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INDEX

ABBE (Prof. Cleveland), Recent Progress in Dynamic Meteorology, 450

Abbe (Prof. Cleveland) and Prof. David P. Todd, Additional Results of the United States Scientific Expedition to West Africa, 563

Abbott (Dr.), East African Zoological Collection, 497

Aberration, Determination of the Constant of, MM. Lœwy and Puiseux, 498

Aberystwith University College, the Physical Laboratory at, 108

Abraham (H.): Thermo-electric Researches, 24; Researches in Thermo-electricity, 95

Acoustics: Kœnig's Superior Beats, Rev. Walter Sidgreaves, 9; the Researches of Dr. R. Kœnig on the Physical Basis of Musical Sounds, Prof. Silvanus Thompson, 199, 224, 249; Squeaking Sand *versus* Musical Sand, H. Carrington Bolton, 30; the Sense of Hearing, Prof. W. Rutherford, 431

Acquired Characters: Are the Effects of Use and Disuse Inherited?, William Platt Ball, Prof. Geo. J. Romanes, F.R.S., 217

Adams (Prof. Henry), Hand-book for Mechanical Engineers, 147

Addy (George), on Milling Cutters, 23

Adie (R. H.): Compounds of Oxides of Phosphorus with Sulphuric Anhydrides, 454; Osmotic Pressure of Salts in Solution; Direct Comparison of Constants of Raoult's Method and those ascertained by Direct Observation of Osmotic Pressure, 478

Adriatic, Austrian Government, Oceanographic Investigation of, 230

Ælurопus melanoleucus, Skin of, procured by Herren Potanin and Beresowski, 355

Aëronautics: Captive Balloons, 17

Affinities of Hesperornis, on the, Dr. F. Helm, 368

Afghanistan, Western, and North-Eastern Persia, J. E. T. Aitchison, F.R.S., 174

Africa, the Nyassaland Region, H. H. Johnston, 45

Africa, the Development of, by Arthur Silva White, 149

Africa, Ptolemaic Geography of, 448

Africa, West, Additional Results of the United States Scientific Expedition to, Profs. Cleveland Abbe and David P. Todd, 563

African Butterfly, an Assumed Instance of Compound Protective Resemblance in an, W. L. Distant, 390

Agaric, the Fly, 88

Agarics: M. C. Cooke on Attractive Characters in Fungi, 57; the Value of Attractive Characters in Fungi, Charles R. Straton, 9, 79, 80; R. Haig Thomas, 79; T. Wemyss Fulton, 269

Agassiz (Alexander), Rate of Growth in Corals, 65

Agriculture: Agricultural Education at Cambridge, 23, 476; Agricultural Education, P. McConnell, 182; Field Experiments at Rothamsted, 189; the Action of Lime on Clay Soils, Alexander Johnstone, 308; James Muir appointed Professor of Agriculture at Yorkshire College, Leeds, 397; Proposed International Agricultural Congress at the Hague, 542; the Wheat Harvest in relation to Weather, 569; Corn and Squirrels, C. P. Gillette, 620

Air, Nocturnal Temperature of the, 63

Air respired by Different Persons, Different Volumes of, Dr. Marcet, 451

Aitchison (J. E. T., F.R.S.), Western Afghanistan and North-Eastern Persia, 174

Ajaccio Magnificent Grotto discovered near, 521

Alabama, History of Education in, Willis G. Clark, 362

Alabama, Varieties of Cotton grown in, P. H. Mell, 521

Alaska: the Geodetic Survey of, 207; Proposed United States Topographical Survey of, 279

Albino Frog, an, J. E. Harting, 383

Albrecht (Herr), Behaviour of Water as to Passage of Different Light Rays, 520

Alcohol from Potatoes, Distillation of, Aimé Girard, 120

Alexeyeff (Prof.), Death of, 398

Alexis and his Flowers, Beatrix F. Cresswell, 79

Algæ: *Tuomeya fluviatilis*, Harvey, 65; Marine, of Berwick-on-Tweed, Edward A. L. Batters, 66; Growing in Mollusk Shells, Bornet and Flahault, 185

Algeria, the Past Winter in, M. Marès, 567

Algebra: Elementary, W. A. Potts and W. L. Sargant, 28; Algebra of Vectors, on the Rôle of Quaternions in the, Prof. J. Wiilard Gibbs, 511; Prof. P. G. Tait, 535, 608; Solutions of the Examples in Elementary Algebra, H. S. Hall and S. R. Knight, 389; Algebraic Symbols in Applied Mathematics, the Meaning of, Prof. Oliver J. Lodge, F.R.S., 513; W. H. Macaulay, 558

Algiers, Earthquake in, 279

Algiers, the Recent Earthquake in, 356

Alpine Flora, with a Suggestion as to the Origin of Blue in Flowers, T. D. A. Cockerell, 207, 533; W. T. Thiselton Dyer, F.R.S., 581

Altitudes, Solar Spectrum at Medium and Low, Dr. Ludwig Becker, 399

Amagat (E. H.), New Method of Studying Compressibility and Expansion of Liquids and Gases, 168

Amat (L.), Transformation of Sodium Pyrophosphite into Hydrogen Sodium Phosphite, 480

Amber Flora, Monographie der baltischen Bernsteinbäume, H. Conwentz, 221

America: an American Geological Railway Guide, James Macfarlane, 8; American Association for Advancement of Science, 42; American Ornithologists' Union, 42, 301; the Four Hundredth Anniversary of the Discovery of, and the Phi Beta Kappa Society, 62; American Journal of Science, 71, 260, 309, 500, 598; American Spiders and their Spinning Work, Henry C. McCook, D.D., 74; Exemption of Educational Books by McKinley Tariff, 88; Life Areas of North America, Dr. C. H. Merriam, 88; American National Academy of Sciences, 131; the Fish-eating Birds of America, W. Palmer, 132; Who are the American Indians?, H. W. Henshaw, 137; Profs. W. B. Scott and H. F. Osborn on the Fossil Mammals of North America, 177; American Meteorological Journal, 185, 285, 382, 548; Garden Scholarships at Missouri, 187; American Blast-Furnace Work, 211; American Morphological Society, 301; Remarkable Ancient Sculptures from North-West America, James Terry, Dr. Alfred R. Wallace, 396; Psychology in America, 506; American Race, the, Dr. Daniel G. Brinton, 556; American Journal of Mathematics, 571

Amigues (E.), a Purely Algebraic Demonstration of the Fundamental Theorem of the Theory of Equations, 336
Amphioxus, on the Atrial Chamber of, Prof. E. Ray Lankester, F.R.S., 70
Amsterdam, Royal Academy of Sciences, 72, 288, 408, 528, 600
Amu-Daria Region, Ornithological Fauna of the, M. Zarudnoi, 334
Anahuac applied by mistake to Plateau of Mexico, the Name, Dr. Seler, 208
Analysis of a Simple Salt, William Briggs and R. W. Stewart, 126
Anatomy and Development of Apteryx, Prof. T. Jeffery Parker, F.R.S., 17
Anatomy of *Corambe testitudinaria*, H. Fischer, 360
Ancient Mounds at Floyd, Iowa, Clement L. Webster, 213
Ancient Sculptures from North-West America, Remarkable, James Terry, Dr. Alfred R. Wallace, 396
Andaman Islands, Naturalized Flora since 1858 of, Dr. Prain, 398
Andamans necessary for Meteorological Purposes, Cable to the, 451
André (G.), Sulphur in Vegetables, 311; the Cause of the Odour of Earth, 528
Andrews (Thos.), the Passive State of Iron and Steel, II., 358
Andromeda Nebula, Variability of the, Isaac Roberts, 379
Anemometer, Huet's, W. J. Lewis, 323; Lind's Anemometer, 323
Angle, Mechanical Trisection of any, Captain A. H. Russell, 547
Animal Kingdom, Natural History of the, for the Use of Young People, 435
Animal Life and Intelligence, C. Lloyd Morgan, Dr. Alfred R. Wallace, 337
Animals, Action of Excessive Cold on, G. Colin, 408
Animals, Classification of, W. E. Fothergill, Emil Selenka, and J. R. A. Davis, 124
Animals, Mutual Aid among, William Elder, 56
Annalen des k.k. naturhistorischen Hofmuseums, Wien, 28
Annals of Harvard College Astronomical Observatory, 301, 356
Annals of a Fishing Village, 389
Annelid Morphology, some Problems of, Prof. E. B. Wilson, 458
Annelids and Crustacea, Prof. T. H. Morgan, 459
Annuaire de l'Académie Royale de Belgique, 301
Annuaire du Bureau des Longitudes, 356
Ant, Caringa, of the Malay Peninsula, the Red, H. N. Ridley, 620
Antelopes of Nyassaland, the, Richard Crawshay, 143
Anthropology: Anthropological Society of Bombay, 62; Conference of Delegates of Corresponding Societies of the British Association, 93, 94; Anthropological Institute, 192, 278, 310, 535; Thumb and Finger-markings as Proofs of Identity, Francis Galton, F.R.S., 192; General Relation of State and Increase of Population in France, Emile Levasseur, 192; the Hill Arrians of India, Rev. A. F. Painter, 212; Ancient Mounds at Floyd, Iowa, Clement L. Webster, 213; Resemblances between Husband and Wife, Hermann Fol, 255; Krilof's Portraits of Typical Representatives of Russian Races, 255; Remarkable Ancient Sculptures from North-West America, James Terry, Dr. Alfred R. Wallace, 396; Australian Aboriginal Weapons and Implements, R. Etheridge, Jun., 545; a Peculiar Gesture of Zuñi and Navajo Indians, J. W. Fewkes, 592
Anticyclones and Cyclones, the Causes of, Henry F. Blanford, F.R.S., 15, 81
Antipathy (?) of Birds for Colour, William White, 608
Anvers (N. D'), the Life Story of our Earth, 103; the Story of Early Man, 103
Ape Heads, Sculptured Anthropoid, James Terry, Dr. Alfred R. Wallace, 396
Apodidæ, Hermaphroditism of the, Rev. H. Bernard, 343
Apteryx, the Anatomy and Development of, Prof. T. Jeffery Parker, F.R.S., 17
Arachnidæ, some Habits of the Spider, 55
Arachnids, on the Origin of Vertebrates from, by Dr. W. Patten, 70
Araucaria Cones: Duke of Argyll, F.R.S., 8; Dr. A. Irving, 29; John L. Plummer, 29; E. Brown, 29; Dr. Maxwell T. Masters, F.R.S., 56; D. Sharp, 57; A. D. Webster, 57; William Gardiner, 80; James Shaw, 81; Adrian Weld-Blundell, H. N. Dixon, 128
Arcana Fairfaxiana, 366
Archæology: Lake-Dwellings of Europe, being the Rhind Lectures in Archæology for 1888, Robert Munro, Prof. W. Boyd Dawkins, F.R.S., 341; Discovery of a Vault full of Mummies, &c., near Thebes, Egypt, 398; Important Discovery by W. Flinders Petrie at Maydoom, Egypt, 591
Architecture, Naval: the Science of Boiler Construction, A. F. Yarrow, 500
Arctic Expedition, Dr. Nansen's coming, 450
Arctic Expedition by Lieut. Robert Peary, U.S.N., the Projected, 426, 619
Arctic Tern, the Flight of, John R. Spears, 319
Argelander-Oeltzen Star Catalogue, Dr. E. Weiss, 357
Argot (M.), Results of Comparisons of Pressure and Temperature Observations at Eiffel Tower with Low-level Stations (1889), 254
Argyll (Duke of, F.R.S.): Araucaria Cones, 8; Darwin on the Unity of the Human Race, 415
Aristotle and Hunter, Jonathan Hutchinson, F.R.S., 377
Arithmetic, the Century, 175
Arithmetic: Key to Arithmetic in Theory and Practice, J. Brooksmith, 294
Arizona, the Fauna and Flora of, Dr. C. H. Merriam, 88
Armengaud (M. Jacques), Death of, 376
Armenia, a Ride through Asia Minor and, Henry C. Barkley, 487
Armenia, Destruction of a Hamlet by Earthquake in, 566
Armitage (E.), the Influenza, 608
Armstrong (H. E., F.R.S.), Andresen's β-Naphthylaminedisulphonic Acid, 478; Chloro- and Bromo-Derivatives of Naphthol and Naphthylamine, 503
Arnaud (A.), Transformation of Cupreine into Quinine, 599
Art Work, Eskimo, John R. Spears, 464
Arthropods, Locomotion of, H. H. Dixon, 223
Asia Minor and Armenia, a Ride through, Henry C. Barkley, 487
Asia Minor, Botanical Results of Herr J. Bornmüller's Expedition to, 43
Asia, Central, G. E. Grum-Grzimailo's Expedition to, 571
Asiatic Society of Japan, 183
Aspergillin, a Vegetable Hæmatin, G. Linossier, 456
Assyriology; the newly-discovered Version of the Story of the Creation, T. G. Pinches, 163
Asteroids: Names of, Dr. Palisa, 379; New, 400, 428, 475, 480, 569; the Question of the Asteroids, Richard A. Gregory, 587
Astronomy: Rotation of Venus, 22; Spectrum of the Zodiacal Light, Prof. C. Michie Smith, 22; D'Arrest's Comet, 22; Astronomical Column, 22, 44, 64, 89, 110, 133, 165, 210, 232, 256, 280, 302, 328, 356, 379, 399, 427, 452, 474, 498, 545, 588; Measures of Lunar Radiation, C. C. Hutchins, 44; Electro-dynamic Origin of Planetary Motion, C. V. Zenger, 48; the Duplicity of α Lyræ, A. Fowler, 64; the Catania Observatory, 65; Washington Observations, Asaph Hall, 65, 188; Prof. Holden on the Parallaxes of Nebulæ, 65; a New Comet (?), 89; Stars 121 and 483 Birm., 90; Apex of the Sun's Way, 90; Orbits of 61 Cygni, Castor, and 70 Ophiuchi, 90; Two New Comets (*e* and *f*, 1890), 90; the Star D.M. + 53° 2684, 90; Rapid Development of a Solar Prominence, Jules Fenyi, 95; a Chaldean Astronomical Annual used by Ptolemy, J. Oppert, 95; J. L. E. Dreyer's Life of Tycho Brahe, 98; Sun, Moon, and Stars, &c., William Durham, 103; the Distances of the Stars, John I. Plummer, 104; Measures of Lunar Radiation, Earl of Rosse, 104; Aid to Astronomical Research, Prof. Edward C. Pickering, 105; the Variations in Latitude, 110; New Asteroids, 111, 400, 428, 475, 569; Asteroid (308), M. Charlois, 480; the Photography of Solar Prominences, G. E. Hale, 133; the Frequency of Meteors, Terby and Van Lint, 133; Variation of Certain Stellar Spectra, Rev. T. E. Espin, 165; British Astronomical Association, 165; Elements and Ephemeris of Zona's Comet, 165; the System of the Stars, Agnes M. Clerke, 169; Greenwich Spectroscopic Observations, 210; Perihelia of Comets, Dr. Holetschek, 233; on the Orbit of α Virginis, Prof. H. C. Vogel, 235; Spectroscopic Observations of Sunspots, Rev. A. L. Cortie, 256; Turin Observatory, 257; the Duplicity of α Lyræ, 257; a Mock Sun, T. Mann Jones, 269;

Stereoscopic Astronomy, 269 ; W. Le Conte Stevens, 344 ; the Photographic Chart of the Heavens, 276, 516, 540, 568 ; Stars having Peculiar Spectra, Prof. Pickering, 280 : Harvard College Observatory, D. W. Baker, 280 ; Dark Transits of Jupiter's Satellites, Mr. Keeler, 302 ; Solar Activity (July–December 1890), Prof. Tacchini, 302 ; Planet or New Star, Dr. Lescarbault, 303 ; Astronomical Lessons, John Ellard Gore, 317 ; Determination of Masses of Mars and Jupiter by Meridian Observations of Vesta, Gustave Leveau, 384 ; Prof. Newcomb on the Transits of Venus in 1761 and 1769, 328 ; Leyden Observatory, 328 ; Personal Equation in Transit Observations, F. Gonnessiat, 336 ; Annuaire du Bureau des Longitudes, 356 ; United States Naval Observatory, 357 ; a New Theory of Jupiter and Saturn, G. W. Hill, 357 ; Argelander-Oeltzen Star Catalogue, Dr. E. Weiss, 357 ; Latitudinal Distribution of Solar Phenomena, P. Tacchini, 360 ; Babylonian Astronomy and Chronology, 369 ; Variability of the Andromeda Nebula, Isaac Roberts, 379 ; Eccentricities of Stellar Orbits, Dr. T. J. J. See, 379 ; a New Nebula near Merope, E. E. Barnard, 379 ; Names of Asteroids, Dr. Palisa, 379 ; Solar Spectrum at Medium and Low Altitudes, Dr. Ludwig Becker, 399 ; a Method of Measuring Atmospheric Dispersion, Prosper Henry, 400 ; Wolf's Relative Numbers for 1890, 400 ; New Asteroids, 400 ; a Hand-book and Atlas of Astronomy, W. Peck, 414 ; Recent Photographs of the Annular Nebula in Lyra, A. M. Clerke, 419 ; the Permanence of Markings on Venus, Dr. Terby, 427 ; Observations of Mars, J. Guillaume, 428 ; New Variable Star, 428 ; New Asteroid, 428 ; Observations of Sun-spots in 1889 Em. Marchand, 432 ; Œuvres Complètes de Christiaan Huygens, A. M. Clerke, 433 ; Photographic Spectrum of the Sun and Elements, Prof. Rowland, 452 ; a Variable Nebula, 453 ; Astronomical and Physical Geography for Science Students, R. A. Gregory, 459 ; the "Capture Theory" of Comets, O. Callandreau, 474 ; Annuaire de l'Observatoire de Bruxelles, 475 ; New Asteroids, 475 ; Determination of the Constant of Aberration, MM. Lœwy and Puiseux, 498 ; New Comet, W. F. Denning, 516 ; Astronomical Congress, 516 ; Stars having Peculiar Spectra, Prof. E. C. Pickering and Mrs. Fleming, 545 ; the Nebula near Merope, Prof. Pritchard, Prof. Barnard, 546 ; Comet *a* 1891, 546 ; Charts of the Constellations, Arthur Cottam, 557 ; the Discovery of Comet *a* 1891 ; W. F. Denning, 558 ; on some Points in the Early History of Astronomy, Prof. J. Norman Lockyer, F.R.S., 559 ; the Solar Corona, 568 ; Prof. Charroppin on the, 568 ; Prof. Frank H. Bigelow on the, 568 ; Mr. Schaeberle on the, 568 ; Prof. Berberich on the Elements of the Comet Barnard-Denning (*a* 1891), 569 ; the Planet Mercury, 569 ; New Asteroid, 569 ; the Question of the Asteroids, Richard A. Gregory, 587 ; a New Variable Star, 590 ; a Meteor, W. Budgen, 608
Aterica meleagris, W. L. Distant, 390
Atkinson (Ll. B. and C. W.), Electric Mining Machinery, 355
Atkinson (W.), Our Latest Glacial Period, 270
Atlantic : Finley's Storm-tracks, &c., Charts of North, 231 ; Pilot Chart for October of North, 108 ; December 1890, 279 ; April, 592 ; Synchronous Daily Weather Charts of the North Atlantic, 59
Atmospheric Dispersion, a Method of Measuring, Prosper Henry, 400
Attractive Characters in Fungi, Dr. M. C. Cooke, 224, 393 ; Worthington G. Smith, 224 ; Dr. T. Wemyss Fulton, 269
Aubert (E.), Simultaneous Emission of Oxygen and Carbon Dioxide by Cacti, 552
Auk Tribe, on the Morphology of the, W. Kitchen Parker, F.R.S., 486
Aurora Problem, a Solution of the, Prof. F. H. Bigelow, 405
Ausiaux (G.), Influence of Exterior Temperature on Heat-production in Warm-blooded Animals, 432
Australia : Australasian Association for the Advancement of Science, 496 ; Australian Batrachia, Geographical Distribution of, J. J. Fletcher, 132 ; Australian Museum, Sydney, 132 ; Australian Aborigines, Rev. John Mathew and Edward Stephens, 185 ; Australian Aboriginal Weapons and Implements, R. Etheridge, Jun., 544 ; Local Magnetic Disturbance of the Compass in North-West Australia, Commander E. W. Creak, F.R.S., 471 ; Weather Forecasting in, 497 ; Australian Year-book for 1891, 591
Austrian Government Oceanographic Investigations of Adriatic, 230
Autobiography of the Earth, Rev. H. N. Hutchinson, 365
Automatic Lamp-lighter, an, Shelford Bidwell, F.R.S., 395
Ayrton (Prof. W. E., F.R.S.) : the Proposed South Kensington and Paddington Subway, 278 ; Proof of Generality of Mr. Blakesley's Formulæ, Tests of a Transformer, 478 ; Measurement of Power Supplied by any Electric Current to any Circuit, 572

Babes (V.) and A. V. Cornil on Bacteria, 195
Babylonian Astronomy and Chronology, 369
Bacilli : Koch's Cure for Consumption, 49 ; Bacillus, Dr. E. Klein on Infectious Diseases, 416, 446 ; *Bacillus anthracis*, 72 ; *Bacillus radicicola*, Accumulation of Atmospheric Nitrogen in Cultures of, Mr. Beyerinck, 600
Backhouse (T. W.), Rainbows on Scum, 416
Bacteria, A. V. Cornil and V. Babes, 195
Bacteria, Exhibition of Drawings of, 107
Bacterium, Life-History of a Pigment, 288
Badgley (Colonel), Dew, 311
Bagnall (James E.) and W. B. Grove, the Fungi of Warwickshire, 413
Bailey (E. H. S.), Halotrichite or Feather Alum, 598
Baker (D. W.), Harvard College Observatory, 280
Baker (J. G., F.R.S.), Flora of Warwickshire, 413
Baker (Sir Samuel W., F.R.S.), Wild Beasts and their Ways, 78
Baker (W. G.), the British Empire, the Colonies and Dependencies, 607
Baldwin (Prof. J. M.) : the Origin of Right- or Left-handedness, 43 ; Hand-book of Psychology, 100 ; Infant Psychology, 256
Ball (Valentine, F.R.S.) : the Great Mogul's Diamond and the Koh-i-nur, 103 ; Cackling of Hens, 583
Ball (William Platt), Are the Effects of Use and Disuse Inherited ?, Prof. Geo. J. Romanes, F.R.S., 217
Balloons, Captive, 17
Baratta (Signor), Photography applied to Mechanical Registration of Motions of Seismo-microphone, 209
Barbier (M.), Relations between Optical Dispersive Power of Alcohols, Ethers, and Fatty Acids, and their Molecular Weight and Constitution, 133
Barkley (Henry C.), a Ride through Asia Minor and Armenia, 487
Barnard (Prof. E. E.), a New Nebula near Merope, 379, 546
Barnard-Denning Comet (*a* 1891), Prof. Berberich on the Elements of, 569
Barometer, Self-recording, Redier and Meyer, 255
Barral (M.), Glycolyptic Power of Human Blood, 528
Barrell (Prof. F. R.), Apparatus for determining Acceleration due to Gravity, 335
Barrett-Hamilton (E. H.), the Irish Rat, 255
Bartlett (A. D.), the Crowing of the Jungle Cock, 319
Bartlett (Edw.), Appointment of, as Curator of Sarawak Museum, 472
Barton (E. A.), the Colonist's Medical Hand-book, 150
Bass Fibre in English Market, West African, 300
Basset (A. B., F.R.S.) : Elementary Treatise on Hydrodynamics and Sound, 75 ; Reflection and Refraction of Light at Surface of Magnetized Medium, 286
Bateson (W.), Nature of Supernumerary Appendages in Insects, 527
Bateson (W. and A.), Variations in Floral Symmetry of certain Plants with Irregular Corollas, 575
Bather (F. A.), Stereom, 345
Batrachia, Geographical Distribution of Australian, J. J. Fletcher, 132
Batrachian Heart, Action of the Nerves of the, in Relation to Temperature and Endocardiac Pressure, G. N. Stewart, 548, 558
Batters (Edward A. L.), Marine Algæ of Berwick-on-Tweed, 66
Bavaria, Coffee Cultivation of, 398
Baxter-Wray, Round Games with Cards, 414
Baynes (Robert E.), Prof. Van der Waals on the Continuity of the Liquid and Gaseous States, 487
Bears and Telegraph Poles, J. D. Pasteur, 184
Beasts, Wild, and their Ways, Sir Samuel W. Baker, F.R.S., 78
Beaumont (Roberts), Colour in Woven Design, 343
Bebber (Dr. J. W. van), Typical Winter Weather Conditions, 567

