed as in the crocodile of several rows of plates careened to the centre, but of two rows only of plates without apparent projecture, the outward part thin, and the inner, by which they are strongly united together, very thick; they cover each other behind like the scales of fish. The fore part of the tail is also covered with two rows of scales only, but these present a longitudinal ridge towards the outward part, which forms two projecting lines, which gradually approach each other towards the hinder part. The back part of the tail, which answers to the crest (*crête en scie*) of the crocodile, has but one row of orbicular plates, which are strongly careened at the centre. The lower plastron exhibits six transverse rows of scales, which are not flexible like those of the crocodile's belly, but all strong and solid, whence the whole plastron could only be moved in one piece. Thus, in the general movements for the purpose of introducing the air into the lungs, the action of the two plastrons is similar to that of the two parts of a bellows. M. de St. Hilaire stated, that from the fact of the posterior aperture of the nostrils being situated at the middle part of the cranium, he had been induced to imagine that the mode of respiration of this animal must have been more nearly allied to that of the tortoise than of the crocodile—a supposition which is fully confirmed by the construction of the plastrons. It appears (in confirmation of the aquatic nature of the *teleo-saurus*) that its posterior members must have been at least double the size of the anterior, resembling the kangaroo in its mal-adaptation for walking; while the manner in which its whole body was closely covered with scales prevented its having the agility in leaping of that animal, so that it was only adapted for the water.

Two-head Lizard.—At the meeting of the Academy of the 28th February, M. Beltrami announced, that in a recent excursion over the Pyrenees, he found a two-headed lizard, with five paws, four of which were naturally formed, but the fifth, which was placed between the two heads, had nine toes. M. Beltrami promised to furnish the Academy, on a future occasion, with a minute account of the habits and mode of life of this animal.

ANALYSIS OF BOOKS, AND SELECTIONS FROM THE TRANSACTIONS OF SCIENTIFIC SOCIETIES.

The Life of Sir Humphry Davy, Bart., LL.D., late President of the Royal Society, &c. &c. &c. By John Ayrton Paris, M.D., F.R.S., &c. &c. 4to. London, 1831.

(Concluded from p. 360.)

IN the year 1808, MM. Gay-Lussac and Thenard succeeded in decomposing potash by chemical means; and Davy soon repeated their experiment. This process afforded the means of procuring potassium more readily in larger quantities than by means of voltaic electricity. The facility of the combustion of the alkalies, and the readiness with which they decomposed water, offered Davy the ready means for determining the proportions of their constituent parts: he thought potash composed of about six parts base and one of oxygen; and soda, as consisting of seven parts base and two of oxygen. The over-excitement and fatigue of his researches upon this occasion, and the irregularity of his habits, threw him into a fever. Such was the alarming state of his disorder, that for many weeks his physicians visited him four times a-day.

The course of lectures on Electro-Chemical Science, which he gave on his recovery, commenced in March, 1808, and the theatre of the Institution overflowed with admiring and interested auditors. At the same period he gave a course in the evening on Geology, which was equally attractive. Having succeeded in decomposing the alkalies, it was natural that he should turn his attention to the earths; he, however, found them much more difficult to conquer. While busily engaged in pursuit of his object, he received a letter from Professor Berzelius, of Stockholm, announcing that, in conjunction with Dr. Pontin, he had succeeded in decomposing baryta and lime by negatively electrising mercury in contact with them, and by such means had actually obtained amalgams of these earths. Davy immediately repeated the experiments with success; and having, by additional experiments, fully established the nature of these bodies and the analogies he had anticipated, he published the result in a memoir to the Royal Society in June, 1808, entitled—'Electro-Chemical Researches on the Decomposition of the Earths; with Observations on the Metals obtained from them, and on the Amalgam of Ammonia.'

It has, however, been doubted whether the change, which ammonia and mercury undergo by voltaic action, merits the name of amalgamation, and whether it may not be referred to a purely mechanical cause; and Dr. Paris observes, in a note, that this opinion is strongly confirmed by Mr. Daniell's paper 'On certain Phenomena resulting from the action of Mercury upon different Metals,' published in the first number of this Journal.

His third Bakerian Lecture was read before the Royal Society in December, 1808: it contained his 'Researches on the nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecompounded; with some observations on Chemical Theory.' These inquiries are continued and extended in a paper read before the Royal Society in February, 1809, and in his fourth Bakerian Lecture, published in that year. The contents of these papers will hardly admit of analysis or abridgment, but Dr. Paris has given an account of the general results: these consist of his account of the mutual action of potassium and ammonia upon each other; in the course of which he obtained an olive-coloured substance, which he was inclined to regard as the metallic base of ammonia: he also believed that nitrogen had been decomposed during the process, and that its elements were oxygen and a metallic base, or oxygen and hydrogen. His attention was called to tellurium by an observation of Ritter, that, of all the metallic substances, it was the only one by which he could not procure potassium through the agency of negative electricity. In pursuing the inquiry, Davy found that tellurium and hydrogen were capable of combining and of forming a gas, to which he gave the name of *telluretted hydrogen;* and that so far from preventing the decomposition of potash, it formed an alloy with potassium when negatively electrified upon the alkali, and had the most intense affinity with it. The results of his inquiry whether sulphur, carbon and phosphorus in their ordinary form may not contain hydrogen, were far from conclusive. He succeeded in decomposing boracic acid, but was anticipated in some of his results by Gay-Lussac and Thenard. He at first proposed to call the base of that acid *boracium;* but, finding it more analogous to carbon than any other substance, he adopted the term *boron.* 'His experiments and reasonings upon muriatic acid, at this period (says Dr. Paris), derive their greatest interest from their fallacy, and the vigour he subsequently displayed in disentangling himself from a web of his own fabrication.'

At this period, on his representation, a splendid voltaic battery was constructed, the means being raised by a subscription among the members of the Royal Institution for the purpose. It consisted of 200 troughs, each containing 10 double plates, arranged in cells of porcelain, and containing, in the whole, a surface of metal of 128,000 square inches. All the phenomena of chemical decomposition were produced with intense rapidity by this combination; and he instituted several experiments with the hope, already alluded to, of decomposing nitrogen.

The evidence by which Davy established the important fact that oxymuriatic acid is a simple body, which becomes muriatic acid by its union with hydrogen, was deduced from a course of experiments conducted with the most consummate skill and perseverance; the results of which were given to the world in his Bakerian Lectures

for 1809 and 1810, and in a subsequent memoir read in February, 1811. Dr. Paris thinks that, after his discoveries in voltaic electricity, these are by far the most important of his labours. When this fact was established,

'it became necessary to alter the nomenclature, since to call a body, which neither contains oxygen nor muriatic acid, by a term which denotes the presence of both, is contrary to those very principles which first suggested it. Having consulted some of the most eminent philosophers, Davy proposed a name, founded upon one of the most obvious and characteristic properties of the oxymuriatic acid, namely, its colour, and called it CHLORINE.'—' In the memoir abovementioned, which was entitled, "On a Combination of Oxymuriatic Gas and Oxygen Gas," he announced the existence of a *protoxide* of chlorine, under the name of *euchlorine;* and in a communication from Rome, in 1815, he described another compound of chlorine and oxygen, containing a still larger proportion of the latter, and which has since been made the subject of a series of experiments by Count Stadion, of Vienna. As it does not exhibit any acid properties, Dr. Henry proposes to call it a *peroxide,* in preference to *deutoxide.* Its discovery was made during an examination of the action of acids on the *hyper-oxymuriates* of Chenevix, undertaken by Davy, in consequence of a statement of M. Gay-Lussac, that a peculiar acid, which he called *chloric acid,* might be procured from the hyper-oxymuriate of baryta by sulphuric acid.'

The chloridic theory may now be considered as fully established: the philosophers, who were so long hostile to its reception, have at length yielded their assent; and the subsequent discovery of *iodine* and *bromine* has confirmed, by beautiful analogies, the views Davy so satisfactorily explained by experiment.

Dr. Paris has asserted Davy's claim to the establishment of this theory, against the claims of priority set up in favour of the French chemists.

In November, 1810, Davy delivered a course of lectures to the Dublin Society, at their invitation, for which he received 500 guineas; and two distinct courses in 1811, on Electro-Chemistry and Geology, for which he received 750*l.*; and before he quitted Dublin, at his second visit, the Provost and Fellows of Trinity College conferred on him the honorary degree of LL.D. In the month of August, 1811, he was requested by a committee to suggest a method to be adopted for ventilating the House of Lords; but his plan appears to have failed, which was a source of vexation to him and of pleasant raillery to others.

On the 8th of April, 1812, he received the honour of knighthood from his late Majesty (then Regent), being the first person on whom, as Regent, he had conferred that distinction; and on the following day, he delivered his farewell lecture in the Theatre of the Royal Institution.

On the 11th of April, 1812, he married Mrs. Apreece, a lady of very considerable fortune.

The first part of 'The Elements of Chemical Science,' a work which he had been some time preparing, was published in June,

1812. It is dedicated to Lady Davy, to whom he offers it 'as a pledge that he shall continue to pursue science with unabated ardour.' Upon this work Dr. Paris has offered some remarks, for which we regret we have not space. He observes that—

'Although it bears the title of "Elements," its plan and execution are rather adapted to the adept than the tyro in science; and though it has not perhaps announced any discoveries which had not been previously communicated to the Royal Society, it has brought together his original results, and arranged them in one simple digested plan—it has given coherence to disjointed facts, and has exhibited their mutual bearings upon each other, and their general relations to previously established truths. Very shortly after the publication, it was asserted that the work could never be completed upon the plan on which it had commenced, which was little less than a system of chemistry, in which all the facts were to be verified by the author; an undertaking too gigantic for the most intrepid and laborious experimentalist to accomplish. There was too much truth in the remark—the life of the author has closed—the work remains unfinished.'

The volume extends only to the general laws of chemical changes, and to the primary combinations of undecompounded bodies.

In October, 1812, he received a letter from M. Ampère, informing him that a compound of chlorine and azote had been discovered at Paris, a fluid which exploded by the heat of the hand; and that the discovery had cost the author an eye and a finger. M. Ampère gave him no details as to the mode of combining them, but his own sagacity led him to the course to be pursued, and Mr. Children having suggested to him that Mr. James Burton, on exposing chlorine to a solution of nitrate of ammonia, had observed the formation of a yellow oil, which he had not been able to collect, Davy availed himself of the hint, and obtained the fluid in question. On exposing it to heat, the tube was shivered to atoms by its explosion, and he received a wound in the transparent cornea of the eye, which was followed by inflammation, and disabled him from pursuing his inquiry. In the following July, he was again wounded in the head and hands in attempting its analysis by the action of mercury, but having taken the precaution of defending his face by a plate of glass attached to a proper cap, no serious consequence ensued. By using smaller quantities, and recently distilled mercury, he succeeded in obtaining results without any violent action: the mercury united with the chlorine, and azote was disengaged, from which he was enabled to conclude that it was composed of four volumes of chlorine and one volume of azote. He suggested the name of *azotane;* but his nomenclature of the compounds of chlorine not having been adopted, the substance is denominated chloride of nitrogen. The results of these experiments were communicated to the Royal Society in two successive papers. In another paper, read July 8, 1813, he establishes, by satisfactory experiments, that the base of fluoric acid is a highly energetic body, not hitherto obtained in an

insulated form; the properties of which are yet unknown: it appears to belong to the class of negative electrics, and to have a powerful affinity for hydrogen and metallic substances. Though this theory was his own suggestion, he acknowledges that we are indebted to M. Ampère for establishing it.

It has been stated that he gave his last public lecture in the Royal Institution in April, 1812; he, however, afterwards delivered an occasional lecture to the managers on his own discoveries, and did not formally resign his professorship until the year 1813.

'At a general monthly meeting of the members, April 5, 1813, the Earl of Winchilsea in the chair, Sir Humphry Davy rose and begged leave to resign his situation of Professor of Chemistry; but he by no means wished to give up his connexion with the Royal Institution, as he should ever be happy to communicate his researches, in the first instance, to the Institution, in the manner he did in the presence of the members last Wednesday, and to do all in his power topromote the interests and success of the Institution.'

On the motion of Earl Spencer, thanks for his inestimable services were voted to him unanimously, and he was elected Honorary Professor of Chemistry, being succeeded as Professor by Mr. Brande. In March of this year, he published his 'Elements of Agricultural Chemistry.' Of this valuable work Dr. Paris has given a complete review, well warranted by the importance of the subject.

Mr. Faraday has furnished to Dr. Paris a relation of the circumstances which attended his introduction to Sir Humphry Davy, which, as they cannot fail to be interesting to readers of this Journal, we shall briefly narrate. 'Bergman (says Dr. Paris) considered the greatest of his discoveries to have been the discovery of Scheele;' and among the numerous services rendered to science by Davy, the amiable conduct which led to the placing Mr. Faraday in the Royal Institution, and thus giving to the world 'a philosopher capable of pursuing that brilliant path of inquiry which his master's genius had so successfully explored,' is not the least estimable; and to use the words of his grateful pupil, 'bears testimony to his goodness of heart.' Mr. Faraday's account is as follows:—

'When I was a bookseller's apprentice, I was very fond of experiment, and very averse to trade. It happened that a gentleman, a member of the Royal Institution, took me to hear some of Sir H. Davy's last lectures in Albemarle-street. I took notes, and afterwards wrote them out more fairly in a quarto volume. My desire to escape from trade, which I thought vicious and selfish, and to enter into the service of science, which I imagined made its pursuers amiable and liberal, induced me at last to take the bold and simple step of writing to Sir H. Davy, expressing my wishes, and a hope that if an opportunity came in his way, he would favour my views; at the same time I sent the notes I had taken at his lectures.'

Davy's answer was kind and encouraging; and shortly after the situation of assistant in the Laboratory of the Royal Institution becoming vacant, he recommended Mr. Faraday.—

'At the same time that he thus gratified my desires (continues Mr. Faraday) as to scientific employment, he still advised me not to give up the prospects I had before me; telling me that science was a harsh mistress, and, in a pecuniary point of view, poorly rewarding those who devoted themselves to her service. He smiled at my notion of the superior moral feelings of philosophic men, and said he would leave me to the experience of a few years to set me right on that matter.'

Mr. Faraday entered upon his appointment in March, 1813, and in October of the same year, accompanied Sir H. Davy in his continental tour, as his secretary and experimental assistant. Napoleon, who had sternly refused his passport to several English noblemen, with a liberality worthy of his character as a patron of science, allowed Sir H. Davy to travel through France; his purpose was to visit the extinct volcanoes in Auvergne, and to examine that which was still in activity at Naples. Dr. Paris has given an interesting account of the occurrences in this tour, principally supplied from the journal of Mr. Faraday and the communications of Mr. Underwood, who, though a *detenu*, had, during the whole war, enjoyed the indulgence of residing in Paris. This gentleman acted there as cicerone to his distinguished friend, and Lady Davy, who accompanied him in his tour.

'Nothing could exceed the liberality, unaffected kindness, and attention with which the *savans* of France received the English philosopher. Their conduct was the triumph of science over national animosity.'

An unknown substance having been accidentally discovered by a manufacturer of saltpetre at Paris, but kept secret by him several years, he at length communicated it to M. Clement, who had found that it might be resolved into a violet-coloured vapour. M. Ampère, having received some of it from M. Clement, transferred it into the hands of Davy. This unknown substance, which had been designated X, was *iodine*. The first opinion of the French chemists was, that it was either a compound of muriatic acid or of chlorine. The first public notice of its existence was given by Clement at the Institute, on the 29th of November, 1813; and at the meeting of the 6th of December, Gay-Lussac, who had only received some X a few days previous, presented a short note, in which he gave the name of *iode* to the body, and threw out a hint as to its great analogy to chlorine, at the same time stating that it might be considered as a simple substance, or as a compound of oxygen. On the 13th of the same month, a letter from Davy to Cuvier was read, in which he offered a general view of its chemical nature and relations; and on the 20th of January, 1814, he communicated to the Royal Society a long and elaborate paper, dated Paris, December 10, 1813, entitled, 'Some Experiments and Observations on a New Substance which becomes a violet-coloured Gas by Heat.'

On the 13th of December, Sir Humphrey Davy was elected a corresponding member of the French Institute, 48 members being present, and Gayton de Morveau being the only person who opposed his election.

He left Paris for Montpelier on the 29th of December, where he remained a month, and worked upon the subject of iodine in the laboratory of M. Berard: from thence he went by way of Turin to Genoa, and then proceeded to Florence, where he experimented in the laboratory of the Accademia del Cimento on iodine, but more particularly on the combustion of the diamond. He quitted Florence on the 3d of April, and entered Rome on the 6th; here he renewed his researches on the combustion of different kinds of charcoal, and transmitted two papers to the Royal Society, the one containing his further experiments on iodine, the other 'On the Combustion of the Diamond and other Carbonaceous Substances.' Dr. Paris observes,

'that the experiments which Davy conducted at Florence and Rome have removed several important errors in regard to the nature of carbonaceous substances; and though they may not encourage the labours of those speculative chemists who still hope to exemplify the old proverb *carbonem pro thesauro*, by manufacturing diamonds out of charcoal, they certainly show that they are less chimerical than those of the wild visionaries who sought to convert the base metals into gold.'

On the 8th of May, he arrived at Naples, and visited Vesuvius and the volcanic country around it. He also took great interest in the excavations, at that time going on at Pompeii under Murat, then King of Naples, who placed at his disposal several specimens of art, which Davy received with a view to investigate the chemical composition of the colours used by the ancients. The results of his inquiry were communicated on the 23d of February, 1815, in a paper 'On a Solid Compound of Iodine and Oxygen,' on April 10, and another 'On the Action of Acids on the Salts usually called Hyper-oxymuriates, and on the Gases produced from them,' on May 4. Before he finally quitted Italy, he again spent three weeks at Naples, during which he experimented on iodine and fluorine in the house of Sementini, and paid several visits to the crater of Vesuvius. He returned to England through Germany and Flanders, and arrived in London in April 1815.

The invention of the Safety Lamp, of which Dr. Paris has given a most interesting detail is, as he observes—

'a discovery which, whether considered in relation to its scientific importance, or to its great practical value, is one of the most splendid triumphs of human genius. It was the fruit of elaborate experiment and close induction; chance or accident, which comes in for so large a share of the credit of human inventions, has no claims to prefer upon this occasion; step by step he may be followed throughout the whole progress of his research, and so obviously does the discovery of each new fact spring from those that preceded it, that we never for a moment lose sight of the philosopher, but keep pace with him during the whole of his inquiry.'

His attention was called to the subject by Dr. Gray (now Lord Bishop of Bristol), then Rector of Bishop Wearmouth, a zealous member of the association which had been formed 'for the Preven-

tion of Accidents in Coal Mines,' in consequence of the fatal calamities which had been of such frequent occurrence by the explosions from fire-damp. Numerous abortive projects had been proposed to the Society, and some few had commanded their attention, when Dr. Gray directed Sir H. Davy's attention to the subject in August, 1815. He was then in Scotland, but immediately began its investigation.

'He commenced with ascertaining the degree of combustibility of the fire-damp, and the limits in which the proportions of atmospheric air and carburetted hydrogen can be combined, so as to afford an explosive mixture. He was then led to examine the effects of the admixture of azote and carbonic acid gas; and the result of those experiments furnished him with the basis of his first plan of security. His next step was to inquire whether explosions of gas would pass through tubes; and on finding that this did not happen if the tubes were of certain lengths and diameters, he proceeded to examine the limits of such conditions; and by shortening the tubes, diminishing their diameters, and multiplying their number, he at length arrived at the conclusion that a simple tissue of wire-gauze afforded all the means of perfect security; and he constructed a lamp, which has been truly declared to be as marvellous in its operation, as the storied lamp of Aladdin—realizing its fabled powers of conducting in safety through "fiends of combustion," to the hidden treasures of the earth.—When it is remembered that the security thus conferred upon the labouring community is not merely the privilege of the age in which the discovery was effected, but must be extended to future times, and continue to preserve human life as long as coal is dug from our mines, can there be found, in the whole compass of art or science, an invention more useful and glorious?'

A subscription was raised by the gentlemen interested in the coal mines of the Tyne and the Wear, and a service of plate was presented to Sir Humphry Davy. The lamp was named after him by the miners, and is now called a *Davy;* and the Emperor of Russia, to whom he had presented a model of the safety lamp, honoured him with the present of a handsome silver gilt vase. He has been heard to declare that the discovery had given him more satisfaction than anything he ever did, as it served the cause of humanity, and would save the lives of thousands of poor labourers. Other interesting results to science have arisen out of the investigation for constructing a safety lamp. We have been made better acquainted with the nature of flame and the circumstances by which it is modified, leading to some practical views connected with the useful arts. These Davy communicated to the Royal Society, in January, 1816, for which, and his previous researches on flame and combustion, the Society adjudged to him the Rumford gold and silver medals. These communications he put into a form more accessible to the practical parts of the community, by reprinting them collectively in an octavo volume, 'On the Safety Lamp, with some Researches on Flame, 1818.'

Having made some experiments on fragments of the papyri, or manuscript rolls, which had been found in the ruins of Herculaneum,

with apparent success, and Mr. Hamilton having represented the circumstance to government, he was authorised to proceed to Naples, and funds were placed at his disposal for paying persons whom it might be necessary to engage in the process. He embarked at Dover for the continent in May, 1818. During his journey he made some interesting observations on the causes of the formation of mists over the beds of river and lakes, which were communicated to the Royal Society in February, 1819. In the process of unrolling the papyri, it appears that Davy was not successful; the failure was not, however, owing to his want of zeal and skill, but solely to the unfortunate condition of the rolls. He had succeeded in partially unrolling some, and the late Rev. Peter Elmsley came to Naples for the purpose of assisting in transcribing what had been recovered. This excited jealousy, which the interference of the English ambassador could not entirely remove; obstructions were thrown in the way of his future operations, and he abandoned the task at the end of two months. He returned to England in 1820; and the death of Sir Joseph Banks, having vacated the chair of President of the Royal Society, Davy was raised to that high honour. Dr. Wollaston, having declined competition, gave him the weight of his influence; and though a feeble attempt was made in favour of Lord Colchester, without his knowledge or concurrence, Davy was elected by an immense majority.

In the winter of 1819, Professor Oersted published an account of his highly important discovery of the intimate relation of electricity and magnetism, which has given birth to a new science termed ELECTRO-MAGNETISM. The discovery was limited to the action of the electric current on needles previously magnetised. Davy applied himself with his characteristic zeal to the repetition and variation of the experiments, and soon ascertained that the uniting conductor itself became magnetic during the passage of electricity through it. He communicated the results of his successive experiments on this subject to the Royal Society, in three papers, published in 1820, 1821, and 1823. The third paper announced the discovery of a new electro-magnetic phenomenon. These researches are inseparably connected with Mr. Faraday's beautiful experiments on magnetic rotation.

Dr. Paris relates in detail the circumstances attendant upon one of the most important discoveries of modern science, *the condensation of the gases;* a discovery which he regards, with justice, as strictly belonging to Mr. Faraday. The circumstances are particularly interesting, because Dr. Paris was an eye-witness of the first result, and publicly stated Mr. Faraday's claim immediately after the event, in a lecture at the College of Physicians. Those who have read Mr. Faraday's paper in the Philosophical Transactions, and that of Sir H. Davy, will have remarked a discrepancy in the statements, which the narrative of Dr. Paris will correct.

'Before the year 1810, the solid substance obtained by exposing chlo-

rine to a low temperature, was considered as the gas itself reduced into that form. Davy first corrected the error, and showed it to be a hydrate, the pure gas not being condensible even at a temperature of 40° Fahrenheit. Mr. Faraday had taken advantage of the cold season to procure crystals of this hydrate, and was proceeding in its analysis, when Sir H. Davy suggested to him the expediency of observing what would happen if it were heated in a close vessel; but this suggestion was made after Mr. Faraday had obtained results which must have led him to the experiments, had he never communicated with Sir H. Davy. On exposing the hydrate, in a tube hermetically sealed, to a temperature of 100°, the substance fused, the tube became filled with a bright yellow atmosphere, and on examination was found to contain two fluid substances: the one, about three-fourths of the whole, was of a faint yellow colour, having very much the appearance of water; the remaining fourth, was a heavy bright yellow fluid lying at the bottom of the former, without any apparent tendency to mix with it. By operating on the hydrate in a bent tube hermetically sealed, Mr. F. found it easy, after decomposing it by a heat of 100°, to distil the yellow fluid to one end of the tube, and thus to separate it from the remaining portion. If the tube was now cut in the middle, the parts flew asunder, as if with an explosion, the whole of the yellow portion disappeared, and there was a powerful atmosphere of chlorine produced; the pale portion, on the contrary, remained, and, when examined, proved to be a weak solution of chlorine in water, with a little muriatic acid, probably from the impurity of the hydrate used. When that end of the tube, in which the yellow fluid lay, was broken under a jar of water, there was an immediate production of chlorine gas. Mr. Faraday soon perceived that the chlorine had been entirely separated from the water by the heat, and *condensed* into a dry fluid by its own abundant vapour. He subsequently confirmed these views by condensing chlorine in a long tube by mechanical pressure.'

To Mr. Faraday's paper, Sir H. Davy thought proper to add a 'note on the condensation of muriatic gas into the liquid form;' and on the 17th of April, he communicated to the Royal Society a paper 'On the application of Liquids, formed by the condensation of Gases, as Mechanical Agents;' which contains the results of several experiments made, with the assistance of Mr. Faraday, on the differences between the increase of elastic force in gases under high and low pressures.

In the latter part of 1823, the government requested the advice of the President and Council of the Royal Society, as to the best mode of manufacturing copper sheets, or of preserving them, while in use as the sheathing of ships, against the corrosive effects of oxidation. Sir H. Davy charged himself with this inquiry, and communicated the results in three memoirs, read in January and June, 1824, and in June, 1825. In these papers he comes to the conclusion, that as copper is but weakly positive in the electro-chemical scale, and can only act upon sea-water when in a positive state, that by rendering it slightly negative, the corroding action of sea-water upon it is prevented; he proposed therefore to render the copper electro-positive by means of the contact of an easily oxidable metal. Having communicated his views to his Majesty's government, an order was made for trying the plan of protection he

suggested upon the bottom of a sailing cutter, under his own superintendence. Models were also constructed and floated in sea-water for several months. The experiment seemed conclusive, and the plan was put into extensive practice. In the month of June, 1824, the Comet steam-vessel was prepared, to afford him the means of performing his experiments upon the best means of protection, in which he made a voyage to Heligoland and the Naze of Norway. When in Denmark, he visited Professor Oersted and Dr. Olbers; and at Stockholm he had a transient interview with Berzelius. In June, 1825, he read before the Royal Society his third paper on Copper sheathing; and the subject was continued in his last Bakerian Lecture, in June, 1826, 'On the Relation of Electrical Changes,' for which the royal medal was adjudged to him by the Royal Society. At the conclusion of this lecture he says, 'A great variety of experiments, made in different parts of the world, have proved the full efficacy of the electro-chemical means of preserving metals, particularly the copper-sheathing of ships; but a hope I had once indulged, that the peculiar electrical state would prevent the adhesion of weeds or insects, has not been realized—and an absolute remedy for adhesions is to be sought for by other more refined means of protection, which appear to be indicated by these researches.'

The vessels, which had their copper-sheathing protected upon the plan proposed by Sir H. Davy, were found to have their bottoms completely covered with sea-weed, shell-fish of various kinds, and myriads of small marine insects, the copper near the protectors being much more foul than at a distance from them; and the inconvenience attending this circumstance was so great, as to cause the plan to be abandoned altogether, after long-continued trial, under various circumstances, in 1828.

Dr. Paris gives a detailed account of similar experiments made in France, on La Constance frigate, which were attended with similar results; and informs us, that further experiments are about to be tried in the British navy, founded on the same principle. Sir H. Davy experienced disappointment and chagrin at the failure of his plan, wholly inconsistent with the merits of the question; but his health was now declining, and this produced, no doubt, that morbid sensibility and irritation which his friends witnessed with pain. At the close of the year 1826, his indisposition increased: he complained of palpitation of the heart, and an affection of the trachea, and was unable to walk without fatigue. In January, 1827, he published the Discourses which he had delivered before the Royal Society, on awarding the Copley Medals, to which was prefixed his Discourse on taking the chair of the Royal Society for the first time, which contains some passages of interest, prophetic of future discoveries. While on a visit to Lord Gage, at the close of 1826, he felt more than usually unwell, and determined to return to London: while on his journey, he was seized with an apoplectic attack at Mayersfield; prompt and copious bleeding on the spot arrested the symptoms

threatening life, and he reached home. As soon as the more immediate danger of the attack had passed away, he was advised to a residence in the South of Europe, and accordingly quitted England with the intention of wintering in Italy. Feeling that his recovery was tardy, and that mental repose was necessary for its advancement, he determined to resign the chair of the Royal Society, and announced his intention in a letter to Mr. Davies Gilbert, who was appointed to fill the chair till the next anniversary, and ultimately was elected to succeed him.

He returned to London in October, 1827, for the benefit of medical advice; again visited his friend Lord Gage in Sussex, and passed a short time with his friend, Mr. Poole, at Stowey. His bodily infirmity was then very great, and his sensibility painfully alive on every occasion. In this period of suffering he amused himself with writing his Salmonia, or Days of Fly-fishing (written in emulation of that delightful piscatory pastoral the 'Complete Angler'). This was published in the spring of 1828. It contains many pleasing illustrations of facts in natural history. Dr. Paris has given some interesting extracts, and thus defends the writer: 'If the advanced age of Walton was pleaded by himself as a sufficient reason for procuring "a *writ of ease*," the friends of Davy may surely claim, at the hands of the critic, an indulgent reception for a congenial work, written in the hour of lassitude and sickness.'

His paper, 'On the Phenomena of Volcanoes,' was read before the Royal Society on the 20th of March, 1828, just before he quitted England on his last journey; and he communicated a paper on the Electricity of the Torpedo, which is dated from Luciana in Illyria, in October, 1828. He concludes the paper by expressing his fear that the weak state of his health will prevent him from pursuing the subject with the attention it seems to deserve. His last production was 'Consolations in Travel, or the Last Days of a Philosopher,' which was published by his brother Dr. Davy, after his decease. He informs us in the preface, that it was composed immediately after 'Salmonia,' under the same painful circumstances. From this exercise of the mind, he says he derived some pleasure and some consolation, when most other sources of consolation and pleasure were closed to him; and he ventures to hope that those hours of sickness may be not altogether unprofitable to persons in health. This work is too generally diffused to render it necessary that we should enter into a detail of its contents, had we space to do so. Dr. Paris has given large extracts, and thus expresses his opinion of its claim to attention:—

'This is a most extraordinary and interesting work; extraordinary, not only from the wild strength of its fancy, and the extravagance of its conceptions, but from the bright light of scientific truth which is constantly shining through its metaphorical tissue, and irradiating its most shadowy imaginings. It may be compared to the tree of the lower regions in the Æneid, to every leaf of which was attached a dream, and yet, however wildly his fancy may dream, his philosophy never sleeps; and in his exit from the land of phantoms, the author can in no instance be accused

of having mistaken the gate of ivory for that of horn. To the biographer, the work is of the highest interest and value, by confirming in a remarkable manner the opinion so frequently expressed in the course of these memoirs, with respect to the diversified talents of Sir Humphry Davy; and above all by elucidating that rare combination of imagination with judgment, which imparted to his genius its most striking particularities.'

Davy's latter days were cheered by the affectionate attentions of Mr. James Tobin, his godson, who was the companion of his travels. He resided at Rome for some months, but declined receiving any visitors; his only solace was to have some one reading to him light works of interest, which was continued even during his meals. As soon as the account of his having sustained another paralytic seizure reached Lady Davy, who was in London, she hastened to join him, and reached Rome in about twelve days. Dr. John Davy, on receiving intelligence of his brother's imminent danger, came to him from Malta: he only partially recovered from this attack, and though there appeared some faint indications of reviving power, his most sanguine friends scarcely ventured to indulge a hope that his life would be much longer protracted. With that restlessness characteristic of his disease, he became extremely desirous of removing from Rome to Geneva. His friends were anxious to gratify his wish, and travelling by easy stages he reached Geneva at three o'clock on the 28th of May, accompanied by Mr. Tobin and his servant, Lady Davy having preceded him to make arrangements for his reception. At four o'clock he dined, ate heartily and was unusually cheerful, he drank tea at eleven, and retired to rest at twelve. His servant, who slept in his room, was very shortly called to attend him, he desired his brother might be summoned, and he expired at a quarter before three without a struggle.

The public authorities of Geneva honoured his remains with a public funeral after the custom of Geneva, which was attended on foot by the magistrates, the professors, and the English residents at Geneva. A tablet has been placed to his memory in Westminster Abbey, by his widow, but, as yet, no national monument has been devised to commemorate the service he has rendered to science, his country, and mankind. It is said that the inhabitants of Penzance and its neighbourhood are about to raise a pyramid of massive granite to his memory, on one of those elevated spots where he delighted in his boyish days to commune with the elements.

'The fame of such a philosopher as Davy,' says his Biographer, 'can never be exalted by any frail memorial which man can raise. His monument is in the great Temple of Nature, his chroniclers are Time and the Elements. The destructive agents which reduce to dust the urn, the statue, and the pyramid, were the ministers of his power, and their work of decomposition is a perpetual memorial of his intelligence.'

Though this analysis of Dr. Paris's work has run out to an unreasonable length, we must indulge in the quotation of his judicious comparison of the characters of two illustrious philosophers; it is an acceptable enlargement of their intellectual portraits so ably drawn by Dr. Henry.

'In contrasting the genius of Wollaston with that of Davy, let me not be supposed to invite a comparison to the disparagement of either, but rather to the glory of both, for by mutual reflection each will glow the brighter.—If the animating principle of Davy's mind was a powerful imagination, generalizing phenomena, and casting them into new combinations, so may the striking characteristic of Wollaston's genius be said to have been an almost superhuman perception of minute detail. Davy was ever imagining something greater than he knew; Wollaston always knew something more than he acknowledged:—in Wollaston, the predominant principle was to avoid error; in Davy it was the desire to discover truth. The tendency of Davy, on all occasions, was to raise probabilities into facts; while Wollaston as continually made them subservient to the expression of doubt.

'Wollaston was deficient in imagination, and under no circumstances could he have become a poet, nor was it to be expected that his investigations should have led him to any of those comprehensive generalizations which create new systems of philosophy. He well knew the compass of his powers, and he pursued the only method by which they could be rendered available in advancing knowledge. He was a giant in strength, but it was the strength of Antæus, mighty only on the earth. The extreme caution and reserve of his manners were inseparably connected with the habits of his mind; they pervaded every part of his character; in his amusements and in his scientific experiments, he displayed the same nice and punctilious observation,—whether he was angling for trout, or testing for elements, he alike relied for success upon his subtile discrimination of minute circumstances.

'By comparing the writings as well as the discoveries of these two great philosophers, we shall readily perceive the intellectual distinctions I have endeavoured to establish—"From their fruits shall ye know them." The discoveries of Davy were the results of extensive views and new analogies, those of Wollaston were derived from a more exact examination of minute and, to ordinary observers, scarcely appreciable differences. This is happily illustrated by a comparison of the means by which each discovered new metals.—The alkaline bases were the products of a comprehensive investigation, which had developed a new order of principles; the detection of palladium and rhodium among the ores of platinum, was the reward of delicate manipulation, and microscopic scrutiny.'—'The chemical manipulations of Wollaston and Davy offered a singular contrast to each other, and might be considered as highly characteristic of the temperaments and intellectual qualities of these remarkable men. Every process of the former was regulated with the most scrupulous regard to microscopic accuracy, and conducted with the utmost neatness of detail. It has been already stated with what turbulence and apparent confusion the experiments of the latter were conducted, and yet each was equally excellent in his own style; and as artists they have not unaptly been compared to Teniers and Michael Angelo. By long discipline, Wollaston had acquired such power in commanding and fixing his attention upon minute objects, that he was able to recognise resemblances, and to distinguish differences, between precipitates produced by re-agents, which were invisible to ordinary observers, and which enabled him to submit to analysis the minutest particle of matter with success. Davy, on the other hand, obtained his results by an intellectual process, which may be said to have consisted in the extreme rapidity with which he seized upon, and applied appropriate means at appropriate moments.'

Acta Academiæ Cæs. Leop. Carol. Naturæ Curiosæ Bonnæ. Vol. XIII. and XIV. 1826—1828: (*On the Blood-vessels of the Head, and on the Internal Ear of some Hybernating Animals.*) By Dr. A. G. Otto.

Dr. Otto's anatomical inquiries, having for object the investigation of the arrangement of certain parts of the vascular system in hybernating animals, have been attended by the establishment of some singular and unexpected facts. Mangili (*Annales du Muséum*, x. 463.) had asserted that in some, and perhaps all, hybernants, the internal carotid artery is wanting, the brain being supplied with blood by the vertebral artery only, and in small quantity; a fact which he supposed to be connected with diminished irritability of the brain, and consequently with the periodical lethargy peculiar to such animals. Saissy, on the other hand, (*Récherches sur la Physique des Animaux Hybernans,*) states, that in such animals the heart and internal vessels are more than ordinarily capacious, whilst the external vessels are smaller, and the cutaneous nerves larger than in other animals: hence, therefore, that the hybernants are more readily rendered torpid by cold.

According to Dr. Otto, hybernating animals do not present these peculiarities; but, on the contrary, have others equally remarkable, though less obvious. The brain is supplied with at least as much blood as in other animals, though the channels through which it passes are differently arranged. The internal carotid appears at first sight to be deficient, the vertebral artery being proportionally very large, uniting with its fellow to form the basilar, which furnishes not only the vessels of the posterior part of the brain and the circle of Willis, but also the anterior cerebral, and the ophthalmic arteries. Neither is the internal carotid really wanting, though its course is very peculiar. In the bear, badger, hare, rabbit, and beaver, the vessel passes through the carotic canal; in the porcupine it is the continuation of the trunk of the internal maxillary, and enters the cranium through the *foramen lacerum anterius;* in the cavy and aguti it is but a lateral branch of the same vessel, and enters the cranium through the *foramen ovale.* In all the hybernants, on the other hand, the internal carotid passes into the cavity of the tympanum by, or near to, the jugular foramen, and, in its course into the cranium, perforates the open ring of the stapes or stirrup-bone of the ear: this structure prevails in bats, hedge-hogs, shrews, moles, lemmings, all mures, myoxi, in the hamster, jerboa, marmot, and squirrel. These are precisely the animals which pass a certain portion of the year in a state of sopor, the apparent exceptions, as in the Mus genus, probably depending on the more artificial habits of these animals, produced by their intercourse with man. The bear and badger, on the contrary, in which the carotid artery follows the ordinary course, do not really hybernate; for, whilst they do not appear to become torpid until the temperature sinks to 13° or 14° (Reaumur), they are even then readily roused, and seem rather weak and morose than somnolent.

The canal in which the carotid is lodged in passing through the ear of the hybernants, is in some cases merely membranous, whilst in others it is completely bony, the stapes, in that case, being fixed to the cavity of the tympanum by the bony tube passing through its ring.

We are not justified in viewing this structure as a cause of hybernation. In all these animals, in fact, the tympanum and other parts of the ear are so large, that in connexion with the bulk of the submaxillary and thymus glands of the pterygoid muscles, and the posterior processes of the lower jaw, that the whole of the base of the cranium is occupied, and no other course is left for the passage of the carotid artery than through the tympanum. Besides, these animals, when torpid, are rolled up, with the head buried between the legs; so that, if the carotid took its ordinary course, it would scarcely fail to be compressed, and the passage of blood through it obstructed.

On a Peculiar System of Visceral Nerves in Insects, analogous to the Sympathetic. By Dr. J. Mueller.

Amongst the numerous questions connected with the study of the sympathetic nerve, none has been more frequently discussed, or more variously decided than that which has for object the determination of the true relations of this nerve, as it exists in the higher classes of animals, and the nervous system at large of the lower classes, including the whole of those without vertebræ. Nor is this one of those minor and comparatively unimportant investigations with which most sciences abound, and of which the results have not any immediate influence on fundamental doctrines. On the contrary, the various modes in which the same anatomical facts have been viewed by different individuals engaged in this inquiry, have uniformly had a direct reference to the physiology of the most important of the systems of which animals are composed.

Ackermann, Reil, and Bichât, with numerous followers, misled by superficial examination and imperfect analogy, assumed and reasoned upon a perfect identity of character between the sympathetic nerve of vertebral, and the ganglionic chain of articulated animals; an assumption on which was raised a superstructure of most important physiological conclusions.

Scarpa, Blumenbach, Cuvier, Gall, J. F. Meckel, Arsaky, and Carus, trusting less to first impressions, and guided rather by the examination of the natural laws regulating the progressive development of parts in the animal scale, showed that there existed sufficient grounds for rejecting this supposed analogy. Meckel and Von Walther expressed the opinion, that the cords extending from the brain into the body of invertebral animals were to be viewed as uniting in themselves the characters of the subsequently disjunct systems (in vertebral animals) of spinal marrow and sympathetic nerve; inclining, in molusca, to the type of the latter; in articulata, of the former. Rudolphi has expressed the same idea in a still

more pointed manner, opposing vertebral animals with a double nervous system; (*Diploneura,*) to invertebral, with a single nervous system, (*Haphloneura;*) and dividing the latter again into *Ganglioneura* and *Myeloneura,* according as they resemble, on the one hand, the ganglionic system of the sympathetic nerve, or, on the other, approach to the type of the spinal marrow.

At a later period, G. Treviranus and E. Weber assimilated the series of ganglia in insects to the ganglia of the spinal nerves, and viewed the connecting cord as the first rudiments of a spinal marrow. Still more recently, when the supposed analogy between the sympathetic nerve and the ganglionic chain of insects had been long refuted and abandoned, Serres and Desmoulins, who have so freely, and with such inadequate acknowledgment, availed themselves of the researches of the Germans, again raised the hypothesis from oblivion, without adverting to, and apparently without being acquainted with, what had already been done by the eminent men above alluded to.

Previous to the attempt to decide the whole question by the description of a sympathetic nerve in insects, distinct from the abdominal chain of ganglia, Dr. Mueller attempts to determine the essential characters of a spinal and a sympathetic system, abstracted from the accidental circumstances of form, position, &c.

A nervous system, consisting of separate ganglia, is not, therefore, necessarily equivalent to the sympathetic system of vertebral animals; neither is that always a spinal marrow which presents itself in the shape of a chord. These circumstances of arrangement are of little importance, and are mainly determined by the shape and peculiar organisation of the animal. In some vertebral animals, *Tetrodon mola,* for instance, all the nerves of the trunk arise from the medulla oblongata, the separate nerves contained within the spinal canal, representing what, in other cases, is the spinal marrow. In *Lophius piscatorius*, the spinal marrow terminates in the upper cervical vertebræ, the nerves, which here, as in the former case, arise by double roots, running separate and parallel through the spinal canal.

On the other hand, there are articulata which do not possess the usual ganglionic chain. Thus, according to G. Treviranus, the ganglia of the phalangidæ are dispersed as in mollusca, whilst in the true arachnidæ the nervous system forms a solid central cord, and in scorpions assumes the usual character of a chain of ganglia. Even as regards the sympathetic nerve itself, the presence of ganglia is by no means an essential character, for in most fishes they are altogether wanting.

The only absolute distinctive characters of the two nervous systems of the trunk are, that the spinal marrow is chiefly destined to preside over voluntary motion, whilst the sympathetic nerve is exclusively devoted to the viscera; that the former is an immediate continuation of the brain, whilst the latter has an independent development, except as regards the filaments by which it is connected with

the brain and spinal marrow. The characters derived from form and position are so little determinate, that the visceral (sympathetic) nerve of insects is usually single, whilst in vertebral animals it is double; that the spinal marrow of insects, on the contrary, for the most part, is formed of separate cords, whilst it is solid in vertebral animals; that the sympathetic nerve of insects is situated on the back, and the nervous system of voluntary motion (spinal marrow) is placed on the abdominal aspect; that in articulata, this same system is knotted, and in fishes a solid cord; the sympathetic, in the one case, swelling into ganglia, which, in the other, (fishes,) as already mentioned, are deficient.

If due attention had been paid to the statements of Swammerdam and Lyonnet, the abdominal chain of ganglia in insects could never have been assimilated to the sympathetic nerve of vertebral animals. Under the name of Nervus Recurrens, the former has described in the larva of the rhinoceros-beetle and of the silk-worm, a nerve arising by two roots from the anterior part of the cerebral ganglion: these roots, after proceeding a little way forwards, turn backwards and approximate to each other, uniting into a ganglion placed above the commencement of the œsophagus; from this proceeds the recurrent nerve; which, after having passed through a second ganglion, runs along the posterior surface of the œsophagus, to be distributed upon the stomach and intestine. The recurrent nerve of the larva of the rhinoceros-beetle is represented in Tab. xxviii. fig. 2, and that of the silk-worm in Tab. xxviii. fig. 3. g. of the Biblia Naturæ.

Lyonnet's description of this nerve in the larva of the *Phalæna cossus* is still more accurate. It here begins with a series of ganglia in the anterior part of the head, above and in front of the brain. The third of these ganglia, reckoning from before backwards, is the largest, and is connected by two sets of lateral cords with cerebral nerves, and by a branch on each side with the ganglion in front of it, which again gives origin to a single filament, by means of which it communicates with the first and anterior of these three frontal ganglia. The nerves derived from these ganglia are distributed upon the œsophagus. The third ganglion, after a short space, is continued into a fourth, to which again succeed several smaller ones. The nerve thus formed, about the middle of the head, perforates the dorsal vessel from above downwards, running between it and the œsophagus, distributing its branches to the surrounding parts, and terminating by dividing into two portions, which are lost in the substance of the stomach. (Lyonnet, Traité Anatomique de la Chenille, &c. Tab. xii. fig. 1., xiii. fig. 1., xviii. fig. 1.)

This nerve has been incidentally noticed by Cuvier in the rhinoceros-beetle's larva, in *Hydrophilus piceus*, and *Locusta viridissima;* by Meckel in the common *Cicada;* by G. Treviranus in *Dytiscus marginalis*, *Sphynx ligustri*, and the common bee; and by Marcel de Serres, though very incorrectly, in some species of *acheta*.

The character and functions of this remarkable nerve are points

that have been almost wholly neglected by systematic writers. Meckel and Treviranus alone have expressed their belief of its analogy with the sympathetic nerve of vertebral animals; the latter, in particular, has endeavoured to show that it corresponds to this nerve only, and not to the par vagum, with which Mercel de Serres is somewhat disposed to identify it.

The term recurrent, by which it has usually been designated, is far from being generally applicable. In fact, it is only in Coleoptera and Lepidoptera, that such is the case: in Orthoptera it arises directly from the posterior margin of the brain. From the mode of its distribution to the viscera only, and especially to the stomach and intestine, to the complete exclusion of the voluntary muscles, the name of visceral nerve is in every respect more suitable.

The following descriptions will serve to show the extent to which this discovery has been already pursued, and to give a general idea of the ordinary arrangement of the visceral nerve.

Orthoptera.—In *Phasma ferula*, which is herbivorous, the brain is situated beneath the commencement of the œsophagus, and the usual nervous ring is wanting. The visceral nerve originates in a small ganglion placed on the dorsal surface of the œsophagus, and communicating by several minute filaments with the anterior part of the chain of ganglia on the abdominal surface. The ganglion thus formed sends a small filament forwards to the corslet, and is continued posteriorly into a long and thicker cord, which, accompanied by a large trachea, is prolonged almost as far as the muscular stomach (gizzard). In the middle of the second thoracic segment, at the same point where the trachea bifurcates, the visceral nerve forms a second ganglion, oval and considerably larger than the first. In the interval between the two ganglia it gives numerous delicate branches to the œsophagus. A great number radiate from the second ganglion, to form an extended and most delicate rete in front of the gizzard; two, much larger than the rest accompany the divisions of the trachea, and, like them, are distributed to the coats of the stomach.

In *Mantis ægyptiaca*, (carnivorous), there is a considerable white ganglion upon the gizzard, from which large branches radiate in all directions, forming a plexus on the capacious membranous stomach in front of the gizzard, penetrating the very short intestine, and extending even to the cœcal pouches, or supposed biliary organs. Anteriorly, the ganglion is continued along the posterior surface of the membranous stomach and œsophagus into a filament, which gives off alternate branches as it advances forwards, becoming gradually smaller in its course.

In *Gryllotalpa vulgaris* (the Mole Cricket,) two nervous filaments of considerable size arise from the posterior edge of the brain, which immediately unite to form the trunk of the visceral nerve. From this are immediately detached two chords, which extend for the space of about an inch along the œsophagus and membranous stomach, as far as the gizzard, giving off branches in their course,

and at the same time increasing in size. Immediately in front of the commencement of the gizzard they unite in a triangular ganglion, from which many filaments radiate, forming a plexus upon the gizzard, and disappearing on the vesicular cœca.

Coleoptera.—In *Dytiscus marginalis* the sympathetic nerve is so large and distinct, that it may easily be traced by the unaided eye. It arises by two very slender roots from the anterior part of the brain, a small ganglion being formed by their union in the anterior and upper part of the head. The trunk of the nerve at its first origin from this ganglion is very slender, but gradually increases in size in its course along the œsophagus. It also gradually bends towards the left side of the canal and of the first stomach, closely following the curves of the latter. Before the termination of the first stomach it expands into a second and smaller ganglion, from which two filaments diverge, to ramify on the second and the beginning of the third stomach.

In the Stag-beetle (*Lucanus cervus*), the frontal ganglion appears as a white triangular mass, situated in front of the brain, with which it is connected by means of two lateral loops. Posteriorly, it is elongated into a very delicate filament, which, passing below the brain, accompanies the narrow œsophagus for a short space, and then bifurcates without expanding into a ganglion; at this point the branches become so minute, that they can no longer be traced even by the aid of glasses.

As the larvæ of the herbivorous Coleoptera with lamellated antennæ present a very complicated structure of the alimentary canal, as compared with the perfect insects, there is ground for supposing that the recurrent, or sympathetic nerve, may be developed in a corresponding degree, particularly when we consider that, in the larva of the rhinoceros beetle, according to Swammerdam and Cuvier, it swells into two ganglia, and accompanies the whole intestinal canal. If such be the case, it would farther follow, that during the process of metamorphosis, the visceral system of nerves must undergo changes analogous to those which are known to take place in the abdominal chain of ganglia, and corresponding to the changes which take place in the organs on which it is distributed.

Hemiptera.—The observation by J. F. Meckel, of the existence of a recurrent nerve in *Tettigonia plebeia*, presents all that is yet known as far as regards this order of insects.

Lepidoptera.—In the larva of a large sphinx, Dr. Mueller observed the same relations of the recurrent nerve as in the descriptions by Lyonnet and Swammerdam, of the *Phalaena cossus* and *Bombyx mori.*

Hymenoptera.—As already mentioned, the recurrent nerve has been discovered by G. Treviranus in the common bee.

Crustacea.—In the cray-fish (*Astacus fluviatilis*), Dr. Mueller supposes that he has seen an elongated frontal ganglion, which extends upwards and downwards, ramifying on the stomach, and communicating by short and delicate filaments with the brain. The

nervous texture in this class of animals, however, is so gelatinous, and so little distinct from the other parts of the body, even when acted on by alcohol, that he by no means places the same reliance on this as on the previous statements.

Articulated Worms.—In these animals, which approach so closely to the other articulata in the arrangement of the nervous system, the visceral nerve is also present. Thus, in *Aphrodite aculeata*, the lateral cords forming the nervous collar around the pharynx, immediately before their junction in the first ganglion of the abdominal chain, 'each send off a filament which may be called recurrent. These nerves are very distinct, and proceed forwards towards the point where the short œsophagus is attached to the stomach; they may be traced by the naked eye along the sides of the latter, which is very long and muscular. Before they reach the intestine, they expand into a ganglion, from which proceed numerous ramifications.' —CUVIER.

The character assigned to this system of nerves (sympathetic or visceral), and the close analogy it presents to the corresponding system in vertebral animals, will hardly be disputed, if, taking a general view of the different degrees of development in which it has already been found to exist, we fix our attention more particularly on the points which establish that character and analogy most precisely. These consist, on the one hand, in the slight and unimportant connexions existing between it and the brain; and on the other, in its independent development, and the direct proportion which that development presents to the degree of complication of the alimentary canal, as is particularly marked in the case of the carnivorous coleoptera, of the carnivorous and herbivorous mantides, and of the other orthoptera.

On the Salivary Glands of Serpents, which are commonly considered not venomous. By H. Schlegel, M.D.

SERPENTS have hitherto been usually separated into two great divisions, viz., those that are, and those are not venomous. As the fact, too, is one that admits of being determined by the examination of the teeth, it is the more extraordinary that there should be great variation in the statements of even the most recent zoologists; and the only manner in which we can explain the doubts that have been suggested as to the venomous nature of certain serpents, is by supposing that those with whom they originated were either ignorant of, or neglected to examine the venomous apparatus. In truth, many have sought the perforation of the venom-fangs at a point where it does not exist, or have allowed themselves to be deceived by the thick membraneous sheaths, in which those fangs are usually buried.

Some naturalists, as Gray and Cuvier, have endeavoured to establish external characters, for the purpose of discriminating these two divisions of serpents; but so far without success, that the genera

Elaps, Naja, and Bungarus agree so closely in general appearance with the Coluber family, as to be with difficulty distinguished.

Of late it has been asserted in many quarters, that the bite of serpents not ordinarily reputed venomous has proved fatal. Professor Natterer communicated similar facts to Dr. Schlegel, at Vienna, on the authority of his brother in Brazil. Professor Reinwardt, during his residence in Java, examined the teeth of Dipsas Lendrophila, not usually considered venomous, and found them similar to those of others of the Coluber family, excepting that the hindermost tooth of the upper jaw on each side is longer than the rest, and perforated. Professor Boie of Leyden pursued the investigation farther, and found that the same structure prevails in all species of the genera Dipsas and Homalopsis. Dr. Schlegel has found similar teeth in the middle and at the posterior part of the upper jaw in the genus Bryophis. They exist also in Bamophis, but are not perforated. In Xenodon there is a single tooth at the back of the jaw, not perforated.

These peculiarities are attended with corresponding variations in the arrangement of the glandular apparatus of the head. In the greater number of true serpents and Boæ, the upper and lower maxillæ are armed with ranges of solid teeth, attached by their roots to the bone, and accompanied on the inner side by a second range, which is connected with the soft parts only. In these cases the upper maxilla is very long, and the teeth are lodged at the bottom of a deep membraneous furrow, forming a distinct sheath for each, from which little more than the point projects. A corresponding glandular mass is ranged along the margin of each jaw, secreting the saliva, and detaching an excretory duct in the situation of each tooth.

In those species which have a perforated tooth posteriorly, as Dipsas, Homalopsis, &c., the teeth in general are more widely separated than in others of the Coluber family. The superior maxilla is also proportionally shorter, a point in which there is an approximation to the truly venomous serpents. The sheath which surrounds this tooth is very capacious, and like that of the proper venomous species, contains one tooth firmly attached, and two, three, or four, moveable, less completely formed, and ready to supply the loss of the principal one ; the latter is perforated along its anterior edge, and has an internal cavity communicating with this fissure.

In Homalopsis monilis (from Java), Coluber monilis, Linn., there is the same structure ; in addition to the ordinary salivary gland placed along the margin of the superior maxilla, as in the other colubrine species, there is a separate gland of considerable size, destined exclusively for the posterior perforated tooth, and giving off a large excretory duct, which penetrates its root, and discharges the secretion of the gland through the fissure on its front edge.

Dr. Schlegel divides the proper venomous serpents into three families, the first containing the colubrine species, as Elaps, Naja,

Bungarus, &c.; the second, truly venomous, as Trigonocephalus, Cophias, Vipera, Pelias, Crotalus, &c.; the third, water-serpents, with the exception of Chersydrus, which is innoxious, and a true Acrochordus. The species of the first family possess the venom apparatus like the rest; the perforated tooth, however, has not merely an aperture above and below, but is also open along the whole length of its front edge, and, consequently, therein approximates to those with the fissured posterior tooth.

In the same manner that the superior maxilla is shortened in those species where there is a perforated posterior tooth, so is it also in the Colubrine family, but is still longer than in the Viperine genera, where the ranges of un-perforated teeth disappear, the venom-fangs alone remaining. The external pterygoid bone is elongated in the same proportion as the superior maxilla is shortened, and the mobility of the latter is obviously in the same degree increased, inasmuch as it is attached to the extremity of a lever of greater length.