Becker (Dr. Ludwig), Solar Spectrum at Medium and Low Altitudes, 399
Becquerel (H.), Phosphorescence of Minerals under Light and Heat, 504
Beddard (Frank E.): the Homology between Genital Ducts and Nephridia in the Oligochæta, 116; Zoological Record for 1889, 316; New Form of Excretory Organ in Oligochætous Annelid, 477
Bedford College and Scientific Teaching for Women only, 425; Opening of Shaen Wing of, 496
Bee, Hive, the Comb of the, Wm. Knight, 80
Bee, the Honey, by T. W. Cowan, Rev. T. Tuckwell, 578
Bees' Cells, the Right Rev. Lord Bishop of Carlisle, 295
Beetle, the Coco-Nut, H. N. Ridley, 476
Behring Sea, Volcanic Eruption in, 279
Belknap (Admiral), on the Depths of the Pacific Ocean, 183
Bell (Arthur John): Whence comes Man?, 460; Why does Man exist?, 460
Bellamy (Edward), Death of, 230
Bemmelen (Van), New Compounds of Mercuric Oxide, 528
Ben Nevis, Meteorology of, Dr. Alexander Buchan, 538
Bengal, Forestry in, 232
Bennett (Alfred W.): the proposed South Kensington and Paddington Subway, 246; the Flight of Larks, 248
Benzoyl Fluoride, M. Guenez, 64
Berberich (Prof.), on the Elements of the Barnard-Denning Comet (*a* 1891), 569
Berberine, Prof. W. H. Perkin, Jun., F.R.S., 335
Beresowski (Herr), Skin of *Æluropus melanoleucus* procured by, 355
Berge's Complete Natural History, 366
Berget (A.), Graphical Method of Determining Relative Values of Gravity in various Places, 504
Berghaus (Dr.), Death of, 131
Berlin, Meeting in Honour of Dr. Schliemann's Memory at, 425
Bernard (Rev. H.), Hermaphroditism of the Apodidæ, 343
Berthelot (Daniel): Derivation of the name of Bronze, 95; Waves caused by Explosions, &c., 264; Sulphur in Vegetables, 311; Volatile Nitrogen Compounds given off by Vegetable Mould, 336; Action of Heat on, and a Reaction of Carbonic Oxide, 528; the Cause of the Odour of Earth, 528; Conductivities of Isomeric Organic Acids and their Salts, 264
Bertin (George), Death of, 425
Besson (M.): Silicon Bromoform, 474; Chlorobromides of Silicon, 592
Beyerinck (M.): Life-History of a Pigment Bacterium, 288; Accumulation of Atmospheric Nitrogen in Cultures of *Bacillus radicicola*, 600
Bidwell (Shelford, F.R.S.): an Automatic Lamp-lighter, 395; Experiments with Selenium Cells, 262
Bigelow (Prof. Frank H.): a Solution of the Aurora Problem, 405; on the Solar Corona, 568
Biology: Mutual Aid among Animals, William Elder, 56; the Laboratory of Vegetable Biology at Fontainebleau, 37; Rate of Growth in Corals, Alexander Agassiz, 65; *Tuomeya fluviatilis*, Harvey, 65; Biological Notes, 65, 184; Marine Algæ of Berwick-on-Tweed, Edward A. L. Batters, 66; Conference of Delegates of Corresponding Societies of the British Association, 92, 93; Biological Terminology, R. J. Harvey Gibson, 175; Illusions of Woodpeckers and Bears with respect to Telegraph Poles, J. D. Pasteur, 184; Algæ Living in Mollusk Shells, Bornet and Flahault, 185; Journal of the Marine Biological Association, 109; Medusæ of Millepora and their Relation to Medusiform Gonophores of Hydromedusæ, S. J. Hickson, 407; Biological Lectures delivered at the Marine Biological Laboratory of Wood's Holl in the Summer Session of 1890, 457; Wood's Holl Biological Lectures, Prof. E. Ray Lankester, F.R.S., 516; the Wood's Holl (Mass.) Laboratory, 591; Projected Station of Marine Biology at Sebastopol, 543; the Marine Laboratory of Luc-sur-Mer, Normandy, 567; Fathers of Biology, Charles McRae, 245; Animal Life and Intelligence, C. Lloyd Morgan, Dr. Alfred R. Wallace, 337; Hermaphroditism of the Apodidæ, Rev. H. Bernard, 343; Biological Station at Plöner See, East Holstein, Projected, 377; the Darwinian Theory of the Origin of Species, Francis P. Pascoe, Prof. R. Meldola, F.R.S., 409; Biological Society of Washington, 425; Biological Expedition to West Indies and Yucatan, Dr. Rothrock's, 426; on the Modification of Organisms, David Syme, Dr. Alfred R. Wallace, 529
Bird (Charles), Elementary Geology, Prof. A. H. Green, F.R.S., 242
Birds of America, the Fish-eating, W. Palmer, 132
Birds, Antipathy of, for Colour, 558; William White, 608
Birds of Norfolk, Henry Stevenson, 313
Birds: a Remarkable Flight of Birds, Rev. E. C. Spicer, 222; on the Flight of Oceanic Birds, Captain David Wilson-Barker, 222
Birds, on the Soaring of, 30; Prof. F. Guthrie, 8; W. Froude, 287
Birds' Eggs, the Protection of Wild, 376
Birds' Nests, E. J. Lowe, F.R.S., 199
Birmingham, Medical Faculty of Queen's College, Removal of, 542
Birth and Growth of Worlds, Prof. A. H. Green, F.R.S., 221
Bishop (Dr. A. W.), Action of Ethyl Dichloracetate on Sodium Derivative of Ethyl Malonate, 550
Bissell (Mary Russell, M.D.), Household Hygiene, Dr. H. Brock, 461
Bizio (Prof. Giovanni), Death of, 619
Black Mountain Expedition, the, 543
Blackie (Prof.), Bistratification in Modern Greek, 527
Blaikie (James) and W. Thomson, a Text-book of Geometrical Deduction, 487
Blakesley (T. H.): the Determination of the Work done upon the Cores of Iron in Electrical Apparatus subject to Alternating Currents, 116; Solution of a Geometrical Problem in Magnetism, 191; Further Contributions to Dynamometry, 478
Blanford (Henry F., F.R.S.): the Causes of Anticyclones and Cyclones, 15, 81; the Genesis of Tropical Cyclones, 81; an Elementary Geography of India, Burma, and Ceylon, 29; the Paradox of the Sun-spot Cycle, 583; Variations of Rainfall at Cherra Poonjee, Assam, 599
Blarez (Ch.), Solubility of Potassium Bitartrate, 432
Blast-Furnace Work, American, 211
Blood, Glycolytic Power of Human, Lépine and Barral, 528
Blood, Herr von Hedin's Hæmatokrit for Determining Volume of Corpuscles, 398
Blood of Vertebrata, Comparative Alkalinity of, René Drouin, 144
Blood-clotting, Experiments on, A. S. Grünbaum, 527
Blow-fly, Anatomy, Physiology, Morphology, and Development of the, B. Thompson Lowne, 77
Blyth (A. Wynter), a Manual of Public Health, Dr. J. H. E. Brock, 267
Boas (Dr. J. E. V.), Lehrbuch der Zoologie für Studirende und Lehrer, 268
Boettger (O.): Reptilia and Batrachia, George A. Boulenger, 361; the Fauna of British India, including Ceylon and Burma, 361
Boiler Construction, the Science of, A. F. Yarrow, 500
Bolton (Prof. H. Carrington), Squeaking Sand *versus* Musical Sand, 30
Bombay: Anthropological Society of, 62; Journal of Natural History Society, 132; Education in, 209; Proposed Attempt to Acclimatize Game Birds Common in other Parts of India in, 543
Bonneville, Lake, Grove Karl Gilbert, Prof. T. G. Bonney, F.R.S., 485
Bonney (G. E.), the Electro-plater's Manual, 579
Bonney (Prof. T. G., F.R.S.): the Origin of the Great Lakes of North America, 203; Destruction of Fish in the late Frost, 295, 368; Temperature in the Glacial Epoch, 373; the North-West Region of Charnwood Forest, 310; Note on a Contact-structure in Syenite of Bradgate Park, 310; Lake Bonneville, Grove Karl Gilbert, 485; Ninth Annual Report of the United States Geological Survey to the Secretary of the Interior, 1887–88, J. W. Powell, 509
Booth's (the late Mr. T. E.), Museum of British Birds, 20
Borgman (Prof. J. J.), a Lecture Experiment Illustrating the Magnetic Screening of Conducting Media, 583
Borneo, British North, Proposed Survey of Mount Kinabalu, 327
Bornet (Ed.), Algæ Growing in Mollusk Shells, 185
Bornmüller (Herr J.), Expedition to Asia Minor, Botanical Results of, 43
Boron Iodide, M. Moissan, 568
Boscastle, Earthquake at, 519

Bosnia, Earthquake in, 279
Botany: Annals of the Royal Botanic Garden, Calcutta, W. Botting Hemsley, F.R.S., 6; Flight of Leaves, R. Haig Thomas, 9; the Coco-de-Mer in Cultivation, William Watson, 19; Augmentation of Botanical Department of University College, London, 20; Edible Fungi for Sale in Modena Market in 1889, 21; the Flora of the Caucasus, Kuznetsoff, 21; Sap, Does it Rise from the Roots?, 27; Araucaria Cones, Duke of Argyll, F.R.S., 8; Dr. A. Irving, 29; John I. Plummer, 29; E. Brown, 29; Dr. Maxwell T. Masters, F.R.S., 56; D. Sharp, 57; A. D. Webster, 57; William Gardiner, 80; James Shaw, 81; Weather-damage to Prague Botanic Garden, 42; Kew and Berlin, Arrangements for Exchange of African Botanical Collections, 42; Flora of Fance, 43; Results of Herr J. Bornmüller's Expedition to Asia Minor, 43; Botanical Mythology of the Hindoos, Dr. Dymoke, 46; the Chrysanthemum, Thomas Child, 56; the Cola Nut, 62; Alexis and his Flowers, by Beatrix F. Cresswell, 79; Kew Gardens, Annual Reports, 87; the Fly Agaric, 88; Botany of British New Guinea, 114, 115; Carboniferous Sporangia, Prof. Bower, 119; Punctaria, Prof. T. Johnson, 119; Sponge, a Boring (*Alectona millari*), V. Jennings, 119; Botanical Enterprise in the West Indies, 129; the Pinks of Central Europe, by F. N. Williams, 149; Materials for a Flora of the Malayan Peninsula, No. 2, Dr. G. King, F.R.S., 149; Destructive Action of Fogs on Plants, 162; Botanical Exploration of the Gambia, 182; Kew Bulletin, 182; Garden Scholarships at Missouri, 187; Proposed Botanical Exploration of the Gambia, 182; Report on the Condition of the State Gardens at Buitenzorg, 196; the Alpine Flora, with a Suggestion as to the Origin of Blue in Flowers, T. D. A. Cockerell, 207; the Alpine Flora, W. T. Thiselton Dyer, F.R.S., 581; Prof. George Henslow, 581; Botany and Zoology of the West Indies, 212; the Geology of Round Island, Prof. John W. Judd, F.R.S., 253; W. T. Thiselton Dyer, F.R.S., 253; Showers of Manna (Edible Lichen) in Turkey in Asia, 255; Attractive Characters in Fungi, Dr. T. Wemyss Fulton, 269; Dr. M. C. Cooke, 393; Thibetan Dried Plants added to the Kew Herbarium, 299; West African Bass Fibre in English Market, 300; Seed-production of Sugar-cane, W. T. Thiselton Dyer, 300; Botanical Researches in British Guiana, Dr. Goebel's, 326; Exhibition of Fibres at London Chamber of Commerce, 326; the Podostemaceæ, Dr. Goebel, 327; Tobacco-Growing in Victoria, 355; India-rubber and Gutta-percha, W. Sowerby, 355; MM. Porta and Rigo's Spanish Expedition, 377; Return of Mr. C. G. Pringle from Mexico, 377; the Ipoh Poison of the Malay Peninsula, 377; Botanical Gazette, 382, 623; on a Collection of Plants from Upper Burma and the Shan States, Brigadier-General H. Collett and W. Botting Hemsley, F.R.S., 386; British Ferns and where Found, E. J. Lowe, F.R.S., 389; Coffee Cultivation in Bavaria, 398; Proposed Government Investigation of Cryptogamic Flora of Ecuador, 398; Gift from Messrs. Dakin of Collection of Samples of Curious Kinds of Tea to the Botanic Society, 398; Naturalized Flora of Andaman Islands since 1858, Dr. Prain, 398; the Flora of Warwickshire, J. G. Baker, F.R.S., 413; Institution of Society for Study of French Flora, 426; Botanical Experiment Station established at Fargo, Dakota, 426; Commercial Botany of the Nineteenth Century, John R. Jackson, 436; Botanisches Centralblatt, 451; Aspergillin, a Vegetable Hæmatin, G. Linossier, 456; Elementary Botany, J. W. Oliver, 461; the Flora of the Revillagigedo Islands, W. Botting Hemsley, 471; Botany-teaching in Indiana, 473; Orchids at Kew, 1890, 473; Additions to the Natural History Museum, 474; Hooker's Flora of British India, 519; Varieties of Cotton grown in Alabama, P. H. Mell, 520; Simultaneous Emission of Oxygen and Carbon Dioxide by Cacti, E. Aubert, 552; Vegetation of Lord Howe Island, W. Botting Hemsley, F.R.S., 565; Variations in Floral Symmetry of Certain Plants with Irregular Corollas, W. and A. Bateson, 575; Persian Tobacco, 591; the Assimilation of Lichens, Henri Jumelle, 624; Influence of Salinity on Quantity of Starch contained in Vegetable Organs of *Lepidium sativum*, Pierre Lesage, 624
Bottomley (Prof. J. T., F.R.S.): Prof. Van der Waals on the Continuity of the Liquid and Gaseous States, 415; Prof. A. W. Rücker, F.R.S., 437
Bouchard (Ch.), Vaccination by Minimum Doses, 576
Bouche, Microbes de la, Dr. Th. David, 174
Boulenger (George A.): Reptilia and Batrachia, O. Boettger, 361; Poisonous Lizards, 383
Bourne (Prof. Edward G.), Recession of the Niagara Gorge, 515
Bourne (Dr. G. A.), a Theory of the Course of the Blood in Earthworms, 334
Bower (Prof.), Carboniferous Sporangia, 119
Boys (Prof. C. V., F.R.S.): Notes on the Habits of some Common English Spiders, 40; Soap-Bubbles, 317; Cutting a Millimetre Thread with an Inch Leading Screw, 439
Brachycephalic Celt, Skeleton of, Worthington G. Smith, 319
Brady (Dr. Henry Bowman, F.R.S.): Death and Obituary Notice of, 278; Prof. M. Foster, F.R.S., 299
Brahe (Tycho), Life of, by J. L. E. Dreyer, A. M. Clerke, 98
Bramwell (Dr. B.), Position of the Visual Centre in Man, 527
Branly (Edward): Variations of Conductivity under different Electrical Influences, 120; Variations of Conductivity of Insulating Substances, 288
Brauner (B.), Volumetric Estimate of Tellurium, II., 503
Brazil, Nepheline Rocks in, II., the Tingua Mass, O. A. Derby, 239
Brennand (Wm.), Photometric Observations of Sun and Sky, 237
Briggs (William) and R. W. Stewart, Analysis of a Simple Salt, 126
Bright Crosses in the Sky seen from Mountain-tops, Dr. R. von Lendenfeld, 464
Brindley (H. H.), Relation between Size of Animals and Size and Number of their Sense-Organs, 119
Brinton (Dr. Daniel G.) the American Race, 556
British Art, Science Museum and Gallery of, at South Kensington, 590, 601
British Association: the Conference of Delegates of Corresponding Societies of the, 92; Coming Cardiff Meeting of, 397, 590; Third Report of the Committee on the Present Methods of Teaching Chemistry, 593
British Astronomical Association, 165
British Empire for 1889, Climatological Table of, 108
British Empire, the Colonies and Dependencies, W. G. Baker, 607
British Ferns and where Found, E. J. Lowe, F.R.S., 389
British Mosses, the Right Hon. Lord Justice Fry, F.R.S., 379, 400
British Museum, New Fossil Mammalia at, 85
British New Guinea, the Scientific Results of the Occupation of, 114
Brock (Dr. J. H. E.): Dr. Newsholme's Lessons on Health, 54; a Manual of Public Health, A. Wynter Blyth, 267; Household Hygiene, by Mary Russell Bissell, 461
Brodie (Fredk. J.): the Sunshine of London, 424; Remarkable Features in Winter of 1890–91, 599
Bronze, Dissociation of the Name, M. Berthelot, 95, 300
Brooksmith (J.), Key to Arithmetic in Theory and Practice, 294
Brown (Prof. Crum), Synthesis of Dibasic Acid by Electrolysis, 575
Brown (E.), Araucaria Cones, 29
Bruce (Dr. A.), Cyclopia in a Child, 527
Brückner (Prof.), Alleged Climatological Periods of 35 Years, 163
Brunton (Dr. T. Lauder, F.R.S.), the Influence of Temperature on the Vagus, 558
Brussels Academy of Sciences, 240, 432, 456
Brussels Observatory, Report of the, 475
Bryan (G. H.), Beats in Vibration of Revolving Cylinder or Bell and their bearing on Theory of Thin Elastic Shells, 215
Buchan (Dr. A.): Relation of High Winds to Barometric Pressure at Ben Nevis Observatory, 527; Meteorology of Ben Nevis, 538
Buchanan (J. Y., F.R.S.): Sulphur in Marine Mud and Nodules and its Bearing on their Modes of Formation, 143; Oceanic and Littoral Manganese Nodules, 287
Buckley (Arabella B.), Through Magic Mirrors, 246
Budapest, Coming International Ornithological Congress at, 472, 542
Buddha, the Glory of, 571
Budgen (W.), a Meteor, 608
Buitenzorg, Report on the Condition of the State Gardens at, 196

Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, 405
Bulletin of the Italian Geographical Society, 63
Bulletin de la Société des Naturalistes de Moscou, 334
Burbury (S. H., F.R.S.),, Modern Views of Electricity, Volta's so-called Contact Force, 268, 366, 439, 515
Burch (G. J.), Variations of Electromotive Force of Cells consisting of certain Metals, Platinum, and Nitric Acid, 142
Burma, India, and Ceylon, Elementary Geography of, Henry F. Blanford, F.R.S., 29
Burmah: Technical Education in, 209; on a Collection of Plants from Upper Burma and the Shan States, Brigadier-General H. Collett and W. Botting Hemsley, 386
Burmese Borderland, Proposed Government Exploration of, 378
Burmese Survey, the, 399
Bursting of a Pressure-Gauge, 318, 366
Burton (Lady), Civil List Pension granted to, 299
Burton (Cosmo Innes), Death and Obituary Notice of, 325
Busin (Prof.), the Diurnal Probability of Rain, 20
Butterflies: an Assumed Instance of Compound Protective Resemblance in an African Butterfly, W. L. Distant, 390
Butterflies Bathing, A. Sidney Olliff, 199
Butterflies, Sale of Captain Yankowsky's Collection of Chinese, 543