The facts above stated may serve to explain the discrepancies in the statements of the consequences of the wounds inflicted by those serpents which have the posterior tooth perforated. When the latter penetrates the wound, fatal effects may ensue; whilst, on the contrary, when the anterior teeth only act, the injury will not have more serious results than those of the bite of an ordinary Coluber.

Is it allowable to suppose that the ancients were acquainted with a serpent of this kind, and that they therefore distinguished their Διψάς, as one, the bite of which caused extreme thirst?

Anatomy of the Nervous and Vascular Systems of Nais diaphana.

By Dr. Gruithuisen.

The head of this animal is characterized by a set of muscular fibres radiating around the pharynx; by means of them that cavity is dilated, and when the prey (infusoria) of the animal is drawn into it, which is done with the rapidity of lightning, they close the mouth, and force the food onwards towards the stomach.

The alimentary canal presents alternate expansions and contractions: the first cavity (stomach) is surrounded by little glands in double rows, the remaining part of the canal is beset with larger glands in single rows. These glands separate the chyle from the contents of the intestine, and convey it into the space between that canal and the general muscular stratum placed beneath the skin.

The nervous system begins as a large knotted cord surrounding the pharynx, and continued inferiorly as a solid cord accompanying the œsophagus, and so broad as to project beyond the lateral edges of that canal. Two nerves proceed from the cerebral ganglion to the extremity of the mouth. The principal nervous cord is irregularly studded with ganglia, and has an imperfect line of division in the longitudinal direction. It gives off numerous branches to the intestinal canal, to the arteries, and veins, and to the muscles of the integuments and bristles, or *seti.*

The circulating system consists of an artery situated on the upper surface of the alimentary canal, beset with numerous little glandular bodies, and having attached to it numerous minute pulmonary branches, extending to the external integuments. Immediately behind the head, the artery bends downwards in a serpentine form towards the under surface of the alimentary canal, and there terminates in the vein, which extends in the same direction to the extremity of the body. The serpentine curve of the vascular system, from the upper to the lower surface of the alimentary canal, must be viewed as the equivalent of a heart, with which it agrees in having a pulsatory motion.

The artery, by its successive contractions from the posterior towards the anterior part of the body, impels the blood, partly by lateral branches into the vein, and partly into its own pulmonary (respiratory) branches, which again re-act, and return to it their aërated blood. The vein, on the other hand, at each diastole of the artery, sends back a portion of its blood, by means of the lateral branches of communication between the two vessels, in which it is assisted by the successive impulses of the blood driven into it by the heart. Hence, the circulation consists partly in an oscillation of the blood between the artery and vein, by means of lateral anastomoses, and partly in a direct current impelled by the heart from the artery into the vein, and from the latter again into the artery.

The mode of increase in these animals, as is well known, from the observations of Réaumur, Bonnet, and Mueller, is by the spontaneous separation of segments of the body, which are rapidly developed into new and self-existent individuals. The astonishment created by the first idea of such a process, is diminished when we consider the simplicity of the whole structure, and the identity of character of all the parts of its most important systems. Thus, the integuments, the subjacent muscles, the simple alimentary canal, the central nervous cord, the artery, and the vein, have nearly the same form and character in each segment of the body. The heart and brain alone are newly-developed parts in the new-formed animal, and even these may perhaps be considered as essentially pre-existent to the change, and merely advanced by it to a higher degree of activity.

The heart first presents itself before the detachment of the new being from the parent stock, as a dilatation of one of the capillary anastomoses between the arterial and venous trunks. The brain, in the same manner, is nothing more than the development of one of the loops or collars surrounding the alimentary canal of the original animal, and so small on its primary form as almost to elude observation.

Memoirs of the Royal Academy of Sciences of the Institute of France. Vol. ix. Paris, 1830.

THE first part of this volume contains a history of the proceedings of the Institute during the year 1826: the mathematical department, by Baron Fourier, perpetual secretary; the physical department, by Baron Cuvier, perpetual secretary; and a biographical memoir of Baron Ramond, and of MM. Hallé, Corvisart, and Pinel, by the same author.

Memoir on the Equilibrium of Fluids, by M. Poisson.

THE author commences with the following views of the nature of molecules, and their mutual actions. The dimensions of the molecules and the spaces by which they are separated are so small, that a line of imperceptible magnitude may comprise a very large number of them: they exert a mutual attraction, and at the same time repel each other, in consequence of the caloric by which they are surrounded: the sphere of action of their forces is imperceptibly small, but comprises a very large number of adjacent molecules; nor do the forces themselves sustain any material diminution, but at a considerable distance, compared with the space intervening between adjacent molecules: without these considerations, the resultant action of each molecule could not be subject to any law of continuity, and, consequently, could not be expressed in function of the co-ordinates of any point; an hypothesis indispensable in the application of analysis to this subject.

The force which a molecule exerts in any given direction is supposed to consist of two parts; the *principal force*, the value of which is the mean value of the difference between the attraction and repulsion, and a *secondary force*. The sphere of action of the latter is much more limited: on this force the chemical action of bodies may depend, and their power of assuming a definite form, as in the process of crystallization; the former is alone considered in the present memoir.

The author then enters into the difference between the constitution of solids and fluids, and of homogeneous and heterogeneous fluids, and successively deduces the following results, *viz.*—

1. The equations of equilibrium of the interior of any fluid.

2. The value of the pressure on any point in the interior of a fluid.

3. The equations of equilibrium of the common surface of two fluids.

4. The equation of equilibrium of the surface of an incompressible fluid.

In conclusion, the author endeavours to establish the two following propositions:—

1. That the phenomena of capillary attraction are due to molecular action, modified not only by the form of the surfaces, according to the theory of Laplace, but also by a peculiar state of compression existing in the superficial stratum of the fluid.

2. That a sphere is the only figure of equilibrium of a fluid uninfluenced by any external force.

Note on the Roots of Transcendental Equations, by M. Poisson.

In this paper the author shews that a rule given by M. Fourier*, for ascertaining the existence of impossible roots in any equation, is not generally applicable to transcendental equations. He then explains the connexion between the roots of an equation of n dimensions, $X=0$, and the parabolic curves represented by the series of equations

$$y = d^m_x X,$$

in which m has successively all values from $m=0$, to $m=n-1$.

In conclusion, the author corrects an opinion he had previously entertained†, *viz.*, that transcendental equations consisting of ascending powers of x, with increasing numerical denominators, might be assimilated to algebraical equations, by neglecting the terms involving high powers of x, but with very large denominators.

Extract from a paper on the integration of partial differential Equations, by M. A. L. Cauchy.

The author of this paper explains a method by which he considers that some formulæ which he has given in the 19th Number of the *Journal de l'Ecole Polytechnique* may be extended to the integration of linear partial differential equations, with variable coefficients.

Extract from a Memoir on some series analogous to that of Lagrange, on symmetrical functions, and on the direct formation of the equations which result from the elimination of unknown quantities between given algebraical equations; by the same Author.

In this extract a method is given for finding the sum of the m^{th} power of the roots of an equation of n dimensions, and for expressing the products of all the roots in terms of the sums of their different powers.

* Théorie Analytique de la Chaleur, p. 373.
† Mémoires de l'Académie, tom. viii. p. 367.

Memoir on the Equation the roots of which are the principal moments of inertia of a solid body, and on some other Equations of the same kind. By the same Author.

M. Cauchy remarks that it has not hitherto been shewn, that the roots of the above equation are possible, except by indirect methods, such as, for instance, the reduction of the cubic equation to a quadratic, by the transformation of coordinates in space: he then proposes a direct demonstration, depending on some theorems enunciated but not demonstrated in this paper.

Memoir on the Movement of a System of Molecules which attract or repel each other at very small distances, and on the Theory of Light. By the same Author.

The author states, that the equations of motion of such a system of molecules may be integrated by the methods given by himself in the 19th Number of the *Journal de l'Ecole Polytechnique*, and that these integrals have led him to form the following conclusions:

1st. If in any system of molecules the electricity of the system be equal in every direction, and a vibratory motion be produced at any point, two spherical undulations, of constant, but unequal velocities, will be propagated from that point. The first of these vibrations will cease, when the initial dilatation of the volume ceases; if, then, we suppose the vibrations to have been originally parallel to a given plane, they will continue so.

2d. If the system be such that the elasticity continues the same about an axis parallel to a given line, in all directions perpendicular to the axis, the equations of motion will contain several coefficients depending on the nature of the system; and a relation may be established between their coefficients, such that the propagation of a vibration first produced at any point may give rise to three undulations, each of which coincides with a surface of the second degree. Furthermore, omitting that undulation which disappears with the dilatation of the volume, when the elasticity again becomes equal in every direction, the surfaces of the two remaining undulations will be reduced to a system comprising a sphere and a spheroid, of which the axis of revolution coincides with the diameter of the sphere.

The author observes, that the remarkable coincidence of this result with the theorem of Huyghens on the double refraction of light in crystals having a single axis, is deserving of our attention; and we may hence be induced to conclude, that the equations of the motion of light coincide with those which express the movement of a system of particles very little disturbed from the position of equilibrium.

Analytical Demonstration of the Law discovered by M. Savart, relative to the vibrations of Solids and Fluids. By the same Author.

It is here shewn that the equations of motion of an elastic body, the particles of which are very little moved from their natural position, may, by a slight modification, be applied to the demonstration of the above proposition.

The author concludes by observing, that it may be proved in the same manner that if the dimensions of a body increase or diminish in a given ratio, and the initial temperature increase or diminish in the same ratio, the duration of the propagation of heat will vary as the square of that ratio.

Memoir on Torsion and the vibrations of Torsion in a rectangular Rod. By the same Author.

In this paper the author obtains some analytical formulæ, from which he deduces the following results:

i. The angle of torsion of a rectangular rod, fixed at one end and free at the other, when measured in a plane perpendicular to the axis of the rod, is as the distance of that plane from the fixed end, and the moment of the force applied at the free end jointly.

ii. If the transverse section of the rod is variable, but continues similar to itself, the angle of torsion will vary inversely as the square of the area of the section. These results, similar to those obtained by M. Poisson for the torsion of a homogeneous cylindrical rod with a circular base, will hold equally for a circular or prismatic rod on any base.

iii. If one transverse dimension of the rod becomes very small, compared with the other, the angle of torsion will vary inversely as the greater dimension, and the cube of the lesser.

iv. The tones produced by the vibrations of torsion of a rectangular rod are invariable, so long as the breadth and thickness of the rod are in the same ratio. This is confirmed by the experiments of M. Savart.

v. If one transverse dimension of the rod becomes very small, compared with the other, the lowest tone produced by the vibrations of torsion is directly as the least dimension, and inversely as the area of a transverse section. This is another of the laws to which M. Savart has been led by experiment.

vi. If the elasticity of the rod is the same in every direction, the tones corresponding with the vibrations of torsion will be directly as the product of the two transverse dimensions, and inversely as the sum of their squares.

vii. When the two transverse dimensions are equal, the lowest tone produced by longitudinal vibrations will be to the lowest tone produced by the vibrations of torsion :: 1.9364... : 1.

[To be continued.]

FOREIGN AND MISCELLANEOUS INTELLIGENCE.

§ I.—*MECHANICAL SCIENCE.*

1. Stiffness and Strength of Timber.

In a series of experiments undertaken by Lieutenant T. S. Brown, to ascertain the relative stiffness and strength of different kinds of pine used in building, it was found that the ratios of the stiffness of white pine, spruce, and southern pine, were as the numbers 1, 1·111 and 1·807. When the weights producing the flexure were increased, it was found that the failures of the wood began at the top. The upper fibres, for rather less than half the depth of the beam, were gradually crushed and broken off in the bending of the specimen; and at last, when no more weight could be supported, a fracture suddenly took place, the lower fibres being drawn asunder *.

2. Proportion between the Metre and English Yard.

M. Francœur, in an elaborate memoir on the proportion between French and English measure, has found that the mètre is equal to 39.37079 English inches, and the English imperial yard equal to $0^{m}.91438348$—numbers which may be relied on with the utmost confidence †.

3. On the Velocity of an Elastic Fluid which flows from a Reservoir into a Gasometer.

In most books of natural philosophy we find the following proposition stated as if it had been founded on data as certain as the pressure of the atmosphere. 'The flowing of an elastic fluid into a vacuum takes place with the velocity due to the height of a column of fluid of the same density with that contained in the vessel, and whose weight would produce the pressure to which the fluid is submitted.' M. Navier has shewn that this could only be true on the supposition, that the density of the fluid in the tube or orifice through which it flows was the same as that in the reservoir, which, from the diminished pressure, is obviously not the case. The results to which he has arrived will for ever banish this false proposition from books of natural philosophy ‡.

4. On the Discharge of a Jet of Water under Water.—(*R. W. Fox, Esq.*)

Having observed that a communication of mine 'On the Discharge of a jet of water under water,' inserted in No. 47, of the Philosophical Magazine, has been noticed in the last number of the Journal of the Royal Institution, I will take this opportunity of mentioning,

* Silliman's Journal, vol. xix. p. 292.
† Mem. de l'Acad. des Sciences, tome x. p. 50. ‡ Ibid.

that where a jet of water is discharged under mercury, the results are the same, under a given force, as when it takes place in water, or air, the quantity discharged being in all cases the same, in the same time.

Hence, it appears that the force with which a moving or spouting fluid recoils is not affected by the surrounding medium, however rare or dense it may be: and thus we may understand why the attempts, which have been made to propel vessels, by forcing water through them against water, have not proved advantageous.

The well known fact that large rivers penetrate, in a direct course, far into the ocean, notwithstanding its agitation by tides and currents, is somewhat analogous; and were it not for this remarkable degree of mobility in water, the sediment, which is now mostly deposited at a considerable distance in the sea, would accumulate near the mouths of rivers, and tend to divert them from their course.

Whilst making my experiments on the jet of water, I noticed that when sand was dropped into the water near the orifice from which the jet issued, it was drawn laterally toward the hole, till it distinctly appeared to enter it, but it was in fact only an optical deception, the grains of sand being carried away by the jet as soon as they came in contact with it, with such great velocity as to be perfectly invisible.

5. Optical Deception upon the Liverpool and Manchester Rail Road.

This rail road consists of two lines of rails, so that in looking from the carriage window of one line, the other is seen, and presents the following somewhat remarkable appearances. While travelling at the rate of from 12 to 15 miles per hour, the rail, together with the roadway, the banks, and other objects, appear, as they do from the window of a stage-coach, to recede, or move in a direction the reverse of that in which the carriage is moving. But when the speed increases to twenty-four or thirty miles in the hour, the rails no longer seem to recede, but to move *with* the carriage, as if they were running along the road at the same rate as the spectator. These different appearances, accompanying the different speeds, are explained without difficulty; they depend on the facts which are familiar to every one who has caused a fluted pencil case, having a plain slider, to revolve in the hand. The case is obviously seen to move, but the slider seems stationary, because, being plain, it presents at every period of its revolution precisely the same appearance to the eye. If the iron rails appeared *always* to move along the road, the explanation of the phenomenon would already have been given. The varying effect produced by varying speeds depends on the circumstance that the iron rails are not, like the slider of the pencil case, *quite* plain, but have slight irregularities occurring at short intervals, which with a moderate velocity are visible, and give to the rail a receding appearance; but when the velocity becomes doubled, the impression on the retina produced by one irregularity is not effaced till it is succeeded by others, so nearly similar, that the appearance of the

rail resembles that which would be given by one without any irregularities at all. The varieties of form and colour in the road and banks are too great, and separated by too long intervals, to be effaced by any possible speed, and therefore they continue to recede from the carriage after the rail seems to have changed the direction of its motion. A. A.

6. A Barometer of a new Construction.—(*Proposed by M. Kupffer.*)

The great danger of particles of air entering into the vacuum of a barometer while in use, and the difficulty of effectually expelling such air by boiling the mercury, have induced M. Kupffer to propose a construction of this instrument by which the error arising from this source, instead of being removed, may be taken into account. The cistern of the barometer is a hollow iron cylinder; the bottom of which can be raised and lowered at pleasure by means of a screw. Besides the ordinary tube of the barometer, another smaller tube, like the short arm of a syphon, proceeds from, and communicates with the cistern, which is quite full of mercury. The height of the mercury in the two tubes, the difference of which is the height of the barometer, may be changed at pleasure by means of the above screw; and the difference of level is measured by a scale, one extremity of which, a steel point, reaches down into the shorter tube, so that it may be brought into contact with the surface of the mercury in it. By the action of the same screw, the space above the mercury in the long tube, which ought to be a vacuum, may consequently be reduced at pleasure to any degree. Now, if we suppose that this space contains some air, let the height of the barometer, which was observed while the space was e, be $=$ A, and let the pressure of the air in the tube at the same moment be $= p$. After reducing the space to the smaller quantity e', let the height of the barometrical column be $=$ B, and the corresponding pressure of the air $= p'$; then will be, by the law of Mariotte, $\frac{p}{p'} = \frac{e'}{e}$ and the corrected height of the barometer $= A + p = B + p'$; whence we find the correction to be applied to A, *viz.* $p = \frac{A - B}{\frac{e}{e'} - 1}$. If $e = 2e'$, or if the space at the second observation is exactly half of what it was at the first observation, we shall have $p = A - B$. Thus the difference of the observed heights will, in that case, be exactly the correction to be applied to the first height for the error arising from the action of the air *.

7. Occultation.

M. Tarkhanoff, the astronomer of an imperial Russian expedition for

* Petersburgh Transactions, 1830.

finding a north-west passage, observed on the 7th May, 1822, the following occultation of Antares by the moon:—

Immersion of the star at the bright limb of the moon $8^h\ 52'\ 34''.93$ mean time; emersion at the dark limb at $10^h\ 5'\ 27''.8$ mean time. Both phenomena were well observed, and the time determined on the preceding and the following days by equal altitudes. The latitude of the place of observation (the isle of Serpents at the North-East Cape) was found by a Dollond reflecting circle $= 22.53'\ 54''.15$. Not having met with corresponding observations in other places, he has calculated the observations by Burckhardt's tables, and finds by these tables, and a compression $= \frac{1}{308.65}$, the longitude of Rio Janeiro by the immersion $= 3^h\ 2'\ 20''.97$, emersion $= 3^h\ 2'\ 18''.51$ west of Paris*.

The mean of the two will therefore give the longitude of Rio Janeiro $= 2^h\ 52'\ 58''.0$ west of Greenwich.

8. Pendulum Observations.

Captain Luetke has communicated to M. Fuss, the Secretary of the Imperial Academy of St. Petersburgh, the following pendulum observations, made during the circumnavigation of the Russian vessel Seniavine. The invariable pendulum employed in this expedition was the same which had before been used by Captain Basil Hall in South America. A series of pendulum experiments had been made with it at the Royal Observatory at Greenwich before leaving England, and the same were repeated at the same place after the return from the expedition. The latter gave $\frac{6}{10}$ of an oscillation more, which Captain Luetke attributes to the knife-edge having been a little blunted. Instead of distributing this difference in arithmetical proportion over the whole interval elapsed, it has been preferred to adopt the mean of the two results. The two sets of observations made at St. Petersburgh for ascertaining the effect of the temperature upon the pendulum, one during a mean temperature of $31°\frac{1}{2}$ Fahr., the other in a temperature of $82°\frac{1}{2}$ Fahr., proved that each degree of the thermometer causes, in 24 hours, a difference of 0.458 oscillation. This exceeds the quantity resulting from Captain Sabine's experiments by 0.033, although the instruments were made of the same bell-metal, and are of the same dimensions. Captain Luetke cannot ascribe this difference to any other cause but a less density of the metal used for his pendulum. In consideration of the various circumstances attending the observations, Captain Luetke is inclined to think that the observations at Greenwich, St. Petersburgh, Petropawlofsk (St. Peter's and St. Paul's), Valparaiso and the Bonin islands, are the best, and will not deviate more than $\frac{1}{10}$ oscillation from the truth; while at Sitka and the island of Ualan the uncertainty may amount to $\frac{1}{4}$ oscillation. The observations least to be depended on are those at the Guahan islands, and at St. Helena, where he cannot

* Petersburgh Trans. 1830.

answer for an error of less than $\frac{1}{2}$ oscillation. The latitudes were determined by numerous sets of circummeridian altitudes of the sun and stars, observed with a sextant of Troughton, a reflecting circle by the same artist, and a reflecting repeating circle by Dollond. The following table contains the number of oscillations of the pendulum at each station in 24 mean solar hours, reduced to the standard temperature of 62° Fahr., to a vacuum, and to the level of the sea. The fourth column contains the corresponding lengths of the seconds' pendulum founded on Captain Kater's measurement of the same at London. With this view the observations at Greenwich were reduced to the station in Portland Place by the difference of oscillations of the two places, as determined by Captain Sabine.

Stations.	Latitudes.	Numbers of Oscillations.	Lengths of Seconds' Pendulum. Inches.
Ualan	5° 21′ 16″ N	86112.83	39.02756
Guahan	13 26 21 N	117.98	03242
St. Helena	15 54 59 S	125.63	03933
Bonin	27 4 12 N	159.24	06980
Valparaiso	33 2 30 S	165.33	07533
London	51 31 8 N	235.80	13929
St Peter's and St. Paul's	53 0 53 N	245.83	14838
Sitka	57 2 58 N	257.44	15810
St. Petersburgh . . .	59 56 31 N	269.08	16950

In order to find the mean value of the ellipticity resulting from all these observations, it is most convenient to combine them by the method of minimum squares. Designating the length of the pendulum at the equator by x, its difference from that at the pole by y, the errors of each partial result by E′, E″, E‴, and assuming the hypothesis of an elliptical figure of the earth, for which we have the length of the pendulum in latitude $L = x + y \sin^2 L$, we shall obtain the following equations of condition :—

Ualan	$39.02765 - x - 0.0087080 \, . \, y = E^{i}$
Guahan	$39.03243 - x - 0.0540157 \, . \, y = E^{ii}$
St. Helena	$39.03933 - x - 0.0752045 \, . \, y = E^{iii}$
Bonin	$39.06980 - x - 0.2070967 \, . \, y = E^{iv}$
Valparaiso	$39.07533 - x - 0.2972962 \, . \, y = E^{v}$
London	$39.13929 - x - 0.6127966 \, . \, y = E^{vi}$
St. Peter's and St. Paul's	$39.14838 - x - 0.6380657 \, . \, y = E^{vii}$
Sitka	$39.15810 - x - 0.7041567 \, . \, y = E^{viii}$
St. Petersburgh . . .	$39.16950 - x - 0.7491220 \, . \, y = E^{ix}$

The following values will then be found by the method of minimum squares :—

$$x = 39.02422 \, ; \; y = 0.091787 \, ; \; \text{ellipticity} = \frac{1}{267.7}.$$

The differences between the observed and calculated lengths of the pendulum and numbers of oscillations will then stand as follows :—

Stations.	Excess of observed length over the calculated length.	Excess of observed oscillations over the calculated number.
Ualan	+ 0.00176	+ 1.94
Guahan	— 0.00216	— 2.38
St. Helena	+ 0.00069	+ 0.76
Bonin	+ 0.00586	+ 6.46
London	− 0.00246	— 2.71
Valparaiso	— 0.00591	— 6.52
St. Peter's and St. Paul's	+ 0.00179	+ 1.97
Sitka	— 0.00117	— 1.29
St. Petersburgh	+ 0.00161	+ 1.77

With the exclusion of Valparaiso, which has a high southern altitude, and the Bonin islands, the following result, which may be assumed as correctly representing the observations in the northern hemisphere, will be obtained,—

$$x = 39.023923;\ y = 0.192535;\ \text{ellipticity} = \frac{1}{269}.$$

The errors will then be as follows:—

Stations.	Excess of observed over calculated length.
Ualan	+ 0.00205
Guahan	— 0.00200
St. Helena	+ 0.00093
London	— 0.00262
St. Peter's and St. Paul's	+ 0.00161
Sitka	− 0 00140
St. Petersburgh	+ 0.00134

The ellipticity here found nearly agrees with the mean results of Captains Freycinet and Duperrey, but is greater than that determined by Captain Sabine.

The detailed description of the observations which Captain Luetke intends to publish will contain the results obtained from a combination of these experiments with those of other travellers *.

9. Dip of the Magnetic Needle at St. Petersburgh.

Observed by M. Hansteen in June, 1828	71° 17′.3
By M. de Humboldt, (applying an instrumental correction) in May, 1829	71° 14′.5
Ditto, in December, 1829	17° 11′.5
By M. Kupffer in May, 1830	71° 11′.3

It would appear from these observations, that the annual decrease of the dip at St. Petersburgh is about 3′ †.

10. On the Direction and Intensity of the Magnetic Force at St. Petersburgh.—(*M. Erman.*)

i. *Variation of the Needle.*—The variation of the needle was determined after the method first proposed by Professor Bessel, of Königsberg, in Schumacher's Astron. Nachr., by means of a transit, the telescope of which is exchanged for a compass, furnished with

* Petersburgh Trans. 1830. † Ibid.

an horizontal cylindrical axis, exactly similar to the axis on which the telescope of the transit revolves. The instrument is first so placed that the telescope describes a vertical line not much differing from the meridian. Transits of stars north and south of the zenith are then observed in the two contrary positions of the axis. The time of the place of observation, and the azimuth of the axis of revolution, will thus be obtained. The compass is next substituted for the telescope, and the difference between the magnetic and astronomical azimuths immediately determined, the compass being furnished with the necessary means for ensuring the parallelism of the diameter of the compass passing through the zero of the divisions, and the line passing through the middle of the cylindrical axis of the compass resting on the Ys of the transit. M. Erman estimates the error which may arise from the determination of the time at 5″, and that from the mean of all the readings of the compass needle at 30″, so that the whole uncertainty of the determination would not exceed 30″ or 40″. There were considerable differences between the observed variations, according to the time of the day in which the observations were made. M. Erman took, therefore, sets of hourly observations, embracing altogether nearly two full days. The observations could not be brought to perfect regularity; but he found in general that the maximum of variation is at about 2h. P. M., and exceeds the mean variation by 10′, whereas the minimum, which takes place between six and eight o'clock A. M., falls short of it by about 8′. It is remarkable that the time of the maximum thus found at St. Petersburgh agrees with that observed in the more western parts of Europe, but that the minimum which, in western Europe, is found to take place at 2^h. A. M., should be found by M. Erman's observations to be six hours later at St. Petersburgh. The mean variation at St. Petersburgh, in the beginning of June, 1828, may be taken at 6° 47′ 20″, which is not likely to deviate more than a minute from the truth.

ii. *Dip of the Needle.*—The dipping compass which has been employed was made by M. Gambey for Professor Hansteen's expedition. The vertical circle is ten inches in diameter, and is divided to ten minutes. The instrument is furnished with two perfectly similar needles, with cylindrical axes that terminate in steel points of about two-tenths of a line in diameter. With these thin ends, the needles rest on convex pieces of agate of a high polish. The artist has endeavoured to construct the needles in such a manner that there should be but a slight difference between the centres of gravity and of figure. This construction allows to dispense with the employment of counterpoises, as invented by Meier, which, in needles of less exact equilibrium, serve to magnify the errors arising from the position of the centre of gravity, in order to be eliminated by calculation. It is easily proved that, if the centre of gravity differs little from the centre of the figure, the formula of Meier is reduced to the taking of the arithmetical mean between the readings before and after the inversion of the poles of the needle in two con-

trary positions of the vertical circle. This method was, accordingly, adopted, and the following observations were made.