Cable to the Andamans necessary for Meteorological Purposes, 451
Cackling of Hens, Prof. Geo. J. Romanes, F.R.S., 516; Valentine Ball, F.R.S., 583
Cacti, Simultaneous Emission of Oxygen and Carbon Dioxide by, E. Aubert, 552
Cadmium, Preparation by Distillation *in vacuo* of Pure Metallic, E. A. Partridge, 44
Cailletet (L.): Method of Determining Critical Temperatures and Pressures of Water, 504; Open Manometer at Eiffel Tower, 599
Calcutta Zoological Gardens, Acquisition of a Greater Bird of Paradise by, 543
California, Northern, Earthquake in, 231
Callandreau (O.), the Capture Theory of Comets, 474
Calliphora erythrocephala, Anatomy, Physiology, Morphology, and Development of the Blow-fly, B. Thompson Lowne, 77
Cambridge: Agricultural Education at, 23; Mathematics at, Prof. George M. Minchin, 151; Philosophical Society, 20, 119, 215, 384, 407, 527
Camera Club, Fifth Photographic Conference, 519
Canada, by Track and Trail, a Journey through, Edward Roper, 532
Canadian Yukon, Meteorology of, W. Ogilvie, 189
Canalization of Pinsk Marshes, 164
Canary Islands, the, John Whitford, 317
Canidæ, a Monograph of the, Dogs, Jackals, Wolves, and Foxes, St. George Mivart, 385
Canoes, Throwing-sticks and, in New Guinea, Prof. Henry O. Forbes, 248; Prof. A. C. Haddon, 295
Cape Colony, Education in, Retirement of Sir Langham Dale, 130
Carcani (M.), Tromometer-Indications Vitiated by Wind, 520
Cards, Round Games with, Baxter-Wray, 414
Carl (Dr. Philip), Death of, 356
Carlier (E. W.), Curious Habit of Larvæ of *Orgyia antiqua*, 255
Carlisle (the Right Rev. Lord Bishop of), Bees' Cells, 295
Carpenter (W. L.), Death of, 230
Carruthers (W., F.R.S.), the Potato Disease, 474
Carus-Wilson (Cecil), Illustrations and Diagrams on Geology, 64
Case-books, Studies of Old, Sir James Paget, 6c6
Casey (Prof., F.R.S.), Death of, 230
Catania, the New Observatory in, 65, 141
Cattle, the Chillingham Wild, and Deer, 132
Caucasus, the Flora of the, Kuznetsoff, 21
Cavendish and Lavoisier, Priestley, Prof. T. E. Thorpe, F.R.S., 1
Cavern discovered at Baden, near Vienna, Remarkable, 587
Cell Theory, Past and Present, Sir William Turner, F.R.S., 10, 31
Celt, Brachycephalic, Skeleton of, Worthington G. Smith, 319
Census of the United States, 73
Census, the Japanese, 378
Cette, Proposed Marine Zoological Station at, Prof. Armand Sabatier, 397
Ceylon and Burma, Fauna of British India, including, R. Bowdler Sharpe, 266; O. Boettger, 361
Ceylon, India, and Burma, Elementary Geography of, Henry F. Blanford, F.R.S., 29
Chabrié (C.), Saponification of Halogen Organic Compounds, 96
Chadwick (W. I.), the Stereoscopic Manual, 103
Chadwick's (Sir Edwin), Bequests for Advancement of Sanitary Science, 130
Chambers's Encyclopædia, 221
Charleston Earthquake, Captain C. E. Dutton, 509
Charlois (M.), Asteroid (308), 480
Charpy (G.), Affinities of Iodine in Solution, 48
Charroppin (Prof.), on the Solar Corona, 568
Chassagny and Abraham, Thermo-electric Researches, 24
Chassagny (M.), Researches in Thermo-electricity, 95
Chattock (A. P.), Modern Views of Electricity, 367, 491
Chemistry: Priestley, Cavendish, and Lavoisier, Prof. T. E. Thorpe, F.R.S., 1; Prof. Curtius' New Gas, Hydrazoic Acid, 21; Azoimide or Hydrazoic Acid, Prof. Curtius, 378; Exercises in Practical Chemistry, A. D. Hall, 29; Preparation of Pure Metallic Cadmium by Distillation *in vacuo*, E. A. Partridge, 44; Practical Inorganic Chemistry, by E. J. Cox, 54; Anleitung zur Darstellung Chemischer Präparate: ein Leitfaden für den Praktischen Unterricht in der Anorganischen Chemie, Dr. Hugo Erdmann, 126; Affinities of Iodine in Solution, H. Gautier and G. Charpy, 48; Conditions of Reactions of Isopropylamines, H. and A. Malbot, 48; Certain Formations on Copper and Bronze, Raphael Dubois, 48; Chemistry of Iron and Steel Making, W. Mattieu Williams, John Parry, 50; Benzoyl Fluoride, M. Guenez, 64; Hyperphosphorus Acids, 72; the Action of Methylene Fluoride upon Bacteria, M. Chabrié, 89; the β-Pyrazol-dicarbonic Acids, M. Maquenne, 96; Saponification of Halogen Organic Compounds, C. Chabrié, 96; Chemical Arithmetic, Part I., by W. Dittmar, F.R.S., 102; Decomposition of Carbon Dioxide by Electricity, Prof. von Hofmann, 109; the Specific Heats of Gases at Constant Volume, Part I., Air, Carbon Dioxide, and Hydrogen, J. Joly, 116; Conditions of Chemical Change between Nitric Acid and certain Metals, V. H. Veley, 118; Relations among Refractive Indices of Chemical Elements, Rev. T. P. Dale, 118; Artificial Production of Chromium Blue, Jules Garnier, 120; Analysis of a Simple Salt, William Briggs and R. W. Stewart, 126; Relations between Optical Dispersive Power of Alcohols, Ethers, and Fatty Acids, and their Molecular Weight and Constitution, Barbier and Roux, 133; Synthetical Production of Cyanogen Compounds by Mutual Action of Charcoal, Gaseous Nitrogen, and Alkaline Oxides, or Carbonates, Prof. Hempel, 164; Chemical Action and the Conservation of Energy, Prof. Spencer U. Pickering, F.R.S., 165; Chemical Action and the Conservation of Energy, Geo. N. Huntly, 246; New Method of Studying Compressibility and Expansion of Liquids and Gases, E. H. Amagat, 168; New Method of Preparing Tetramethylbenzidine, Charles Lauth, 168; Molecular Dispersion, Dr. J. H. Gladstone, F.R.S., 198; the Isolation of Hydrazine, $\begin{array}{c}NH_2\\|\\NH_2\end{array}$, A. E. Tutton, 205; Alloys of Sodium and Lead, Greene and Wahl, 216; Refraction and Dispersion in Isomorphous Series of Two-axial Crystals, F. L. Perrot, 216; Reduction of Nitrates to Nitrites by Seeds and Tubercles, Émile Laurent, 240; Volatile Nitrogen Compounds given off by Vegetable Mould, M. Berthelot, 336; Influence of Salinity on Quantity of Starch contained in Vegetable Organs of *Lepidium sativum*, Pierre Lesage, 624; Cadmium and Magnesium Analogues of Zinc Methide and Ethide, Dr. Löhr, 256; Chemical Society, 261, 454, 477, 503, 549; Jubilee of, 277, 376, 397, 491; Dr. W. J. Russell's Address, 440; Sir Lyon Playfair, 491; Sir William Grove, 491; Magnetic Rotation of Saline Solutions, W. H. Perkin, F.R.S., 261; Action of Light on Ether in presence of Oxygen and Water, Dr. A. Richardson, 261; Halogens and Asymmetrical Carbon Atom, F. H. Easterfield, 261; New Method of determining Specific Volumes of Liquids, &c., Prof. S. Young, 261; Molecular Condition of Metals when alloyed with each other, C. T. Heycock and F. H. Neville, 262; Spectra of Blue and

Yellow Chlorophyll, Prof. W. N. Hartley, F.R.S., 262; Action of Heat on Nitrosyl Chloride, J. J. Sudborough and G. H. Miller, 262; Conductivities of Isomeric Organic Acids and their Salts, Daniel Berthelot, 264; Trithienyl, Adolphe Renard, 264; New Series of well-crystallizing Salts of Iridium-Ammonium, Dr. Palmaer, 280; Physical Properties and Molecular Constitution of Simple Metallic Bodies, P. Jacobin, 288; a New Class of Organic Bases, Dr. Stoehr, 301; Influence of Dissolvents on Rotary Power of Camphols and Isocamphols, A. Haller, 311; Sulphur in Vegetables, Berthelot and André, 311; Berberine, Prof. W. H. Perkin, Jun., 335; Re-determination of Atomic Weight of Rhodium, Prof. Seubert and Dr. Kobbé, 356; Elementary Systematic Chemistry for the use of Schools and Colleges, Prof. William Ramsay, F.R.S., 364, 391; Sodium Amide, M. Joannis, 399; Reaction of Hypochlorites and Hypobromites on Phtalimide and Phtalamide, Drs. Hoogewerff and Van Dorp, 408; Hand-Dictionary of Chemists and Physicists, Carl Schaedler, 414; Final Results of Prof. Seubert's Re-determination of Atomic Weight of Osmium, 427; Solubility of Potassium Bitartrate Ch. Blarez, 432; Distribution of Sea-Salt according to Altitude, A. Muntz, 432; Discovery of a New Metal, Titanate of Manganese, Dr. Hamberg, 452; Magnetic Rotation, W. Ostwald, 454; Vapour Density of Ammonium Chloride, Pullinger and Gardner, 454; Formation of Explosive Substances from Ether, Prof. P. T. Cleve, 454; Does Magnesium combine with Hydrocarbon Radicles?, Masson and Wilsmore, 454; Compounds of Oxides of Phosphorus with Sulphuric Anhydride, R. H. Adie, 454; Combustion of Magnesium in Water Vapour, G. T. Moody, 454; Photographic Chemistry, Prof. Meldola, F.R.S., 473; Silicon Bromoform, M. Besson, 474; a Treatise on (New Edition), Roscoe and Schorlemmer, 474; Action of Reducing Agents on $\alpha\alpha'$-Diacetylpentane, Synthesis of Dimethyldihydroxyheptamethylene, Kipping and Perkin, 477; Osmotic Pressure of Salts in Solution, Direct Comparison of Physical Constants by Raoult's Method and those ascertained by Direct Observation of Osmotic Pressure, R. H. Adie, 478; Derivatives of Piperonyl, F. M. Perkin, 478; Andresen's β-Naphthylaminedisulphonic Acid, Armstrong and Wynne, 478; Transformation of Sodium Pyrophosphite into Hydrogen Sodium Phosphite, L. Amat, 480; Fermentation of Starch by Action of Butyric Ferment, R. Varet, 480; Colour Absorption Spectrum of Liquid Oxygen, M. Olszewski, 498; Gold-coloured Allotropic Silver, M. C. Lea, 500; Crystalline Form of Calcium Salt of Optically Active Glyceric Acid, A. E. Tutton, 503; Fermentation induced by Friedländer's Pneumococcus, Frankland, Stanley, and Frew, 503; Volumetric Estimation of Tellurium, II., B. Brauner, 503; Chloro- and Bromo-Derivatives of Naphthol and Naphthylamine, Armstrong and Rossiter, 503; Transformations accompanying Combustion of Iron by Diamond, M. F. Osmond, 504; Action of Heat on, and a Reaction of, Carbonic Oxide, M. Berthelot, 528; the Odour of Earth caused by an Organic Compound of the Aromatic Family, Berthelot and André, 528; New Compounds of Mercuric Oxide, Van Bemmelen, 528; Influence of Temperature on Viscosity of Fluid Methyl Chloride, L. M. J. Stoel, 528; Poisonous Compound of Nickel and Carbon Monoxide, M. Hanriot, 544; Reproduction of Hornblende in Crystals, M. Kroustchoff, 545; Molecular Refraction and Dispersion of various Substances, Dr. J. H. Gladstone, F.R.S., 549; the Crystalline Alkaloid of *Aconitum napellus*, Dunstan and Ince, 550; Asymmetry of Nitrogen in Substituted Ammonium Compounds, S. B. Schnyver, 550; Crystallographical Characters of Aconitum from *Aconitum napellus*, A. E. Tutton, 550; Acetylcarbinol, W. H. Perkin, Jun., 550; Action of Ethyl Dichloracetate on Sodium Derivative of Ethyl Malonate, Bishop and Perkin, 550; Benzoylacetic Acid and some of its Derivatives, V., Perkin and Stenhouse, 550; Syntheses with aid of Ethyl Pentanetetracarboxylate, Perkin and Prentice, 550; Boron Iodide, M. Moissan, 568; Synthesis of Dibasic Acids by Electrolysis, Brown and Walker, 575; the Virial, with Special Reference to Isothermals of Carbonic Acid, Prof. Tait, 575; Asymmetry and Production of Rotary Power in Chlorides of Compound Ammoniums, Le Bel, 576; Solutions, Prof. William Ramsay, F.R.S., 589; Chlorobromides of Silicon, M. Besson, 592; the Present Methods of Teaching Chemistry, Third Report of the British Association Committee on, 593; Allotropic Silver, M. C. Lea, 598; Volumetric Composition of Water, E. W. Morley, 598; Certain Points in Estimation of Barium as Sulphate, F. W. Mar, 598; Holotrichite or Feather Alum, E. H. S. Bailey, 598; Transformation of Cupreine into Quinine, E. Grimaux and A. Arnaud, 599; Amidoisoxazol, M. Hanriot, 599; Aspergillin, G. Linossier, 600: Daubreelite, Artificial Reproduction of, S. Meunier, 600; Co-existent Phases of a Mixture, M. Van der Waals, 600; Salts of Sub-oxide of Silicon, M. Güntz, 620; Two New Forms of Sulphur, M. Engel, 624; Action of Urea on Sulphanilic Acid, J. Ville, 624
Child (Thomas), the Chrysanthemum, 56
Chili, Earthquakes in, 24
Chillingham Wild Cattle and Deer, 132
Chin-Lushai Country, New Map of, 209
China: Ethnology of the Northern Shan States, 62
China, South-West, a Journey in, A. E. Pratt, 570
China, Western, Three Years in, Alexander Hosie, 290
Christy (Thomas), Various Specimens of Honey, 383
Chronology, Babylonian Astronomy and, 369
Chrysanthemum, the, Thomas Child, 56
City and Guilds of London Institute and County Councils, 429
City and Guilds of London Institute, the Electrical Department, 619
Clark (Latimer, F.R.S.), a Dictionary of Metric and other Useful Measures, 487
Clark (Willis G.), History of Education in Alabama, 362
Clarke (W. E.), the Irish Rat, 255
Class-book of Geology, Archibald Geikie, F.R.S., Prof. A. H. Green, F.R.S., 242
Class-book on Light, R. E. Steel, 606
Classification of Animals, W. E. Fothergill, Emil Selenka, and J. R. A. Davis, 124
Clavaud (Prof.) Death of, 254
Clay Soils, Action of Lime on, Alexander Johnstone, 308
Clayden (Arthur W.), Photographs of Meteorological Phenomena, 55
Clerke (Agnes M.): J. L. E. Dreyer's Life of Tycho Brahe, 98; the System of the Stars, 169; Recent Photographs of the Annular Nebula in Lyra, 419; Œuvres Complètes de Christiaan Huygens, 433
Cleve (Prof. P. T.), Formation of Explosive Substance from Ether, 454
Climate, Glacial, Prof. N. S. Shaler, 155
Climatological Table for British Empire for 1889, 108
Clouds, Iridescent, J. Lovel, 464
Clouds, Luminous, O. Jesse, 59
Co-adaptation (Prof. Geo. J. Romanes, F.R.S.), 490, 582; Prof. R. Meldola, F.R.S., 557
Coal and Plant-bearing Beds of Palæozoic and Mesozoic Age in Eastern Australia and Tasmania, by B. O. Feistmantel, J. S. Gardner, 148
Coal Dust and Explosions in Coal Mines, 354
Coal Measures, Plants represented in the, Prof. W. C. Williamson, F.R.S., 355, 454
Coal-mining in 1850 and 1890, M. Tonge, 544
Cockerell (T. D. A.): the Alpine Flora, with a Suggestion as to the Origin of Blue in Flowers, 207; Neo-Lamarckism and Darwinism, 533
Cocks and Hens, E. J. Lowe, F.R.S., 583
Coco-de-Mer in Cultivation, William Watson, 19
Coco-nut Beetles, H. N. Ridley, 476
Coffee Cultivation in Bavaria, 398
Cola Nut, the, 62
Colardeau (E.), Method of Determining Critical Temperatures and Pressures of Water, 504
Cold of 1890-91, E. J. Lowe, F.R.S., 294
Cold on Animals, Action of Excessive, G. Colin, 408
Colin (G.), Action of Excessive Cold on Animals, 408
College of Science, Durham, Newcastle-upon-Tyne, 519
Collett (H., Brigadier-General) and W. Botting Hemsley, F.R.S., on a Collection of Plants from Upper Burma and the Shan States, 386
Colonial Exhibition at Paris, Proposed International, 542
Colonist's Medical Hand-book, E. A. Barton, 150
Colorado, Colonel Holabird's Explorations in, 44
Colour, Antipathy (?) of Birds for, William White, 608
Colour in Woven Design, Roberts Beaumont, 343
Colours, Photography of, G. Lippmann, 360
Columbus reproduced in *Cosmos*, recently discovered Portrait of, 619
Comets: D'Arrest's, 22; a New Comet (?), 89; Two New Comets (*e* and *f* 1890), 90; W. F. Denning, a New, 516; Zona's Comet, 111; Elements and Ephemeris of Zona's Comet,

165; Perihelia of Comets, Dr. Holetschek, 233; the Capture Theory of, O. Callandreau, 474; Comet *a* 1891, 546; the Discovery of Comet *a* 1891, W. F. Denning, 558; Comet Barnard-Denning (*a* 1891), Prof. Berberich on the Elements of, 569
Commercial Botany of the Nineteenth Century, John R. Jackson, 436
Compass, on Local Magnetic Disturbance of the, in North-West Australia, Commander E. W. Creak, F.R.S., 471
Conchology: Dispersal of Freshwater Shells, H. Wallis Kew, 56; the Conchological Fauna of the Sahara, Dr. P. Fischer, 312
Congress, the Astronomical, 516
Congress, Coming International Geographical, 301
Congress, the Coming International Ornithological, 130, 472, 542
Congress of French Geographical Societies, National, 473
Congress of Geologists, the Coming International, 376
Congress of German Geographers, 519
Congress at the Hague, Proposed International Agricultural, 542
Congress, International, of Hygiene and Demography, 241, 300, 472
Congress at Moscow, Proposed International Scientific, 207
Conroy (Sir John, Bart.), Change in Absorption Spectrum of Cobalt Glass produced by Heat, 406
Conservation of Energy, Chemical Action and the, Geo. N. Huntly, 246
Constant of Aberration, Determination of the, MM. Lœwy and Puiseux, 498
Consumption: Dr. Koch's Cure for, 25, 49; Prof. Virchow on the, E. H. Hankin, 248; Nature of Koch's Remedy, 265
Contact Force, Volta's so-called Modern Views of Electricity, S. H. Burbury, 268; Prof. Oliver J. Lodge, F.R.S., 268
Continuity of the Liquid and Gaseous States, Prof. Van der Waals on the, Robert E. Baynes, 488
Conwentz (Dr. H.), Monographie der baltischen Bernsteinbäume: Vergleichende Untersuchungen über die Vegetationsorgane und Blüthen, sowie über das Harz und die Krankheiten der baltischen Bernsteinbäume, 221
Coode (J. M.), Exceptional Mode of Hunting adopted by Panther, 132
Cooke (Dr. M. C.), Attractive Characters in Fungi, 57, 224, 393
Coppin (Liévin), the New Commercial Museum at Rome, 497
Coral Islands and Reefs, Dr. Langenbeck, 293
Coral, Rate of Growth in, Alexander Agassiz, 65; Prof. Heilprin, 473
Coral Rocks of West Indies, Jukes-Brown and Harrison, 383
Corambe testitudinaria, Anatomy of, H. Fischer, 360
Corfield (Dr. W. H., F.R.S.), International Congress of Hygiene and Demography, 511
Cornil (A. V.): and V. Babes, on Bacteria, 195
Cornu (A.): Ultra-Violet Limit of Solar Spectrum, 216; Determination of Direction of Vibration in Polarized Light, 336
Corona, the Solar, 568; Prof. Charroppin on the, 568; Prof. Frank H. Bigelow, 568; Mr. Schaeberle, 568
Cortie (Rev. A. L.), Spectroscopic Note (D_3), 210; Spectroscopic Observations of Sun-spots, 256
Coste (F. H. Perry), on Frozen Fish, 516
Cottam (Arthur), Charts of the Constellations, 557
Cotterill (James H., F.R.S.), the Steam Engine considered as a Thermodynamic Engine, J. Larmor, 123
Cotterill (James H., F.R.S.) and J. H. Slade, R.N., Lessons in Applied Mechanics, 461
Cotton Grown in Alabama, Varieties of, P. H. Mell, 520
County Councils and Technical Education, 97, 602
Cowan (T. W.), the Honey Bee, Rev. T. Tuckwell, 578
Cox (E. J.), Practical Inorganic Chemistry, 54
Cox (H. J.), Waterspout at Newhaven, Connecticut, 285
Crab-eater, Capture of a Young, 63
Crane (Miss Agnes), Formation of Language, 534
Crawshay (Richard), the Antelopes of Nyassaland, 143
Creak (Commander E. W., F.R.S.), on Local Magnetic Disturbance of the Compass in North-West Australia, 471
Creation, the Newly-discovered Assyrian Version of the Story of the, T. G. Pinches, 164
Cremation, the Case for, Frederic Passy, 231
Cresswell (Beatrix F.), Alexis and his Flowers, 79
Croll (James, F.R.S.): Obituary Notice of, 180; the Philosophical Basis of Evolution, 434
Cromer, Mud Glaciers of, William Sherwood, 515
Crookes (Dr. Wm.), Electricity in Transition, 278
Crosses, Bright, seen in the Sky from Mountain Tops, Dr. R. von Lendenfeld, 464
Crow and Squirrels, C. P. Gillette, 620
Crowing of the Jungle Cock, Prof. Henry O. Forbes, 295
Crustacea and Annelids, Prof. T. H. Morgan, 459
Crustaceans in Bois de Boulogne Lakes, hitherto Unknown, Jules Richard, 280
Crustacea, on the Origin of Vertebrates from, W. H. Gaskell, F.R.S., 70
Cryptogamia: the Value of Attractive Characters to Fungi, Charles R. Straton, 9; M. C. Cooke, 57; R. Haig Thomas, 79; Attractive Characters in Fungi, 151; Attractive Characters in Fungi, T. Wemyss Fulton, 269; British Mosses, the Right Hon. Lord Justice Fry, 379, 400; the Fungi of Warwickshire, James E. Bagnall and W. B. Grove, 413
Crystallography: Krystallographisch-chemischen Tabellen, Dr. A. Fock, 197; Dr. Fock on the Relationship between Optical Activity of Substances in Solution and Hemihedrism of their Crystalline Forms, 327
Crystals of Platinum and Palladium, J. Joly, 541
Cumbrae, the Naturalist of, being the Life of David Robertson, Rev. T. R. R. Stebbing, 532
Cunningham (J. T.), the Common Sole considered both as an Organism and as a Commodity, 193
Curlew, the Australian, Colonel Legge, 89
Curtius (Prof.): New Gas, Hydrazoic Acid, 21; Azoimide or Hydrazoic Acid, 378
Cutting a Millimetre Thread with an Inch Leading Screw, Prof. C. V. Boys, F.R.S., 439
Cyclone Tracks of South Indian Ocean, Dr. Meldrum's Atlas of, 620
Cyclones and Anticyclones, the Causes of, Henry F. Blanford, 15, 81