	In the town, May 14, 1828. Dip.	In the botanic garden, June 3, 1828. Dip.
The vertical circle having its face East	71° 51′.75	70° 55′.75
	53.00	54.75
West	70° 11.0	71 52.00
	15.5	71 54.25
After the inversion of the poles		
The vertical circle having its face East	72° 23′.00	70° 7′.0
	19.00	6.0
West . . .	70 4.50	72 11.5
	70 4.75	15.0
Results . . .	71° 7′ 49″	71° 17′ 2″
Mean between the two .	71° 12′ 25″	

iii. *Intensity of the Magnetic Force.*—Messrs. Humboldt and Hansteen having ascertained the intensity of the magnetic force at Berlin, that which obtains on the magnetic equator being, as usual, considered as unity, nothing was required but to compare the magnetic forces at Berlin and at St. Petersburgh by the same instruments, and by the same methods. For this purpose, M. Erman employed two needles horizontally suspended. The first needle, which shall be called A, is a small cylinder of the best tempered steel that could be procured, in figure and dimensions exactly like the cylinders used by M. Hansteen for determining the same intensity in western Europe. This needle had been magnetized to saturation about a year before, and had not been retouched since, as it was to be presumed that the magnetic state that had been induced after the lapse of a year would be longer preserved than any which might be produced by retouching the needle. The second needle (B) is a bar of six inches in length, which was tempered and magnetized by Coulomb. This is the same which has been used by M. Humboldt for comparing the intensities in Europe with that on the magnetic equator, and it is well known that this needle hardly changed its intensity at all during the whole course of his travels. The times of the oscillations were observed by a chronometer beating five times in two seconds. The time was noted for every ten oscillations, and a hundred of them were counted at each time. The result has been deduced by the method of minimum squares, and by a reduction of the observed times of the oscillations to that of an infinitely small oscillation, by means of the observed elongations of the first and the hundredth oscillations.

Duration of an Oscillation.

	At Berlin.	At St. Petersburgh.
Of needle A . .	3″.0990	3″.2086
Of needle B . .	4″.6161	4″.7852

The intensity of that part of the magnetic force which acts in a horizontal direction is therefore, at St. Petersburgh,

$$\text{By needle A} = \frac{(3.09\ \ 90}{(3\ .208} = 0.933 \qquad \text{By needle B} = \frac{(4.6161)^2}{(4.7852)^2} = 0.931$$

(the same at Berlin being assumed as unity.)

In order to deduce from these results the ratio of the entire force at the two places, the dip at Berlin was taken at 68° 39′ 30″, as determined in the same manner, and by the same instrument with which the preceding result for St. Petersburgh was obtained. With these data, and using the mean of the results deduced from the two needles, the following expression is found for the intensity of the magnetic force at St. Petersburgh; that of Berlin being considered as unity:

$$0.932.\ \frac{\text{secant}\,(71^\circ\ 12'\ 25'')}{\text{secant}\,(68^\circ\ 39'\ 30'')} = 1.0526\ *.$$

11. Variation of the Needle.

(*From a Letter of Professor Hansteen to M. Kupffer.*)

Orenburg, Jan. 1, 1830.

The magnetical observations in the eastern parts of Siberia prove that there is a considerable western variation in places eastward of the line of no variation which passes near Irkutsk. Professor Hansteen has likewise discovered the line of no variation which had already been established by the observations of M. Schubert. The observations of Captain Wrangel had led M. Kupffer to believe that the variations on both sides of this line of no variation had equal signs; but the very accurate observations of M. Hansteen now prove, that they change sign from one side to the other of that line; and that, consequently, this line of no variation which traverses Siberia has the same property as those which pass near Kasan, and through the United States of America †.

12. On the Figure of the Magnetic Equator.

The following are deductions from observations made during the voyages in the *Coquille*, by L. J. Duperrey, commander of the expedition.

It had been previously known that the magnetic equator, or that line surrounding the globe where the needle arranges itself in a horizontal position, was not a regular great circle of the sphere, but an irregular line cutting the equator in two points. From the observations made by M. Duperrey, combined with those of Captain Sabine and others, its true figure may now be considered as ascertained with a considerable degree of accuracy. M. Duperrey crossed the magnetic equator six times; but it was not so much from observations made on that line, as from those made about thirty degrees

* Petersburgh Transactions, 1830. † Idem, 1831.

on each side of it, that its true figure has been deduced. If we could determine the inclination of the dipping needle at any place, then we could, by a trigonometrical formula, ascertain the latitude of that point where the dip would be nothing; or, in other words, the corresponding point in the magnetic equator. The formula which we employ for the purpose is the following:—The tangent of the magnetic latitude is equal to half the tangent of the inclination of the dipping needle.

From these principles it is found that the node of the magentic equator, or that point where it crosses the equator, is near the island of St. Thomas, about 3° 20′ to the east of the meridian of Paris. It then advances rapidly towards the north-east, across the continent of Africa, and the Red Sea at the Straits of Babelmandel. It then stretches almost parallel to the equator for a short distance, and gradually declines, passing through the south of Hindostan, and touching the northern extremity of the island of Ceylon. It then stretches in an irregular line through Malacca, the northern point of the island of Borneo, stretching onwards to the south of the Carolinas, and again crossing the equator at about 175 degrees east of Paris. It then passes on, making a small angle with the equator, till it reaches about 100 degrees west from Paris, when it begins to deviate rapidly from the equator, and sweeps through South America, at its greatest distance from the equator, being about sixteen degrees. It then rises, in a very irregular line, through the Atlantic Ocean, till it reaches the island of St. Thomas.

By tracing this line on a map it will be seen, that the two points at which it crosses the equator are almost diametrically opposite; that in the ocean it declines very little from the equator, but where it approaches islands it feel their influence, and its deviation increases; and when it reaches the massy continents of Africa and America, their influence seems to be powerfully felt, and its deviation from the equator becomes greatest. If the magnetic equator be the resultant of electric currents, circulating perhaps at no very great depth below the surface of the earth, then it is obvious that such a country as South America, abounding with metalliferous veins, ought to have a decided influence on the needle, and here we observe that its deviation is the greatest*.

13. New Dipping Needle.

In dipping needles formed for ascertaining the dip in different latitudes, the axis must be made cylindrical. But in the one made by M. Gambey, at Paris, to be used at St. Petersburgh, the axis is a knife-edge, as in a fine hydrostatic balance. The edge is placed exactly in the centre of gravity of the whole compound needle, and so fixed, that when the needle dips 71° the edge rests perpendicularly on two agate plates. It can, therefore, only be employed at those places where the dip is but a little more or less than 71°, and is intended to ascertain minute variations of the inclination at the same place †.

* Annales de Chimie et de Physique. † Idem.

14. Powerful Electro-Magnets.

Professor Henry, of the Albany Academy, and Dr. Ten Eyck, have extended the ingenious experiments of Mr. Sturgeon, by adopting the principle of Professor Schweiger's galvanometer, and produced magnetic effects on soft iron, which we could scarcely have expected from the feeble voltaic current which they employed. The circumstances on which the increase of electro-magnetic power depends, are, first—an increase of the mass of soft iron; and secondly—an increase in the number of coils without increasing the length of the wire.

A cylindrical bar of soft iron, ten inches long and half an inch diameter, was bent into the form of a horseshoe, and wound with thirty feet of copper wire, covered with silk thread. With a pair of plates two inches and a half square, dipped into dilute acid, the soft iron became a magnet capable of raising fourteen pounds. A wire of the same length as the first was wound over it, and the ends soldered to the copper and zinc plates: the effect was now doubled, and the temporary magnet actually supported twenty-eight pounds. With plates of four inches by six, it supported more than fifty times its own weight.

But the greatest effect which has ever been produced on soft iron by voltaic electricity was, by using a bar of iron two inches square and twenty inches long, having the edges rounded and being bent into the form of a horseshoe magnet. Around the horseshoe 540 feet of copper bell-wire was wound in nine coils of sixty feet each. These coils were not continued from one end of the magnet to the other; but each of them was wound round a space of the horseshoe about one inch long, leaving the ends of the lines projecting and properly numbered. By soldering the alternate ends to a copper cylinder, and the others to a smaller cylinder of zinc, containing only two-fifths of a square foot, and placing the one within the other in dilute acid, the following extraordinary effect was produced. When the armature of soft iron was placed across the ends of the horseshoe, and weights added till the temporary magnet could support no more, it was found that the total weight amounted to 650 pounds, an astonishing effect for such a small battery, and requiring only half a pint of dilute acid for its submersion. With a larger battery the weight raised was 750 pounds, which seemed to be the maximum of magnetic power which could be developed in that bar by voltaic electricity. This appears to be the strongest magnet ever constructed, either by the ordinary modes of magnetizing steel bars, or by the voltaic current. Mr. Peal's magnet weighs fifty-three pounds, and lifts 310 pounds, or about six times its own weight; whereas this temporary one weighs only twenty-one pounds, and raises more than thirty-five times its own weight.

When the ends of the wires were united so as to form a continuous wire of 540 feet, the weight raised was only 145 pounds.

When a battery containing twenty-five double plates, and presenting the same surface with the cylindrical battery, was employed,

it was found that a greater electro-magnetic effect was produced with a long copper wire than with a short one, for when the ends of the battery were connected with the coil surrounding a small horseshoe magnet, it raised only seven ounces; but when one-fifth of a mile of copper-wire was interposed, it raised eight ounces. The author gives his opinion of the cause of this remarkable effect in the form of a query. 'On a little consideration, however, the above result does not appear so extraordinary as at first sight, since a current from a trough possesses more projectile force, to use Professor Hare's expression, and approximates somewhat in intensity to the electricity from the common machine. May it not also be a fact, that the galvanic fluid, in order to produce the greatest magnetic effect, should move with a small velocity, and that, in passing through one-fifth of a mile, its velocity is so retarded as to produce a greater magnetic action?'

Dr. Ten Eyck varied these experiments, so as to get a small temporary magnet which should raise the greatest number of times its own weight. With a small horseshoe of round iron slightly flattened, one inch in length, and $\frac{6}{10}$ inch in diameter, and wound with three feet of brass wire, it raised, by means of the cylindrical battery, 420 times its own weight. The author remarks, 'the strongest magnet we can find described, is one worn by Sir Isaac Newton in a ring, weighing three grains; it is said to have raised 746 grains, or 250 times its own weight.' Hence it is evident that a much greater degree of magnetism can be developed in soft iron by a galvanic current, than in steel, by the ordinary method of touching *.

15. On the Intensity of the Earth's Magnetism.—(*Kupffer.*)

i. During a scientific journey in the neighbourhood of Mount Elbrouz, undertaken by the Academy of Sciences at St. Petersburgh, M. Kupffer and M. Lenz made a series of important magnetical observations, of which we shall give a short abstract.

Observations had frequently been made at different elevations above the surface of the sea, and nearly at the same place, on the intensity of the earth's magnetism; but, contrary to what might be expected, no diminution of intensity had been observed in rising above the level of the sea. MM. Gay-Lussac and Biot, during their aërial ascent, had made the same needle oscillate at the surface of the earth, and at the height of 6000 mètres, without observing any diminution in the time of an oscillation. Hence it was concluded that no sensible diminution of the intensity of the earth's magnetism had taken place at that elevation. In this conclusion, an element, which has considerable effect on the magnetism of the needle itself, namely, temperature, was entirely overlooked. The effect of temperature on the magnetism of needles has been carefully observed by M. Kupffer, and entered as an element in the data from which the intensity of the earth's magnetism was deduced.

* Silliman's Journal. See our account of Professor Moll's experiments, at page 379 of this volume.

When a magnetic needle is heated, the coercitive force of the steel is diminished, and the strength of the magnet considerably impaired. Besides, the magnetism of the earth acts by induction on a needle, and, when the temper is not very hard, is a quantity which increases or diminishes the magnetism of the needle, according to the position in which it is placed.

It is also necessary to pay attention to the hour of the day at which the observations are made. At eleven o'clock A. M., the earth's intensity is greatest; at five o'clock P. M., its intensity is least. Another element, which M. Kupffer has made to enter into the calculation, is the difference of intensity depending on the difference of latitude and longitude of the places of observation.

The observations were made in the valley of Malka and on Kharbis, at an elevation of 4500 feet. After making the proper corrections due to the causes which we have mentioned, it was found that the duration in one oscillation of an excellent needle, made by Gambey, was .063 of a second, which, for every 1000 feet of perpendicular ascent (supposing the diminution uniform), is .014 of a second. In comparing these observations with those of MM. Gay-Lussac and Biot, M. Kupffer has arrived at the following remarkable law :—' That the increase of intensity which a needle, of the usual degree of hardness, acquires by a diminution of temperature, according to the height, is almost entirely compensated by the diminution of the intensity in the magnetic forces of the earth, due to that elevation.' Since the magnetic intensity of the earth is thus found to diminish at a small height above the surface, it appears obvious the cause must reside nearer the surface than was formerly supposed, and affords an additional proof that the declination and dip of the needle depend on electric currents circulating about the earth at no very considerable depth.

ii. It had been long known that, in our hemisphere, the north pole of a magnetic needle marches towards the west from eight o'clock in the morning till two o'clock in the afternoon, and returns towards the east during the rest of the day; but its oscillations during the night are so irregular, that it has been impossible to ascertain whether or not there be a corresponding period during the night. These irregularities have been carefully attended to by M. Arago, and he has generally found, that during those periods when the oscillations of the needle were very irregular, the aurora borealis has been visible in the north. Hence it has, I think, been too hastily concluded, that the aurora borealis has a decided effect on the needle. It appears more probable that the aurora borealis, and the disturbing force on the needle, are both effects of the same cause—the unknown cause of all terrestrial magnetic phenomena. M. Kupffer has observed at Kasan, that the irregular march of the needle took place on the same day, and almost at the same instant, with those observed at Paris. Could a trifling electrical atmospheric phenomenon in the north of Scotland produce the same effect on a needle in Paris, almost directly south of it, and on

one in a place so remote as Kasan? From the position of these two places, it is obvious that these irregularities cannot in any way be connected with the diurnal motion of the sun, as the hourly variations obviously are. What is most remarkable in the *irregular* variations is, that the motion of the needle is much more frequently towards the east than in the contrary direction.

From a series of observations on the irregular variations at different places, M. Kupffer has discovered that there is a regular period during the night in which the needle has a uniform movement as it has during the day. He finds that at St. Petersburgh the needle marches towards the west from nine o'clock in the evening till almost one o'clock in the morning; that the arc travelled over by the needle is three minutes, whereas the diurnal arc travelled over from nine in the morning till two in the afternoon is nearly fifteen minutes at St. Petersburgh, and twelve at Kasan. The observations have been chiefly made at the same moment at St. Petersburgh, Nicolaïeff, and Kasan; and such is the coincidence, that choose any hour in a whole table of observations, at which the needle has suffered irregular oscillations at St. Petersburgh, and it will be found to have suffered similar perturbations at Nicolaïeff. In a long series of observations, only two exceptions have been found to this general rule. The author observes, the sign of the diurnal variation changes in crossing the irregular line called the magnetic equator, and gives a simple rule for remembering the direction in which the daily variation takes place. 'If we stand at the southern extremity of the needle, in the northern magnetic hemisphere, with the face to the needle, its north pole turns towards the left from eight o'clock in the morning till two in the afternoon; the same thing takes place from eight o'clock in the evening till two in the morning; but from two o'clock till eight, whether in the morning or evening, it marches towards the right. Over the whole of the southern magnetic hemisphere, the periods are the same, but the deviations are in the opposite directions.' The author remarks, that the extent of the hourly variations depends on the length of the projection of the needle on the plane of the magnetic equator; that in all probability it depends on the diurnal revolution of the magnetic equator, and is probably a phenomenon closely allied to those of revolving discs of metal discovered by M. Arago*.

II.—*CHEMICAL SCIENCE.*

1. Matteuci on the Origin of the Action of the Voltaic Pile.

A highly important discussion is at present in progress, relative to the original source of electricity in the voltaic pile, not originating with, but to a considerable extent renewed by, the endeavours of M. A. de la Rive, to prove that chemical action is the sole cause;

* Voyage dans les Environs du Mont Elbrouz dans le Caucase. Par M. Kupffer, Membre de l'Académie des Sciences de St. Pétersbourg.

contact of dissimilar metals having no effect. This has been vigorously controverted by MM. Pfaff, Marianini, &c.: but we wish here merely to quote the experiment of M. Matteuci, who has joined in the investigation.

He was convinced by Pfaff's experiments, that electricity could be developed by contact only; but to be more convinced, he made certain experiments on frogs. First, he assured himself that there was no chemical action between distilled water perfectly free from air, and zinc, either alone or in contact with copper. After contact of many hours, the most sensible tests could not shew the presence of the oxide of zinc or copper. It would, therefore, be very unjust (in the present question) to assume that there is chemical action *because* there is development of electricity. Being sure upon this point, a prepared frog was then suspended from a rod of zinc, which was fixed at the bottom of a gas jar, and connected with a long copper wire, so that nothing more was required to produce the well known contraction than to touch the muscles of the legs with the copper wire.

To remove every suspicion of chemical action, the frog was washed in distilled water, freed from air, so as to remove all animal fluid. It was then suspended by the nerves from the zinc, the jar filled with distilled water, and then with pure hydrogen: but on touching the limbs with the copper wire, the same contractions took place as in common air. The experiment was repeated in vacuo, carbonic oxide, carbonic acid, and oxygen, sometimes dry and sometimes damp, but always with the same result.

M. Matteuci, therefore, remains convinced that the mere contact of different metals is able to develope electricity; although he admits with most philosophers, that chemical action exerts an influence over this force just as heat does in thermo-electric experiments.*

2. Conducting Powers of Liquified Gases.—(*K. T. Kemp.*)

By making liquified sulphurous acid gas a part of the circuit in a galvanic battery of 250 pairs of plates, shocks were received, water was decomposed, and the galvanometer was acted on as if a continuous metallic communication had existed. Liquid sulphurous acid is therefore an excellent conductor of electricity. Cyanogen, on the contrary, was found to be a perfect non-conductor, even to a voltaic current from 300 pairs of plates. Liquified chlorine was also found to be a perfect non-conductor of electricity from a battery of 250 pairs of plates. The author then tried liquified ammoniacal gas, but could not ascertain whether it was a conductor or non-conductor of electricity. It is, in all probability, a non-conductor †.

3. Generation of Steam by heated Metal.

In boilers of high pressure engines, the heat applied to a part not

* Ann. de Chimie, xlv. 106. † Edin. Journal of Nat. and Geog. Science.

containing water, sometimes raises that part of the boiler to a dull red heat, which suddenly coming in contact with a portion of the contained water, generates steam with such rapidity as to burst the boiler. In order to ascertain the effects of different metals, raised to different temperatures, in generating steam from boiling water, a series of experiments have been undertaken by Mr. Johnson, of Philadelphia, which contain the following results. He found by immersing iron raised to different temperatures in boiling water, that more steam was generated in a given time by iron of a red heat, just visible in day light, than by the same piece of iron raised to a white heat. This may arise from the greater quantity of steam forming an atmosphere around the white hot iron, and thus preventing the water coming in contact with the iron. The steam generated bears a direct relation to the weight of the metal, being about one pound of steam for every nine pounds of iron. In comparing cast iron with malleable, he found that cast iron raised to the same temperature generates more than wrought iron, being about one pound of steam for every eight pounds and a quarter of iron*.

4. On the Preparation of Iodic Acid.—(*Serullas.*)

In the course of experimental investigations, M. Serullas had made out that iodic acid was insoluble in alcohol, and that the perchloride of iodine acted upon, and was decomposed by, water. Upon these two points he has founded a process for preparing pure iodic acid in an economical manner. For this purpose the perchloride of iodine, saturated to the utmost with chlorine, and in the solid state, must be prepared, and a small quantity of water, or, what is better, a solution of the perchloride put into the flask with it; some fragments of glass, for the purpose of detaching the solid chloride from the vessel, are also to be added, and then this mixture of glass, fluid and solid chloride of iodine is to be transferred by a funnel into a stoppered 8 or 10-ounce phial, in which it may afterwards be strongly agitated—the funnel retains the fragments of glass, which may be washed with a little saturated solution of perchloride. The bottle is then to be violently shaken to reduce the solid chloride to powder, that all parts may come in contact with the liquid, and that it may be freed from subchloride as much as possible. The whole is then to be poured into a capsule, as much liquid as possible decanted, and then small quantities of ether, or strong alcohol, added, agitating the whole with a glass rod. Almost immediately the solid part becomes white, and the liquid yellow: it is then to be decanted, and the washing repeated, until the liquid comes off colourless. In this way the acid is obtained as a white crystalline powder, perfectly pure, which, being dried and pressed under the finger, feels like very fine sand; or it may be dissolved, filtered, and crystallized by mixture with sulphuric acid, as formerly described †.

* Silliman's Journal, vol. xix. p. 292. † Quarterly Journal of Science, xxix. 411.

It is important to wash away all the subchloride, which may be done by using colourless saturated solutions of the perchloride. It appears probable, from the change of colour, that the first contact of water converts nearly the whole of the perchloride into iodic acid*.

5. On the Precipitation of the Vegeto-Alkalies by Iodic Acid.—(*Serullas.*)

On a former occasion M. Serullas shewed that solution of iodic acid, added to solutions of the vegeto-alkaline salts, produced an abundant precipitate of acid iodate. In this way the smallest quantity of a vegeto-alkali may be shown by this acid, or by a solution of perchloride of iodine, because of the iodic acid there present. The sensibility is so great that the acid may be considered in this respect as one of the most delicate tests which chemists possess: the hundredth part of a grain of quinia or cinchonia, dissolved in several thousand times its weight of alcohol, may in this way be precipitated, and the precipitate quickly collected. The iodic acid should be so dilute as not to be precipitated itself by the alcohol; that is always the case when solution of the perchloride of iodine is used. All the vegeto-alkalies are not equally evident to the test, but the least sensible is discovered when one-fifth of a grain is present.

Great excess of the acid is required in these experiments; it should, therefore, be added by drops. From this circumstance, the alkalies are no test of iodic acid. M. Serullas hopes to found on these effects a process for trying the strength of Peruvian bark.

The iodine with which the acid is formed ought to be quite pure, and the alcohol also, with which the iodic acid is washed, ought to be purified; the latter by mixture with a few drops of sulphuric acid, and distillation to remove lime; the former by using iodine precipitated from its alcoholic solution by water.

The precipitates formed by iodic acid in alcoholic solutions of the vegeto-alkalies, when dry, are decomposed, with explosion, by temperatures of 240° to 248° F.; even when heated upon paper, upon using a tube, the detonation is of considerable force. The results are easily understood; the effect forms a character of the compounds †.

6. On the Action of Bromic and Chloric Acids on Alcohol.—(*Serullas.*)

In consequence of some suspicions relative to a process for the preparation of bromic acid, in which alcohol was used, M. Serullas mixed about equal quantities of solution of that acid and strong alcohol; the liquids immediately became coloured, much heat was evolved, the fluids boiled, and very powerful vapours of acetic ether

* Ann. de Chimie, xlv. 68. † Ibid.

escaped. The acid acted nearly as powerfully as nitric acid under similar circumstances.

Concentrated chloric acid, poured into strong alcohol, acted powerfully; producing ebullition, and vapours of acetic acid. When the quantity of alcohol was small, the whole was converted into strong acetic acid; and when there was still less alcohol, inflammation occurred.

Dried folded filtering paper dipped into chloric acid takes fire, and exhales the odour of nitric acid.

In the acetification of alcohol by these hydrogenative agents, no carbonic acid is produced.

The bromic and chloric acids had been prepared by the process of Mr. Wheeler, namely, adding silicated hydrofluoric acid in excess to the heated bromate or chlorate of potassa, continuing the heat till the excess was driven off, cooling and filtering the liquid, evaporating with the usual precaution, and refiltering the concentrated liquid through glass *.

7. On Perchloric Acid and its Fossil Formation.—(*Serullas.*)

The chloric acid just described, differed a little from that described by other chemists; but, upon heating it, it was found to become the same. A singular result of the action of heat, applied to a greater extent, was observed in the change of one part only into chlorine and oxygen; the other part, nearly one-third, became *perchloric acid*, probably often considered by chemists as chloric acid. After distilling chloric acid for some time, therefore, the aqueous products may be rejected, but there remains in the retort, adhering to its sides, a dense colourless liquid, which, by raising the heat sufficiently at last upon all the surface of the retort, may be driven over into a clean receiver.

This is perchloric acid, and, though very concentrated, it will not inflame paper like chloric acid, but enables it to burn with small sharp detonations. It may be distilled at high temperature without further change. At first it has a reddish colour, from a little manganese, perhaps; but that passes off on a second distillation. It is not altered when heated with muriatic acid or alcohol; the process is one which will give perchloric acid far more readily than that of the discoverer Stadion.

A perchlorate of potash was made with this acid and the alkali. This being decomposed carefully by heat, gave 46 of oxygen per cent. This result corresponds with the composition of the acid already given, *i. e.* 1 proportion of chlorine and 7 of oxygen †.

* Ann. de Chim. xlv. p. 204. † Ibid., p. 270.

8. On the Spontaneous Inflammation of Pulverized Charcoal.—(*Aubert.*)

Spontaneous inflammation of pulverized charcoal occurred in 1802, at the powder-works of Essonne; in 1824 at those of Bouchet; in 1825 at those of Esquerdes; and in 1828 at those of Metz. The latter gave occasion to an investigation, of which the following is an account.

Pulverized charcoal had to be prepared for the Polytechnic School; the operation was commenced on the 31st of March, 1828; the charcoal was made from dry bourdine (peach?), by distillation, 25 per cent. of charcoal being obtained. Twenty-four hours after, the charcoal was weighed, and then triturated for five hours with bronze balls from 7 to 10 millimeters (0.275–0.394 of an inch) in diameter, in leather tuns 1 meter (39.37 inches) in diameter, and 1.15 meters (45.27 inches) in height, making 30 revolutions in a minute. Each vessel contained about 10 kil. (22 lbs.) of charcoal, and balls to the amount of 35 kil. (77 lbs.); the trituration was continued three hours. The triturated charcoal was spread out during a new trituration, and ultimately put into casks. During six years that charcoal with sulphur has been thus triturated, no accident of the kind has occurred, and the temperature of the tuns never rose more than 45° or 54° F. above that of the workshops. Nevertheless, on the 3rd of April, 80 kil. (176 lbs.) of the charcoal, pulverized on the preceding days, and put into a cask, had inflamed spontaneously.

A new operation produced the same results; the same quantity, triturated in one day, was divided and put into two barrels; on the morrow fire appeared in the barrel containing that portion triturated in the morning; the other gradually heated, but did not inflame. Thus pulverized, the charcoal is excessively divided, it assumes an oily appearance, occupies only one-third the space it has whilst in cylinders, and contains six-thousandths of bronze.

In examining the cause of this action, attention was first paid to the mode of carbonization: one portion of charcoal was distilled at a high temperature in close vessels; 100 parts of wood yielded 25 of black carbon: the other was made in open cast-iron vessels. Forty-eight hours after their manufacture, these charcoals were triturated as before for three hours, but as 40 kil. (88 lbs.) were required for their ignition, whilst only about a third of that could be pulverized at once, each was done at thrice, and the products put, as obtained, into two barrels for the two kinds of charcoal. The barrels were placed side by side; each contained about 42 gallons, and were covered with cloth. During the trituration the temperature of the tuns above the surrounding space was alike, and was from 27° to 43° F.

Each cask, with its 42 kil. (92½ lbs.) of charcoal, was closed by a wooden cover with two holes, one at the middle, the other at the

edge, for thermometers; the time, from the end of the carbonization to the arranging the casks, was sixty hours. In about sixteen hours the maximum of temperature occurred; it was, for the distilled charcoal, 41° C. (74° F.) above that of the place, which was at 12° C. (53°.6 F.), and for the other 28° C. (50°.4 F.). Inflammation did not take place apparently, because the casks were well covered, and thus access of air prevented.

On repeating the experiment, in every point, except that free access of air was allowed, inflammation of the distilled charcoal took place. When put into the cask it was at 11°, in two hours it had become 40°, in twenty-three hours 70°, twenty-nine hours 75°, and in thirty-three hours from the pulverization it inflamed. When about to inflame, smoke was seen to rise from the centre, especially from the thermometer-hole; this gradually increased, and the inflammation was first observed about five centimetres (1.97 inches) below the surface of the charcoal; it rapidly extended to within twice that distance from the staves. On tilting the cask, a layer of charcoal, 15 or 20 centimetres (4.9 or 7.87 inches) thick, fell out, from which proceeded long jets of fire; the rest did not appear to be inflamed, the hand could easily be held in it; but that gradually also heated, and next morning was on fire, though the night had been very cold. The other charcoal heated, but did not inflame. During the trituration of the charcoal, the air in the casks was not altered. It did not precipitate lime-water, and contained no carbonic acid.

Twenty-five parts of each of these charcoals were put, immediately after pulverization, into capsules, and into jars full of air, confined by water. In three days the charcoal absorbed much air, and increased by 1½ part; the other charcoal had also absorbed much air, and increased by 1.3 part.

Many other experiments were then tried: charcoal allowed to remain exposed to air twenty-four hours before trituration did not inflame. The mass also had great influence; upon doubling the quantity of the charcoal made in open vessels, that also inflamed twenty-two hours after pulverization. In this case the heat was highest near the surface rather than lower down, and the augmentation in weight of 84 kil. was only 280 grammes, or 1-300dth part; the inflammation took place only in a small quantity of the charcoal near the middle of the upper layer. A capsule of lime-water placed on the carbon did not indicate formation of carbonic acid before the charcoal inflamed.

In another experiment, two equal portions of 42 kil. each were placed, one in an open and the other in an inclosed vessel; the first inflamed, the second did not; so that the absorption of air appears to be the cause of inflammation.

On allowing five or six days to pass between the carbonization and pulverization, no combustion of the powder occurred. When circumstances, as to age, &c., were the most favourable possible, portions of 45 and 30 kil. inflamed, a portion of 15 kil. did not.

Sulphur and saltpetre mixed with charcoal diminish its absorbing power and tendency to inflame *.

9. Power of Carbon to destroy the Bitterness of certain Bodies.

M. Duburga observed that charcoal destroyed the bitterness of a tincture of gentian root, whilst it had no action on that of the centaury; in consequence of which observation, Dr. Kopff made many experiments on different bitter substances, and found great varieties of action. Each experiment was made with two ounces of distilled water, twenty grains of bitter extract of the particular plant, and about sixty grains of the recently-pulverized charcoal; they were digested at temperatures from 78° to 86° F., and examined at intervals, being compared with similar solutions without the charcoal. Wormwood, centaury, gentian, quassia, were not changed; orange-peel, camomile, yarrow, soapwort, and Iceland moss, lost all their bitterness. Endive, rhubarb, &c., &c., were nearly deprived of their bitterness.

When animal charcoal, freed from phosphate of lime, &c., by digestion in muriatic acid, was used in place of vegetable charcoal, similar results were obtained †.

10. Method of preparing Selenium from the Sulphuret.—(*By M. Magnus.*)

The best manner of preparing selenium from its compound with lead, consists, according to Professor Mitscherlich, in fusing it with an equal weight of nitre; this method being, however, inapplicable to the obtaining of the metal from its sulphuret, Berzelius has proposed to use caustic potash instead of nitre, a process which is, however, too laborious and too expensive, particularly in those cases where the quantity of selenium is very small, as, for instance, in the sulphuret obtained during the manufacture of sulphuric acid. The following method will, then, be found preferable. The sulphuret being powdered, and mixed with eight times its weight of the peroxide of manganese, is heated in a retort; the sulphur becomes changed into sulphurous acid, and the peroxide into the protoxide, and at the neck of the retort a sublimate forms, which consists of pure selenium. If manganese has been employed in excess, the sublimate becomes oxidized after the complete evolution of sulphurous vapour, and forms crystallized selenious acid. The sulphurous acid is passed through water, from which the small quantity of selenious acid which might possibly be carried over, is precipitated in a metallic state. The sublimate will always be found to contain some sulphur, from which it, however, is easily separated, by fusing it repeatedly with the peroxide of manganese, or with caustic potash.

* Ann. de Chim., xlv. p. 73. † Journ. de Pharm., 1831, p. 172.

11. On the Compounds of Ammonia with Anhydrous Salts.—(*By H. Rose.*)

In order to combine ammonia with anhydrous salts, I have employed the following method:—A current of ammonia was passed through caustic potash, and then over a certain quantity of the salt, until, on repeatedly weighing the compound, no further increase of weight could be observed in it. The salt itself was placed in an oval bulb, connected at both sides with long tubes of small diameter, from which the atmospheric air was carefully shut out. I must, however, observe that the addition of weight in the compound was always rather less than what it ought to have been, because, before the experiment, the air in the bulb was atmospheric, and after it ammonial. The experiments were all made at common temperatures, and whenever any evolution of heat was observed, the current of ammonia was diminished. In most cases ammonia was rapidly taken up at the beginning of the experiment, but gradual absorption went on for a long time, so that, in many instances, the experiment was continued for two days.

In most of the compounds which ammonia thus forms with anhydrous salts, it combines in a different ratio from what is observed in its compound with hydrates; and the following description of some of these combinations will perhaps serve to point out the laws by which their formation is governed.

Ammonia and Sulphate of Manganese.—The absorption of ammonia is slow, and unattended by any evolution of heat; the compound forms a white powder, which, after some time, becomes brownish. 0.51 parts of the salt take up 0.328 parts of ammonia, or 100 parts of salt absorb 43.68 of ammonia, which is equal to 1 atom of the former, and 4 atoms of the latter. In the open air it emits ammonia; the solution in water deposits protoxide of manganese; on being strongly heated, the ammonia is driven away; the remainder becomes white, and is perfectly soluble in water.

Ammonia and Sulphate of Zinc.—Ammonia is rapidly taken up by this sulphate with considerable evolution of heat; the compound is a voluminous white powder, consisting of 100 parts of the sulphate and 51.22 of ammonia; 1 atom of the salt, and 5 atoms of ammonia, would be equal to 100 parts of the former, and 53.3 F. of the latter. It is not completely soluble in water, and deposits oxide of zinc. On being strongly heated, it boils and emits ammonia, and a small quantity of sulphite of ammonia.

Ammonia and anhydrous Sulphate of Copper.—The absorption of ammonia by this sulphate is very rapid, and attended with evolution of heat: the salt increases in volume, and becomes of a blue colour; 0.54 parts of salt absorb 0.292 parts of ammonia, or one hundred parts of the former take up 53.97 of the latter, which would correspond to 1 atom of the sulphate to 5 atoms of ammonia, or 100 parts of the former to 53.77 of the latter. It is completely soluble

in water, which becomes of an intense blue colour. If strongly heated, it melts and emits ammonia, water, and sulphite of ammonia; the remainder is of a brownish colour, and consists of metallic copper and undecomposed sulphate of copper.

A similar compound is obtained from sulphate of copper and the solution of ammonia; it differs, however, from the above, inasmuch as it consists, according to Berzelius, of 1 atom of sulphate, 1 atom of water, and 4 atoms of ammonia.

Ammonia and Sulphate of Nickel.—The sulphate absorbs ammonia very rapidly, and under much evolution of heat; it is a white powder, with a slight violet hue; 0.481 parts of salt, combined with 0.317 parts of ammonia, or 100 parts of the former with 65.91 of the latter, equal to 1 atom of salt, and 6 atoms of ammonia. It is soluble in water, with a deposit of hydrate of the oxide of nickel, and by a gentle heat emits ammonia, sulphite of ammonia, and water.

The combination of Ammonia with Sulphate of Cobalt is very rapid; the compound forms a white powder, with a feeble red hue; 0.543 parts of salt unite with 0.361 parts of ammonia, or 100 parts of the former with 66.48 of the latter, which corresponds to 1 atom of salt and 6 atoms of ammonia, or 100 parts of the sulphate and 66.33 of ammonia. It is soluble in water, to which it imparts a reddish tint, and forms a deposit of the hydrate of oxide of cobalt.

Ammonia and Sulphate of Cadmium unite very rapidly, and with much evolution of heat; the compound is a white powder, consisting of 100 parts of the sulphate with 48.69 of ammonia, equal to 1 atom of the former, and 6 atoms of the latter, or 100 parts of sulphate with 49.56 of ammonia. It is not quite soluble in water, and deposits much oxide of cadmium; on being heated, ammonia and a small quantity of sulphite of ammonia are emitted.

Ammonia and Sulphate of Silver combine very slowly, forming a white powder, of which 100 parts consist of sulphate, and 11.82 of ammonia, which corresponds to 1 atom of the former, and 2 atoms of the latter, equal to 100 parts of salt, and 10.99 of ammonia. It is completely soluble in water, and, on being heated, emits ammonia and sulphite of ammonia.

Ammonia and Nitrate of Silver combine very rapidly, with much evolution of heat; the nitrate first melts, and then changes into a white mass, which is perfectly soluble in water, and, on being heated, evolves ammonia. 10.12 parts of the nitrate take up 0.298 parts of ammonia, which corresponds to 100 parts of the former, and 29.55 parts of the latter, or 1 atom of nitrate, and 6 atoms of ammonia.

The anhydrous sulphate of magnesia, the nitrates of soda and of baryta, the phosphate of copper, and bichromate of potash, could not be made to combine with ammonia.

Compounds of Chlorides and Ammonia are very similar.—The ab-

sorption of ammonia by the *chloride of calcium* was very rapid at the beginning of the experiment, but it took a long time before the absorption was terminated. The compound is a white powder of about twenty times the volume of the chloride, and completely soluble in water; 0.99 parts of the chloride combine with 1.186 parts of ammonia, or 100 parts of the former with 118.96 of the latter, so that the compound may be considered as consisting of 1 atom of chloride, and 8 atoms of ammonia, equal to 100 parts of the former, and 122.80 of the latter.

Chloride of Strontium and Ammonia is very similar to the compound of the chloride of calcium; 0.783 parts of the salt were found to have united with 0.662 parts of ammonia, or 100 parts of the former with 84.52 of the latter. This corresponds to 1 atom of salt and 8 atoms of ammonia, or 100 parts of the former, and 86.88 of the latter; on being heated the compound emits its ammonia.

Chloride of Copper and Ammonia.—The combination proceeds very rapidly at the beginning, but it continues a long time before the chloride is saturated. The compound is blue, like the compound of sulphate of copper and ammonia, and of much greater volume than the chloride; 2.357 parts of chloride unite with 1.737 parts of ammonia, or 100 parts of the former with 73.70 of the latter, which may be considered as 1 atom of chloride, and 6 atoms of ammonia. The solution in water is blue. If the compound is exposed to the air, it loses its ammonia, and changes its colour to green; on being heated, it melts and becomes brown, evolving ammonia and muriate of ammonia; the remainder consists of chloride of copper.

Chloride of Nickel very much increases in volume whilst combining with ammonia; the compound is a white powder with a slight violet hue, consisting of 100 parts of chloride, and 74.84 of ammonia, or 1 atom of the former, and 6 atoms of the latter. The solution in water is of a bluish colour, and forms a deposit of the hydrate of the oxide of nickel. Strong heat reduces part of the chloride with the evolution, first of ammonia, and then of muriate of ammonia.

Chloride of Cobalt and Ammonia forms a voluminous white powder, with a slight reddish hue; the absorption is very rapid, and takes place with evolution of heat; 0.473 parts of chloride combine with 0.248 parts of ammonia, or 100 of the former with 51.43 of the latter. This corresponds with 1 atom of chloride, and 4 atoms of ammonia, equal to 100 parts of the former, and 52.84 of the latter. The solution in water is reddish-brown, and forms a deposit of green oxide of cobalt; on being heated, the compound emits ammonia, muriate of ammonia, and water.

Chloride of Lead and Ammonia combine very slowly, and without any change of colour or volume; 15.15 parts of chloride were found to have taken up 0.141 parts of ammonia; which would correspond to 2 atoms of the former, and 3 atoms of the latter, or 100 parts of chloride, and 927 of ammonia.

Chloride of Silver absorbs but a small quantity of ammonia;

0.642 parts of chloride took up only 0.115 parts of gas, which is 100 parts of the former to 17.91 of the latter; 1 atom of chloride unites accordingly with 3 atoms of ammonia, or 100 parts of the former with 17.92 of the latter.

If *Chloride of Mercury* is brought into contact with ammonia, it becomes black, but on a gentle heat, or by exposure to air, as well as by acids, the white colour is restored, and ammonia emitted; 1.382 parts of the chloride were found to combine with 0.102 parts of ammonia, or 100 parts of the former with 7.38 of the latter, which corresponds to 1 atom of chloruret and 1 atom of ammonia, or 100 parts of the former and 7.21 of the latter.

At the common temperature, the *Chloride of Mercury* was found to combine very slowly with ammonia; but on applying heat to the chloride, the absorption was much more rapid; afterwards 0.865 parts of chloride were found to have taken up 0.050 of ammonia, or 100 parts 5.78 of ammonia. The compound does not dissolve in water, but in every other respect resembles the chloride; if boiled with water, it forms a yellow precipitate; on being heated, it is first melted, and then volatilized, without emitting its ammonia. It may be considered as consisting of 1 atom of chloride, and 1 atom of ammonia, equal to 100 parts of the former and 6.27 of the latter.

Chloride of Antimony and ammonia do not combine at the common temperature; but on being fused, in contact with ammonia, the chloride is changed into a brittle substance, which does not become fluid in the air, like the chloride. It was examined in the following manner:—1.675 parts of the compound were dissolved in tartaric acid, the solution was diluted with water, and the antimony precipitated by sulphuretted hydrogen; to the filtered liquid a small quantity of a solution of sulphate of copper, and then a solution of the nitrate of silver was added, by means of which 2.833 parts of chloride of silver were precipitated. The compound accordingly consisted of 100 parts of antimony, and 8.19 of ammonia, or 1 atom of the former and 2 atoms of the latter, which are equal to 100 parts of chloride, and 7.29 of ammonia.

The *Chlorides of Sodium and Barium* could not be made to combine with ammonia.

Of the compounds of ammonia with bromides, iodides, and cyanides, I have examined but very few: of which I shall only mention the following:—

At the common temperature, the *Bromide of Quicksilver and Ammonia* do not combine; but if the *bromide is heated in an atmosphere of ammonia*, its weight increases by 3.41 per cent., which may, perhaps, be considered as = 1 atom of bromide, and 1 atom of ammonia, equal to 100 parts of the former, and 4.78 of the latter. In its properties, it is very similar to the compound of chloride of mercury and ammonia.

Under the action of ammonia, the red colour of the *Iodide of Quicksilver* is changed into a dirty white; 100 parts of the iodide

were found to have increased by 7.01, which would correspond to 1 atom of the iodide, and 2 atoms of ammonia, equal to 100 parts of the former, and 7.54 of the latter. By exposure to air, ammonia is rapidly evolved, and the red colour of the iodide restored.

Cyanide of Mercury combines so very slowly with ammonia, that I was unable to continue the experiment for a sufficient length of time. After several days, 0.760 parts of the cyanide had acquired 0.060 parts additional weight, which would be 7.90 of ammonia to 100 parts of cyanide. The compound is perfectly soluble in water, and, on being heated, loses its ammonia.

Some oxides were also submitted to the above method, but without obtaining any results; the oxides of copper and zinc at least did not seem to combine with ammonia.

The above experiments seem to me to be of interest, inasmuch as they shew that some anhydrous salts combine with ammonia, whilst others do not, and that in the compounds there exists a definite ratio of the elements. It is further worthy of remark, that in some compounds, the salts of which are very similar one to another, the proportion of ammonia should be so very different. The compounds which anhydrous salts, and the chlorides, bromides, &c. form with ammonia, appear to me to have some analogy to the combination of these substances with water; for of those salts, which possess similar properties, some form hydrates, and others do not: in some, the water of crystallization is in a different proportion to what it is in others; and in all, it is in a definite ratio to the elements of the salt. Lastly, most hydrates emit their water of crystallization very easily; but in a few it is rather fixed; this is also the case with the compounds of ammonia and anhydrous salts*.

12. Test of the Protoxide and Peroxide of Iron.—(*Berzelius.*)

To determine the exact quantity of the oxides of iron in a substance which is soluble in acids, Berzelius proposes the following method:—A bottle, with muriatic acid, is closed air-tight, after all atmospheric air has been expelled by means of carbonic acid; the substance which is to be examined is carefully weighed and dissolved in the acid by a gentle heat. What remains insoluble, is washed with boiling water (which must be free from air), and this, as well as the solution, is put into a bottle containing a certain quantity of silver in powder and water, which has been freed from its air by boiling. The bottle is now carefully closed, and the liquid digested at a little below 212° F., and frequently shaken. After it has become colourless, which sometimes requires about twenty-four hours' digesting, it is filtered, and the metallic powder washed, dried, and weighed; the excess of weight consists of chlorine, 44.26 of which correspond to 97.84 of peroxide in the substance; the rest, if it had been ascertained that the substance contains more iron, consists of protoxide†.

* From Poggendorff's Annalen, Bd. xx. St. 1. page 147.
† Berzelius, Lehrb. Band 4. S. 757.

13. A NEW METAL, VANADIUM, ASSOCIATED WITH IRON.—(*Sefström.*)

This new metal, which has an interest far beyond that of many new substances, because it is connected with the physical and valuable properties of iron, was discovered by M. Sefström, and has been briefly described in a letter from M. Berzelius to M. Dulong, from which the following is an extract:—

M. Sefström, director of the School of Mines at Fahlun, whilst engaged in examining a variety of iron remarkable for its extreme softness, observed the presence of a substance, the properties of which differed from those of all other known bodies, but its quantity was so small as would have rendered it tedious and expensive to collect sufficient for a correct examination of its properties. This iron was from the mine of Taberg in Smöland: the ore merely contained traces of the substance. Finding that the pig iron contained far more of this principle than the wrought iron, M. Sefström thought that the scoriæ formed during the conversion of the pig iron into wrought metal might be a more abundant source,—a conjecture confirmed by experience; so that sufficient having been procured, he went to M. Berzelius during the Christmas holidays, to complete its examination. For the present the substance is called *Vanadium*, after a Scandinavian divinity.

Vanadium combines with oxygen to form an oxide and an acid. The acid is red, pulverulent, fusible, and on solidifying becomes crystalline. It is slightly soluble in water, reddens litmus, and forms yellow neutral salts and orange bisalts. Its combinations with acids or bases has the singular property of suddenly losing their colour—they resume it only on becoming solid again, and being then redissolved, preserve their colour. This phenomenon appears to have some analogy with the two states of phosphoric acid and of phosphates.

Hydrogen, at a white heat, reduces vanadic acid, leaving a coherent mass, having a feeble metallic lustre, and being a good conductor of electricity, but it is not certain that the reduction is complete. Vanadium, thus obtained, does not combine with sulphur when heated to redness, in its vapour. The oxide of vanadium is brown, or nearly black, and dissolves readily in acids. The salts are of a deep brown colour, but, by the addition of a little nitric acid, effervesce and become of a fine blue colour.

Vanadic acid combined with another acid is reduced by sulphuretted hydrogen, and even by nitrous acid, to that blue matter which appears to be a compound of vanadic acid with the oxide of vanadium, analogous to those compounds formed by tungsten, molybdenum, iridium, and osmium. The oxide and acid of this metal together produce other combinations, green, yellow, and red, all soluble in water.

When the oxide of vanadium is produced in the humid way, it is

soluble both in water and alkalies. The presence of a salt renders it insoluble, and upon this effect may be founded a process for its preparation.

The vanadates, when dissolved in water, are decomposed by sulphuretted hydrogen, and converted into sulfa salts, of a fine red colour.

The chloride of vanadium is a very volatile, colourless liquid, producing thick red fumes in the air. The fluoride is sometimes colourless, sometimes red, but always fixed. Before the blowpipe vanadium colours fluxes of a fine green colour, in that respect resembling chrome *.—See page 562.

14. Combustion of an Alloy of Tin and Lead.—(*R. W. Fox.*)

When tin and lead have been strongly heated together (in the flame of a blowpipe for instance), the alloy continues ignited a considerable time after it has been removed from the flame, throwing out numerous and brilliant ramifications without cessation, till the whole becomes oxidated, if the quantity be small. The addition of gold does not impede the process, and it appears to be converted into a purple oxide, though I have as yet only slightly examined it. With platinum in combination, the oxidation is more partial, and a porous alloy remains, which is easily pulverised.

The metals may be treated on mica, or any other imperfect conductor, capable of resisting a high temperature. The resulting oxides emit a bright light when acted upon by the blowpipe, owing probably to the presence of the oxide of tin, which yields an intense light, and so does the oxide of zinc; but the white ashes of the burnt leaves of shrubs or trees exceed all other substances, in this respect, that I am acquainted with, not excepting lime.

15. Vauquelin's Process for obtaining Metallic Chromium.

The following is his own account of the process. ‘When attempts are made to obtain chromium from the oxide and carbon, they never succeed well, whatever the heat employed. Chromic acid is reduced with less difficulty, and from 72 parts 24 of metal were obtained. The muriate of chromium is the most favourable substance, and the following, which is the correct process, has not been yet described. Chromate of lead in very fine powder is to be digested in four or five times its weight of muriatic acid until all is dissolved. The liquid is to be evaporated to dryness, the residue digested in alcohol, which dissolves the chloride of chromium; the solution evaporated to a syrup, and then formed into a ball with sufficient oil, and, if necessary, a little charcoal. This being put into a crucible lined with charcoal, and that placed in another containing powdered charcoal,

* Ann. de Chimie, xlv. 332.

and the whole heated well for about an hour, will yield the metal chromium *.'

16. On the Absorption of Oxygen at high Temperatures, by Silver.—(*Gay-Lussac.*)

Mr. Lucas remarked that fused silver in contact with air absorbed oxygen, which it threw off upon becoming solid. This property is analogous to its power, as observed by Pelletier, of combining, when hot, with double the quantity of phosphorus which it can retain when solid. In the experiments described by Lucas, only small absorptions of oxygen occurred, and sometimes none. More certain results are obtained by fusing the silver in a porcelain tube, and passing a current of oxygen gas over it. After twenty-five or thirty minutes of high temperature the current of gas may be stopped, and the heat allowed to fall; a partial vacuum is soon produced in the tube, because of the diminution of heat; but at the moment when the silver solidifies, there is an abundant evolution of oxygen gas.

A simpler and more advantageous process is to throw small quantities of nitre upon silver retained in fusion in a crucible. In about half an hour the crucible is to be withdrawn and to be plunged into the water brought under a bell glass. No accident need be feared. There is time to get the crucible under the jar, but immediately after a large quantity of oxygen is disengaged. In one experiment it amounted to twenty-two times the bulk of the silver †. If the metal be allowed to fall drop by drop into cold water, large bubbles of oxygen gas will be observed rising through the water, and the metal will acquire a rough and dull aspect. It is to be remarked, that silver containing a little copper absorbs oxygen by its affinity for the latter metal, preserving it from oxidation ‡. But the purer it is, the more oxygen it takes up; and a few hundredths of copper prevent the absorption altogether.

It is of course to this power of absorbing oxygen at a high temperature, and evolving it on solidification, that the phenomenon of vegetation and shooting as it occurs in assaying is to be attributed. It is very difficult to prevent very fine silver from vegetating, but very easy with such as is alloyed with copper, lead, or gold §. It is also to this same property of silver of becoming oxidized when hot, that the loss which occurs in cupellation is to be attributed, and the

* Ann. de Chim., xlv. p. 110.

† When nitre is decomposed by heat, it frequently yields a peroxide of potassium, which, in contact with water, evolves much oxygen. There is no reason to fear any error on M. Gay-Lussac's part from this cause, but it is necessary that those who repeat the experiments be careful also to avoid it.

‡ The phenomena of polling in the copper works would seem to shew that a similar property belongs, in a slight degree, to copper.

§ We have understood from practical men that the effect is easily prevented by charcoal dust being applied over the silver before it is allowed to solidify. The reason of its utility would seem to be very apparent.

absorption of the metal by the cupel, especially towards the close of the operation *.

17. Robiquet on a new Metallic Dye.

A stuff dyed of a clear bluish-grey colour was taken to M. Robiquet as able to stand the action of every agent without change of tint, a character which M. Robiquet ascertained it to deserve. Concluding that it was metallic, it was also concluded that it must be chloride of silver, from its colour and characters; on boiling the cloth in ammonia, however, no silver, or chloride of silver, was dissolved,—the colour, indeed, became brighter. On incinerating the substance and digesting the ashes in ammonia, and then in nitric acid, both solvents dissolved silver, the first having taken up muriate of silver, and the latter having dissolved the metal.

As it was not likely that any chloride would be decomposed and brought into the metallic state by incineration, it was supposed that the silver had been applied at first as a nitrate and then converted into a chloride; the parts which had penetrated deepest having escaped the converting action. Imitations of the dye were therefore made by dipping the cloth first into solution of nitrate of silver, then drying it, immersing it in a solution of a muriate or of chloride of lime, and immediately upon withdrawing it, exposing it to light; the colour was at once developed, and the success was perfect. By using different strengths of solution of silver, different tints were obtained.

Upon trying the application in a large way, a curious cause of failure occurred. Unless the whole be exposed to the light at once, the colour is not uniform; the parts exposed at different times are dissimilar, and hence cloudiness is produced. This may be obviated in some situations, but not in others, where space is limited.

In printed goods it is supposed that some good applications of this idea may be made †.

18. Purple Precipitate of Silver, Gold, &c. &c.

Fischer has shewn that protosalt of tin yields with solutions of silver, platina, palladium, and tellurium, precipitates similar to those produced with solution of gold. Frick has shewn that the silver precipitate may be prepared of great purity, by using a very pure protonitrate of tin, and after adding it to the solution of silver, adding also dilute sulphuric acid. The addition is supposed to prevent the further oxidation of the tin by the free nitric acid, and so alter the precipitate. The protonitrate of tin is to be prepared by decomposing the protomuriate by nitrate of lead. In the purple precipitate of silver, the combination is as strong as in Cassius' purple; the substance is not decomposed either by muriatic acid or ammonia.

* Ann. de Chim., xlv. p. 221. † Jour. de Pharm., 1831, p. 162.

In preparing the purple precipitate of Cassius, Fischer, who first pointed out the superiority of the protonitrate of tin in the above experiment to the other salts of that metal, also uses the same solution. It very much surpasses the protomuriate, and is always successful, whether used in a weak or a concentrated state.

When protonitrate of mercury is poured into solution of gold, according to Fischer, a blue grey precipitate is obtained, quite in analogy with the purple of Cassius. It is composed of deutoxide of mercury and suboxide of gold, and is not decomposed by muriatic acid; that substance only dissolves a little mercury, and makes the colour of the remaining precipitate pass to a clear grey white*.

19. Œnometer or Alcohometer.—(*By M. Emile Tabarie.*)

This instrument is intended to supply the manufacturer with the means of ascertaining the quantity of spirit in any vinous liquid. The principle consists in boiling the wine (for instance) in the open air, allowing the alcohol to escape, and making up the bulk of the residue by the addition of pure water. The difference between the density of this mixture and the original liquid indicates the quantity of alcohol which was present. The apparatus consists of a small boiler heated by a spirit lamp; a horizontal cross bar near the bottom, when left uncovered by the liquid, indicates when the ebullition has proceeded so far as to ensure dissipation of all the alcohol. The densities before and after ebullition are ascertained by a hydrometer with a double scale. For correction of temperature, a thermometer with double scale is used, one scale being the Centigrade, and the other a peculiar division, intended to simplify the operation. Tables accompany the instrument. The whole is intended for the distillers of the centre of France, and costs about forty francs †.

20. On the Manufacture of Sulphuric Ether.—(*C. Wittstock.*)

The remark of MM. Fourcroy and Vauquelin, that the sulphuric acid employed in the fabrication of ether undergoes very little change, led to the conclusion that ether would be formed as long as there was a fresh supply of alcohol to the acid. This supposition was confirmed by the experiments of M. Gay-Lussac; and since then the fabrication of ether has been considerably improved by MM. Boullay, Geizer, and others. I have for some time employed the following method; and as I am disposed to consider it more simple and less expensive than any other, a short description of it may perhaps be acceptable to the reader.

A mixture of nine parts of sulphuric acid (s. g. 1.84 — 1.85), and five parts of alcohol (s. g. 0.835) are put into a green glass retort of one foot in diameter, with a glass tube inserted at its upper part. This tube

* Jour. de Pharm. 1831, p. 175. † Ann. de Chimie, xlv. 222.

is 4 lines in diameter, and bent at a right angle; the shorter arm, which, at its extremity, has only one line in diameter, is plunged one inch deep in the mixture; the longer arm, of about three or four feet length, with a cock near its further end, leads into a bottle, with alcohol. The receiver consists of a refrigerator, viz., a wooden tube, filled with water, by which the distilled ether is kept cool, and two copper vessels, the one within the other, so that there is a distance of about two inches between their sides. The neck of the retort leads into the intermediate space between the two copper vessels, which is thus filled with the distilled liquid, and from which the liquid may flow off by another tube. The apparatus is used in the following manner:—When the mixture is boiling, the cock of the glass tube is opened, and the supply of alcohol thus kept up, so that the quantity of liquid in the retort remains always the same; this is continued until eight times the original quantity of alcohol has been used, which will be the case in about twenty hours, if the original mixture consisted of 25lbs. of sulphuric acid, and 14lbs. of alcohol. The first rectification of the ether thus obtained yields about its third of ether of .725 sp. gr., which may of course be considerably increased by repeated rectifications, besides about twenty to twenty-five per cent. of alcohol are regained, which may be subsequently used again, particularly for the supply of alcohol to the mixture.