Dale (Rev. T. P.), Relations among Refractive Indices of Chemical Elements, 118
Dale (Sir Langham), Retirement of, 130
Dana (J. D.), Long Island Sound in Quaternary Period, 260
Dark Transits of Jupiter's Satellites, Mr. Keeler, 302
Darkness of London Air, 222
D'Arrest's Comet, 22
Darwin (Charles, F.R.S.), Multiple Origin of Races, 535
Darwin on the Unity of the Human Race, Duke of Argyll, F.R.S., 415
Darwin (Prof. G. H., F.R.S.), on Tidal Prediction, 320, 609
Darwinian Theory of the Origin of Species, Francis P. Pascoe, Prof. R. Meldola, F.R.S., 409
Darwinism and Neo-Lamarckism, Prof. George Henslow, 490, 581; T. D. A. Cockerell, 533; the Modification of Organisms, David Syme, Dr. Alfred R. Wallace, 529
Daubrée (M.): Mechanical Action on Rocks of Gas at High Pressure or in Rapid Motion, 120; Experiments on Mechanical Action on Rocks of Gases at High Temperatures and Pressures and in Rapid Motion, 311
David (Dr. Th.), Microbes de la Bouche, 174
Davies (Wm.), Death of, 398
Davis (H. R.), Pectination, 367
Davis (J. R. A.) and Emil Selenka, a Zoological Pocket-book or Synopsis of Animal Classification, 124
Dawkins (Prof. W. Boyd, F.R.S.), Lake-Dwellings of Europe, being the Rhind Lectures for 1888, Robert Munro, 341
Dawson (Bernard), Gas Furnaces, 332
Deaf in America, History of Articulating System for, G. G. Hubbard, 231
Deane (Rev. G.), the Future of Geology, 303
Death, Weismann and Maupas on the Origin of, E. G. Gardiner, 458
Decimal Measure-System of Seventeenth Century, Prof. J. H. Gore, 309
Decimal Metric System, the, J. E. Dowson, 354
Decimal System, Mr. Goschen and the, 326
Deer and Chillingham Wild Cattle, 132
Defforges (Commandant), Resistance of various Gases to Movement of Pendulum, 408
Deighton (Horace), Elements of Euclid, 127
Delebecque (A.), Soundings of Lake Leman, 264
Demography, International Congress of Hygiene and, 241; Dr. W. H. Corfield, 511

Dendy (Arthur), Comparative Anatomy of Sponges, 333
Denning (W. F.): New Comet, 516; the Discovery of Comet *a* 1891, 558
Dentine, J. H. Mummery, 501
Denudation, Dry, Prof. Johannes Walther, 556
Depopulation of France, the, Ed. Marbeau, 183
Derby (O. H.), Nepheline Rocks in Brazil, II., the Tingua Mass, 239
Design of Structures, S. Auglin, 436
Deslandres (H.), Spectrum of α Lyræ, 432
Deslandres (H.), Application of New Method of Investigating Feeble Bands in Banded Spectra to Hydrocarbon Spectra, 552
Determination of the Constant of Aberration, MM. Lœwy and Puiseux, 498
Determining Specific Gravity, a Method of, Prof. W. J. Sollas, F.R.S., 404
Determinism and Force, Dr. James Croll, F.R.S., 434; Prof. Oliver J. Lodge, F.R.S., 491; Prof. C. Lloyd Morgan, 558
Devonshire County Council, Grants for Scientific Lectures by, 326
Dew, Colonel Badgley, 311
Dewar (Prof. James, F.R.S.), the Scientific Work of Joule, 111
Diamond, the Great Mogul's, and the Koh-i-nur, Valentine Ball, F.R.S., 103
Diamond, Transformations Accompanying Carburation of Iron by, F. Osmond, 504
Dianthus, the Pinks of Central Europe, F. N. Williams, 149
Diphtheria, a Cure for Tetanus and, E. H. Hankin, 121
Disease, the Relation of Ground Water to, Baldwin Latham, 94
Disk, Whirling Ring and, Prof. Oliver J. Lodge, F.R.S., 533
Disks, Spinning, J. T. Nicolson, 514
Dispersion, Molecular, Dr. J. H. Gladstone, F.R.S., 198
Distant (W. L.), an Assumed Instance of Compound Protective Resemblance in an African Butterfly, 390
Dittmar (W., F.R.S.), Chemical Arithmetic, Part I, 102
Dixon (E. T.), the Foundations of Geometry, 554
Dixon (H. H.), Locomotion of Anthropods, 223
Dixon (H. N.), Araucaria Cones, 128
Dogs, Jackals, Wolves, and Foxes: a Monograph of the Canidæ, St. George Mivart, F.R.S., 385
Dohrn (Dr. Anton), Zoological Station at Naples, 465
Doppler's Principle, R. W. Stewart, 80
Dorp (Dr. Van), Reaction of Hypochlorites and Hypobromites on Phtalimide and Phtalamide, 408
Douvillé (H.), Age of Strata cut by Panama Canal, 456
Dowson (J. E.), the Decimal Metric System, 354
Dreyer (J. L. E.), Tycho Brahe, A. M. Clerke, 98
Drills, Rock, 499
Drouin (René), Comparative Alkalinity of Blood of Vertebrata, 144
Dry-rot Fungus, F. T. Mott, 129
Drygalski (Dr. E. von), Proposed Expedition to Greenland, 326
Dublin Royal Society, 551
Dubois (Raphael), Certain Formations on Copper and Bronze, 48
Duck and Auk Tribes, on the Morphology of the, W. Kitchen Parker, F.R.S., 486
Dunstan (Prof. Wyndham R.), the Threshold of Science, C. R. Alder Wright, F.R.S., 314
Dunstan (W. R.), Crystalline Alkaloid of *Aconitum napellus*, 550
Duplicity of α Lyræ, 257
Dupuis (N. F.), Streamers of White Vapour, 175
Durham (W.), Astronomy, Sun, Moon, and Stars, 103
Durham College of Science, Newcastle-upon-Tyne, 519
Dutch Coast, Tides off the, 450
Dutton (Captain C. E.), Charleston Earthquake, 509
Dyer (W. T. Thiselton, F.R.S.): Seed Production of Sugar-Cane, 300; the Geology of Round Island, 253; Multiple Origin of Races, 535; the Alpine Flora, 581
Dymoke (Dr.), the Botanical Mythology of the Hindoos, 46

Early Man, the Story of, N. D'Anvers, 103
Earth, Life Story of our, N. D'Anvers, 103
Earth, the Autobiography of the, Rev. H. N. Hutchinson, 365
Earthquakes: Earthquakes in Chili, 24; Earthquake of June 1890 in Persia, Dr. Jellisew, 42; Earthquakes in the North-East of Scotland, 62; in Inverness-shire, 108; List of Recorded Earthquakes in Central and Northern Japan, August 11, 1888, to December 31, 1889, W. B. Mason, 131; Earthquake in Mexico, 131; in Northern California, 231; in Texas, 254; in Bosnia, Granada, Mexico, Algiers, and Geneva, 279; the Recent Earthquake in Algiers, 356; Charleston Earthquake, Captain C. E. Dutton, 509; Earthquake at Boscastle, 519; Destruction of a Hamlet by an Earthquake in Armenia, 566
Earthworms, Alvan Millson on, 179; a Theory of the Course of the Blood in, Dr. G. A. Bourne, 334
Easterfield (F. H.), Halogens and Asymmetrical Carbon Atom, 261
Eccentricities of Stellar Orbits, Dr. T. J. J. See, 279
Ecuador, Proposed Government Investigation of Cryptogamic Flora of, 398
Edinburgh Botanic Garden, Thousands of Sea-Gulls in, 278
Edinburgh Royal Society, 143, 239, 287, 335, 431, 479, 527, 575
Edmunds (Lewis) and A. Wood Renton, the Law and Practice of Letters Patent for Inventions, 53
Education: Technical, in Switzerland, 21; Technical Education and the County Councils, 97, 602; Technical Education in India, 109, 209; Conference of National Association for Promotion of Technical Education, 130; the Technical Instruction Act and the Local Taxation Act, 167; Technical Education in Lancashire, 326; Sir H. Roscoe's Bill, 397; the Electrical Department of City and Guilds of London Institute, 619; Exemption by McKinley Tariff of Educational Books, 88; Geographical Teaching in United States, 131; Education in Cape Colony, Retirement of Sir Langham Dale, 130; Agricultural Education, P. McConnell, 182; Agricultural Education in Würtemberg, Lord Vaux of Harrowden, 567; Educational Review, 183; Manual Training in Education, C. M. Woodward, 220; Association for Improvement of Geometrical Teaching, 230, 278; History of the Articulating System in America for the Education of the Deaf, Gardiner G. Hubbard, 231; Report of the Oxford University Extension Delegacy, 355; Secondary Education in Scotland, 325; Mansion House Meeting in aid of University and King's Colleges Extension Funds, 397; Right Hon. G. J. Goschen and the proposed Increase in the Annual Grant to the University Colleges of England, 425; History of Education in Alabama, Willis G. Clark, 362; Scientific Education for Women only, Bedford College, London, 425; Opening of the Shaen Wing of Bedford College, 496; Mason Science College, 591
Edwards (Aubrey), Fight between Cock Wrens, 209
Effects of Use and Disuse Inherited, Are the, William Platt Ball, Prof. Geo. J. Romanes, F.R.S., 217
Eggs and Larvæ of some Teleosteans, on the, Ernest W. L. Holt, 510
Eggs, the Protection of Wild Birds', 376
Egypt, the Preservation of Ancient Monuments of, 230
Egypt, a Pyramid Temple discovered by Mr. Petrie at Maydoom, 591
Egyptian Monuments, the State of, 182, 230
Ekholm (Nils), on the Determination of the Path taken by Storms, 63
Elasmobranch Fishes, Dr. Anton Fritsch on, 293
Elder (William), Mutual Aid among Animals, 56
Elderton (William A.), Maps and Map-drawing, 196
Electricity: Thermo-electric Researches, Chassagny and Abraham, 24; Electro-dynamic Origin of Planetary Motion, C. V. Zenger, 48; Researches in Thermo-electricity, MM. Chassagny and Abraham, 95; Elementary Manual of Electricity and Magnetism, Andrew Jamieson, 102; Magnetism and Electricity, J. Spencer, 127; the Determination of the Work done upon the Cores of Iron in Electrical Apparatus subject to Alternating Currents, T. H. Blakesley, 116; Variations of Electromotive Force of Cells consisting of certain Metals, Platinum, and Nitric Acid, Burch and Veley, 142; United States Electric Light Association, 108; the Electric Light, A. Bromley Holmes, 196; Electric Lighting in St. Louis, 497; the Electric Light for Photographic Self-registering Instruments, Dr. H. Wild, 519; Variations of Conductivity under different Electrical Influence, Ed. Branly, 120; the Pyro-electricity of Tourmaline, Riecke, 120; Electric Railway from St. Petersburg to Archangel, Proposed, 163; Board of Trade Committee on Electrical Standards, 181; Additional Notes on Secondary Batteries, Dr. J. H. Gladstone, F.R.S., and W.

Hibbert, 190; Proposed Historical Exhibition of Electrical Apparatus, 231; a Treatise on Electro-metallurgy, W. G. McMillan, 244; the Art of Electrolytic Separation of Metals, J. G. Gore, F.R.S., 244; a Treatise on Electro-metallurgy, W. G. McMillan, 244; Leçons sur l'Électricité professées à l'Institut Électro-Technique Montefiore annexé à l'Université de Liége, Eric Gerard, 245; Experiments with Selenium Cells, Shelford Bidwell, F.R.S., 262; Alternate Current Condensers, James Swinburne, 262; a Pocket-book of Electrical Rules and Tables, John Munro and Andrew Jamieson, 268; Modern Views of Electricity, Volta's so-called Contact Force, S. H. Burbury, F.R.S., 268, 366, 439, 515; Prof. Oliver J. Lodge, F.R.S., 268, 367, 463; A. P. Chattock, 367, 491; Electricity *in transitu*, Dr. William Crookes, 278; Variations of Conductivity of Insulating Substances, E. Branly, 288; Electricity as a Locomotive Power, a New Peril for Scientific Teaching Institutions, 299; the Alternating Electric Wire between Ball and Point, E. L. Nichols, 309; Electric Mining Machinery, Ll. B. and C. W. Atkinson, 355; Indicator of Charge of Accumulators, M. Roux, 356; Effect of Temperature on Conductivity of Solutions of Sulphuric Acid, Miss H. G. Klaasen, 384; an Automatic Lamp-lighter, Shelford Bidwell, F.R.S., 395; Electric Discharge through Rarefied Gases without Electrodes, Prof. J. J. Thomson, 384; Experiments in Illustration of Prof. Minchin's Paper on Photo-electricity, 407; Application of Maxwell's Principles to Electrical Phenomena in Moved Bodies, H. A. Lorentz, 408; the first Electrical Launch ever built for the English Government, 450; the Human Eye and Electricity, Prof. Dubois, 452; Electrical Exhibition at St. Pancras Vestry Hall, 472; Proof of Generality of Blakesley's Formulæ, Tests of a Transformer, Ayrton and Taylor, 478; Further Contributions to Dynamometry, T. H. Blakesley; 479; Interaction of Longitudinal and Circular Magnetizations in Iron and Nickel Wires, Prof. C. G. Knott, 480; Electrostatic Wattmeters, James Swinburne, 501; the Theory of Dissociation of Electrolytes into Ions, Prof. S. U. Pickering, F.R.S., 548; Some Points in Electrolysis, J. Swinburne, 549; Synthesis of Dibasic Acids by Electrolysis, Brown and Walker, 575; Electro-plater's Manual, G. E. Bonney, 579; Hertz's Experiments, 536; Measurement of Power supplied by any Current to any Circuit, Ayrton and Sumpner, 572; a Lecture Experiment illustrating the Magnetic Screening of Conducting Media, Prof. J. J. Borgman, 583; Exodus of Electricians from U.S.A. to Europe, 619; the City and Guilds of London Institute and Electricity, 619; a Property of Magnetic Shunts, Prof. S. P. Thompson, 623; an Alternating Current Influence Machine, James Wimshurst, 623

Eliot's Cyclone Memoirs, III., 620

Ellipsoidal Harmonics, W. D. Niven, F.R.S., 189

Ellis (Dr. Alex. J., F.R.S.), Death and Obituary Notice of, 20

Emerson (P. H.), Wild Life on a Tidal Water, 366

Embalming Dead Bodies by Galvanoplastic Method, Dr. Variot, 163

Emin Pasha, 162

Encyclopædia Britannica, Zoological Articles Contributed to the, E. Ray Lankester, F.R.S., 607

Encyclopædia, Chambers's, 221

Endocardiac Pressure, the Action of the Nerves of the Batrachian Heart in Relation to Temperature and, G. N. Stewart, 548, 558

Energy, Chemical Action and the Conservation of, Prof. Spencer U. Pickering, F.R.S., 165; Geo. N. Huntly, 246

Engel (M.), Two New Forms of Sulphur, 624

Engineering: Lessons in Applied Mechanics, by J. H. Cotterill, F.R.S., and J. H. Slade, 461

Engineers, Institution of Mechanical, 22

Engineers, Mechanical, Hand-book for, Prof. Henry Adams, 147

English Patent Law, Lewis Edmunds and A. Wood Renton, 53

Entomology: Entomological Society, 71, 191, 311, 383, 503, 551; American Spiders and their Spinning Work, Henry C. McCook, D.D., 74; Anatomy, Physiology, Morphology, and Development of the Blow-fly, B. Thompson Lowne, 77; the Comb of the Hive-bee, Wm. Knight, 80; Bees' Cells, Right Rev. the Lord Bishop of Carlisle, 295; the Honey Bee, by T. W. Cowan, Rev. W. Tuckwell, 578; the Phosphorescent Light of Insects, Prof. Langley, 88; Oviposition of a Spider (*Agelena labyrinthica*), C. Warburton, 119; Some Habits of the Spider, W. H. Hudson, 151; Changes in Markings and Colouring of Lepidoptera caused by Subjecting Pupæ to Different Temperature Conditions, Merrifield, 191; Butterflies Bathing, A. Sidney Olliff, 199; Appointment of Mr. A. S. Olliff as Government Entomologist, N.S. Wales, 254; Curious Habit of Autumn Larvæ (*Orgyia antiqua*), E. W. Carlier, 255; *Bipalium kewense* in a New Locality, A. E. Shipley, 407; the Coco-nut Beetle, H. N. Ridley, 476; *Selenia illustraria*, P. Merrifield, 503; Nature of Supernumerary Appendages in Insects, W. Bateson, 527; the Red Ant (Caringa) of the Malay Peninsula, H. N. Ridley, 620

Epidemics: Dr. E. Klein on Infectious Diseases, their Cause, Nature, and Mode of Spread, 416, 446

Epping (Rev. Joseph) and the Rev. J. H. Strassmaier, on Babylonian Astronomy and Chronology, 369

Erdmann (Dr. Hugo), Anleitung zur Darstellung chemischer Präparate, ein Leitfaden für den praktischen Unterricht in der anorganischen Chemie, 126

Erosive Action of Frost, 319

Eskimo Art Work, John R. Spears, 464

Espin (Rev. T. E.), Variations of Certain Stellar Spectra, 165

Essex Field Club, the, 325

Essex and the Technical Instruction Act, 337, 357

Etheridge (R., Jun.), Australian Aboriginal Weapons and Implements, 544

Ethnography: Retirement of Adrian Jacobsen, 183; the American Race, Dr. Daniel G. Brinton, 556; of the Northern Shan States, 62; Who are the American Indians?, H. W. Henshaw, 137; Rev. John Mathew and Edward Stephen on Australian Aborigines, 185; Indian Ethnography, Prof. Alfred C. Haddon, 270; the Aino Race of Japan, 427; Primitive Folk, Elie Reclus, 580

Etymological Dictionary of the German Language, Friedrich Kluge, 532

Euclid, Elements of, Horace Deighton, 127

Euclid Revised, Supplement to, R. C. J. Nixon, 581

Euclid's Elements of Geometry, A. G. Layng, 343; 607

Europe, Central, the Pinks of, F. N. Williams, 149

Europe, Lake-Dwellings of, being the Rhind Lectures in Archæology for 1888, Robert Munro, Prof. W. Boyd Dawkins, F.R.S., 341

Eustace (J. M.), Notes on Trigonometry and Logarithms, 55

Evans (Surgeon J. F.), the Pathogenic Fungus of Malaria, 430

Everett (A. H.), Wild Swine of Palawan and the Philippines, 416

Everett (Prof. J. D., F.R.S.): Weights Proceeding by Powers of 3, 104; Weighing with a Ternary Series of Weights, 198

Evolution and Heredity, Prof. H. F. Osborn, 458

Evolution, the Philosophical Basis of, James Croll, F.R.S., 434

Evolution: the Principle of Lamarck and the Inheritance of Somatic Modifications, Prof. Giard, 328

Ewing (Prof. J. A., F.R.S.): Model Illustrating a Molecular Theory of Magnetism, 239; Stresses in a Whirling Disk, 514

Exact Thought, the Subject-Matter of, A. B. Kempe, F.R.S., 156

Exhibition of Electrical Apparatus, Proposed Historical, 231

Exhibition at Paris, Proposed International Colonial, 542

Exhibition, Proposed Palermo, 301

Exhibition at St. Pancras Vestry Hall, Electrical, 472

Expedition, the Black Mountain, 543

Expedition, Herr H. Leder's proposed Zoological and Botanical, to Siberia, 451

Expedition by Lieutenant Robert Peary, U.S.N., Projected Arctic, 619

Expedition, a New Russian Scientific, to the Pamir, 591

Expedition, the Pilcomayo, 107

Expeditions, Geographical: Grombchevsky's, in Tibet, &c., 352; Bonvalot and Prince Henri d'Orléans in Central Asia, 353; British East Africa Company's Expedition to Uganda, 353

Extermination, 505

Eye and Electricity, the Human, Prof. Dubois, 452

Fahrenheit Thermometer-Scale, Principle of Construction of, Arthur Gamgee, F.R.S., 119

Fathers of Biology, Charles McRae, 245

Fauna of British India, including Ceylon and Burma, R. Bowdler Sharpe, 266; O. Boettger, 361

Faye (H.), Hypothesis of Spheroidal Shape of Earth, 288

February Sunshine, 453

Feistmantel (B. O.), on the Coal and Plant-bearing Beds of Palæozoic and Mesozoic Age in Eastern Australia and Tasmania, J. S. Gardner, 148
Fenyi (Jules), Rapid Development of a Solar Prominence, 95
Ferns, British, and where Found, E. J. Lowe, F.R.S., 389
Ferrel (Prof. William), the High-pressure Area of November 1889 in Central Europe, with Remarks on High-pressure Areas in General, 466
Fewkes (J. W.), a Peculiar Gesture of Zuñi and Navajo Indians, 592
Fibres at London Chamber of Commerce, Exhibition of, 326
Field Experiments at Rothamsted, 189
Finley (Lieutenant J. P.), Storm Track, &c., Charts of North Atlantic, 231
Fire-ball Meteor, Rev. A. Freeman, 150; Robert Hunter, 151; James Turle, 176
Fischer (Dr. P.), the Conchological Fauna of the Sahara, 312
Fish: the Common Sole, Rev. W. Spotswood Green, 56; J. T. Cunningham, 104, 193; Scientific Investigations of the Fisheries Board for Scotland, Dr. T. Wemyss Fulton, 56; Thomas Scott, 56; the Crab-Eater, 63; Dr. T. W. Fulton on the Scotch Sea Fisheries, 108; Hermaphroditism in Fish, Prof. G. B. Howes, 384; Frozen Fish, E. J. Lowe, F.R.S., 391; R. McLachlan, F.R.S., 440; Oswald H. Latter, 464; Dr. James Turle, 464; F. H. Perry Coste, 516; R. McLachlan, F.R.S., 535; E. Main, 535; Rev. E. Hill, 345; Prof. T. G. Bonney, F.R.S., 295, 368; Steam Yacht *Harlequin* chartered by Government for Fishing Experiments off Irish Coast, 426
Fisher (Prof. W. R.), Forestry in North America, 247; Cultivation of India-rubber, 390
Fishes, Fossil, Catalogue of the, in the British Museum, Part II., by Arthur Smith Woodward, Prof. E. Ray Lankester, F.R.S., 577
Fishing Village, Annals of a, 389
Flahault (Ch.), Algæ growing in Mollusk Shells, 185
Flames in various Azimuths, Illuminating Power of Flat Petroleum, A. M. Mayer, 309
Fleming (Mrs.), Stars having Peculiar Spectra, 545
Fletcher (J. J.), Geographical Distribution of Australian Batrachia, 132
Fletcher (L., F.R.S.), the Supposed Occurrence of Widespread Meteoritic Showers, 295
Flora, Alpine, W. T. Thiselton Dyer, F.R.S., 581; Prof. George Henslow, 581
Flora of France, 43
Flora of the Revillagigedo Islands, W. Botting Hemsley, 471
Flora of Warwickshire, J. G. Baker, F.R.S., 413
Flowers, the Origin of Blue in, T. D. A. Cockerell, 207
Flight of Birds, Remarkable, Rev. E. C. Spicer, 222
Flight of Larks, the, Alfred W. Bennett, 248
Flight of Oceanic Birds, on the, Capt. David Wilson-Barker, 222; John R. Spears, 319
Floods in Queensland from Excessive Rainfall, 326
Flower (Prof. W. H., F.R.S.), the Proposed South Kensington and Paddington Subway, 246
Fluor-Spar of Quincié, 72
Fock (Dr. A.): Krystallographisch-chemischen Tabellen, A. E. Tutton, 197; on the Relationship between Optical Activity of Substances in Solution and Hemihedrism of their Crystalline Forms, 327
Fogs, Effect of, on Cultivated Plants, 107, 163; Dr. D. H. Scott, 129
Fol (Hermann), Resemblance between Husband and Wife, 255
Folie (F.), Periodical Changes in Declination of Stars, a Possible Cause of Latitude Variation, 240
Folk Lore: Payment of "Wrath Silver," 42; Hand-book of Folk Lore, G. L. Gomme, 125; Tongues in Trees and Sermons in Stones, Rev. W. Tuckwell, 581; Primitive Folk, Elie Reclus, 580
Fontainebleau, the Laboratory of Vegetable Biology at, 37
Forbes (Prof. Henry O.): Note on the Disappearance of the Moa, 105; Crowing of the Jungle Cock, 295; Throwing-Sticks and Canoes in New Guinea, 248
Force, Contact, Volta's so-called, Modern Views of Electricity, S. H. Burbury, 268; Prof. Oliver J. Lodge, F.R.S., 268
Force and Determinism, Dr. James Croll, F.R.S., 434; Prof. Oliver J. Lodge, F.R.S., 491; Prof. C. Lloyd Morgan, 558
Forestry in Bengal, 232
Forestry in North America, Prof. W. R. Fisher, 247
Formation of Language, W. J. Stillman, 491; Miss Agnes Crane, 534; C. Tomlinson, F.R.S., 534
Forsyth-Major (Prof. C. J.), New Fossil Mammalia Discovered at Samos, 85
Fossil Fishes in the British Museum, Catalogue of the, Part II., by Arthur Smith Woodward, Prof. E. Ray Lankester, F.R.S., 577
Fossil Mammals of North America, Profs. W. B. Scott and H. F. Osborn, 177
Foster (Prof. M., F.R.S.), Obituary Notice of Henry Bowman Brady, F.R.S., 299
Fothergill (W. E.), Zoological Types and Classifications, 124
Fowler (A.), the Duplicity of α Lyræ, 64
Fowler (W. Warde), a Swallow's Terrace, 80, 318
Foxes, Dogs, Jackals, Walves, and, a Monograph of the Canidæ, St. George Mivart, F.R.S., 385
France: Flora of, 43; the Depopulation of, Ed. Marbeau, 183; Emile Levasseur, 192; Foundation of a Natural History Society for West France, 377; the Destruction of Wolves in, 356; National Congress of French Geographical Societies, 473; French Association for Advancement of Science, a New Medal, 591; Magnetic Anomalies in, M. Mascart, Prof. A. W. Rücker, F.R.S., 617
Frankland (P. F.), Fermentations induced by Friedländer's Pneumococcus, 503
Franklin (Benjamin), Centennial of the Death of, 39
Freeman (Rev. A.), a Large and Brilliant Fireball Meteor, 150
Fremy (M.), Artificial Rubies, 432
Fresenius's Quantitative Analysis, 436
Freshwater Shells, Dispersal of, H. Wallis Kew, 56
Frew (W.), Fermentation induced by Friedländer's Pneumococcus, 503
Fritsch (Dr. Anton), Fauna der Gaskohle und der Kalksteine der Permformation Böhmens, 293
Frog, an Albino, J. E. Harting, 383; Development of Oviduct in, E. W. MacBride, 407
Frost, Destruction of Fish by, Prof. T. G. Bonney, F.R.S., 295, 368; Rev. E. Hill, 345
Frost, the Erosive Action of, 319
Frost, the Great, of the Winter of 1890–91, 270
Froude (late W.), the Soaring of Birds, 287
Frozen Fish: E. J. Lowe, F.R.S., 391; R. McLachlan, F.R.S., 440; Oswald H. Latter, 464; Dr. James Turle, 464; F. H. Perry Coste, 516; R. McLachlan, F.R.S., 535; E. Main, 535
Fruit, British, the Encouragement of the Growth of, 62
Fry (Right Hon. Lord Justice Fry, F.R.S.), the British Mosses, 379, 400
Fulton (Dr. T. Wemyss): the Scientific Investigations of the Fisheries Board for Scotland, 56; on Scotch Sea Fisheries, 108; Attractive Characters in Fungi, 269
Functions, Theory of, Dr. H. A. Schwartz, Prof. O. Henrici, F.R.S., 321, 349
Fungi: the Value of Attractive Characters to, 79, 80, 151; Charles R. Straton, 9; Dr. M. C. Cooke, 57, 224, 393; R. Haig Thomas, 79; Worthington G. Smith, 224; Dr. T. Wemyss Fulton, 269; Edible Fungi for sale in Modena Market in 1889, 21; Fungus-poisoning at Nismes, 356; Fungi of Warwickshire, James E. Bagnall and W. B. Grove, 413
Furnaces, Gas, Bernard Dawson, 332

Gallery of British Art at South Kensington, Science Museum and, 590, 601
Galton (Francis, F.R.S.), the Patterns in Thumb and Finger Marks, 117, 192
Gambia, Proposed Botanical Exploration of the, 182
Games, the Hand-book of, Vol. I., 28; Vol. II., 510
Garden Scholarships at Missouri, 187
Gardiner (E. G.), Weismann and Maupas on the Origin of Death, 458
Gardiner (William), Araucaria Cones, 80
Gardner (J. A.), Vapour Density of Ammonium Chloride, 454
Gardner J. S.), on the Coal and Plant-bearing Beds of Palæozoic and Mesozoic Age in Eastern Australia and Tasmania, by B. O. Feistmantel, 148
Garnier (Jules), Artificial Production of Chromium Blue, 120
Gas Furnaces, Bernard Dawson, 332
Gaseous Illuminants, Prof. V. B. Lewes, 233, 257, 282

Gaseous States, Prof. Van der Waals on the Continuity of the Liquid and, Prof. J. T. Bottomley, F.R.S., 415; Prof. A. W. Rücker, F.R.S., 437; Robert E. Baynes, 488

Gaseous Theory of Solution, a Deduction from the, Prof. Orme Masson, 345; Prof. Spencer U. Pickering, 488

Gases at Constant Volume, the Specific Heat of, Part I, Air, Carbon Dioxide, and Hydrogen, J. Joly, 116

Gaskell (W. H., F.R.S.), on the Origin of Vertebrates from Crustacea, 70

Gautier (Émile), Obituary Notice of, 518

Gautier (H.), Affinities of Iodine in Solution, 48

Geikie (Archibald, F.R.S.): Class-book of Geology, Prof. A. H. Green, F.R.S., 242; History of Volcanic Action in Britain during Earlier Geological Ages, 455

Geneva, Earthquake in, 279

Geodesy, the Burmese Survey, 399

Geodetic Survey of Alaska, the, 207

Geography: an Elementary Geography of India, Burma, and Ceylon, Henry F. Blanford, F.R.S., 29; Colonel Holabird's Explorations in Colorado, 44; the Nyassaland Region, H. H. Johnston, 45; Bulletin of the Italian Geographical Society, 63; Matabeleland, by Lieut. E. A. Maund, 91; the Pilcomayo Expedition, 107; the Scientific Results of the Occupation of British New Guinea, 114; Geographical Teaching in the United States, the Brooklyn Institute, 131; the Development of Africa, by Arthur Silva White, 149; the First Crossing of Greenland, by Fridtjof Nansen, 172; Western Afghanistan and North-Eastern Persia, J. E. T. Aitchison, F.R.S., 174; Maps and Map Drawing, William A. Elderton, 196; New Map of Chin-Lushai Country, 209; the Lake System, &c., of Tabreez, Colonel Stewart, 232; Three Years in Western China, Alexander Hosie, 290; Coming International Geographical Congress, 300; the Canary Islands, John Whitford, 317; Proposed Survey of Mount Kinabalu, 327; Grombchevsky in Tibet, &c., Expedition of, 352; Bonvalot and Prince Henri d'Orléans' Travels in Central Asia, 353; British East Africa Company's Expedition to Uganda, 353; Congress of German Geographers, 354, 519; Applied Geography, J. Scott Keltie, 365; the Exploration of the Tundras of North-East Russia, Th. Tchernysheff, 397; Ptolemaic Geography of Africa, 448; Physical and Astronomical Geography for Science Students, R. A. Gregory, 459; National Congress of French Geographical Societies, 473; Measurement of 52nd Parallel in Europe, M. Vénukoff, 480; a Ride through Asia Minor and Armenia, Henry C. Barkley, 487; G. E. Grum-Grzimailo's Expedition to Central Asia, 571; Our Present Knowledge of the Himalayas, Colonel H. C. B. Tanner, 621; a Journey in South-West China, A. E. Pratt, 570; Peculiar Method of Sand Transportation by Rivers, J. C. Graham, 260; Soundings of Lake Leman, A. Delebecque, 264; Evidence of Northern and Eastern Extension of Gulf Stream in Arctic Regions, 377; Periodical Variation in Earth's Axis caused by Periodical Transference of Ocean Water-Masses, Herr Lamp, 520; Silica and Siliceous Formations in Modern Seas, Murray and Irvine, 527

Geology: an American Geological Railway Guide, James Macfarlane, 8; Geological Photographs Committee, 42; Illustrations and Diagrams on Geology, Cecil Carus-Wilson, 64; Conference of Delegates of Corresponding Societies of the British Association, 92, 93; the Porphyritic Rocks of Jersey, Prof. de Lapparent, 94; Geological Society, 94, 143, 214, 239, 310, 359, 383, 455, 526, 550, 598; Anniversary Medals of, 254; Annual Meeting, 325; the Geology of Korea, T. H. Holland, 95; Glacial Striæ and Morainic Gravel in Norwegian Lapland far Older than the Ice Age, Dr. Hans Reusch, 106; Geological Survey of New Zealand, 109; Geology of British New Guinea, 114; Mechanical Action on Rocks of Gas at High Pressure and in Rapid Motion, M. Daubrée, 120; the Rocks of North Devon, Dr. Henry Hicks, F.R.S., 143; Analysis of Manganese Nodules, Dr. John Gibson, 143; Sulphur in Marine Muds and Nodules and its bearing on their Modes of Formation, J. Y. Buchanan, F.R.S., 143; the Palæozoic Fishes of North America, Prof. John Strong Newberry, 146; on the Coal and Plant-bearing Beds of Palæozoic and Mesozoic Age in Eastern Australia and Tasmania, by B. O. Feistmantel, J. S. Gardner, 148; Fossil Mammals of North America, Prof. W. B. Scott and H. F. Osborn, 177; Prof. T. G. Bonney, F.R.S., on the Origin of the Great Lakes of North America, 203; Action of Water upon Stones in Severn Weir, H. G. Marten, 214; Physical Geology of Tennessee, &c., Prof. Edward Hull, F.R.S., 215; Geology of the Kama Region, Krotoff and Netchaieff, 232; Nepheline Rocks in Brazil, II., the Tingua Mass, O. A. Derby, 239; Class-book of Geology, Archibald Geikie, F.R.S., Prof. A. H. Green, F.R.S., 242; Elementary Geology, Charles Bird and Prof. A. H. Green, F.R.S., 242; the Geology of Round Island, W. T. Thiselton Dyer, F.R.S., 253; Prof. John W. Judd, F.R.S., 253; Long Island Sound in Quaternary Period, J. D. Dana, 260; Deformation of Iroquois Beach and Birth of Lake Ontario, J. W. Spencer, 260; Geological Work during last Oxford Summer Meeting, 279; Hypothesis of Spheroidal Shape of Earth, H. Faye, 288; the Future of Geology, Rev. G. Deane, 303; the North-West Region of Charnwood Forest, Hill and Bonney, 310; Note on a Contact-structure in Syenite of Bradgate Park, Prof. T. G. Bonney, F.R.S., 310; Vegetation of the Carboniferous Age, Prof. Williamson, 355; the Darent Valley, Prof. Joseph Prestwich, F.R.S., 359; the Autobiography of the Earth, Rev. H. N. Hutchinson, 365; Temperature in the Glacial Epoch, Prof. T. G. Bonney, F.R.S., 373; the Coming International Congress of Geologists, 376; Geology of the West Indies, Jukes-Brown and Harrison, 383; the Histology and Physiology of Granite, Prof. W. J. Sollas, F.R.S., 412; Discovery of Vertebrate Life in Lower Silurian (Ordovician) Strata, C. D. Walcott, 425; History of Volcanic Action in Britain during Earlier Geological Ages, Dr. A. Geikie, F.R.S., 455; Geology of Southern Transvaal, W. H. Penning, 455; Age of Strata cut by Panama Canal, H. Douvillé, 456; Easter Excursion of the Geologists' Association to Isle of Wight, 473; Lake Bonneville, Grove Karl Gilbert, Prof. T. G. Bonney, F.R.S., 485; Ninth Annual Report of the United States Geological Survey to the Secretary of the Interior 1887–88, J. W. Powell, Prof. T. G. Bonney, F.R.S., 509; Mud Glaciers of Cromer, William Sherwood, 515; Recession of the Niagara Gorge, Prof. Edward G. Bourne, 515; Manod and the Moelwyns, Jennings and Williams, 526; Flamborough Head Drifts, G. W. Lamplugh, 550; a Phosphatic Chalk with *Belemnitella quadrata* at Taplow, A. Strahan, 551; Die Denudation in der Wüste, Prof. Johannes Walther, 556; the Cross Fell Inlier, Nicholson and Marr, 598; Igneous Rocks of South of Man, Bernard Hobson, 599

Geometry: Higher Geometry, W. J. Macdonald, 8; Solution of a Geometrical Problem in Magnetism, T. H. Blakesley, 191; Association for the Improvement of Geometrical Teaching, 230, 278; the Applications of Geometry to Practical Life, Prof. Karl Pearson, 273; Euclid's Elements of Geometry, A. G. Layng, 343; Euclid's Elements of Geometry, Books III. and IV., H. M. Taylor, 607; a Text-Book of Geometrical Deduction, James Blaikie and W. Thomson, 487; the Foundations of Geometry, E. T. Dixon, 554

Gerard (Eric), Leçons sur l'Électricité professées à l'Institut Électro-Technique Montefiore annexé à l'Université de Liége, 245

German Geographers' Congress, 519

German Language, an Etymological Dictionary of the, Frederick Kluge, 532

Giard (Prof.), the Principle of Lamarck and the Inheritance of Somatic Modifications, 328

Gibb (Thomas), an Introduction to the Study of Metallurgy, Prof. W. C. Roberts-Austen, F.R.S., 530

Gibbs (Prof. J. Willard), on the Rôle of Quaternions in the Algebra of Vectors, 511

Gibson (Dr. John), Analysis of Manganese Nodules, 143

Gibson (R. J. Harvey), Biological Terminology, 175

Gilbert (Grove Karl), Lake Bonneville, Prof. T. G. Bonney, F.R.S., 485

Gillette (C. P.), Corn and Squirrels, 620

Girard (Aimé), Distillation of Alcohol from Potatoes, 120

Girdle of the Earth, Tension of a, Prof. A. S. Herschel, F.R.S., 513

Glacial Climate, Prof. N. S. Shaler, 155

Glacial Epoch, Temperature in the, Prof. T. G. Bonney, F.R.S., 373

Glacial Period, our Latest, W. Atkinson, 270

Glacial Striæ and Morainic Gravel in Norwegian Lapland far Older than the Ice Age, Dr. Hans Reusch, 106

Glaciation, Dr. Nansen on, Dr. A. Irving, 541

Glaciers, Mud, of Cromer, William Sherwood, 515

Gladstone (Dr. J. H., F.R.S.): Additional Notes on Secondary Batteries, 190; Molecular Dispersion, 198; Molecular Refraction and Dispersion of Various Substances, 549
Glaisher (James), Monthly Meteorological Observations taken at Sarona, Syria, 473
Goebel's (Dr.), Botanical Researches in British Guiana, 326
Gomme (G. Lawrence, F.S.A.), Hand-book of Folk-Lore, 125
Gonnessiat (F.), Personal Equation in Transit Observations, 336
Goods Waggons, Tube-frame, M. R. Jefferds, 22
Gore (G., LL.D., F.R.S.), the Art of Electrolytic Separation of Metals, Theoretical and Practical, 244
Gore (John Ellard), Astronomical Lessons, 317
Gore (Prof. J. H.), Decimal Measure-system of Seventeenth Century, 309
Goschen (Right Hon. G. J.), and the Decimal System, 326; and the Proposed Increase in the Annual Grant to the University Colleges of England, 425
Gosse (Philip Henry, F.R.S.), the Life of, Edmund Gosse, 603
Gotch (Francis) and Victor Horsley, F.R.S., on the Mammalian Nervous System, 428
Göttingen Royal Society of Sciences, 120
Graham (J. C.), Peculiar Methods of Sand Transportation by Rivers, 260
Granada, Earthquake in, 279
Granites of Leinster, Prof. W. J. Sollas, F.R.S., 412
Grasses of the South-West, Dr. Geo. Vasey, 389
Gravity: Apparatus for Determining the Acceleration Due to, Prof. F. R. Barrell, 385; a Method of Determining Specific, Prof. W. J. Sollas, F.R.S., 404; Graphical Method of Determining Relative Values of Gravity in Various Places, A. Berget, 504
Great Mogul's Diamond and the Koh-i-nur, Valentine Ball, F.R.S., 103
Green (Prof. A. H., F.R.S.): Birth and Growth of Worlds, 221; Class-book of Geology, Archibald Geikie, F.R.S., 242; Elementary Geology, Charles Bird, 242
Green (Rev. W. Spotswood), the Common Sole, 56
Green Sun, a, Chas. T. Whitmell, 440
Greene (W. H.), Alloys of Sodium and Lead, 216
Greenland, the First Crossing of, by Fridtjof Nansen, 172
Greenland, Projected Expedition under Dr. C. von Drygalski to, 279, 326
Greenwich Spectroscopic Observations, 210
Gregory (Richard A.), the Question of the Asteroids, 587
Gregory (R. A.), Physical and Astronomical Geography for Science Students, 459
Gresham College Lectures, 520
Grimaux (E.), Transformation of Cupreine into Quinine, 579
Grotto discovered near Ajaccio, Magnificent, 520
Grove (Sir William, F.R.S.), Chemical Society's Jubilee, 491
Groves (Henry), Death and Obituary Notice of, 519
Grünbaum (Dr.), Experiments on Blood-clotting, 527
Grzimailo (G. E. Grum), Expedition to Central Asia, 571
Guenez (M.), on Benzoyl Fluoride, 64
Guiana, Dr. Goebel's Botanical Researches in British, 326
Guide-books to Switzerland, Scientific, 43
Guignard (Léon), "Attractive Spheres" in Vegetable Cells, 480
Guillaume (C. E.), Practical Solution of Problem of Emergent Thermometer by Employment of Correcting Stem, 288
Guillaume (J.), Observations of Mars, 428
Guillemard (Arthur G.), Great Waterfalls, 105, 129
Guillemard (F. H. H.), the Life of Ferdinand Magellan, 294
Gulf Stream in Arctic Regions, Evidence of Northern and Eastern Extension of, 377
Gulland (Dr.), Development of Adenoid Tissue, 239
Güntz (M.), Salts of Sub-oxide of Silver, 620
Gutta-percha, India-rubber and, W. Sowerby, 355

Haddon (Prof. Alfred C.): Indian Ethnography, 270; Throwing-Sticks and Canoes in New Guinea, 295
Hæmatokrit, an Instrument for Determining Volume of Corpuscles in Blood, Herr von Hedin, 398
Hæmoglobin in Blood according to Conditions of Existence, Amount of, A. Müntz, 360
Hague, the, Proposed International Agricultural Congress at, 542
Hail in Würtemberg, 1828–87, Injury from, Herr Bühler, 473
Hale (G. E.), the Photography of Solar Prominences, 133
Hall (Asaph), Saturn and its Rings, 65
Hall (A. D.), Exercises in Practical Chemistry, 29
Hall (H. S.) and S. R. Knight, Solutions of the Examples in Elementary Algebra, 389
Haller (A.), Influence of Dissolvents on Rotary Power of Camphols and Isocamphols, 311
Hamberg (Dr.), Discovery of a New Metal, Titanite of Manganese, 452
Hankin (E. H.): a Cure for Tetanus and Diphtheria, 121; Prof. Virchow on the Consumption Cure, 248
Hann (Prof. J.): the Causes of Anticyclones and Cyclones, 15, 81; on the High-pressure Area of November 1889 in Central Europe, with Remarks on High-pressure Areas in General, Prof. William Ferrel, 466
Hanriot (M.): Poisonous Compound of Nickel and Carbon Monoxide, 545; Amidoisoxazol, 599
Harding (C.), the Great Frost of 1890–91, 431
Harding (J. S., Jun.), Catalogue of the Meteorological Society's Library, 497
Harmonics, Ellipsoidal, W. D. Niven, F.R.S., 189
Harmonics, Tables of Spherical, Prof. J. Perry, F.R.S., 118
Harrison (Prof. J. B.), Coral Rocks of West Indies, 383
Hart (Prof. Samuel): a Remarkable Ice-Storm, 317; Snow on the Branches of Trees, 391
Harting (J. E.): Widgeon with Hanging Feather Tassel, 311; an Albino Frog, 383
Hartley (Prof. W. N., F.R.S.), Spectra of Blue and Yellow Chlorophyll, 262
Harvard College Observatory, D. W. Baker, 280
Harvey (Prof. W. H.), *Tuomeya fluviatilis*, 65
Hatteras Porpoise Fishery, the, F. W. True, 497
Haycraft (Dr J. B.): Method of Determining Density of Liquid in Small Quantities, 479; the Minute Structure of Striped Muscle, 286
Hazell's Annual for 1891, 109
Hazen (Prof. H. A.): the Tornado, 128; Observations and Studies with Sling Hygrometer on Mount Washington, 285
Health, Lessons on, by Dr. Arthur Newsholme, Dr. J. H. E. Brock, 54
Health, a Manual of Public, A. Wynter Blyth, Dr. J. H. E. Brock, 267
Heat Capacity and Fusion Heat of Substances, Determinations of, to Test Validity of Person's Absolute Zero, S. U. Pickering, 214
Heat and Light Problems, R. Wallace Stewart, 28
Heat, Mr. A. S. Merry on, 327
Heavens, Photographic Chart of the, 540, 615
Hector (Sir James, F.R.S.), Science in New Zealand, 521
Hedin (Herr von), Hæmatokrit for determining Volume of Corpuscles in Blood, 398
Heen (P. de), Velocity of Evaporation of Boiling Liquids, 456
Heilprin (Prof.), Date of Growth of Coral, 473
Helm (Dr. F.), on the Affinities of Hesperornis, 368
Helmholtz (Prof.), Seventieth Birthday of, 277; Proposed Commemoration of, 354
Heloderma, the Poison Apparatus of the, Dr. R. W. Shufeldt, 514
Hempel (Prof.), Synthetical Production of Cyanogen Compounds by Mutual Action of Charcoal, Gaseous Nitrogen, and Alkaline Oxides or Carbonates, 164
Hemsley (W. Botting, F.R.S.): Annals of the Royal Botanic Garden, Calcutta, 6; and Brigadier-General H. Collett, on a Collection of Plants from Upper Burma and the Shan States, 386; Flora of the Revillagigedo Islands, 471; Vegetation of Lord Howe Island, 565
Henrici Prof. O., F.R.S.), Theory of Functions, Dr. H. A. Schwartz, 321, 349
Henry (Prosper), a Method of Measuring Atmospheric Dispersion, 400
Hens, Cackling of: Prof. Geo. J. Romanes, F.R.S., 516; V. Ball, F.R.S., 583
Hens and Cocks, E. J. Lowe, F.R.S., 583
Henshaw (H. W.), Who are the American Indians?, 137
Henslow (Prof. George), Neo-Lamarckism and Darwinism, 490, 581
Hepworth (Captain), Wind Systems and Trade Routes between Cape and Australia, 215