Of 124 lbs. of alcohol of 0.835 sp. gr., 22 lbs. were regained; the quantity of pure ether of 0.720 to 0.725 sp. gr. at 14° R., amounted to 59 lbs., and of sulphuric acid 25 lbs. were used. The expenses of fuel, apparatus, attendance, &c. does not raise the price of the ether to more than twice that of its weight of alcohol*.

21. On Columbine; a New Vegetable Principle.—(*By M. Wittstock.*)

M. Wittstock has succeeded in obtaining from columbo root, a crystallised substance, to which he, on account of its similarity to some other vegeto-alkalies, has given the name of columbine (*Columbia*). The method of preparing it consists simply in distilling the alcoholic extract to about its third; after a few days yellowish-brown crystals are deposited, which, after having been washed with water, and again dissolved in alcohol with a little animal charcoal, yield colourless prismatic crystals. They may also be obtained by letting the etherial infusion evaporate, and M. Wittstock states that two drachms of the root are sufficient for the experiment, provided the specific gravity of the ether is 0.725. Columbia is extremely bitter, inodorous, and without any effect on vegetable colours; boiling alcohol of sp. gr. 0.835 dissolves from $\frac{1}{40}$ to $\frac{1}{30}$ of its weight; cold alcohol, ether and water, take it up in much less proportion, yet the solutions are intensely bitter. It is also soluble in essential oils and alkaline

* Extracted from Poggendorff's Ann., Band xx. St. 2, page 461.

solutions, from the latter of which it is precipitated by acids. Acetic acid dissolves it nearly in the same proportion as boiling alcohol; the solution is intolerably bitter and yields regular crystals. By nitric, sulphuric, and muriatic acids, as well as by intense heat, it is decomposed, without any evolution of ammonia. The solutions in alcohol and acetic acid are not changed by the tincture of galls, nitrate of silver, acetate of lead, or any other metallic salt.

It is very probable that the astringent property of columbo resides in the substance in question; and as the method of preparing it is so very simple, it would perhaps be worth while to try its medicinal effects *.

22. On the Composition of Camphor and Camphoric Acid.—(*By J. Liebig.*)

The researches of MM. Brandes (Schweigger's Journal, s. 38, 269,) and Bouillon Lagrange, (Ann. de Chim., t. xxiii. and xxvii.) on camphoric acid, have led to very different results; for, according to the latter chemist, the camphorates of potash, soda, and baryta, are almost insoluble in water, whilst the former found them very soluble and almost deliquescent in the open air. This and other differences are, however, only apparent, and, as will be seen, may be accounted for by the different kinds of camphoric acid which were examined by these two chemists. It is sufficiently known that camphor, when digested with strong nitric acid of s. g. 1.425, dissolves into yellowish liquid, which, on continuing the digestion gradually disappears; the solution, when left to cool, deposits, a great quantity of white non-transparent crystals, which, when boiled with water, communicate to its vapours a smell of camphor. This crystallised substance is Bouillon's camphoric acid; its salts are very insoluble, and it consists of a mixture of camphor and camphoric acid, as may be ascertained by dissolving the former in the latter substance by a gentle heat. If the crystals are again submitted to the action of nitric acid, transparent crystals of camphoric acid are obtained, the compounds of which with bases correspond with the camphorates described by Brandes. This acid I considered as pure, although, on boiling it with water, it communicated to the vapours a camphoric smell. In order to ascertain its atomic weight, the camphorate of lead was decomposed by sulphuric acid, and it was found that

0.785 of camphorate of lead yielded 0.527 of the sulphate, and
1.129 . . . 0.772

so that the equivalent of camphoric acid would be 140.34; it does not contain any water of crystallisation. The proportion of its elements was determined by burning crystallised camphoric acid

* Poggendorff's Annal. 1830, p. 298.

and camphorate of lead, after they had been previously dried under the air-pump with oxide of copper; the result was:—

0.261	gramm. of the camphorate yielded at		27″	8‴	and	24°	Cent.	166	CC	of gas.
0.202	,,		27	10	,,	23	,,	120		,,
0.100	,,	acid	27	11	,,	22	,,	122		,,
0.100	,,	,,	27	11	,,	21	,,	124		,,

and 0.849gr. of the camphorate of lead yielded 0.260 of water.

A hundred parts of acid accordingly consist of

61.4098	of carbon
6.8070	hydrogen
31.7832	oxygen;

and the atomic proportion would be

	By Experiment.			By Calculation.
Carbon	8.61828	= 12 atoms	=	9.1724
Hydrogen	0.95429	= 15	,,	0.9360
Oxygen	4.46143	= 4	,,	4.0000
	14.03200			14.1084

in which, however, the results of experiment and calculation, particularly with regard to the proportion of carbon and oxygen, are too much at variance to be supposed correct.

I accordingly digested another portion of the crystallised substance with nitric acid, until, when boiled with water, no smell of camphor could be perceived in its vapours. A part of this acid was boiled with a solution of acetate of lead, and the precipitate decomposed by sulphuric acid; 1.105 of the camphorate yielded 0.760 of the sulphate, which makes the atomic weight of camphoric acid = 135.67. And 0.220 gram. of the camphorate of lead at 14° Cent., and 331‴, gave 121 CC of gas, and 0.290 gr. gave 0.090 of water, so that the result is:—

	In Hundred Parts.		By Experiment.	By Calculation.
Carbon	76.4370	= 10 atoms	56.167	56.29
Hydrogen	9.3597	= 15 ,,	6.981	6.89
Oxygen	50.0000	= 5 ,,	36.852	36.82

Supposing this analysis, which was made with all possible care, to be correct, camphoric acid would contain 15 atoms of hydrogen, if 1 volume is equal to 1 atom of hydrogen, and 7½ atoms if 1 volume is equal to 2 atoms: it need hardly be mentioned, how much the former of the two hypotheses, in this instance at least, is preferable to the latter.

On treating camphor with nitric acid, no effervescence nor any evolution of carbonic acid is observed. From a number of analyses, the two following appear to me to be the nearest to truth.

0.100 of camphor yielded at 23° and 27″ 9‴ Barom. 162 cc. of gas.
0.100 „ „ 21° 27 9 „ 164 „
and 0.255 of camphor gave 0.230 of water,
„ 0.225 „ „ 0.191 „
of which the mean result would be,

Carbon	81.763	or 12	atoms.
Hydrogen	9.722	18	„
Oxygen	8.535	1	„

or 6 (2C + 3 H) + O.

Camphoric acid, being expressed by 10 C + 15 H + 5 O, consists accordingly of camphor with 5 atoms of oxygen for eyery atom, but so that 5 atoms of camphor would form 6 atoms of camphoric acid*.

23. Use of Mica in minute Chemical Analyses.

Mica is not broken or burnt by flame; M. Voges, therefore, separates a thin film with a knife, and if he has to heat a substance, puts it upon the mica and a lamp beneath. If many successive experiments are to be made, a wet cloth removes the remains of former experiments. Slight detonations, concentrations, evaporations, combustions, reductions of metals, &c., may thus be made with great facility. The thinness of the mica allows the heat to pass with great readiness; its transparency allows changes of colour to be seen. The blowpipe cannot often be used because it renders the mica white. On concentrating some drops of acetic, nitric, sulphuric, muriatic, phosphoric, and tartaric acids upon mica by the flame of a candle, phosphoric acid was the only one which left a visible mark. Caustic potassa might be fused on it without sensible action. Nitrate of silver, reduced upon it by heat, allowed the metal to be wiped off, leaving no mark. Nitrate and acetate of lead could be decomposed upon it without change. Tartrate of potash and antimony, calomel, sulphur, &c. could be heated powerfully on it without alteration to the plate. Mica, therefore, may be used as a very valuable agent in the laboratory †.

24. On Perforating and Cutting Glass, Earthenware, &c. (*Mr. Marsh.*)

Although many persons are acquainted with methods of perforating glass, &c. the following easy one I find is not so generally known as it deserves to be, and I am therefore induced to explain the mode of operating I employ, in order to produce the best results. Circumstances often conspire to render the process valuable to persons situated at a distance from large manufacturing towns, and especially to those who are living in places where they cannot readily obtain

* Extracted from Poggendorf's Annal., Bd. xx. St. 1. p. 41. sq.
† Journal de Pharmacie, 1831, p. 113.

the various chemical apparatus they may require for their purpose. I have frequently found the knowledge to be of importance, not only as regards the time saved (and time is equal to money); but the facility with which the various broken and otherwise useless articles of domestic economy may be converted by its aid into convenient and useful chemical apparatus.

It will be unnecessary at present to enter into a detailed account of the various apparatus that may be constructed, by any person acquainted with the process—it being my intention, on another occasion, to describe a variety of such arrangements. My present object is to describe the method by which the effect may be produced, with certainty and despatch.

The only tools requisite in this process are a few worn out three-edged handsaw files; these being generally made of cast steel, retain, when ground, a very fine point, which is of the utmost importance. In order, however, to give them the requisite degree of hardness, it is necessary to make their ends, for about an inch, red hot, and then plunge them into cold water; by this treatment they become hard and brittle: care is therefore required in grinding them to a proper point; this is easily effected on a common grindstone. I generally give them a few rubs on a fine oil stone after the grinding, so as to produce a very fine point.

A cylindrical piece of any sort of wood, about two inches long, terminated by a half round end, having a hole about the tenth of an inch in diameter through its axis, may either be fastened into a common bench vice, or on a table; this constitutes the only support required.

Suppose that a glass to cover the face of a wheel barometer is wanted, through which it is sometimes necessary to make a perforation for the purpose of passing the screw of the nonius through: a proper piece of glass being selected is to be marked with a dot of ink on the place where the intended perforation is to be made; the glass is then to be held horizontally by the left hand, on and immediately over the hole in the wood support above-mentioned. A three edged file having been hardened, and ground to a fine point in the manner before described, is held firmly between the forefinger and thumb of the right hand, precisely in the position that a pen or pencil is retained when writing. The pointed steel is then to be repeatedly impinged against the glass over the spot intended to be perforated, taking care not to use too much violence; in a short time, the outer surface is removed, and, by a continuation of the process, a conical piece is forced from the under surface of the glass through the hole in the wood support: the perforation so produced, never exceeds in size a pin head, but may be made as large as required by holding it over the hole in the support, and working round its edge with a fine pointed file. In this way, after a little practice, and in a very few minutes, may be perforated with ease all descriptions of glass, from the thinnest crown to the thickest plate, without any danger. In-

deed, I have frequently made four or five perforations in the space of an inch square, without any fear of starring the glass, as it is technically called.

When it is required to perforate glass globes, or the upper part of wine bottles, the wood support is of course unnecessary, as the figure of the vessel gives sufficient strength without it. Wine glasses or tumblers may also be easily perforated in a similar manner; but I mostly employ another process for them. These being made of a softer sort of glass, require only to be moved by the hand backwards and forwards in the manner of drilling, on the sharp point of the file, with the occasional assistance of a little oil and emery. Indeed, any sort of glass may be perforated in this manner with ease, but I think not so quickly as by the method of punching.

All the varieties of china and earthenware may be perforated by either of the above processes with certainty; and the ingenious experimentalist will find no difficulty in turning to account many otherwise useless articles by its assistance.

It may not be amiss to mention here an easy method, which I have occasionally employed with success, for separating the bottoms of phials from the other parts, &c.; I pour a small quantity of sand or emery into the angular turned up part of the vessel, with a few drops of water to moisten it; then, by means of a piece of wood having a sharp point, I press the moistened sand, &c. into contact with the glass, and by gently turning the bottle round, bringing the point of the wood and the sand into contact with every part of the lower end of the phial in succession; by these means, the surface is quickly scratched, and immediately after a fracture takes place all round the bottle, which instantly separates the bottom; this effect does not take place with all sort of bottles, but in very many it does. J. M.

---

§ III. *NATURAL HISTORY, &c.*

1. Circulation of Fluids in Vegetables.

A brief account of some remarkable observations upon this phenomenon has reached us just as we were going to press. We should have been glad to have inserted the paper entire, but it is indispensable that the coloured figures that illustrate it, should accompany the author's remarks, which are moreover so desultory, that we cannot but hope they will be followed by some more exact statement. Professor Ch. H. Schultz of Berlin, the author of these observations, has discovered that there is a visible circulation of fluid in all plants, not only in the lower orders, such as Chara, Nitella, &c. in which it has been long since described by Amici, but also in the most perfect flowering plants. He more particularly describes its appearance in the stipulæ of Ficus elastica, Alisma plantago, and

some Cichoraceæ. It is stated, that in these plants, the veins of the foliaceous parts consist of what the author calls *vital vessels*, surrounding the spiral vessels; the duty of the latter is to absorb the nutritive fluid, which they assimilate, and to conduct it into the *vital vessels*, which form what is more properly called the system of circulation. This is observed readily both in monocotyledons and dicotyledons, by aid of the microscope. The progressive movement of the nutritive fluid takes places in canals, which are more or less flexuose, and which traverse the cellular tissue by the side of other vessels of the same nature, in which the motion takes place in an opposite direction. At intervals are anastomosing branches, which connect the two *torrents*, and by which the liquid passes from one into the other, altering its direction; so that the whole constitutes a sort of vascular plexus, the arrangement of which varies according to species. The observations of Professor Schultz have been confirmed by Messrs. Cassini and Mirbel, who were appointed by the French Academy to examine the facts. They state that the manner in which the subjects are prepared by M. Schultz is very simple. In the Ficus carica he merely cut away, with a sharp instrument, the cuticle, so as to lay bare the vessels and the parenchyma, and then placed the preparation in water under the microscope. But the most satisfactory illustration of the phenomenon was found in the Chelidonium, an entire leaf of which, when examined in water, by means of transmitted light in bright sunshine, exhibited the movement beautifully, in its more delicate and transparent veins. M. Schultz considers it very important, that a flat, not a concave, mirror should be employed for this examination *.

2. Structure of Leaves.

An important memoir upon this subject has appeared from the pen of M. Adolphe Brongniart. According to this observer, there is a great difference between leaves that grow in water, and those that grow in air. In the latter, there is a regularly formed cuticle on both surfaces, which is perforated by openings of a peculiar nature, which are what botanists call *stomata*. In the former, there is no epidermis, and consequently no stomata. This difference of structure is in direct relation with the respective functions of aërial and submerged leaves, and with the respiration of plants. The functions of leaves are to present the water, mucilage, sugar, &c. which is pumped up from the earth through the roots, to the action of the atmospheric air and light through the medium of extremely thin transparent membranes. In leaves that grow in air, the cellules that contain the fluids destined to be thus elaborated are inclosed within a covering called the cuticle, which protects the tender membranes from coming too rapidly in contact with the atmosphere, and which, by aid of the preparations, or stomata, above alluded to, retard evaporation, and control respiration according to their number, size, &c. But

* Annales des Sciences.

submerged leaves have no need of protection from rapid evaporation, nor of any mechanical contrivance by which a too active influence might be exerted upon them by the atmosphere; and besides, the atmospheric air by which they are to be acted upon is itself dissolved in the circumambient water. Hence such leaves have no cuticle.

With regard to the stomata, the author shews, by various observations, that the common opinion, of there being openings through the cuticle into the cavernous parenchyma of the leaf, is just; and consequently, that they are not closed up by a membrane, as is the opinion of Turpin and Raspail, and as has been more recently stated by Mr. Brown. His best proof of this is that which he has drawn from an inspection of very young unexpanded leaves of the Narcissus and Lily, examined near the bulb. In these the stomata are circular evident perforations, surrounded by a circular elevated rim. The paper, which is published in the *Annales des Sciences* for December last, is accompanied by highly magnified drawings.

3. Germination of Seeds at the Surface of Mercury.

Some experiments have been instituted by Professor Mulder to determine the force with which roots are developed. He placed seeds of the bean and buckwheat in glasses containing mercury, covered over with water, laying them upon the surface of the mercury, and taking care that they were just about to germinate. The very next day the beans had forced their radicles into the mercury; but those of the buckwheat ran along the surface, forming a sort of net-work by their interlacing, and not making the smallest impression upon the mercury. This experiment was instituted on the 26th of September; on the 26th of October he found many of the bean-roots had ramified beneath the mercury, between it and the sides of the glass; but what was especially worthy of remark, in every instance the root was curved back upon itself in the water at its origin. Hence the author concludes, that there is an *internal force* which propels the roots, and which, while it sometimes yields to external circumstances, is never wholly destroyed.

4. Fertilization of Plants.

M. Amici has lately indicated a simple mode of witnessing the manner in which the fertilization of vegetables takes place. When M. Adolphe Brongniart published his opinions upon this subject he asserted that, upon the bursting of a grain of pollen, an internal very delicate membrane is protruded, which acts as a *boyau*, or sheath, to the vivifying particles that are emitted, and which insinuates itself between the cellules of the tissue of the stigma, thus establishing the ingress of the fertilizing matter into the system of the ovarium. It has since been a common opinion, that M. Brongniart was, in that respect, deceived by some optical illusions, and that the sheath or boyau in question has no existence. The object of M. Amici is to

shew how these sheaths may be distinctly seen, so as to leave no doubt of their existence. 'If,' he says, 'you take the stigma of Hibiscus Syriacus, or the Althæa frutex, and compress it gently between two plates of glass, it will become transparent; and the observer will perceive not only the sheath introduced into the tissue of the stigma, but may behold the vivifying particles circulating within them. In addition to this fact, M. Amici asserts, that each grain of pollen frequently protrudes from twenty to thirty sheaths, and that each sheath penetrates from the stigma to the foramen of the ovulum, there being always one sheath for each ovulum. 'If,' he remarks, 'I am asked how such a sheath is to traverse the tissue of these styles, which are very long, my answer is, that perhaps when it has once effected an entrance within the tissue of the stigma, it receives from the tissue itself such nourishment and increase of matter as may be necessary.' We scarcely need add, that it is highly desirable, that these observations should be repeated by others.

5. Structure of the Radish Root.

It is well known to most observers that at the summit of the root of the common radish, at the very base of the stem, or at that place which the French call the *collet*, the English the *neck*, is an appendage, at first resembling a membranous sheath, enwrapping the young root, and subsequently, as the root distends, becoming two loose straps hanging down on each side of the root. The nature of this appendage was unknown until the late ingenious L. C. Richard discovered the existence of two modes of germination, called the *exorhizal* and *endorhizal*, and suggested that the radish was an example of the latter mode; a notion which has been generally admitted by recent writers, notwithstanding the circumstance, that, if endorhizal, the radish would offer an exception to a very general law that endorhizal germination goes along with endogenous growth. M. Turpin has lately demonstrated that the fleshy supposed root of the radish belongs to the ascending axis, not to the descending one, and that, consequently, it belongs to the system of the stem, and not to that of the root. In the next place, he asserts, that the tumour, which ultimately becomes the radish, is in the beginning cylindrical, and that its cuticle loses, at a very early period, the power of distension; in short, that it dies, and separates from the subjacent living matter, just as dead bark separates from liber and young wood, in old stems. Now, this premature death of the cuticle is connected with the rapid lateral distension of the tumour, the cause of the existence of the two appendages in question, which are nothing more than two straps of dead cuticle, rent asunder by the gradual but rapid distension of the part that they originally ensheathed.

6. Russet in Apples.

Mr. John Williams, C.M.H.S., in a paper recently published in the

Transactions of the Horticultural Society of London, attributes the cause of apples becoming russet to the alternating temperature, light, shade, dryness, and moisture, which occur many times in the course of a day, when July or August are showery. Continued rain, preceded and followed by a cloudy sky, does not seem to produce the same effect, but the sudden intense light which commonly succeeds a shower at the time the fruit is wet, injures the skin, and occasions small cracks, like the net-work upon a melon.

7. Medicinal Use and Effect of the Ava Root.

The intoxicating property of the Ava root, the cutaneous eruption which succeeds its use, and the renovating effect it has upon the constitution, have been noticed ever since the discovery of the Society Islands. Mr. Collie, late surgeon of H. M. S. Blossom, in her voyage to the Pacific, observes that 'a course of it is most beneficial in renovating the constitutions which have been worn out by hard living, long residence in warm climates, without, however, affections of the liver, and by protracted chronic diseases; more especially, if the disorder be such as by humoral pathologists would be attributed to an acrid or attenuated state of the blood.' He had an opportunity of seeing a gentleman, a foreigner, who had undergone a course of it, to remove a cutaneous affection, said to have been similar to St. Anthony's fire. It had affected, at different times, almost every part of the body, going from one place to another, but had been particularly obstinate in one leg. He took two doses a day, of half a pint each, one before breakfast, and one before dinner, by which his appetite was sharpened, and by the time he had finished his meal, a most pleasing state of half intoxication had come on, so that he was just able to go to his couch, where he enjoyed a sound and refreshing sleep. About the second or third week the eyes became suffused with blood, and the cuticle around them began to peel; and then the whole surface of the body assumed the appearance above described. The first dose is continued for a week, or so, according to the disease, and then gradually left off. The skin clears at the same time, and the whole system is highly benefited.

Mr. Collie administered the ava, and had an opportunity of seeing its first effects upon a man affected with chronic superficial ulceration, affecting the greater part of the toes, and the anterior part of the soles of the feet; and from what he observed, he had no hesitation in saying, that if he could have procured and applied a suitable dressing for the ulcers, with appropriate support to the œdematous extremities, the plan which he adopted would have succeeded; and even with these disadvantages, he was inclined to think that a cure would be effected if the man abstained from liquor*.

* Beechey's Voyage to the Pacific and Behring's Straits, Part II. p. 434.

8. Mexican Domestic Bees. (Melipona Beechei.)

Captain Beechey, when at Xalisco, obtained two hives constructed by these bees, which he brought to England in H. M. S. Blossom. One of them has been presented to M. Huber, and the other to the Linnean Society. They are formed of hollow trees, a portion of which, of between two and three feet in length, has been cut off, and a hole is bored through the sides into the hollows at about the middle, and the ends of the hives stopped up with clay. These hives are usually suspended on a tree in a horizontal position, with the opening into the cavity directed also horizontally, and are speedily taken possession of by the bees. Their interior arrangement differs materially from that of the European bee, some of the layers of the comb assuming a vertical and some a horizontal position, the cells of the latter being most numerous. All the combs, both vertical and horizontal, are composed of a single series of cells applied laterally to each other, and not, as in the European bee-hive, of two series, the one applied against the extremities of the other. The cells appear destined solely for the habitation of the young bees. The combs are placed together, at some distance from the opening of the hives, and surrounding them are several layers of wax, as thin as paper, irregular in their form, and placed at some little distance from each other: externally to these are placed the sacs for containing the honey, which are generally large and rounded in form. They vary in size, some of them exceeding an inch and a half in diameter. They are supported by processes of wax from the wood of the cavity, or from each other, and are frequently placed side by side; but their disposition is altogether irregular, and bears some resemblance to that of a bunch of grapes. Some of the honey sacs are placed apart from the others, and form a distinct cluster.

From this irregular position of the honey sacs, a most important advantage is gained by the cultivators of the Mexican hive bee, as, in order to possess themselves of the honey, all that is necessary is, to remove the plug from the end of the cavity employed as a hive, and to introduce the hand and withdraw the honey. The store of the laborious bee is thus transferred to the proprietor of the hive, without injuring, and almost without disturbing, its inhabitants. The end of the hive is then again stopped up, and the bees hasten to lay in a fresh store of honey. A hive treated in this way affords, during the summer, at least two harvests.

The bee itself, by which this nest is constructed, is smaller than the European hive bee; its abdomen especially being much shorter. It is distinguished also from the European race of hive bees by the form of the first joint of its hinder tarsi, which is that of a triangle, with its apex applied to the tibia. Its technical characters are intermediate between the two genera *melipona* and *trigona* of M. Latreille, one of the mandibles being toothed, and the other nearly

entire. It has a leaning towards the *trigona*, but its general appearance is entirely that of a *melipona*, approaching very closely to that of *melipona favosa*, Latr., *apis favosa*, Fab.

Some curious anecdotes are related by the possessors as to the manners of these bees; one of which deserves to be recorded. They assert, that at the entrance of each hive a sentinel is placed to watch the outgoings and incomings of his fellows. and that this sentinel is relieved at the expiration of twenty-four hours, when another assumes his post and duties for the same period. Of the duration of this guard some doubts may be reasonably entertained; but of its existence ample evidence has been obtained by repeated observation. At all times a single bee was seen occupying the hole leading to the nest, who, on the approach of another, withdrew himself within a small cavity apparently made for this purpose on the left hand side of the aperture, and thus allowed the passage of the individual entering or quitting the hive, the sentinel constantly resuming his station immediately after the passage had been effected. During how long a time the same individual remained on duty could not be ascertained; for, although many attempts were made to mark him by introducing a pencil tipped with paint, he constantly eluded the aim taken. With the paint thus attempted to be applied to the bee the margin of the opening was soiled, and the sentinel, as soon as he was free from the annoyance he suffered from the thrusts repeatedly made at his body, approached the foreign substance to taste it, and, evidently disliking the material, he withdrew into his hive. A troop of bees was soon observed to advance towards the place, each individual bearing a small particle of wax, or of propolis, in his mandibles, which he deposited in his turn upon the soiled part of the wood. The little labourers then returned to the hive, and repeated the operation until a small pile rose above the blemished part, and consequently relieved the inhabitants from the annoyance*.

9. Mean Meteorological Results.

M. Bouvard, from the examination of more than a hundred thousand observations, made at the Royal Observatory of Paris, and embracing a period of eleven years, deduces the following conclusions: —The mean height of the barometrical variations is $0^{mm}.15$.

That from nine o'clock in the morning till three in the afternoon, the variation from the mean height is about double of that from three to nine in the afternoon. Hence it is necessary, in determining the mean pressure of the atmosphere, to pay attention to the times of observation. During the months of February, March, and April, the variation from the mean is greater than in the months of November, December, and January; and that during the rest of the year the barometer, between the hours of nine A. M. and three P. M., only suffers slight oscillations about the mean height. The height

* Beechey's Voyage, App. p. 613.

of the barometer is also affected by winds from different quarters; and by taking the mean of the heights when the winds blow from quarters diametrically opposite, nearly the same result is obtained as when the atmosphere is tranquil, the one depressing the barometer as much as the other raises it. M. Bouvard has found that the tides caused in the atmosphere by the action of the moon, and consequently the variations in the barometer due to that cause, are so small, that they may, in all cases, be entirely neglected. The author also gives the mean temperature of the year, and registers the kind of weather which takes place, which the curious English reader may wish to compare with the climate of London. At Paris there are 182 days in which the sky is covered with clouds, 184 in which the sky is partially covered with clouds; 142 days it rains; 58 days of frost; 180 of foggy weather; 12 of snow; 9 of hail, and 14 days of thunder, &c.

The mean quantity of rain on the observatory is 482 millemetres (18.97 inches), and in the court of the observatory, 24 metres (75.4 yards) below the platform, 365 millemetres (14.37 inches)*.

10. Climate of England.

In a paper recently published in the Transactions of the Horticultural Society of London, Mr. Knight says that he entertains no doubt whatever but that our winters are generally a good deal less severe than formerly,—our springs more cold and ungenial,—our summers, and particularly the latter parts of them, as warm, at least, as they formerly were, and our autumns considerably warmer. In accounting for these changes, our author observes, that within the last fifty years, very extensive tracts of ground, which were previously covered with trees, have been cleared, and much waste land has been inclosed and cultivated; and by means of drains and improvements in agriculture, the water from the clouds has been more rapidly carried off. From these circumstances, the ground becomes more dry in the end of May than it was formerly, and it consequently absorbs and retains much more of the warm summer rain than it did in an uncultivated state; and as water in cooling is known to give out much heat to surrounding bodies, much warmth must be communicated to the ground, and this cannot fail to affect the temperature of the autumn. The warm autumnal rains, in conjunction with those of summer, operate powerfully upon the temperature of the winter, and, consistently with this hypothesis, Mr. Knight asserts that he has observed, that during the last forty years, when the summer and autumn have been very wet, the succeeding winter has been mild; and that when north-east winds have prevailed after wet seasons, the winter has been cold and cloudy, but without severe frost, probably owing to the ground upon the opposite shores of the continent being in a state similar to that on this side the Channel.