Herdman (Prof. W. A.), the Common Sole considered both as an Organism and a Commodity, J. T. Cunningham, 193
Heredity: are the Effects of Use and Disuse Inherited?, William Platt Ball, Prof. Geo. J. Romanes, F.R.S., 217
Heredity, Evolution and, Prof. H. F. Osborn, 458
Héricourt (J.), Toxicity of Soluble Products of Tuberculous Cultures, 504
Hermaphroditism of the Apodidæ, Rev. H. Bernard, 343
Hermaphroditism in Fish, Prof. G. B. Howes, 384
Herschel (Prof. A. S., F.R.S.), Tension of a Girdle of the Earth, 513
Hertz's Experiments, 536
Hesperornis, on the Affinities of, R. W. Shufeldt, 176; Dr. F. Helm, 368
Hewitt (W.), Elementary Science Lessons, 414
Heycock (C. T.), Molecular Condition of Metals when Alloyed with each other, 262
Hibberd (Shirley), Death of, 62; Obituary Notice of, 87; Proposed Memorial to, 107, 182
Hibbert (W.), Additional Notes on Secondary Batteries, 190
Hicks (Dr. Henry, F.R.S.), the Rocks of North Devon, 143
Hickson (S. J.), Medusæ of Millepora and their Relation to Medusiform Gonophores of Hydromedusæ, 407
Higgs (George), Bisulphite Compounds of Alizarin-blue and Cœrulin as Sensitizer for Rays of Low Refrangibility, 525
High Pressure Area of November 1889 in Central Europe, with Remarks on High Pressure Areas in General, Prof. William Ferrel, 466
Hill (Rev. E.): the North-West Region of Charnwood Forest, 310: Destruction of Fish by Frost, 345
Hill (G. W.), a New Theory of Jupiter and Saturn, 357
Hill Arrians of India, Rev. A. F. Painter, 212
Himalayas, our Present Knowledge of the, Colonel H. C. B. Tanner, 621
Hindoos, the Botanical Mythology of the, Dr. Dymoke, 46
Hiorns (A. H.), Mixed Metals, or Metallic Alloys, Prof. W. C. Roberts-Austen, F.R.S., 388
Hiron-Royer, Toads and Wasps, 232
Histology and Physiology of Granite, Prof. W. J. Sollas, F.R.S., 412
Hobson (Bernard), Igneous Rocks of the South of Man, 599
Höffding (Prof. Harald), Outlines of Psychology, translated by Mary E. Lowndes, 553
Hofmann (Prof. von), Decomposition of Carbon Dioxide by Electricity, 109
Holabird (Colonel), Explorations in Colorado, 44
Holden (Prof.), on the Parallaxes of Nebulæ, 65
Holetschek (Dr.), Perihelia of Comets, 233
Holland (T. H.), the Geology of Korea, 95
Hollis (W. Ainslie), Phosphograms, 532
Holmes (A. Bromley), the Electric Light, 196
Holstein, Projected Biological Station at Plöner Sea, East, 377
Holt (Ernest W. L.), on the Eggs and Larvæ of some Teleosteans, 510
Honey Bee, the, T. W. Cowan, Rev. T. Tuckwell, 578
Honey, Various Specimens of, Thomas Christy, 383
Honeycomb Appearance of Water, J. Shaw, 30
Hoogewerff (Dr.), Reaction of Hypochlorites and Hypobromites on Phtalimide and Phtalamide, 408
Hooker's (Sir Joseph, F.R.S.), Flora of British India, 519
Horace Mann School, Boston, Mass., Twenty-first Anniversary of, 231
Horsley (Victor, F.R.S.) and Francis Gotch, on the Mammalian Nervous System, 428
Horticulture: the Encouragement of the Growth of British Fruit, 62; Peach-trees Injured by Contact with Galvanized Wire during Hard Frost, Rev. W. Wilks, 377
Hosie (Alexander), Three Years in Western China, 290
Hoskyns-Abrahall (Rev. J.): a Beautiful Meteor, 416; the Intelligence of the Thrush, 583
Household Hygiene, Mary Russell Bissell, M.D., Dr. H. Brock, 461
Howes (Prof. G. B.): the Morphology of the Sternum, 269; Hermaphroditism in Fish, 384
Hubbard (Gardiner G.), History of Articulating System for Deaf in America, 231
Hudson (W. H.), some Habits of the Spider, 151
Huet's Anemometer, W. J. Lewis, 323
Hüfner (Herr), Behaviour of Water as to Passage of Different Light-rays, 520
Hughes (William R.), Constance Naden, a Memoir, 343
Hull (Edward, F.R.S.), Physical Geology of Tennessee, &c., 215
Human Race, Darwin on the Unity of the, Duke of Argyll, F.R.S., 415
Hunt (Dr. T. Sterry, F.R.S.), Potassium Salts in Sea-water, 463
Hunter and Aristotle, Jonathan Hutchinson, F.R.S., 377
Hunter (Robert), a Large and Brilliant Fire-ball Meteor, 151
Huntly (Geo. N.), Chemical Action and the Conservation of Energy, 246
Hutchins (C. C.), Measures of Lunar Radiation, 44
Hutchinson (Rev. H. N.), the Autobiography of the Earth, 365
Hutchinson (Jonathan, F.R.S.), Hunter and Aristotle, 377
Huygens (Christiaan), Œuvres Complètes de, A. M. Clerke, 433
Hydrazine, $\begin{array}{c}\mathrm{NH_2}\\ |\\ \mathrm{NH_2}\end{array}$, the Isolation of, A. E. Tutton, 205
Hydrodynamics and Sound, an Elementary Treatise on, A. B. Basset, 75
Hygiene: Dr. Newsholme's Lessons on Health, 54; a Manual of Public Health, A. Wynter Blyth, Dr. J. H. E. Brock, 267; Household Hygiene, Mary Taylor Bissell, M.D., Dr. H. Brock, 461; International Congress of Hygiene and Demography, 241, 300, 472; Dr. W. H. Corfield, 511
Hypophosphorus Acids, 72

Ice, Effects of Pressure on, R. W. Wood, Jun., 309
Ice on French Rivers, the Formation of Ground, 450
Ice-Storm, a Remarkable, Prof. Samuel Hart, 317
Ichthyology: the Scientific Investigations of the Fisheries Board for Scotland, Dr. T. Wemyss Fulton, 56; Thomas Scott, 56; the Common Sole, Rev. W. Spotswood Green, 56; J. T. Cunningham, 104; the Common Sole considered both as an Organism and a Commodity, J. T. Cunningham, 193; the Crab-eater, 63; Presence of a Sternum in *Notidanus indicus*, Prof. T. Jeffery Parker, F.R.S., 142; Fauna der Gaskohle und der Kalksteine der Permformation Böhmens, Dr. Anton Fritsch, 293; Destruction of Fish in the late Frost, Prof. T. G. Bonney, F.R.S., 295, 368; Rev. E. Hill, 345; Frozen Fish, E. J. Lowe, F.R.S., 391; R. McLachlan, F.R.S., 440; Oswald H. Latter, 464; Dr. James Turle, 464; F. H. Perry Coste, 516; R. McLachlan, F.R.S., 535; E. Main, 535; on the Eggs and Larvæ of some Teleosteans, Ernest W. L. Holt, 510; Catalogue of the Fossil Fishes in the British Museum, Part II., Arthur Smith Woodward, Prof. E. Ray Lankester, F.R.S., 577
Illuminants, Gaseous, Prof. V. B. Lewes, 233, 257, 282
Impact, Prof. Tait on, 287
Implement, Notable Palæolithic, Worthington G. Smith, 345
Ince (W. H.), Crystalline Alkaloid of *Aconitum napellus*, 550
India: Burma, and Ceylon, an Elementary Geography of, Henry F. Blanford, F.R.S., 29; India, Anthropology of, 62; Geographical Text-books for, 64; Technical Education in, 109; the Reported Oil-fields in, 131; British, Fauna of, including Ceylon and Burma, R. Bowdler Sharpe, 266; Indian Ethnography, Prof. Alfred C. Haddon, 270; the Fauna of British India, including Ceylon and Burma, O. Boettger, 361; Hooker's Flora of British India, 519; Annals of the Royal Botanic Garden, Calcutta, W. Botting Hemsley, F.R.S., 6
Indiana, Botany-Teaching in, 473
Indians, Micmac, Dictionary of the Language of the, Rev. Dr. S. T. Rand, 102
India-rubber and Gutta-percha, W. Sowerby, 355
India-rubber, Cultivation of, Prof. W. R. Fisher, 390
Indoor Games, 28
Infectious Diseases, their Nature, Cause, and Mode of Spread, Dr. E. Klein, F.R.S., 416, 443
Influenza, the, E. Armitage, 608
Institution of Civil Engineers, Anniversary, 230
Institution of Mechanical Engineers, 22, 254, 332, 499
Institution of Naval Architects, 500
Instruments, Surveying and Levelling, William Ford Stanley, 374
International Congress of Hygiene and Demography, 241, 300; Dr. W. H. Corfield, 511
International Geographical Congress, Coming, 300
Internationales Archiv für Ethnographie, 89, 231, 544

Inventions, the Law and Practice of Letters Patent for, Lewis Edmunds and A. Wood Renton, 53
Irish Coast, Steam Yacht *Harlequin* Chartered by Government for Fishing Experiments off, 426
Iron and Steel Making, Chemistry of, W. Mattieu Williams, John Parry, 50
Irvine (Robert), Silica and Siliceous Formations in Modern Seas, 527
Irving (Dr. A.): Araucaria Cones, 29; Dr. Nansen on Glaciation, 541

Jackals, Wolves, Dogs, and Foxes, a Monograph of the Canidæ, St. George Mivart, F.R.S., 385
Jackson (John R.), Commercial Botany of the Nineteenth Century, 436
Jacobin (P.), Physical Properties and Molecular Constitution of Simple Metallic Bodies, 288
Jacobsen (Adrian), Retirement of, 183
Jade used for Ancient Implements in Europe and America, Source of, F. W. Rudler, 310
James (William), the Principles of Psychology, Prof. C. Lloyd Morgan, 506
Jamieson (Andrew), Elementary Manual of Magnetism and Electricity, 102
Jamieson (Andrew) and John Munro, a Pocket-Book of Electrical Rules and Tables, 268
Jamieson (Rev. Dr. George), a New Psychology, 100
Japan, the Aino Race of, 427
Japan, List of Recorded Earthquakes (August 11, 1888, to December 31, 1889) in Northern and Central, W. B. Mason, 131
Japanese Census, the, 378
Jefferds (M. R.) Tube-Frame Goods Waggons, 22
Jellisew (Dr.) the Earthquake of June 1890 in Persia, 42
Jennings (A. V.), Manod and the Moelwyns, 526
Jennings (Vaughan), a Boring Sponge (*Alectona millari*), 119
Jesse (O.), Luminous Clouds, 59
Joannis (M.), Sodium Amide, 399
Johns Hopkins University, the, 89
Johnson (Prof. T.), Punctaria, 119
Johnston (H. H.), the Nyassaland Region, 45
Johnstone (Alexander), Action of Lime on Clay Soils, 308
Joly (J.): the Specific Heat of Gases at Constant Volume, Part 1, Air, Carbon Dioxide and Hydrogen, 116; Crystals of Platinum and Palladium, 541
Jones (T. Mann), Mock Sun, 269
Joule (James Prescott), the Scientific Work of, Prof. James Dewar, F.R.S., 111
Journal of Botany, 382, 474, 623
Journal of the Camera Club, 426
Journal of the Institution of Electrical Engineers, 451
Journal of the Straits Branch of Royal Asiatic Society, 620
Judd (Prof. John W., F.R.S.), the Geology of Round Island, 253
Jukes-Brown (A. J.), Coral Rocks of West Indies, 383
Jumelle (Henri), the Assimilation of Lichens, 624
Jungle Cock, the Crowing of the, Prof. Henry O. Forbes, 295; A. D. Bartlett, 319
Jupiter and Saturn, a New Theory of, G. W. Hill, 357
Jupiter's Satellites, Dark Transits of, Mr. Keeler, 302

Kama Region, Geology of the, Krotoff and Netchaieff, 232
Kansu, Skin of *Æluropus melanoleucus*, procured by Herren Potanin and Beresowski, in, 355
Keeler (Mr.), Dark Transits of Jupiter's Satellites, 302
Keltie (J. Scott), Applied Geography, 365
Kempe (A. B., F.R.S.), the Subject-matter of Exact Thought, 156
Kent and Surrey, Rainfall of, 63
Kerr (Mr. J. Graham), the Pilcomayo Expedition, 107
Kew (H. Wallis), Dispersal of Freshwater Shells, 56
Kew: Bulletin, 62, 182, 300, 377, 473, 591; Kew Gardens, Annual Report, 87; Changes in Staff of, 277; Kew Committee, Report of, 278; the Guide to, 425; Orchids at, 1890, 473; the Training of Gardeners at, 591; Thibetan Plants (dried) added to the Kew Herbarium, 299
Keynes (John Neville), the Scope and Method of Political Economy, 387
King (Dr. G., F.R.S.), Materials for a Flora of the Malayan Peninsula, No. 2, 149
Kipping (F. S.), Action of Reducing Agents on $\alpha\alpha'$-Diacetylpentanes, Synthesis of Dimethyldihydroxyheptamethylene, 477
Kirchoff (Dr. Alfred), Fluctuation of Latitude, 88
Kirkpatrick (Prof. E. A.), Suggested Records of Certain Facts of Child Development, 163
Kiwi (*Apterix*), Prof. T. Jeffery Parker, F.R.S., on the Anatomy and Development of, 17
Klaassen (Miss H. G.), Effect of Temperature on Conductivity of Solutions of Sulphuric Acid, 384
Klein (Dr. E., F.R.S.), Infectious Diseases, their Cause, Nature, and Mode of Spread, 416, 446
Kleinstrick (Herr), Wax floats in Water above 18° C., 520
Kluge (Friedrich), an Etymological Dictionary of the German Language, 532
Knight (S. R.) and H. S. Hall, Solutions of the Examples in Elementary Algebra, 389
Knight (William), the Comb of the Hive-Bee, 80
Knott (Prof. C. G.), Introduction of Longitudinal and Circular Magnetizations in Iron and Nickel Wires, 480
Knowledge, the Relativity of, Dr. James McCosh, 531
Kobbé (Dr.), Redetermination of Atomic Weight of Rhodium, 356
Koch (Prof. Dr. Robert): the Cure of Consumption, 25, 49, 66, 265, 281
Kœnig (Dr. R.), the Researches of, on the Physical Basis of Musical Sounds, Prof. Silvanus P. Thompson, 199, 224, 249
Kœnig's Superior Beats, Rev. Walter Sidgreaves, 9
Koh-i-nur, the Great Mogul's Diamond and the, Valentine Ball, F.R.S., 103
Kopske (Wilh.), on Photographic Retouching, 55
Korea, the Geology of, T. H. Holland, 95
Kovalevsky (Prof. Sophie), Obituary Notice of, 375
Krilof's Portraits of Typical Representatives of Russian Races, 255
Krotoff (P.), Geology of the Kama River, 232
Kroutschoff (M.), Reproduction of Hornblende in Crystals, 545
Krystallographie, Physikalische, Dr. M. Liebisch, 126
Krystallographisch-chemischen Tabellen von Dr. A. Fock, A. E. Tutton, 197
Kuznetsoff, the Flora of the Caucasus, 21

Lacaze-Duthiers (M. de), Oyster Culture in Experimental Fish-Pond of Roscoff Laboratory, 456
Ladd (George Trumbull), Outlines of Psychology, a Text-book of Mental Science for Academies and Colleges, Prof. C. Lloyd Morgan, 506
Lagerheim (Dr. G. von), Appointment of, by Ecuador Government to investigate Cryptogamic Flora, 398
Lake Bonneville, Grove Karl Gilbert, Prof. T. G. Bonney, F.R.S., 485
Lake-Dwellings of Europe, being the Rhind Lectures in Archæology for 1888, Robert Munro, Prof. W. Boyd Dawkins, F.R.S., 341
Lakes of North America, the Origin of the Great, Prof. T. G. Bonney, F.R.S., 203
Lala (Ulysse): Compressibility of Mixed Air and Carbon Dioxide, 144; Compressibility of Mixtures of Air and Hydrogen, 432
Lamarck, the Principle of, and the Inheritance of Somatic Modifications, Prof. Giard, 328
Lamp (Herr), Periodical Variation in Earth's Axis caused by Periodical Transference of Ocean Water-Masses, 520
Lamp-lighter, an Automatic, Shelford Bidwell, F.R.S., 395
Lamplugh (G. W.), Flamborough Head Drifts, 550
Lancashire, Technical Education in, 326
Lancaster (A.), Cold Winters generally followed by Cold Summers, 426
Langdon-Davies (C.), an Explanation of the Phonophore, and more especially of the Simplex Phonophore Telegraph, 531
Langenbeck (Dr.), Die Theorieen über die Entstehung der Koralleninseln und Korallenriffe und ihre Bedeutung für geophysische Fragen, 293
Langley (Prof.), the Phosphorescent Light of Insects, 88
Language, Formation of, W. J. Stillman, 491; Miss Agnes Crane, C. Tomlinson, F.R.S., 534
Lankester (Prof. E. Ray, F.R.S.): on the Atrial Chamber of Amphioxus, 70; Wood's Holl Biological Lectures, 516;

Zoological Articles contributed to the Encyclopædia Britannica, 607
Lantern, on erecting Prisms for the, Prof. S. P. Thompson, 623
Lanterns, Optical Projection, Lewis Wright, 555
Lapland, Norwegian, Glacial Striæ and Morainic Gravel in, far Older than the Ice Age, Dr. Hans Reusch, 106
Lapparent (Prof. de), the Porphyritic Rocks of Jersey, 94
Larks, the Flight of, Alfred W. Bennett, 248
Larmor (J.): the Steam-Engine considered as a Thermodynamic Engine, James H. Cotterill, F.R.S., 123; Diffraction at Caustic Surfaces, 384
Latham (Baldwin), the Relation of Ground Water to Disease, 94
Latitude, Fluctuation of, Dr. Alfred Kirchoff, 88
Latitude Variation, Periodical Changes in Declination of Stars a possible Cause of, F. Folie, 240
Latitude, the Variations in, 110
Latter (Oswald H.), Frozen Fish, 464
Launch, the First Electric, built for English Government, 450
Laurent (Émile), Reduction of Nitrates to Nitrites by Seeds and Tubercles, 240
Lauth (Chas.), New Method of preparing Tetramethylbenzidine, 168
Lavoisier, Priestley, Cavendish and, Prof. T. E. Thorpe, F.R.S., 1
Layng (A. G.), Euclid's Elements of Geometry, 343
Le Bel (J. A.), Asymmetry and Production of Rotary Power in Chlorides of Compound Ammonium, 576
Le Chatelier (H.), Influence of Tempering on Electrical Resistance of Steel, 264
Lea (M. C.), Gold-coloured Allotropic Silver, 500, 598
Leading Screw, Cutting a Millimetre Thread with an Inch, Prof. C. V. Boys, F.R.S., 439
Leaves, Extraordinary Flight of, R. Haig Thomas, 9
Lecture, Scientific, Arrangements, Royal Institution, Gresham College, Victoria Hall, 520
Lecky (Mr.), Sun Record in Botanic Gardens, 450
Leder (Herr H.), Proposed Zoological and Botanical Expedition to Siberia, 451
Leeds, James Muir appointed Professor of Agriculture at the Yorkshire College, 397
Left-handedness, the Origin of Right or, Prof. J. M. Baldwin, 43
Legge (Colonel), the Australian Curlew, 89
Leinster, Granites of, Prof. W. J. Sollas, F.R.S., 412
Lendenfeld (Dr. R. von), Bright Crosses in the Sky seen from Mountain Tops, 464
Leonid Meteors, 210
Lepidoptera, Changes in Markings and Colouring of, caused by subjecting Pupæ to Different Temperature Conditions, C. Merrifield, 191
Lepidoptera, Sale of Captain Yankowsky's Collection of Chinese Butterflies, 543
Lepine (R.), Glycolytic Power of Human Blood, 528
Lesage (Pierre), Influence of Salinity on Quantity of Starch contained in Vegetable Organs of *Lepidium sativum*, 624
Lescarbault (Dr.), Planet or New Star, 303
Levasseur (Émile), General Relation of State and Increase of Population in France, 192
Leveau (Gustave), Determination of Masses of Mars and Jupiter by Meridian Observations of Vesta, 384
Levelling and Surveying Instruments, William Ford Stanley, 374
Lewes (Prof. V. B.), Gaseous Illuminants, 233, 254, 282
Lewis (W. J.), Huet's Anemometer, 323
Leyden Museum Notes, 501
Leyden Observatory, 328
Liagre (Lieut.-General), Death of, 299
Lichen in Turkey and Asia, Showers of Manna, Edible, 255
Liebisch (Dr. M.), Physikalische Krystallographie, 126
Life-Areas of North America, Dr. C. H. Merriam, 88
Life Story of Our Earth, N. D'Anvers, 103
Light, a Band of, 129
Light, B. A. Smith on Newton's Rings, 55
Light, a Class-Book on, R. E. Steel, 606
Light, Determination of Direction of Vibration in Polarized, A. Cornu, 336
Light and Heat Problems, R. Wallace Stewart, 28
Light at Surface of Magnetized Medium, Reflection and Refraction of, A. B. Basset, F.R.S., 286
Light, the Theory of, Thomas Preston, 53
Light-Rays, Behaviour of Water as to Passage of Different, Hüfner and Albrecht, 520
Lightning, Peculiar Effects on Eggs of, R. H. Scott, F.R.S., 215
Lime, Action of, on Clay Soils, Alexander Johnstone, 308
Lind's Anemometer, 323
Linnean Society, 72, 119, 192, 311, 383, 479, 503, 526, 574
Linnean Society of New South Wales, 544; Annual General Meeting of, 450
Linossier (G.), Aspergillin, a Vegetable Hæmatin, 456, 600
Lippmann (G.), Photography of Colours, 360
Liquid and Gaseous States, Prof. Van der Waals on the Continuity of the, Prof. J. T. Bottomley, F.R.S., 415; Prof. A. W. Rücker, F.R.S., 437; Robert E. Baynes, 487
Lizard, Poisonous, Mr. Boulenger and Prof. C. Stewart, 383
Local Taxation Act and the Technical Instruction Act, the Working of, 167
Lock (J. B.), Elementary Statics, 55
Lockhart (J. G.), the Moose, 109
Lockjaw, Relation of Gangrenous Septicæmia to, M. Verneuil, 48
Lockyer (Prof. J. Norman, F.R.S.), on some Points in the Early History of Astronomy, 559
Locomotion of Arthropods, H. H. Dixon, 223
Lodge (Prof. Oliver J., F.R.S.): Modern Views of Electricity, Volta's so-called Contact Force, 268, 367, 463; Ratio of Centimetre to Inch, 463; Force and Determinism, 491; the Flying to Pieces of a Whirling Ring, 439, 461, 533; the Meaning of Algebraic Symbols in Applied Mathematics, 513
Lœwy and Puiseux (MM.), Determination of the Constant of Aberration, 498
Logarithms, Trigonometry and, J. M. Eustace, 55
Löhr (Dr.), Cadmium and Magnesium Analogues of Zinc Methide and Ethide, 256
London Air, the Darkness of, W. Hargreaves Raffles, 152, 222; George White, 318
London Fogs, Effect of, on Cultivated Plants, 107
London, the Smoke Nuisance in, 426
London, Sunshine of, Fredk. J. Brodie, 424
London University College, Appeal for Funds, 289
London-Paris Telephone, 475
Longitudes, Annuaire du Bureau des, 356
Lord Howe Island, Vegetation of, W. Botting Hemsley, F.R.S., 565
Lorentz (H. A.), Applications of Maxwell's Principles of Electrical Phenomena in Moved Bodies, 408
Love (A. E. H.), Present State of Theory of Thin Elastic Shells, 310
Lovel (J.), Iridescent Clouds, 464
Lovell (Kate), Nature's Wonder Workers, 532
Lowe (E. J., F.R.S.): Birds' Nests, 199; the Cold of 1890-91, 294; British Ferns and Where Found, 389; Frozen Fish, 391; Cocks and Hens, 583
Lowndes (Mary E.), Translation of Prof. Harald Höffding's Outlines of Psychology, 553
Lowne (B. Thompson), Anatomy, Physiology, Morphology, and Development of the Blow-fly, 77
Luminous Clouds, O. Jesse, 59
Lunar Radiation, Measures of, C. C. Hutchins, 44; the Earl of Rosse, F.R.S., 104
Lydekker (R.), Generic Identity of Sceparnodon and Phascolonus, 214
Lyra, Recent Photographs of the Annular Nebula in, A. M. Clerke, 419
Lyræ (α), the Duplicity of, 257; A. Fowler, 64

Maas, River, Floods in the, 72
Macao, Volcanic Eruption at, 182
Macaulay (W. H.), the Meaning of Algebraic Symbols in Applied Mathematics, 558
MacBride (E. W.), Development of the Oviduct in Frog, 407
McConnell (P.), Agricultural Education, 182
McCook (Henry C., D.D.), American Spiders and their Spinning Work, 74
McCosh (Dr. James), the Prevailing Types of Philosophy, 531

McCoy (Sir Frederick), Prodomus of the Zoology of Victoria, 389
Macdonald (Rev. James), the South African Doctrine of Souls, 307
Macdonald (John) and Prof. John Milne, F.R.S., on Vibration-Recorders, 154
Macdonald (W. J.), Higher Geometry, 8
Macfarlane (James), an American Geological Railway Guide, 8
Machinery, Electric Mining, Ll. B. and C. W. Atkinson, 355
McKinley Bill on Scientific Instruments, Effect of, 108
McLachlan (R., F.R.S.), Frozen Fish, 440, 535
Maclear (J. P.), a Bright Green Meteor, 30
MacMahon (Major P. A., F.R.S.), Weighing by a Series of Weights, 113
McMillan (W. G.), a Treatise on Electro-Metallurgy, 244
McRae (Charles), Fathers of Biology, 245
Magellan (Ferdinand), the Life of, F. H. H. Guillemard, 294
Magic Mirrors, Through, Arabella B. Buckley, 246
Magnetism and Electricity: Elementary Manual of, Andrew Jamieson, 102; Magnetism and Electricity, J. Spencer, 127; Illustration of Ewing's Theory, Prof. S. P. Thompson, 190; Solution of a Geometrical Problem in Magnetism, T. H. Blakesley, 191; Model Illustrating a Molecular Theory of Magnetism, Prof. Ewing, 239; Magnetism and Light, A. B. Basset, F.R.S., 286; Distribution of Magnetism in Alpine Regions, Sella and Oddone, 378; Magnetic Rotation, W. Ostwald, 454; Magnetic Disturbance of the Compass in North-West Australia, Commander E. W. Creak, F.R.S., 471; Magnetic Proof Pieces and Proof Planes, Prof. S. P. Thompson, 549; Magnetic Screening of Conducting Media, Prof. J. J. Borgman, 583; Magnetic Anomalies in France, M. Mascart, Prof. A. W. Rücker, F.R.S., 617; a Property of Magnetic Shunts, Prof. S. P. Thompson, 623
Main (E.), on Frozen Fish, 535
Mair (Major), on the Disappearance of the Moa, 105
Malaria, the Pathogenic Fungus of, Surgeon J. F. Evans, 430
Malay Peninsula, the Ipoh Poison of the, 377; the Red Ant (Caringa) of the, H. N. Ridley, 620
Malayan Peninsula, Materials for a Flora of the, No. 2, Dr. G. King, F.R.S., 149
Malbot (H. and A.), Conditions of Reaction of Isopropylamines, 48
Mallock (A.), Photographic Perspective and the use of Enlargement, 517
Mammalia, New Fossil, Discovered at Samoa, 85
Mammalian Nervous System, Francis Gotch and Victor Horsley, F.R.S., 428
Mammals, Fossil, of North America, Profs. W. B. Scott and H. F. Osborn, 177
Mammoth and Man in America, 63
Man and the Mammoth in America, 63
Man, Whence Comes, Arthur John Bell, 460; Why does Man Exist?, Arthur John Bell, 460
Manganese Nodules in Deep Sea, Dr. John Murray, 287; Oceanic and Littoral Nodules, J. Y. Buchanan, 287
Mann (N. M.), Orbits of 61 Cygni, Castor, and 70 Ophiuchi, 90
Manna, Edible Lichen, in Turkey in Asia, Showers of, 255
Manual Training in Education, C. M. Woodward, 220
Map of Chin-Lushai Country, New, 209
Maps and Map-Drawing, William A. Elderton, 196
Maquenne (M.), the β-Pyrazoldicarbonic Acids, 95
Mar (F. W.), Certain Points in Estimation of Barium as Sulphate, 598
Marbeau (Ed.), the Depopulation of France, 183
Marcet (Dr.), Different Volumes of Air Respired by Different Persons, 451
Marchand (Em.), Observations of Sun-spots in 1889, 432
Marès (M.), the Past Winter in Algeria, 567
Marey (M.), a Photo-Chronographic Apparatus to analyze all kinds of Motion, 48
Marine Biology: Proposed Station in Queensland, W. Saville-Kent, 255; Biological Lectures delivered at the Marine Biological Laboratory of Wood's Holl in the Summer Session of 1890, 457, 516, 591
Marine Zoology: Marine Zoological Station at Cette, Proposed, Prof. Armand Sabatier, 397; the Naturalist of Cumbrae, being the Life of David Robertson, Rev. T. R. R. Stebbing, 532
Marr (J. E.), the Cross Fell Inlier, 598
Marriott (William), the Royal Meteorological Society's Exhibition, 446
Mars, Observations of, J. Guillaume, 428
Marshall (John, F.R.S.), Death of, 230
Marshes, Canalization of Pinsk, 164
Marten (H. J.), Action of Water upon Stones in Severn Weir, 214
Marvin (Prof.), Wind-pressures and Measurement of Wind Velocities, 548
Mascart (M.), Magnetic Anomalies in France, Prof. A. W. Rücker, F.R.S., 617
Mason (P. B.), Squirrels in Cold Weather, 544
Mason (W. B.), List of Earthquakes recorded (August 11, 1888, to December 31, 1889) in Northern and Central Japan, 131
Masson (Prof. Orme): a Deduction from the Gaseous Theory of Solution, 345; Does Magnesium Combine with Hydrocarbon Radicles?, 454
Masters (Dr. Maxwell T., F.R.S.), Araucaria Cones, 56
Matabeleland, by Lieut. E. A. Maund, 91
Mathematics: Elementary Text-book of Trigonometry, R. H. Pinkerton, 7; Higher Geometry, W. J. Macdonald, 8; Elementary Algebra, W. A. Potts and W. L. Sargant, 28; Heat and Light Problems, R. Wallace Stewart, 28; Weighing by a Ternary Series of Weights, J. Willis, 30; Weighing with a Ternary Series of Weights, 198; J. Willis, 198; Prof. J. D. Everett, F.R.S., 198; Major P. A. MacMahon, F.R.S., 113; Elementary Statics, the Rev. J. B. Lock, 55; Notes on Trigonometry and Logarithms, Rev. J. M. Eustace, 55; Elementary Treatise on Hydro-dynamics and Sound, A. B. Basset, 75; Doppler's Principle, R. W. Stewart, 80; Award of the De Morgan Medal to Lord Rayleigh, F.R.S., 83; Mathematical Society, 95, 192, 384, 479, 575; Nowhere can Mathematics be learned as at Cambridge, Prof. George M. Minchin, 151; the Century Arithmetic, 175; Ellipsoidal Harmonics, W. D. Niven, F.R.S., 189; Weights proceeding by Powers of 3, Prof. J. D. Everett, F.R.S., 104; Tables of Spherical Harmonics, Rev. J. Perry, F.R.S., 118; the Subject-Matter of Exact Thought, A. B. Kempe, F.R.S., 156; on Stokes's Current Function, R. A. Sampson, 261; Key to Arithmetic in Theory and Practice, J. Brooksmith, 294; Theory of Functions, Dr. H. A. Schwartz, Prof. O. Henrici, F.R.S., 321, 349; a Purely Algebraic Demonstration of the Fundamental Theorem of the Theory of Equations, E. Amigues, 336; Some hitherto Unproved Theorems in Determinants, 335; a Problem of Elimination connected with Glissettes of Ellipse and Hyperbola, 335; a Dictionary of Metric and other Useful Measures, Latimer Clark, F.R.S., 487; Spinning Disks, J. T. Nicholson, 514; Tension of a Girdle of the Earth, Prof. A. S. Herschel, F.R.S., 513; the Meaning of Algebraic Symbols in Applied Mathematics, Prof. Oliver J. Lodge, F.R.S., 513; Mechanical Trisection of any Angle, Captain A. H. Russell, 547; on the Rôle of Quaternions in the Algebra of Vectors, Prof. J. Willard Gibbs, 511; Quaternions and the Algebra of Vectors, Prof. P. G. Tait, 535; the Rôle of Quaternions in the Algebra of Vectors, Prof. P. G. Tait, 608; the Foundations of Geometry, E. T. Dixon, 554; the Meaning of Algebraic Symbols in Applied Mathematics, W. H. Macaulay, 558; Supplement to Euclid Revised, R. C. J. Nixon, 581. *See also* Geometry.
Mathew (Rev. John) and Edward Stephens, on Australian Aborigines, 185
Matter, Properties of, Prof. P. G. Tait, 78
Maund (Lieutenant E. A.), Matabeleland, 91
Mawley (E.), Phenological Observations for 1890, 215
Maximowicz (K. I.), Death of, 398; Obituary Notice of, Dr. Otto Stapf, 449
Maxwell (James Clerk), the Scientific Papers of, Right Hon. Lord Rayleigh, F.R.S., 26
Mayall (Mr.) and the New Objective 1·6 N.A., an Explanation, 47
Mayer (A. M.): Illuminating Power of Flat Petroleum Flames, 309; Physical Properties of Vulcanite, 309
Measure-system of Seventeenth Century, Decimal, Prof. J. H. Gore, 309
Measures, a Dictionary of Metric and other Useful, Latimer Clark, F.R.S., 487
Measuring Atmospheric Dispersion, a Method of, Prospe Henry, 400

Mechanics: Institution of Mechanical Engineers, 22, 332, 499; Hand-book for Mechanical Engineers, Prof. Henry Adams, 147; Weighing with a Ternary Series of Weights, 198; J. Willis, 30, 198; Prof. J. D. Everett, F.R.S., 198; Prof. P. G. Tait on Impact, 287; Present State of the Theory of Thin Elastic Shells, A. E. H. Love, 310; the Bursting of a Pressure-Gauge, Frederick J. Smith, 318; Bursting of a Pressure-Gauge, Newton and Co., 366; the Flying to Pieces of a Whirling Ring, Prof. A. M. Worthington, 463, 583; Lessons in Applied Mechanics, by J. H. Cotterill, F.R.S., and J. H. Slade, 461; Mechanical Trisection of any Angle, Captain A. H. Russell, 547. *See also* Whirling Ring.

Medical Faculty of Queen's College, Birmingham, Removal of, 542

Medical Hand-book, the Colonist's, E. A. Barton, 150

Medicine at St. Petersburg, Foundation of Institute for Experimental, 208

Medusæ of Millepora and their Relation to Medusiform Gonophores of Hydromedusæ, S. J. Hickson, 407

Meek (Alexr.), Museums for Public Schools, 180

Meldola (Prof. R., F.R.S.): Handbuch der Photographie, Prof. Dr. H. W. Vogel, 3; Co-adaptation, 557; the Darwinian Theory of the Origin of Species, Francis P. Pascoe, 409

Meldrum (Dr.): Atlas of Cyclone Tracks of South Indian Ocean, 620; Eliot's Cyclone Memoirs, III., 620

Mell (P. H.), Varieties of Cotton Grown in Alabama, 521

Mercadier (E.), Intensity of Telephonic Effect, 288

Mercury, the Planet, 569

Meridian Conference, United States and the Proposed Universal Prime, 108

Merle's (Walter) Weather Records, 1337–44, Proposed Publication of, 592

Merope, a New Nebula near, Prof. Barnard, 379, 546; Prof. Pritchard, 546

Merriam (Dr. C. H.), the Fauna and Flora of Arizona, 88

Merrifield, Changes in Markings and Colourings of Lepidoptera caused by Subjecting Pupæ to Different Temperature Conditions, 191

Merrifield (F.), *Selenia illustraria*, 503

Merry (Mr. A. S.), on Heat, 327

Merulius lacrymans, Dry-rot Fungus, F. T. Mott, 129

Mesozoic and Palæozoic Age in Eastern Australia and Tasmania, the Coal and Plant-bearing Beds of, B. O. Feistmantel, 148

Metallurgy: Metal-Turning, by a Foreman Pattern Maker, 175; American Blast-Furnace Work, 211; the Art of Electrolytic Separation of Metals, Theoretical and Practical, G. Gore, F.R.S., 244; Influence of Tempering on Electrical Resistance of Steel, H. Le Chatelier, 264; Instrument for Detecting Flaws in Metal Castings, Captain de Place, 279; Native Nickel in Sands of Elvo Torrent, Piedmont, Alfonso Sella, 312; the Passive State of Iron and Steel, II., Thos. Andrews, 358; Mixed Metals or Metallic Alloys, A. H. Hiorns, Prof. W. C. Roberts-Austen, F.R.S., 388; an Introduction to the Study of, Prof. W. C. Roberts-Austen, F.R.S., Thomas Gibb, 530

Meteorology: the Causes of Anticyclones and Cyclones, Henry F. Blanford, F.R.S., 15, 81; the Genesis of Tropical Cyclones, Henry F. Blanford, F.R.S., 81; the Aurora, Forces concerned in Development of Storms, M. A. Veeder, 20; the Diurnal Probability of Rain, Prof. P. Busin, 20; Meteorological Observations for 1886 at Stations of Second Order, 20; Remarkably Warm Winter at St. Petersburg, 42; Synchronous Daily Weather Charts of North Atlantic, 42; Photographs of Meteorological Phenomena, Arthur W. Clayden, 55; Luminous Clouds, O. Jesse, 59; Rainfall of Kent and Surrey, 63; Nils Ekholm on the Determination of the Path taken by Storms, 63; T. Tuhlin on the Nocturnal Temperature of the Air, 63; Weather Service of United States, 87; Temperature Variations in Lakes, Rivers, and Estuaries, 92, 93; Royal Meteorological Society, 94; Exhibition of, William Marriott, 446; Catalogue of the Royal Meteorological Society's Library, J. S. Harding, Jun., 497; Climatological Table for the British Empire for 1889, 108; Pilot Charts of the North Atlantic, 108, 208, 279; Meteorological Report of the United States for the Year ending June 30, 1890, 131; the Darkness of London Air, W. Hargreaves Raffles, 152; Glacial Climate, Prof. N. S. Shaler, 155; alleged Climatological Period of Thirty-five Years, Prof. Brückner, 163; New Station (Tientsin) in Telegraphic Communication with the Shanghai Observatory, 164; Streamers of White Vapour, N. F. Dupuis, 175; Report of the Meteorological Council for the Year ending March 31, 1890, 182; Russian Meteorology Review, 183; Meteorology of the Canadian Yukon, W. Ogilvie, 189; Distribution of Atmospheric Pressure in Russia and Asia, 1836–1885, General de Tillo, 208; Peculiar Effect of Lightning on Eggs, R. H. Scott, F.R.S., 215; Wind Systems and Trade Routes between Cape and Australia, Captain Hepworth, 215; Phenological Observations for 1890, E. Mawley, 215; Finley's Storm-Track, &c., Charts of North Atlantic, 231; the Monsoons of the Yang-tse-Kiang, 232; Meteorologitcheskiy Sbornik, 232; Results of Comparisons of Pressure and Temperature Observations at the Eiffel Tower with Low-level Stations (1889), M. Angot, 254; Self-recording Barometer, Redier and Meyer, 255; Prediction of Cold Waves from Signal Service Weather Maps, T. Russell, 260; the Great Frost of the Winter of 1890–91, 270; the Cold of 1890–91, E. J. Lowe, F.R.S., 294; the Great Frosts of 1890–91, C. Harding, 431; Report of the Kew Committee for Year ending October 31, 1890, 278; Waterspout at Newhaven, Connecticut, H. J. Cox, 285; Observations and Studies with Sling Hygrometer on Mount Washington, Prof. Hazen, 285; Practical Solution of Problem of Emergent Liquid Column of Thermometer by Employment of Correcting Stem, C. E. Guillaume, 288; Dew, Colonel Badgley, 311; a Remarkable Ice-Storm at Hartford, Prof. Samuel Hart, 317; the Darkness of London Air, George White, 318; the Erosive Action of Frost, 319; Floods from Rainfall in Queensland, 326; Establishment of a Meteorological Station at Noumea, C. L. Wragge, 326; H. C. Russell's Meteorological Observations in New South Wales during 1888, 377; Meteorological Bibliography of Mexico, Señor R. A. Santillan's, 398; a Solution of the Aurora Problem, Prof. F. H. Bigelow, 405; the Sunshine of London, Fredk. J. Brodie, 424; Cold Winters generally followed by Cold Summers, A. Lancaster, 426; Extraordinary Dryness of February 1891, G. J. Symons, F.R.S., 426; the Tides off the Dutch Coasts, 450; Recent Progress in Dynamic Meteorology, Prof. Cleveland Abbe, 450; Cable to the Andamans necessary for Meteorological Purposes, 451; February Sunshine, 453; Actinometric Observations at Kief, 1890, M. Savélief, 456; Iridescent Clouds, J. Lovel, 464; Bright Crosses in the Sky seen from Mountain Tops, Dr. R. von Lendenfeld, 464; Prof. William Ferrel on the High-Pressure Area of November 1889 in Central Europe, with Remarks on High-Pressure Areas in General, 466; Meteorological Observations taken at Sarona, Syria, Monthly, James Glaisher, 473; Injury from Hail in Würtemberg between 1828–87, Herr Bühler, 473; Temperature of the Clyde Sea-Area, Dr. John Murray, 480; Weather Forecasting in Australia, 497; G. J. Symons, F.R.S., on the History of Rain-Gauges, 504; Influence of Height of Rain-Gauges on Rainfall Records, Dr. Hellmann, 520; Relation of High Winds to Barometric Pressure at Ben Nevis Observatory, Dr. A. Buchan, 527; Meteorology of Ben Nevis, Dr. A. Buchan, 538; Royal Meteorological Institute of the Netherlands, Appointment of Dr. Maurice Snellen as Director, 542; the U.S. National Weather Service, 543; Mean Temperature and Rainfall for 1890 in Germany and British Isles, 543; Wind Pressures and Measurement of Wind Velocities, Prof. Marvin, 548; Die Denudation in der Wüste, Prof. Johannes Walther, 556; Additional Results of the United States Scientific Expedition to West Africa, Profs. Cleveland Abbe and David P. Todd, 563; the Past Winter in Algeria, M. Marès, 567; Typical Winter Weather Conditions, Dr. van Bebber, 567; the Wheat Harvest in Relation to Weather, 569; the Paradox of the Sun-spot Cycle, Henry F. Blanford, F.R.S., 583; the Formula for Wind Velocities (see *ante*, p. 548) as Observed with Robinson's Cup Anemometer, 592; Weather in Atlantic during March, 592; Effect of Difference of Exposure on Reading of Thermometers, Dr. Sprung, 592; Proposed Publication of Walter Merle's Weather Records, 1337–44, 592; Remarkable Features in Winter of 1890–91, F. J. Brodie, 599; Rainfall of February 1891, H. S. Wallis, 599; Variations of Rainfall at Cherra Poonjee, Assam, H. F. Blanford, 599; Open Manometer at Eiffel Tower, L. Cailletet, 599; on Tidal Prediction, Prof. G. H. Darwin, F.R.S., 609; Dr. Meldrum's Atlas of Cyclone Tracks of South Indian Ocean, 620