Supposing the ground to contain less water in the commence-

* Mem. de l'Académie, tom. x. p. 9.

ment of winter, on account of the operation of the drains and improvements before mentioned, more of the water afforded by dissolving snows and cold rains in winter will necessarily be absorbed by it; and in the end of February, however dry the ground may have been at the winter solstice, in the end of February, it will almost always be found saturated with water; and as the influence of the sun is as powerful on the last day of February as on the 15th of October, and it is the high temperature of the ground in the latter period which occasions the difference of temperature in those opposite seasons, Mr. Knight thinks it cannot be doubted, that if the soil be rendered more cold by the absorption of water at nearly the freezing temperature, the weather of the spring must be, to some extent, injuriously affected *.

11. On the Earthquake at Odessa on the 26th of November, 1829.—(*M. Hauy*).

On the 14th (26th) of November, 1829, at 3h. 58 minutes in the morning, M. Hauy was awakened by slight vibrations, which appear to have been the beginning of the earthquake. They increased during two-thirds of a minute, and then a severe shock ensued, which lasted some seconds. The vibrations then abated, but increased again for a minute, when a second very severe shock, which was much stronger than the first, was felt. After this, the motion decreased at first, and was next again about twelve or fifteen seconds on the increase, when the third shock took place. It was weaker than the first, and lasted only some moments. A new interval followed, in which a diminution of the oscillatory motion was again succeeded by an augmentation of it, which had lasted about a quarter of a minute, when the fourth and last shock took place, which was equal in intensity to the third, and lasted about three or four seconds. The tremulous motion, although constantly decreasing, continued about one minute and a half longer. At 4h. 2′ 2″ every thing was again at rest; during the four minutes which the earthquake lasted, the trembling motion continued without interruption. The cracking of a wooden partition in the bed-room enabled M. Hauy to count 152 complete oscillations in the space of thirty seconds. He frequently observed the barometer during the phenomenon, but could not discover any change; neither did M. Challaye, the French Consul, who observed the barometer almost without intermission, observe any motion in the mercurial column. During a good part of the night, the weather was calm, the sky overcast, and the thermometer at 0° R. (32° F). One of M. Hauy's friends, Dr. Hennan, saw, to the eastward, a strong and very distinct brightness in the sky, analogous to an aurora borealis, veiled, however, by the clouds which covered the atmosphere. The visible maximum of the intensity of this light was at the horizon;

* Trans. Hort. Soc. Lond., vol. vii. p. 4.

it disappeared almost instantaneously, five or six minutes after the termination of the earthquake. An engineer of the name of Chatillon communicated to M. Hauy the following observation. He had in his room a decanter half full of water, the inside of which, above the water, had been covered with vapour, in consequence of the lowering of the temperature during the night. This vapour had disappeared in all the places which the water had touched during its oscillations, and thus M. Chatillon was enabled to ascertain the direction as well as the magnitude of the oscillations which had taken place in the water. He found that the axis of the oscillations had never changed; and that the direction of the oscillations was parallel to a vertical plane, whose azimuth was 2° east of north, while he found the variation of the needle 10° west.

The following is an extract from the meteorological register kept by M. Challaye, for the two days preceding and following the earthquake here described.

	Time.	Barometer. Eng. inches.	Therm. Réaumur.	Wind.	Weather.
24	9 A.M.	29.8	3½	N.E.	Cloudy.
,,	3 P.M.	29.85	2	N.E.	Fine.
,,	9 P.M.	29.9	3½	N.E.	Dull, slightly overcast.
25	9 A.M.	30	2	N.E.	Cloudy.
,,	3 P.M.	30	½	N.E.	A little cloudy.
,,	9 P.M.	30.05	0	N.E.	Overcast, calm.
Earthquake		30.05	0	. .	Perfect calm.
26	9 A.M.	30	+1	S.E.	Overcast.
,,	3 P.M.	29.95	+1½	E.S.E.	Rain, mixed with snow.
,,	9 P.M.	30	0	E.N.E.	Heavy fall of snow.
27	9 A.M.	30	−1½	E.N.E.	Cloudy.
,,	3 P.M.	30.05	−1	E.	Cloudy.
,,	9 P.M.	30.05	−1½	N.E.	Thick snow, violent wind.

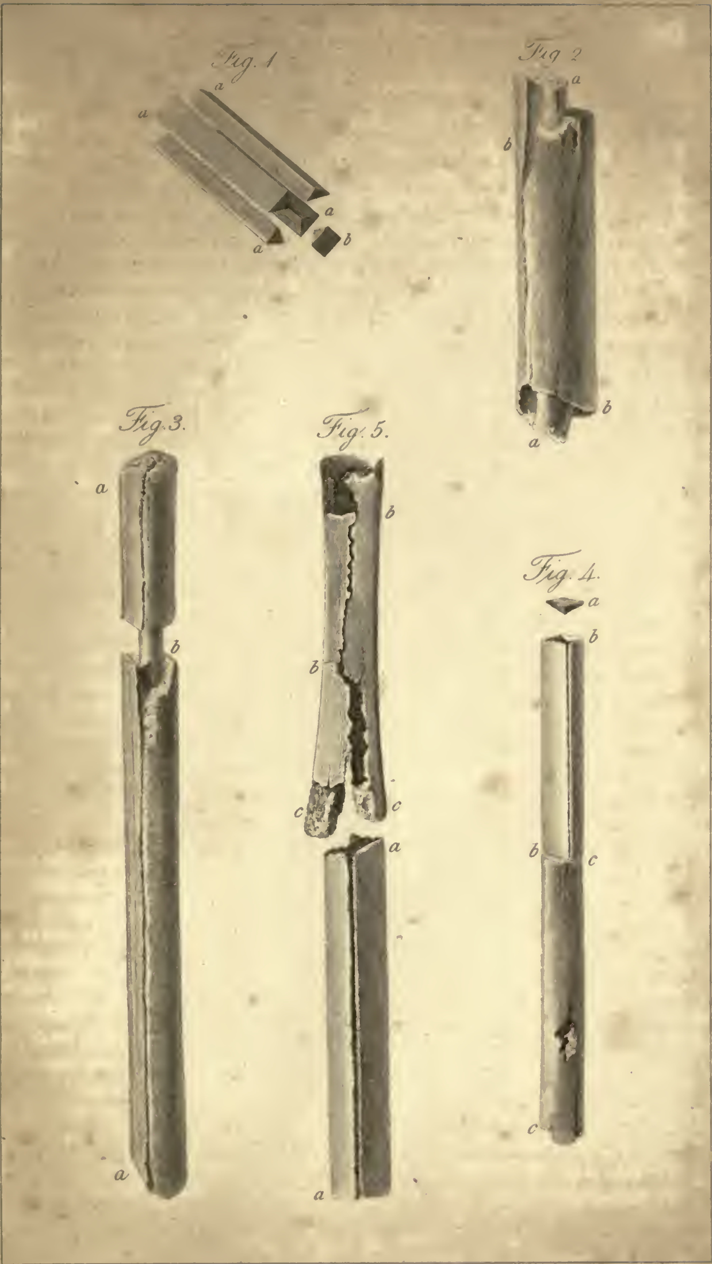
12. Geography of Siberia.

M. Hansteen has determined the longitude of the town of Yenisseisk in Siberia, by astronomical observations, to be 92° 10′ 59″ east of Greenwich. He has connected, by the transposition of two chronometers, other places with this town, and thus determined their geographical position, of which we give the following ones:—

Places.	Latitude.	Longitude.
Town of Yenisseisk . . .	58° 27′ 19″	92° 10′ 59″ East.
Mouth of the Elotchikha .	61 29 51	90 11 0
Town of Touroukhansk . .	65 54 56	87 32 25 *.

* Petersburgh Transactions, 1831.

Plate I.



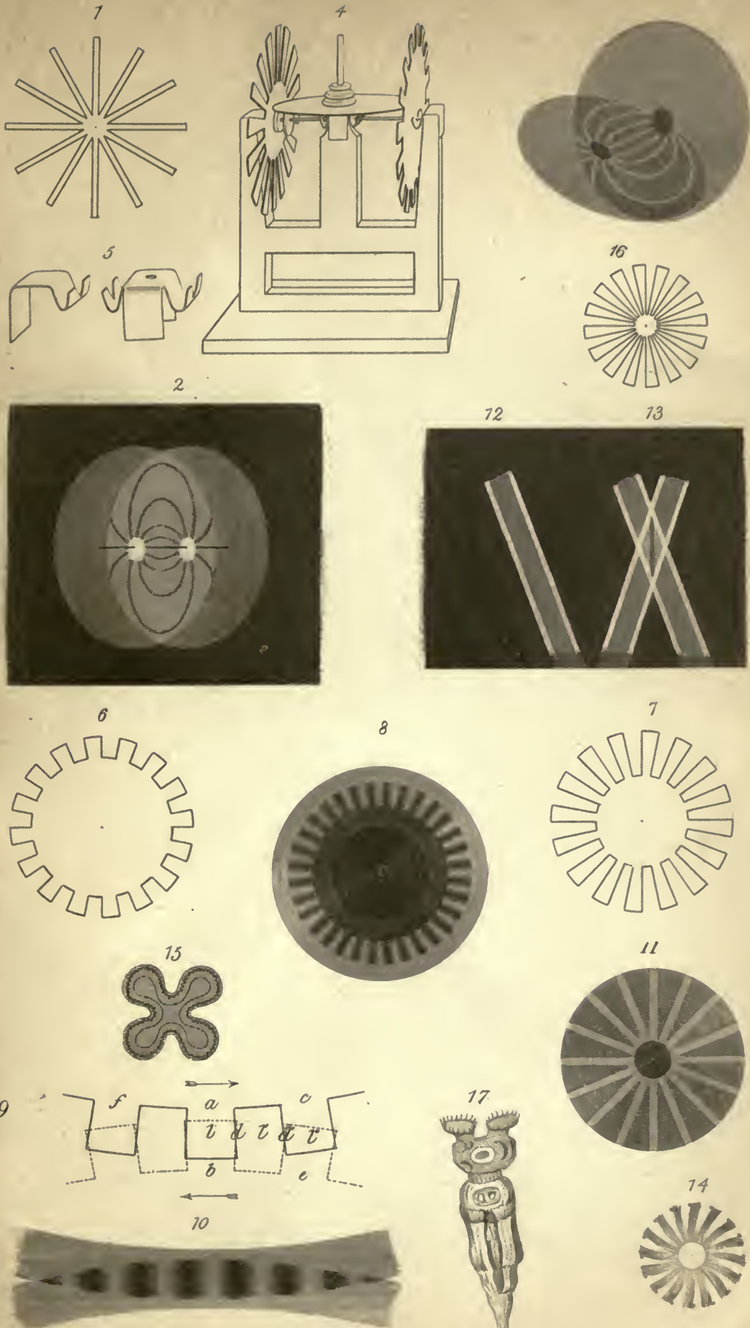
J Basire del et sculp

Action of Mercury upon Tin bars.

Published by John Murray 183[illegible].

Plate VI.

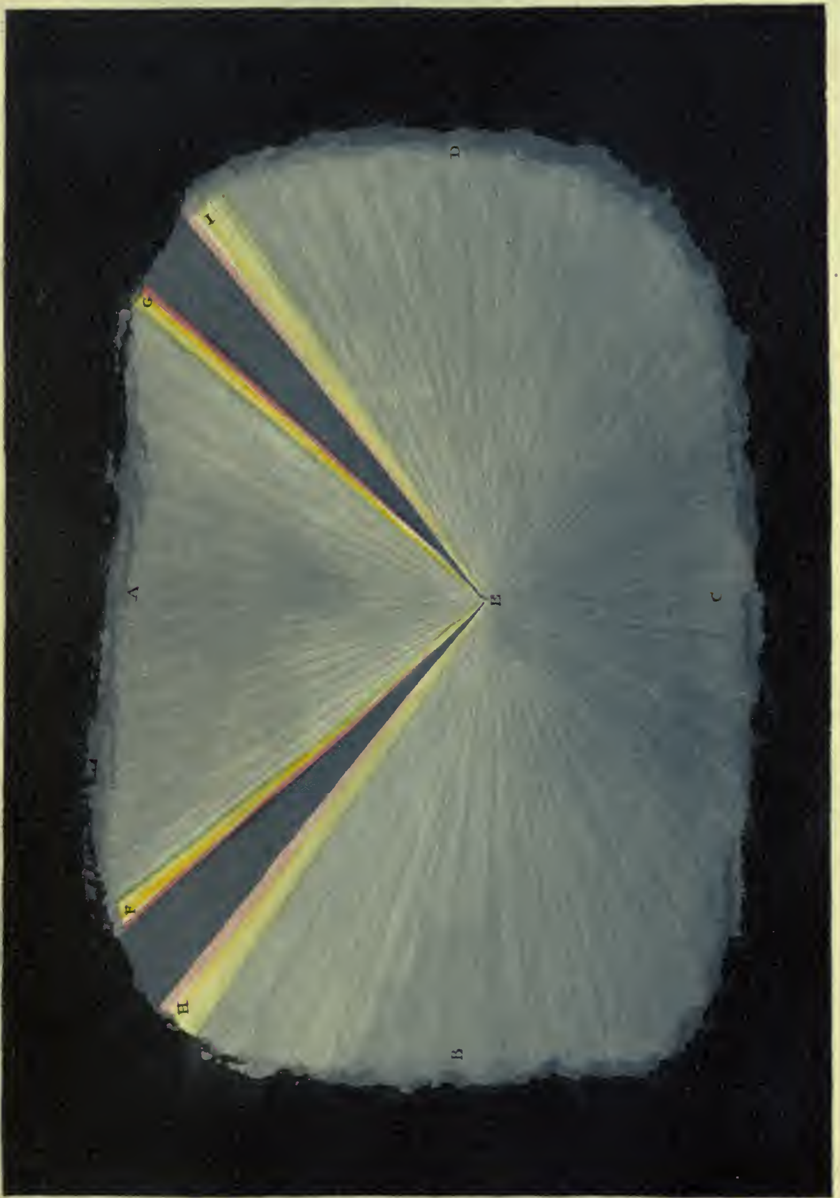
Published by John Murray 183[illegible]



London, Published by John Murray, Jan.y 1831.

Pl. 4.

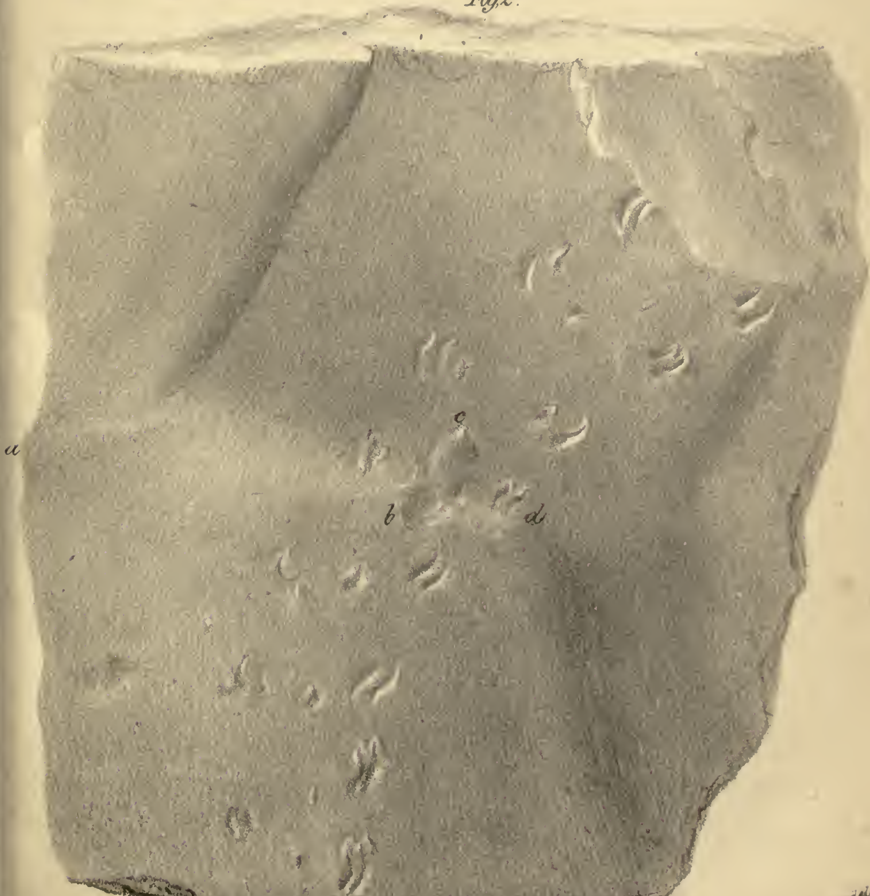
THE RAIN-BOW.



London. Published by John Murray. January. 1831.

Fig. 1

Fig. 2.



harf. del et lithog.

London, Published by John Murray, 1831

Printed by C. Hullmandel

INDEX.

Academy of Sciences in Paris, proceedings of, 558.
Acid, account of pyrophosphoric, 167.
—— hydrocyanic, produced under uncommon circumstances, 169.
—— malic, preparation and composition of, 178.
—— para-tartaric, 395.
—— nitric, distillation of, 402.
Aconitum ferox, account of the poison obtained from the, 365, 366.
Acoustic rainbow, 375.
Action of chlorine on carburetted hydrogen, 169.
—— of the galvanic pile upon living animal substances, 186.
—— of mixed nitrate and muriate of ammonia on glass, 385.
—— of oil of turpentine on the nervous system, 565.
Acupuncturation of the arteries, 565.
Adriaansz's claim to the invention of telescopes, 320.
Aëorolite, account of one that fell in Georgia, in 1829, 415.
Affections of the vocal organs, 565.
Agricultural labours of the ancients regulated by the rising of remarkable stars, 459.
Ahrweiler, account of the mode of vaulting used at the church at, 225.
Ainger, Mr., on the darkness between the primary and secondary rainbows, 281—remarks upon the theory of M. Biot, 282—geometrical illustrations, *ibid*.—the phenomena more clearly explained, 291.
—— on machinery employed in pencil making, 555.
Ainsworth's, Mr., observations on Mr. Rennie's paper relating to the cleanliness of animals, 261.
—— remarks on the ages of rocks, 547.
Air, changes effected in the atmospheric, by leaves, 93.
—— change of, in eggs, during incubation, 435.
Aire, quantity of gaseous bodies in the waters of the, 45.
Almond, Mr., on the mode of regulating the supply of water between rivulets and canals, 307.
Alloys of lead and tin, action of mercury upon, 1.
—— —— peculiar property of, 404.
Alumine, remarks upon its power of purifying water, 41.
Amalgamation, mode of reducing metals by, 145.
Amici, M., on the growth of vegetables, 422.
—— experiments on the leaves of celadine, 560.
Ammonia in native oxide of iron, 174.
—— compounds of, with anhydrous salts, 620.
Amusat's, M., cases in which the twisting of arteries had been performed with success, 564.
Analysis of books, 142, 337, 571.
—— of chloride of gold, and potassium, 409.
—— —— and sodium, 410.
—— of a plant, 87.
Anatomical investigation of the structure of the eyes in insects, 152.
Andrieux's, Dr., improved galvanic apparatus, 564.
Animal poisons, cure of, by the application of common salt, 189.
—— substances, action of the galvanic battery upon living, 186.
Animals, on the tract of, in the forest marble, 538.
Animalculæ, existence of, in snow, 193.
Ants, care of, to secure warmth, 517.
Apatite, electric experiments upon, 79.
Ape, remarks upon the Barbary, 500.
Apophillite, analysis of, 443.
Apparent hydrostatic anomaly with laurel oil, 161.
Apples, russet in, 638.
Arago's, M., observations on the aurora borealis, 558.
Ararat, Parrot's expedition to, 419.
Aratus, remarks upon, relating to the rising of the Dog-star, 471.
Argonautic expedition, observations upon the, 62.

Arsenic, a good test for, 173.
Arteries, twisting of the, 564.
Atmosphere, tides in the, 559.
Atmospheric phenomena, described by Professor Strehlke, 432.
Atomic weight of titanium, 175.
Ava root, medicinal use of the, 639.
Aubert, M., on the spontaneous inflammation of charcoal, 617.
Aurora borealis, account of an irised, 198.
——— by Mr. Christie, 262.
——— influence of, on the magnetic needle, 429.
——— account of, by Dr. Moll, 519.
——— observations upon the, by the Hon. C. Harris, 522.
——— by M. Arago, 558.
——— height of the luminous arch above the earth's surface, 525.

B.

Baker's account of the wheel animalcula, 221—remarks on the apparent circular motion, 222.
Barometer, improved mountain, 555.
——— new construction of a, 601.
Battery for electro-magnetic purposes, remarks upon the construction of a, 35.
Becquerel's error in estimating the conducting power of wires, 36.
——— on the electrical state of bodies by the action of heat, 568.
Bees, account of Mexican domestic, 640.
Beltrami's, M., account of a two-headed lizard, 570.
Bennati, on the mechanism of the human voice, 185.
——— on the affections of the vocal organs, 565.
Berzelius's remarks upon para-tartaric acid, 395.
——— method of preparing urea, 401.
——— on the combination of chloride of gold, with chloride of potassium and sodium, 409.
——— test of the protoxide and peroxide of iron, 624.
Bevan, on the compression of lead, 157.
——— on the power of horses, 159.
Bi-carbonate of soda, mode of preparing, 385.
Bicheno, Mr., on the plant intended by the shamrock of Ireland, 453.
Biot, M., on remarks upon his theory of rainbow, 282.
——— his calculation of the commencement of the Egyptian year, 473.
Birds, peculiar cleanliness of, 25.
——— of prey, on the vision of, 192.
——— their care to secure warmth, 505.
Bismuth, crystallization of, 393.
——— and its alloys, expansion of, during congelation, 411.
Blood, mode of preserving, 398.
——— presence of manganese in, 399.
Bodies, change of volume in, when they combine together, 160.
Bonijol's apparatus for decomposing water by atmospheric electricity, 376.
——— for decomposing potash, *ibid.*
Botany of India, by Dr. Wallich, analysis of the, 360.
Bonnycastle's, Captain, observations on the phosphorescence of the sea, 194.
Boullay, M., on the change of volume in bodies when they combine together, 160.
——— on ulmin, 179.
Boussingault, M., on ammonia in native oxide of iron, 174.
Bowdoin's, Mr., account of an irised aurora borealis, 198.
Braconnet, M., on caseum and milk, 181.
Brande, Mr., on the electro-chemical decomposition of the vegeto-alkaline salts, 250.
——— on the relation of the vegeto-alkalies to the common alkalies, and to certain proximate principles of vegetables, 547.
Brewster, Dr., on the laws of elliptic polarization, as exhibited in the action of metals upon light, 340.
Bromic and chloric acids, action of alcohol upon, 615.
Bromide of carbon, mode of preparing, 171.
Brongniart, on the smut in corn, 420.
——— on the structure of leaves, 421, 636.
Brown's, Lieutenant, experiments on the stiffness and strength of timber, 599.
Browne's moving molecules, easy mode of exhibiting, 369.
Bullet, mode of preventing its discharge from a gun, by the finger, 368.
Burnett, Mr., on the development of the several organic systems of vegetables, 83—remarks upon the functions of plants, 84—analysis of a plant, 87—remarks on the death of trees, when the roots are too thickly covered with earth, 89—observations on the motion of the sap in plants, 90—changes

produced in atmospheric air by leaves, 95.
Bussy's, M., method of obtaining magnesium, 562.

C.

Cadmus, remarks upon the origin of the fable, 60.
Calculous diseases, on the tendency to, by Dr. Yelloly, 346.
Cambium, remarks upon, in the formation of wood and bark, 478.
Camphor, composition of, 631.
Canals, mode of regulating the supply of water between rivulets and, 307.
Caseum and milk, memoir on, 181.
Cat, natural cleanliness of the, 23.
—— mode of cleaning itself, *ibid.*
—— its care to secure warmth, 497.
—— difference between the wild and domestic, 499.
Cathedral at Cologne, remarks upon the vaulting of the choir, 235.
Cavalier, M., on discoloured chloride of silver, 393.
Cauchy, M. A. L., on the integration of differential equations, 596—on series, *ibid.*—on the moments of inertia of a solid, 597—on a system of molecules, *ibid.*—on the theory of light, *ibid.*—demonstration of the law relating to solids and fluids, 598—memoir on torsion, *ibid.*
Ceres, remarks upon the fabulous history of, 59.
Chameleon, its antipathy to black, 194.
Charcoal most suitable for the manufacture of gunpowder, 131.
—— new process in the manufacture of, 184.
Chemical notation, observations on the necessity of a, by Rev. W. Whewell, 437.
Chevalier, M., on the thermal water of Chaudes Aigues, 417.
Chloride of silver, remark upon the discoloured, 393.
—— of gold, and potassium, analysis of, 410.
—— and sodium, analysis of, *ibid.*
Chlorine, action of, upon carburetted hydrogen, 169.
—— an antidote to hydrocyanic acid, 188.
Chlorophane, electrical experiments upon, 77.
—— its phosphorescence restored by electricity, 78.
Cholera morbus, observations on, by Dr. Jahinichen, 567.
Christie, Mr., on the permanence of magnetism in steel bars, 243.
—— on the aurora borealis, 262.
—— on the height of a luminous arch of the aurora borealis above the surface of the earth, 525.
Chronology of the Egyptians, 458.
Circulation in vegetables, 424.
—— in plants, remarks on the, 561.
Clarke, Mr., on pyrophosphates, 167.
Cleaning instrument of the larva of the glow-worm described, 17.
Clement's experiment, easy mode of repeating it, 369.
Clemson's, Mr., mode of preparing piperin, 395.
Climate of England, remarks upon the, by Mr. Knight, 642.
Clover (*Trifolium repens*), not the original emblem of Ireland, 453, 454.
Cold, never sufficiently intense to stop the evaporation of water, 70.
Coloured bands, experiments on, by Mr. Quitelet, 164.
Columbine, a new vegetable principle, 630.
Comet, account of a, by Mr. Dabadie, 241.
Compression of fluids, experiments upon the, 375.
Contributions to the physiology of vision, No. I., 101; No. II., 534.
Copper and zinc, voltaic action produced from a few grains of, 32.
Coriolis's, M., experiments on the compression of lead, 158.
Coxe's, Dr., mode of preparing phosphuret of lime, 173.
Cowper, Mr., on recent improvements in paper making, 552.
Cuticular pores of plants, 419.
Cutting instruments, new mode of setting, 13.
Cuvier's theory of vision, 536.
Cyanogen in the blood, 186.

D.

Dabadie's, M., account of a new comet, 241.
Dahlias, new mode of multiplying, 424.
Danaides, remarks upon the fable of the, 68.
Daniell, Mr. J. F., on the phenomena resulting from the action of mercury upon different metals, 1—action of mercury upon an alloy of lead and tin, *ibid.* —remark upon the nature and result

of this experiment, 2—upon pure tin, *ibid.*—upon lead, 3—upon zinc, *ibid.* —upon silver, 4—upon gold, *ibid.*—upon tin, 5, 6, 7—remarks upon hammering metals, 6—experiment with spongy platinum, 10—general observations upon the process and results of these experiments, 11.

Daniell, Mr. J. F., on a new register pyrometer, 338.

Davy, Sir Humphry, analysis of the life of, by Dr. Paris, 347—account of his birth and family, *ibid.*—education, *ibid.*—his first experiments in chemistry, 348—appointed chemical assistant to the Pneumatic Institution of Bristol, 349—interesting experiment with two pieces of cane, 351—new galvanic experiments, 352—appointed assistant lecturer in chemistry to the Royal Institution, *ibid.* — galvanic battery without metallic substance, 853—appointed professor of chemistry to the Royal Institution, 354—elected Fellow of the Royal Society, 356— —decomposition of the alkalies, 358 —his chloridic theory, 572—his marriage, 573—Mr. Faraday's first introduction to him, 575—invention of the safety-lamp, 577—death, 583—comparison of the characters of Davy and Wollaston, 583.

Daubeny, Dr., on the occurrence of iodine and bromine in mineral waters of South Britain, 337.

D'Aubuisson, M., on the resistance opposed to water moving in pipes, 157.

Degree, the exact measure of a, 370.

De Lassaux's mode of erecting light vaults over churches and similar spaces, 224.

Detonating matches, Dr. Ure on, 140.

Diamonds made phosphorescent by heat, after electrization, 32.

Dipping needle, improvement in the construction of the, 608.

Domes, account of, erected at Vienna, without centering, 224, 225.

Donati, Dr., on the phenomena observed during the last eruption of Vesuvius in 1828, 296 — sudden shock and eruption, 298—discharge of liquid lava, 300—explosion of gas, 304—vertical section of the great cone, 306.

Doolittle, Mr., account of the manufacture of charcoal by, 184.

Drowning, restoration from, by insufflation of the lungs, 190.

Drummond, Lieutenant, on the illumination of light-houses, 344.

Duberga, M., on the power of carbon to destroy bitterness in certain bodies, 619.

Dumas, M., on oxamide, 382.

Duperrey, L. J., on the figure of the magnetic equator, 607.

Du Petit Thouars, remarks upon his theory of the growth of wood, 479, 480.

Dutrochet's remarks upon the circulation in celadine, 561.

E.

Eggs, change of air in, during incubation, 435.

Egyptian chronology, by Professor Renwick, 458 — agricultural labours of the ancients regulated by the heliacal rising of remarkable stars, 459—earliest settlement of a colony in Egypt, 462—Sothic period, 463 —Chaldean records used by Ptolemy, 464 — remarks upon the origin of letters, 466—chronologies compared, 468.

Elastic fluid, remark upon the velocity of an, 599.

Elaterium, new principle obtained from, 532.

Electrical accumulation, laws of, 380.

——— relation of bodies to heat, 568.

Electricity increased by a double copper plate, 34.

——— of the winds, 198.

——— effects of, upon fluor spar, 271.

Electro-magnetic telegraph, remarks upon an, 37.

——— magnet, account of a powerful, by Professor Moll, 379.

——— magnets, account of powerful, by Professor Henry and Dr. Ten Eyck, 609.

Elevation of the Morea, 563.

Elk, description of the horns of the Prussian, 118.

Elliptic polarization, laws of, as exhibited in the action of metals upon light, 340.

Elm-trees, cure of wounds in, 200.

Emmett, Professor, on iodide of potassium as a test for arsenic, 173.

——— on the preparation of nitrogen, 384.

English yard, proportion between the, and French metre, 599.

Erman, M., on the direction and intensity of the magnetic force at St. Petersburgh, 604.

Eye, surgical recovery of an, 191.

Eye globules in the humours of the, 185.

F.

FABULOUS history of Greece, elucidation of some portions of the, by Mr. Sankey, 57.
Faraday, Mr., on the limits of vaporisation, 70.
——— on a peculiar class of optical deceptions, 205, 333, 334.
——— on wheel animalculæ, 220.
——— on Clement's paradoxical experiment, 369.
——— his introduction to Sir H. Davy, 575.
——— his condensation of the gases, 579, 580.
Feathers, mode of restoring the elasticity of, when damaged, 427.
Fern owl (*Caprimulgus Europœus*), description of the cleaning instrument of the, 21.
Fischer, Rev. J., on the cure of animal poisons by the application of common salt, 189.
Flea, remarks upon the care of the, to secure warmth, 518.
Flies, remarks upon the construction of their feet, 24.
Fluids, experiments on the compression of, 375.
——— on the equilibrium of, 595.
Fluor spar, electric experiments upon, by T. Pearsall, 81.
——— effects of repeated electric discharges upon, 270, 271.
Flourens, M., experiments on the action of oil of turpentine upon the nervous system, 565.
Force of terrestrial magnetism, 374.
Fox, Mr., on the electro-magnetic properties of the metalliferous veins of Cornwall, 345.
——— on the discharge of a jet of water under water, 368, 599.
Fredonia, village of, lighted by natural gas, 203.
French method of purifying nitre, 123, 124.
Fruit-trees, mode of preserving, from the bites of hares, 200.
Fruit, progress of maturation in, 559—mode in which the saccharine matter is produced, 560.
Fuch's, M., mode of extracting potash from feldspar, 184.

G.

GALVANIC currents during the decomposition of water, 166.
Galvanic pile, action of the, upon living animal substances, 186.
——— apparatus improved, for medical purposes, 564.
Galvanism, on the application of, 566.
Galvanometer, description and use of the, in electro-magnetic researches, 29.
——— experiments with, 32.
Gamboa, F. X. de, on the mining ordinances of Spain, 142.
Gas, village lighted by natural, 203.
Gases, emission of light during the compression of, 381.
Gay Lussac, M., on the absorption of oxygen by silver at high temperatures, 627.
Geoffrey de St. Hilaire, M., on the fossil remains of the Teleo Saurus, 569.
Gergonne, M., on the apparent projection of stars upon the moon's disc, 163.
Gipsies, hardihood of, 505.
Glass, elasticity of the threads of, 29, 556.
——— mode of preparing, 31.
——— method of perforating, 633.
Glow-worm, cleanliness of the grub of the, 15—description of its cleaning instrument, 17—proved to be carnivorous, 20—remarks upon the light, 21.
Gold, action of mercury upon, 4.
——— on the crystallization of, by Professor Henslow, 176.
——— on the composition of fulminating, 394.
——— on chloride of, and potassium, by Berzelius, 409.
——— and platina district of Russia, 418.
——— and silver, produce of, in the Russian empire, 434.
Gothic architecture most appropriate for churches, 224.
Greece, fabulous history of, 57.
Grew's experiment on the formation of wood, 477.
Growth of vegetables (Amici), on the, 422.
Gruithnisens, Dr., anatomy of the Nais diaphana, 593.
Guijot and Admyrauld, MM., on the seat of the sense of taste, 425.
Gunpowders and detonating matches, Dr. Ure on the manufacture of, 121.
——— table of different compositions, 136.

H.

HAIL, remarks upon the theory of the formation of, 415.

Hall's, Dr. M., observations on Dr. Arnott's explanation of the nature of stammering, 253.
——— on the mechanism of vomiting, 265.
Hammering, remarks upon the, of metals, 6.
Hancock, Dr., upon an apparent hydrostatic anomaly with laurel oil, 161.
Harris, Mr., on the laws of electrical accumulation, 380.
——— on the power of various substances to intercept magnetism, 549.
——— Hon. Charles, on the aurora borealis, 522.
Hayes, A. A., on the production of prussic acid under uncommon circumstances, 169.
Hennell, Mr., on elatinum, 532.
Henslow, Professor, on the crystallization of gold, 176.
Hermes, remarks upon the fable of, 65.
Herschell, Mr., observations upon the utility of his chemical nomenclature, 440, 442.
Herodotus, remark upon a passage in, by Professor Renwick, 459.
Hieroglyphic system, observations upon the, by Professor Renwick, 458.
Home, Sir E., structure of the feet of flies first observed by, 24.
Houlton, Mr., bulbous root of great antiquity produced by, 196.
——— market state of hyosciamus by, 196.
Horns of the Prusian elk and American moose deer, 118.
Horses, on the power of, 159.
Horus Apollo, remarks upon the antiquity of, 470.
Huber Burnand, M., on the snow in the winters 1829 and 1830, 196.
Human voice, memoir on the mechanism of the, in singing, 185.
Humboldt's, M., account of the gold and platina district of the Russian empire, 418.
——— on the produce of gold and silver in the Russian empire, 434.
——— Map of heights, account of, 563.
Hybernating animals, observations on the blood vessels of the head of, 585.
Hyosciamus, market state of, 196.

I.

Ichthyology, 429.
India, mirage of Central, 201.
Indian birds, notice of Mr. Gould's collection of, 428.
Indigo, new kind of, 397.
Influence of the age of parents upon the sex of children, 199.
Insects, on the structure of the eyes of, 152.
——— on a peculiar system of visceral nerves in insects, analogous to the sympathetic, 586.
Iodic acid, on the preparation of, 614.
——— precipitation of the vegeto-alkalies by, 615.
Iodide of potassium, a test for arsenic, 173.
Iodine, on the disorders arising from the long-continued use of, 187.
——— and bromine in mineral waters, 337.
Irised aurora borealis, account of an, 198.
Iron, persalts of, reaction of the, and neutral carbonates, 388.

J.

Jahn, Dr., on the use of iodine, 187.
Jahinichen on cholera morbus, 567.
Johnson, Dr., on the vision of birds of prey, 192.
———'s, Mr., experiments on steam generated by heated metal, 613.
Josephus, remarks upon his chronology, 469.
Journal of the weather at Madagascar, 50, 51.

K.

Kater, Captain, on the error in standards of linear measure, 343.
Kelkoa or planera tree, wood of the, recommended for useful purposes, 559.
Kemp, K. T., on the conducting powers of liquified gases, 613.
Knapp, Mr., remark upon his conjecture respecting the light of the glow-worm, 20—relating to the cleanliness of animals, 26.
Knight, Mr., on the means of giving a fine edge to razors and cutting instruments, 13.
——— remarks upon the climate of England, 642.
Kupffer, M., on the influence of the aurora borealis on the magnetic needle, 429.
——— new construction of a barometer, 601.
——— on the intensity of the earth's magnetism, 610.

L.

Lancets, new mode of giving a fine edge to, 13.

Languages, remarks upon the analysis of, 57.
Lassau's, M. de, description of a mode of erecting light vaults over churches and similar spaces, 224.
Laurel oil, apparent hydrostatic anomaly with, 161.
Lead, action of mercury upon, 3.
—— resistance of, to pressure, 157.
Leaves, M. Brongniart on the structure and functions of, 421, 636.
Leroux's, M., memoir on salicine, and its powers as a febrifuge, 177.
Letters, remarks upon, by Professor Renwick, 466.
Liebeg, M., on the preparation and composition of malic acid, 178.
——— on magnesium, 411.
——— on the composition of camphor and camphoric acid, 631.
Light-houses, on the illumination of, by Lieutenant Drummond, 344.
Light, emission of, during the compression of gases, 381.
—— of the glow-worm, remarks upon the, 20.
Limits of vaporization, 70.
Linear measure, error in standards of, 343.
Linley's, Mr., account of a remarkable instance of anomalous structure in the trunk of an exogenous tree, 476.
——— remarks upon the theories of the formation of wood, 477—nature of plants explained, 479.
Lippershey, Hans, the original inventor of the telescope, 324.
Lithia, mode of preparing, 386.
Lithotricity, remarks on, by Dr. Civiale, 564.
Lœwig, M., on the preparation of bromide of carbon, 171.
Lubbock's, Mr., researches in physical astronomy, 342.
Luetke's, Captain, pendulum observations, 602.
Luminous figures produced by rapid alternations of light and shade, 102—by pressure on the eye-ball, 104—by galvanism, 107—rings occasioned by lateral pressure, 115.
Lungs, insufflation of the, in cases of drowning, 190.
Lyall's, Mr., remarks upon the weather at Madagascar, 47—journal, 50, 51—general observations, 56.

M.

Madagascar, remarks upon the weather at, 47.
Magnesium, M. Liebeg on, 411.
——— mode of obtaining, 562.
Magnetic needles, mode of preparing for electro-magnetic experiments, 31.
——— curve, geometric properties of the, 311—description of an instrument for describing it, 315.
——— needle, dip of the, at St. Petersburgh, 604.
——— force, intensity of the, at St. Petersburgh, 604.
——— equator, figure of the, 607.
Magnetism, on the permanence of, in steel bars, 243.
——— power of bodies to intercept, 549.
——— intensity of the earth's, 610.
Majendie and Desmoulin's observations on the disappearance of luminous objects, 535, 537.
Magnus's, M., mode of preparing selenium, 619.
Malic acid, preparation and composition of, 178.
Manganese, method of ascertaining the value of the ores of, 293.
Marsh, Mr., on the mode of perforating glass, 633.
Marx, Professor, on the expansion of bismuth upon congelation, 411.
Maunoir's, M., account of the surgical recovery of an eye, 191.
Matteucci's, M., experiments on living animal substances, 186.
——— on the origin of the action in the voltaic pile, 612.
Maturation of fruit, 559.
Mental spectra, 114.
Mercury, action of, upon different metals, 1.
Metallic specula, quantity of light reflected by, 162.
——— rods, power of, to decompose water after the connexion with the pile is broken, 167.
Meteor and aërolite in Georgia, 415.
Meteoric stones, remarks upon a theory of the formation of, by Mr. Faraday, 72.
Meteorological results, 641.
Metre and English yard, proportion between the, 599.
Mica, use of, in minute chemical analysis, 633.
Microscope, first invention of the, 484.
Milk and caseum, memoir upon, 181.
Mineralogy, Professor Whewell's remarks upon the necessity of the employment of notation in, 437.
Mirage of Central India, 201.

Missol, Dr. F. de, on the use of salicine in intermittent fevers, 566.
Mitscherlich, M., on the distillation of nitric acid, 402.
Moll, Dr., on the first invention of telescopes, 319—continuation, 483.
——— account of a powerful electro-magnet constructed by, 379.
——— on the aurora borealis, 519.
Moon's disc, apparent projection of stars upon the, 163.
——— occultation of antares by the, 601.
Morin, M., on the action of chlorine upon carburetted hydrogen, 169—action of chlorine on alcohol, 170—upon ether, 171.
Müller, on the structure of the eyes in insects and crustacea, 152—on the eyes of the murex tritonis, 155.
——— on a peculiar system of visceral nerves in insects analogous to the sympathetic, 586.
Mure, Dr., on animalcula in snow, 193.

N.

Nais diaphana, anatomy of the, 593.
Nebulous striæ seen in the dark, 108.
Necker, M. L. A., on the relation between the general direction of the stratification of the earth and the lines of equal magnetic intensity in the northern hemisphere, 372.
Nervous system, action of oil of turpentine upon the, 565.
New remedy in pulmonary complaints, 565.
Nile, remarks upon the rising of the, 460.
Nitrate and muriate of ammonia, action of, upon glass, 385.
Nitre, mode of purifying, 121—French process described, 123.
Nitric acid, distillation of, 402.
Nitrogen, mode of preparing, 384.

O.

Occultation of antares by the moon, 601.
Œnometer or alchometer, account of the, by M. Tabarie, 629.
Oersted's, Professor, experiments on the compression of fluids, 375.
Opium, singular effect of, 426.
Optical deceptions, Mr. Faraday on a peculiar class of, 205.
Optic nerve, place of insertion of the, not entirely insensible to light, 109.
Ornithology, 427.
Otto, Dr., on the blood-vessels of the head of hybernating animals, 585.
Oxalamede, account of, by Mr. Faraday, 552.
Oxygen, absorption of, by silver at high temperatures, 627.

P.

Palaprat's, Dr., experiments on the application of galvanism, 566.
Paper, recent improvements in making, 552.
Para-tartaric acid, account of, 395.
Parents, influence of the age of, upon the sexes of children, 199.
Paris's, Dr., Life of Sir Humphry Davy, 347, 571.
Parrot's, account of, expedition to Ararat, 419.
Pearsall, Mr. T., on the effects of electricity upon minerals that are phosphorescent by heat, 77—table of results, 80.
——— further experiments on the communication of phosphorescence and colour to bodies by electricity, 267—on the influence of structure upon phosphorescent bodies, 274—on the coloration of fluor spars by the action of electricity, 277.
Pendulum experiments, 338.
——— observations, 602.
Perchloric acid, experiment upon, by M. Serullas, 563.
——— and its fossil formation, 616.
Perevoschtchikoff, M., on the formation of hail, 415.
Perrault's, M. J., new remedy for pulmonary complaints, 565.
Perseus, remarks upon the fable of, 63.
Persez and Nonah, MM., on chlorine as an antidote to prussic acid, 188.
Peschier's mode of preparing salicine, 397.
Petri, M., on the means of improving wool, 192.
Phillips, Mr., on submuriates of iron, 387.
Philosophical Transactions for the Year 1830, Part II., extracts from the, 337.
Phosphorescence given to bodies by electricity, 268.
Phosphorus, pulverization of, 385.
——— inflammation of, by charcoal, 385.
Phosphuret of lime, mode of preparing, 173.
Piorry, M., on restoration from drowning, 190.
Piperin, mode of preparing, 395.
Plants, nature of, explained, 479.
Platina, action of mercury upon, 10.
Potatoes, precautions in planting, 199.
——— preservation of, when frozen 200.

Potter, Mr., on the quantity of light reflected by metallic specula, 162.
Potash, mode of obtaining, from felspar, 184.
Poisson, M., on the equilibrium of fluids, 595.
——— on the roots of transcendental equations, 596.
Prevost's, M., experiments on an appearance of decomposition of white light, by the motion of the body which reflects it, 535.
Purkinje's Essay on the Subjective Phenomena of Vision, analysis of, 102.
Pyrometer, account of a new register, by Mr. J. F. Daniell, 338.
Pyrophosphoric acid, and the pyrophosphates, remarks upon, 167.

Q.

Quesneville's, Fils, mode of preparing lithia, 386.
——— process for the crystallization of bismuth, 393.
Quetelet's, M., experiments on coloured bands produced by plane mirrors, 165.
Quicksilver employed in reducing ores, 145.
——— mines in New Spain, 150.

R.

Radish-root, remarks upon the structure of the, 638.
Rainbows, remarks upon the darkness between the primary and secondary, 281.
Razors, new mode of setting, 13.
Rennie, Mr., on the peculiar habits of cleanliness in some animals, and particularly the grub of the glow-worm, 15—description of its cleaning instrument, 17—proved to be carnivorous, 19—remarks upon its light, 20—cleanliness of the cat, 23.
——— on the contrivances of some animals to secure warmth, 496.
Renwick, Professor, on the earliest epoch of Egyptian chronology, 458—remarks upon the rising of the Nile, 460—earliest settlement of a colony in Egypt, 462—remarks upon the chronology of Josephus, 469.
Retina, its central vessels rendered visible, 110—explanation of this phenomenon, 111—its more minute vessels rendered visible, *ibid.*—its central point apparently free from vessels, 112—its insensibility to feebly illuminated objects when continuously presented, 534.
Ribers and Donne, MM., on the globules in the humours of the eye, 185.
Rich, Dr., on the use of nitrogen in respiration, 186.
Ritchie's, Mr., description and application of a torsion galvanometer, 29—experiments showing its great delicacy, 32—remarks upon the construction of a battery for electro-magnetic purposes, 35—conducting power of wires, 36.
Ritchie, Mr., on the elasticity of torsion in the threads of glass, 556.
Rive, M. de la, on the relative action of sulphuric acid and zinc, 388.
Robinson's improved mountain barometer, 555.
Robiquet on a new metallic dye, 628.
Rock-salt in Armenia, 386.
Rose, M., on the atomic weight of titanium, 175.
——— on two ores of tellurium from the Altai mountains, 399.
——— H., on the compounds of ammonia with anhydrous salts, 620.
Royal Academy of Sciences of the Institute of France, Vol. IX., extracts from the, 595.
Royal Institution, proceedings of the, 547.

S.

Sabine's, Captain, experiments with the pendulum, 338—experiments for the correction of variation of temperature, *ibid.*
Salicine, powers of, as a febrifuge, 177.
——— remarks upon, by MM. Pelouze and Jules Gay Lussac, 396.
——— process for preparing, 397.
——— use of, in intermittent fevers, 566.
Salt, on the application of common, in cases of animal poisons, 189.
Sankey, Mr., on the fabulous history of Greece, 57—remarks upon the origin of the fable of Ceres, 59—of Proteus, 60—observations upon the Argonautic expedition, 62—fable of Perseus, 63—Hermes, 65—Danaus, 68.
Sap, remarks upon the motion of, in plants, 90.
Saturn's ring, peculiar appearance of, 376.
Schlegel, Dr., on the salivary glands of serpents, 591.
Scorpions, observations upon the eyes of, 153.
Scrope, Mr. G. P., on the ripple marks and tracts of certain animals in the forest marble, 538.
Sea, phosphorescence of the, in the gulf of St. Lawrence, 194.
Seeds, germination of, on the surface of mercury, 637.

Sefstrom's, M., account of a new metal, 562, 625.
Selenium, sale of, 184.
——— mode of preparing, 619.
Serpents, remarks upon the salivary glands of, 591.
Serullas, M., experiment on perchloric acid, 563.
——— on the preparation of iodic acid, 614.
——— on the precipitation of the vegeto-alkalies, by iodic acid, 615.
——— on the action of bromic and chloric acids on alcohol, 615.
——— on perchloric acid, and its fossil formation, 616.
Shamrock, remarks upon the, by Mr. Bicheno, 453.
Silver, action of mercury upon, 4.
Silliman's, Professor, observations on the galvanic currents during the decomposition of water, 166.
Simmons's, Mr., account of a new species of British snake, 193.
Size for artists, 165.
Smith's, Mr., mode of preparing bicarbonate of soda, 385.
Snail, curious formation of its winter cell, 501.
Smut in corn, 420.
Snake, new species of, 193.
Snow of the winter 1829-30, 196.
——— peculiar fall of, 198.
——— animalcula in, 193.
——— remarks on the non-conducting power of, 515—and advantage some animals take of it, *ibid.*
Sound, singular natural, 204.
Soubeiran, M., on the reaction of per-salts of iron, and neutral carbonates, 388.
South's, Sir James, remarks upon the apparent projection of stars upon the moon's disc, 163.
Spiders, their mode of cleaning themselves, 27.
——— mechanical powers of, 426.
Spinal marrow in insects, remarks upon the functions of the, 587.
Spongy platinum, experiment with, and mercury, 10.
Stammering, observations on, by Dr. Hall, 253.
Stars, apparent projection of on the moon's disc, 163.
Steam, generation of, by heated metal, 613.
——— engines, boilers of, injured by the rapid deposition of sulphate of lime, 42.
Stromeyer, M., on pyrophosphoric acid and the pyrophosphates, 167, 168.
Strehlke, Professor, on atmospheric phenomena, 432.
Sturgeon, monography of, 569.
Svanberg, M., on the temperature of the planetary space, 370.
Substances phosphorescent when heated have this property restored by electricity, 77, 79, 80.
Sulphate of lime, deposition of, from hard water, 42.
Sulphur, modes of refining, 126.
Sulphuric acid and zinc, on the relative action of, 388.
——— ether, manufacture of, 629.
Surgical recovery of an eye, 191.
Symbols, list of, by Professor Whewell, 450.

T.

Tananarivou, observations upon the weather at, 47.
Taste, seat of the sense of, 425.
Teleo-saurus, memoir on the fossil remains of the, 569.
Telescopes, on the first invention of, 319.
Tellurium, M. Rose on, 399.
Temperature of the planetary system, 370.
Terrestrial magnetism, table of the intensity of, 374.
Tides in the atmosphere, 559.
Timber, rended by lightning, 195.
——— experiments on the strength of, 599.
Tin and lead, combustion of an alloy of, 626.
Titanium, atomic weight of, 175.
Torsion, elasticity of, in threads of glass, 556.
——— memoir on, by M. Cauchy, 598.
Turpin's, M., account of the oxalade of lime in plants, 422.
Turner's, Dr., on the mode of ascertaining the commercial value of ores of manganese, 293.
Twisting of the arteries in cases of amputation, 564.

V.

Vaporization, on the limits of, by Mr. Faraday, 70.
Vaults, mode of erecting, without centring, 224.
Vegeto-alkaline salts, electro-chemical decomposition of the, by Mr. Brande, 250.
Vegetables, on the growth of, by M. Amici, 422.
——— circulation in, 424, 635.
Venadium, account of the new metal, 562, 625.

Vesuvius, phenomena observed in the last eruption of, in 1828, 296.
——— and Pompeii, account of, 554.
Village, lighted up by natural gas, 203.
Vision, contributions to the physiology of, 101, 534.
——— of birds of prey, remarks on the, 192.
Vocal organs, affections of the, 565.
Voltaic pile, origin of the action of the, 612.

U.

Ulmin, or ulmic acid, and azulmic acid, account of, by M. Boullay, 179.
Ulm minster, remarks upon the mode of vaulting used at, 233.
Ure, Dr., on gunpowders and detonating matches, 121—mode of purifying nitre, *ibid.*—process used in France, 123—modes of refining sulphur, 126—charcoal most suitable for gunpowder, 131—chemical examination of gunpowder, 136—detonating matches, 140.
Urea, mode of preparing, by Berzelius, 401.

W.

Wallich's, Dr., descriptions and figures of a select number of unpublished East India plants, 360.
——— numerical list of dried specimens of plants in the East India Company's Museum, 360.
Water, remark upon the report of the Commissioners on the supply of, to the metropolis, 41.
——— practical and philosophical observations on natural, 38.
——— resistance to, when moving in pipes, 157.
——— mode of regulating the supply of, between rivulets and canals, 307.
——— on the discharge of a jet of, under water, 368.
Waterspout on the lake of Neufchatel, 200.
Wave, remarks upon the formation of a, 540.
Weather, Mr. Lyall's observations on the, at Madagascar, 47.
West, Mr., on natural waters, 38—water from peat lands, *ibid.*—on the deposition of sulphate of lime from hard waters, 42—on the solvent powers of waters, 43—on the gaseous contents of waters, 44.
Wheel animalculæ, nature of the appearance they present, 220.
Whewell, Rev. W., on the employment of notation in chemistry, 437—advantage of using symbols in chemistry, 438, 439—objections to the foreign system, 441—proposed system, with examples, 442—remarks upon the letters representing the elements of bodies, 446, 447—list of symbols recommended, 450.
——— written nomenclature for chemical compounds, 394.
White-bait, experiments to preserve, 426.
Wires, remarks upon the conducting power of metallic, 36.
Wittich, Mr., on the horns of the Prussian elk, and American moose-deer, 118.
Wittstock, M., on the manufacture of sulphuric ether, 629.
——— on columbine, a new vegetable principle, 630.
Wollaston, Dr., remarks upon his improvement of the galvanic battery, 33,
Wood, mode of charring, at low temperatures, 397.
——— change of colour in, 398.
——— remarks upon the formation of, 477.
——— sorrel, remarks upon the, as the original emblem of Ireland, 454.
Wool, on the means of improving, 192.

Y.

Yard and metre, proportion between the English, 599.
Yarrell's, Mr., experiments to preserve white-bait alive, 426.
Year, remarks upon the Egyptian, 462.
Yelloly, Dr., on calculous diseases, 346.

Z.

Zinc, action of mercury upon, 3.
——— and sulphuric acid, on the relative action between, 388.
Zoology, 569.

END OF VOLUME THE FIRST.

London: Printed by W. Clowes, Stamford-street.

ERRATA.

Page	Line
320	4 from bottom, *for* Chartres *read* Castres.
325	5 from bottom, *for* vitro-crystal lines *read* vitrocristallines.
329	16 from top, *for* Bussi *read* Russii.
ib.	22 from top, *for* Teannin *read* Jeannin.
ib.	23 from top, *for* Bussi *read* Russi.
330	25 from top, *for* Tanz *read* Jansz.
ib.	2 from bottom, *for* Tanz *read* Jansz.
331	6 from top, *for* Borel *read* Boreel.
332	5 from top, *for* Borel *read* Boreel.
ib.	7 from bottom, *for* that *read* thus.
ib.	1 from bottom, *for* produced *read* procured.
483	6 from top, *for* Tausz or Taussen *read* Jansz or Janssen.
484	14 from top, *for* Tausz *read* Jansz.
485	33 from top, *for* Peirese *read* Peiresc.
487	26 from top, *for* Padrea *read* Padua.
488	6 from top, *for* Badorere *read* Badovere.
ib.	26 from top, *for* ne piu aggiunto *read* ne piu fu aggiunto.
ib.	30 from top, *for* Badorere *read* Badovere.
489	14 from top, *for* Maurus, of Nassau *read* Maurice of Nassau.
490	24 from top, *for* Tagemann *read* Jagemann.
492	20 from top, *for* Zanssen *read* Janssen.
494	36 from top, *for* Hore *read* Hove.
ib.	37 from top, *for* Peirese *read* Peiresc.
496	12 from top, *for* Zansz *read* Jansz.
ib.	14 from top, *for* Zansz *read* Jansz.
ib.	18 from top, *for* Cinoculus *read* Binoculus.
597	17 from top, *for* electricity, *read* elasticity.