Meteors: a Bright Green Meteor, J. P. Maclear, 30; the Frequency of Meteors, MM. Terby and Van Lint, 133; a Large and Brilliant Fireball Meteor, Rev. A. Freeman, 150; Robert Hunter, 151; Fireball of December 14, James Turle, 176; the Leonid Meteors, 210; a Beautiful, Rev. J. Hoskyns-Abrahall, 416; Remarkable Meteor, 556; Fall of a Great Meteor at Sea, 590; W. Budgen, 608; the Turgaisk Meteorite, E. D. Kislakovsky, 334; Meteorite of Oschansk, 228; the Supposed Occurrence of Widespread Meteoritic Showers, L. Fletcher, F.R.S., 295
Metric System, the Decimal, J. E. Dowson, 354
Metric and other Useful Measures, a Dictionary of, Latimer Clark, F.R.S., 487
Meunier (S.), Artificial Reproduction of Daubreelite, 500
Mexico: the Name Anahuac applied by Mistake to Plateau of, Dr. Seler, 208; Earthquake in, 131, 279; Return of Mr. C. G. Pringle from, 377; Señor R. A. Santillan's Meteorological Bibliography of, 398
Meyer (M.), Self-recording Anemometer, 255
Miall (L. C.), Object-Lessons from Nature, a First Book of Science, 511
Micmac Indians, Dictionary of the Language of the, Rev. Dr. S. T. Rand, 102
Microbes de la Bouche, Dr. Th. David, 174
Microscopy: the Cell Theory, Past and Present, Sir William Turner, F.R.S., 10, 31; Swift and Son's Improved Students' Microscope, 47; Quarterly Journal of Microscopy, 70; Microscopical Study of the Skin of Toads and Salamanders, Herr Schultz, 209
Miller (G. H.), Action of Heat on Nitrosyl Chloride, 262
Millimetre Thread, Cutting a, with an Inch Leading Screw, Prof. C. V. Boys, F.R.S., 439
Milling Cutters, George Addy, 23
Millson (Alan), Earthworms, 179
Milne (Prof. John, F.R.S.) and John Macdonald, Vibration-Recorders, 154
Minchin (Prof. George M.): Nowhere can Mathematics be Learned as at Cambridge, 151; Photo-electricity, 334; Experiments in Illustration of Paper on Photo-electricity, 407
Mineralogy: the Great Mogul's Diamond and the Koh-i-nur, Valentine Ball, F.R.S., 103; Source of Jade used for Ancient Implements in Europe and America, F. W. Rudler, 310; Celebration of Fiftieth Anniversary of Prof. Arcangelo Scacchi, 376; Phosphorescence of Minerals under Light and Heat, H. Becquerel, 504; Mineralogical Magazine, 567
Mines, Coal Dust and Explosions in Coal Mines, 354
Mining Industries of New Zealand, 316
Mining Machinery, Electric, Ll. B. and C. W. Atkinson, 355
Mining: Rock Drills, 499
Mirrors, through Magic, Arabella B. Buckley, 246
Missouri, Garden Scholarships at, 187
Mivart (St. George, F.R.S.), Dogs, Jackals, Wolves, and Foxes, a Monograph of the Canidæ, 385
Moa, Note on the Disappearance of the, Prof. H. O. Forbes, 105
Mock Suns, T. Mann Jones, 269
Modena Market in 1889, Edible Fungi for Sale in, 21
Modern Views of Electricity, S. H. Burbury, F.R.S., 366, 439, 515; Prof. Oliver J. Lodge, F.R.S., 367; A. P. Chattock, 367, 491
Modifications of Organisms, on the, David Syme, Dr. Alfred R. Wallace, 529
Mogul's Diamond, the Great, and the Koh-i-nur, Valentine Ball, F.R.S., 103
Moissan (M.), Boron Iodide, 568
Molecular Dispersion, Dr. J. H. Gladstone, F.R.S., 198
Mollusca of British New Guinea, 114
Mollusk Shells, Algæ growing in, Bornet and Flahault, 185
Monaco's (Prince of), New Steam Yacht, 519
Monsoons of the Yang-tse-Kiang, 232
Montessus (M. de), Relation of Earth-tremors to Seasons, 456
Monuments: the State of Egyptian, 182; the Preservation of, 230
Moody (G. T.), Combustion of Magnesium, 454
Moose: Measures of Lunar Radiation, C. C. Hutchins, 44; the Earl of Rosse, F.R.S., 104
Moon, the, J. G. Lockhart, 109
Morainic Gravel and Glacial Striæ in Norwegian Lapland older than the Ice Age, Dr. Hans Reusch, 106
Morbology: Relation of Gangrenous Septicæmia to Lockjaw, M. Verneuil, 48; Accumulation of Atmospheric Nitrogen in Cultures of *Bacillus radicicola*, Mr. Beyerinck, 600
Morgan (De), Medal awarded to Lord Rayleigh, F.R.S., 83
Morgan (Prof. C. Lloyd): Animal Life and Intelligence, Dr. Alfred R. Wallace, 337; Outlines of Physiological Psychology, a Text-book of Mental Science for Academies and Colleges, George Trumbull Ladd, 506; the Principles of Psychology, William James, 506; Force and Determinism, 558
Morgan (Prof. T. H.), on the Relationships of Sea-spiders, 459
Morley (E. W.), Volumetric Composition of Water, 600
Morphology: the Morphology of the Sternum, Prof. G. B. Howes, 269; Stereom, F. A. Bather, 345; on the Affinities of Hesperornis, Dr. F. Helm, 368; some Problems of Annelid Morphology, Prof. E. B. Wilson, 458; on the Morphology of the Duck and Auk Tribes, W. Kitchen Parker, F.R.S., 486
Moscow, Proposed International Scientific Congress at, 207
Mosses, the British, the Right Hon. Lord Justice Fry, F.R.S., 379, 400
Motais (M.), Myopia, a Product of Civilization, 163
Motion, a Photo-chronographic Apparatus to analyze all kinds of, M. Marey, 48
Mott (F. T.), Dry-rot Fungus, 129
Mounds, Ancient, at Floyd, Iowa, Clement L. Webster, 213
Mud Glaciers of Cromer, William Sherwood, 515
Muir (Mr. James), appointed Professor of Agriculture at Yorkshire College, Leeds, 397
Muir (Dr. Thomas): some hitherto Unproved Theorems in Determinants, 335; a Problem of Elimination connected with Glissettes of Ellipse and Hyperbola, 335
Multiple Origin of Races, W. T. Thiselton Dyer, F.R.S., 535
Mummery (J. H.), Dentine, 501
Munro (John) and Andrew Jamieson, a Pocket-book of Electrical Rules and Tables, 268
Munro (Robert), Lake-dwellings of Europe, being the Rhind Lectures in Archæology for 1888, Prof. W. Boyd Dawkins, F.R.S., 341
Müntz (A.): Hæmoglobin in Blood according to Conditions of Existence, Amount of, 360; Distribution of Sea-salt according to Altitude, 432
Murray (Dr. John): Manganese Nodules in Deep Sea, 287; Temperature of Clyde Sea-area, 480; Silica and Siliceous Formations in Modern Seas, 527
Muscle, the Minute Structure of Striped Muscle, J. B. Haycraft, 286
Museum at Noumea, Projected Government, 474
Museum Report, U.S. National, 620
Museum at Rome, the New Commercial, 497
Museum, the Science, 495
Museum, Sydney Australian, 132
Museums for Public Schools, Alexr. Meek, 180
Museums in Siberia, Natural History, 163
Music, a New Musical Instrument, Dr. Shohei Tanaka, 521
Musical Sand *versus* Squeaking Sand, Prof. H. Carrington Bolton, 30
Musical Sounds, the Researches of Dr. R. Kœnig on the Physical Basis of, Prof. Silvanus P. Thompson, 199, 224, 249
Mutual Aid among Animals, William Elder, 56
Mycology: the Value of Attractive Characters to Fungi, Charles R. Straton, 9; Attractive Characters in Fungi, 79, 80, 151; R. Haig Thomas, 79; T. Wemyss Fulton, 269; the Fungi of Warwickshire, James E. Bagnall and W. B. Grove, 413
Myology of the Raven, R. W. Shufeldt, 101
Myopia a Product of Civilization, M. Motais, 163
Mythology, the Botanical, of the Hindoos, Dr. Dymoke, 46

Naden, Constance, a Memoir, William R. Hughes, 343
Nansen (Dr. Fridtjof), the First Crossing of Greenland, 172; Coming Arctic Expedition, 450; on Glaciation, Dr. A. Irving, 541
Naples, Zoological Station of, 392; Dr. Anton Dohrn, 465
Natural History of the Animal Kingdom for the Use of Young People, 435
Natural History, Berge's Complete, 366
Natural History, the Essex Field Club, 325
Natural History: the Life of Philip Henry Gosse, F.R.S., by his Son, Edmund Gosse, 603

Natural History Museum at South Kensington, Additions to, 378
Natural History Society for West France, Foundation of, 377
Natural History Transactions of Northumberland, Durham, and Newcastle-on-Tyne, 109
Naturalist of Cumbrae, being the Life of David Robertson, Rev. T. R. R. Stebbing, 532
Naturalist's Occupation, the, Prof. C. O. Whitman, 457
Nature, Object Lessons from, a First Book of Science, L. C. Miall, 511
Nature's Wonder-Workers, Kate Lovell, 532
Nautical Surveying, Vice-Admiral Shortland, 8
Naval Observatory, United States, 357
Nebula in Lyra, Recent Photographs of the Annular, A. M. Clerke, 419
Nebula near Merope, a New, E. E. Barnard, 379, 546; Prof. Pritchard, 546
Nebula, a Variable, Roberts, 453; Bigourdan, 453
Nebula, Variability of the Andromeda, Isaac Roberts, 379
Nebulæ, Prof. Holden on the Parallaxes of, 65
Neo-Lamarckism and Darwinism, Prof. George Henslow, 490, 581; T. D. A. Cockerell, 533
Neptunia, New Italian Monthly Marine Scientific Journal, 543; 567
Nervous System, the Mammalian, Francis Gotch and Victor Horsley, F.R.S., 428
Netchaieff (A.), Geology of the Kama Region, 232
Netherlands, Royal Meteorological Institute of, Dr. Maurice Snellen appointed Director, 542
Neville (F. H.), Molecular Condition of Metals when Alloyed with each other, 262
New Guinea, British: the Scientific Results of the Occupation of, 114; Geology, Ornithology, Reptiles, and Botany of, 115; Throwing-sticks and Canoes in, Prof. Henry O. Forbes, 248; Prof. A. C. Haddon, 295
New South Wales: Appointment of Mr. A. S. Olliff as Government Entomologist, 254; Meteorological Observations during 1888 in, H. C. Russell, 377
New Zealand: Geological Survey of, 109; the Mining Industries of, 316; Science in, Sir James Hector, F.R.S., 521
Newberry (Prof. John Strong), the Palæozoic Fishes of North America, 146
Newcastle-upon-Tyne, Durham College of Science, 519
Newcomb (Prof.), Transits of Venus in 1761 and 1769, 328
Newsholme (Dr. Arthur), Lessons on Health, Dr. J. H. E. Brock, 54
Newton and Co., the Bursting of a Pressure-Gauge, 366
Newton's Rings, B. A. Smith, 55
Niagara Falls, the Recession of, 131; Prof. Edward G. Bourne, 515
Nichols (E. L.), the Alternating Electric Wire between Ball and Point, 309
Nicholson (Prof. H. A.), the Cross Fell Inlier, 598
Nickel in Sands of Elvo Torrent, Piedmont, Alfonso Sella, 312
Nicolson (J. T.), Spinning Disks, 514
Nipher (F. E.), Electric Lighting in St. Louis, 497
Nismes, Fungus-poisoning at, 356
Niven (W. D., F.R.S.), Ellipsoidal Harmonics, 189
Nixon (R. C. J.), Supplement to Euclid Revised, 581
Nogues (A. F.), Earthquakes of May 1890, Chili, 24
Norfolk, the Birds of, Henry Stevenson, 313
Norfolk and Norwich Naturalists' Society, 566
North America: the Origin of the Great Lakes of, Prof. T. G. Bonney, F.R.S., 203; Forestry in, Prof. W. R. Fisher, 247; Remarkable Ancient Sculptures from North-West America, James Terry, Dr. Alfred R. Wallace, 396
Norwegian Lapland, Glacial Striæ and Morainic Gravel in, far Older than the Ice Age, Dr. Hans Reusch, 106
Notidanus indicus, on the Presence of a Sternum in, Prof. T. Jeffery Parker, F.R.S., 516
Noumea: Establishment of Meteorological Station at, C. L. Wragge, 326; Projected Government Museum at, 474
Nuovo Giornale Botanico Italiano, 382
Nyassaland Region, the: H. H. Johnston, 45; the Antelopes of, Richard Crawshay, 143

Object Lessons from Nature, a First Book of Science, L. C. Mial, 511
Observatories: the New Observatory at Catania, 65, 141; Turin Observatory, Signor Porro, 257; Harvard College Observatory, D. W. Baker, 280; the Leyden, 328; United States Naval, 357; Annuaire de l'Observatoire de Bruxelles, 475; the Vatican, 496; Meteorology of Ben Nevis, Dr. Alexander Buchan, 538
Oceanic Birds, on the Flight of, Capt. David Wilson-Barker, 222; John R. Spears, 319
Oceanographic Investigation of Adriatic, Austrian Government, 230
Oceanography, Results of the *Pola* Expedition, 567
Oddone (Signor), Distribution of Magnetism in Alpine Regions, 378
Ogilvie (W.), Meteorology of the Canadian Yukon, 189
Oil at Sea, the Use of, 451
Oil-fields in India, the Reported, 131
Oliver (Prof. Daniel, F.R.S.), Retirement of, 277
Oliver (J. W.), Elementary Botany, 461
Olliff (A. S.), Appointment as Government Entomologist (New South Wales), 254
Olliff (A. Sidney), Butterflies Bathing, 199
Olszewski (M.), Colour Absorption Spectrum of Liquid Oxygen, 498
Ophthalmology: a Membrane lining the Fossa Patellaris of Corpus Vitreum, Connection between Suspensory Ligament of Crystalline Lens and Lens Capsule, Prof. T. P. A. Stuart, 406; Cyclopia in a Child, Dr. A. Bruce, 527; Position of the Visual Centre in Man, Dr. B. Bramwell, 527
Optical Lantern, on Erecting Prisms for the, Prof. S. P. Thompson, 623
Optical Projection, Lewis Wright, 555
Optics, B. A. Smith on Newton's Rings, 55
Optics: Diffraction at Caustic Surfaces, J. Larmor, 384
Oppert (J.), a Chaldæan Astronomical Annual used by Ptolemy, 95
Orang, Lesser (*Simia morio*), Additions to Zoological Gardens of, 618
Orbit of α Virginis, on the, Prof. H. C. Vogel, 235
Orchids at Kew, 1890, 473
Organisms, on the Modification of, David Syme, Dr. Alfred R. Wallace, 529
Orgyia antiqua, Curious Habit of Larvæ of, E. W. Carlier, 255
Orientation, Prof. J. Norman Lockyer, F.R.S., on Some Points in the Early History of Astronomy, 559
Origin of Races, Multiple, W. T. Thiselton Dyer, F.R.S., 535
Origin of Species, the Darwinian Theory of the, Francis P. Pascoe, Prof. R. Meldola, F.R.S., 409; Co-adaptation, Prof. Geo. J. Romanes, F.R.S., 490; Prof. R. Meldola, F.R.S., 557
Ormerod (G. W.), Death of, 254
Ornithology: on the Soaring of Birds, Prof. F. Guthrie, 8, 30, 287; Prof. T. Jeffery Parker, F.R.S., on the Anatomy and Development of Apteryx, 17; the late Mr. T. E. Booth's Museum of British Birds, 20; American Ornithologists' Union, 42; a Sparrow's Courtship, 63; Our Fancy Pigeons, by George Ure, 79; a Swallow's Terrace, W. Warde Fowler, 80, 318; Robert H. Read, 176; the Australian Curlew, Colonel Legge, 89; the Myology of the Raven, R. W. Shufeldt, 101; Pectination, E. B. Titchener, 103, 248, 368; H. R. Davies, 367; the Flight of Larks, Alfred W. Bennett, 248; Note on the Disappearance of the Moa, Prof. H. O. Forbes, 105; Ornithology of British New Guinea, 114; the Coming International Ornithological Congress, 130; the Fish-eating Birds of America, W. Palmer, 132; Robin's Nest in a Flat Hand-basket, Nath. Waterall, 163; Nests of the Red-backed Shrike, Robert H. Read, 176; Birds' Nests, E. J. Lowe, F.R.S., 199; Fight between Cock Wrens, Aubrey Edwards, 209; a Remarkable Flight of Birds, Rev. E. C. Spicer, 222; on the Flight of Oceanic Birds, Captain David Wilson-Barker, 223; the Flight of Larks, Alfred W. Bennett, 248; the Fauna of British India, including Ceylon and Burma, R. Bowdler Sharpe, 266; Thousands of Sea-gulls in Edinburgh Botanic Garden, 278; the Crowing of the Jungle Cock, Prof. Henry O. Forbes, 295; A. D. Bartlett, 319; F. J. Jackson's African Collection of Birds, 301; American Ornithologists' Union, 301; Widgeon with Hanging Feather Tassel, J. E. Harting, 311; the Birds of Norfolk, Henry Stevenson, 313; on the Flight of Oceanic Birds, John R. Spears, 319; a Rare Visitor (Water-rail), O. A. Shrubsole, 319; Starlings Eaten by Rooks, 326; Stoliczka's Collection of Yarkand Birds, 327; Ornithological Fauna of the Amu-Daria Region, M. Zarudnoi, 334; on the Affinities of Hesperornis, Dr. F. Helm, 368; the Intelligence of the Thrush, Rev. John Hoskyns-Abrahall

383 ; Coming International Ornithological Congress at Buda-pest, 472, 542 ; on the Morphology of the Duck and Auk Tribes, W. Kitchen Parker, F.R.S., 486 ; Birds of Sussex, 497 ; Lord Lilford's Birds of the British Isles, 497 ; Cackling of Hens, Prof. Geo. J. Romanes, F.R.S., 516 ; V. Ball, F.R.S., 583 ; Cocks and Hens, E. J. Lowe, F.R.S., 583 ; Acquisition of a Greater Bird of Paradise by the Calcutta Zoological Gardens, 543 ; Proposed Attempt to Acclimatize in Bombay Game Birds Common in other Parts of India, 543 ; Antipathy of Birds for Colour, 558 ; William White, 608
Osborn (Prof. H. F.), Evolution and Heredity, 458
Osborn (Prof. H. F.) and Prof. W. B. Scott, on the Fossil Mammals of North America, 177
Oschansk, the Meteorite of, 228
Osmium, Final Results of Prof. Seubert's Re-determination of Atomic Weight of, 427
Osmond (F.), Transformations accompanying Carburation of Iron by Diamond, 504
Ostwald (W.), Magnetic Rotation, 454
Otago University Museum, Notes from the, Prof. T. Jeffery Parker, F.R.S., 141
Ovine Pathology : a Treatise on the Diseases of the Sheep, John Henry Steel, 78
Oxford Summer Meeting, Geological Work during the last, 279
Oxford University Extension Delegacy, Report of, 355
Oxygen, Colour and Absorption Spectrum of Liquid Oxygen, M. Olszewski, 498
Oyster-Culture in Experimental Fishpond of Roscoff Laboratory, De Lacaze-Duthiers, 456

Pacific Ocean, the Depths of the, Admiral Belknap, 183
Paddington and South Kensington Subway Railway, 217, 246
Paget (Sir James, V.P.R.S.) : Louis Pasteur, 481 ; Studies of Old Case-books, 606
Painter (Rev. A. F.), the Hill Arrians of India, 212
Palæobotany : Organization of Fossil Plants of Coal Measures, xvii., Prof. W. C. Williamson, F.R.S., 454
Palæolithic Implements, Notable, Worthington G. Smith, 345
Palæolithics : Discovery of Prehistoric Workshops in Vaucluse, M. Rousset, 183
Palæontology : New Fossil Mammalia Discovered at Samos by Prof. C. J. Forsyth-Major, 85 ; the Palæozoic Fishes of North America, Prof. John Strong Newberry, 146 ; on the Coal and Plant-bearing Beds of Palæozic and Mesozoic Age in Eastern Australia and Tasmania, B. O. Feistmantel, 148 ; Fossil Mammals of North America, Profs. W. B. Scott and H. F. Osborn, 177 ; Generic Identity of Sceparnodon and Phascolonus, R. Lydekker, 214 ; Skeleton of Brachycephalic Celt, Worthington G. Smith, 319 ; the Mont Dol Elephants, M. Sirodot, 408 ; K. v. Zittel's Reptiles, 420, 440 ; Great Find of Elephants' Remains at Mont Dol, Brittany, 473 ; Catalogue of the Fossil Fishes in the British Museum, Part II., Arthur Smith Woodward, Prof. E. Ray Lankester, F.R.S., 577
Palæozoic Fishes of North America, Prof. John Strong Newberry, 146
Palæozoic and Mesozoic Age in Eastern Australia and Tasmania, the Coal and Plant-bearing Beds of, B. O. Feistmantel, 148
Palawan and the Philippines, Wild Swine of the, A. H. Everett, 416
Palermo, Proposed Exhibition at, 301
Palestine Exploration Fund : Removal of the Siloam Inscription, 280 ; Quarterly Statement of, 619
Palisa (Dr.), Names of Asteroids, 379
Palladium, Crystals of Platinum and, J. Joly, 541
Palmaer (Dr.), New Series of Well-Crystallizing Salts of Iridium-Ammonium, 280
Palmer (W.), the Fish-eating Birds of America, 132
Panther, Exceptional Mode of Hunting adopted by, J. M. Coode, 132
Parallaxes of Nebulæ, Prof. Holden on the, 65
Paris Academy of Sciences, 24, 48, 72, 95, 120, 144, 168, 192, 216, 239, 264, 288, 311, 336, 360, 384, 408, 431, 456, 480, 504, 528, 552, 576, 599, 624
Paris-London Telephone, 475
Paris, Proposed International Colonial Exhibition at, 542
Parker (Prof. T. Jeffery, F.R.S.) : the Anatomy and Development of Apteryx, 17 ; Notes from the Otago University Museum : X. Three Suggestions in Biological Terminology, 141 ; XI. the Presence of a Sternum in *Notidanus indicus*, 142, 516
Parker (W. Kitchen, F.R.S.), on the Morphology of the Duck and Auk Tribes, 486
Parry (John), the Chemistry of Iron and Steel Making, W. Mattieu Williams, 50
Partridge (E. A.), Preparation of Pure Metallic Cadmium by Distillation *in vacuo*, 44
Pascoe (Francis P.), the Darwinian Theory of the Origin of Species, Prof. R. Meldola, F.R.S., 409
Passy (Frederic), the Case for Cremation, 231
Pasteur (J. D.), Illusion of Woodpeckers and Bears with Respect to Telegraph Poles, 184
Pasteur (Louis), Sir James Paget, V.P.R.S., 481
Patent Law, English, Lewis Edmunds and A. Wood Renton, 53
Pathology : Conditions which Modify Virulence of Tubercle Bacillus, A. Ransome, M.D., F.R.S., 286 ; Toxicity of Soluble Products of Tuberculous Cultures, Héricourt and Richet, 504
Patten (Dr. W.), on the Origin of Vertebrates from Arachnids, 70
Pearson (Prof Karl) : the Applications of Geometry to Practical Life, 273 ; the Flying to Pieces of a Whirling Ring, 488
Peary (Lieutenant Robert, U.S.N.), Projected Arctic Expedition by, 619
Peck (W.), a Hand-book and Atlas of Astronomy, 414
Peckham (Mr. and Mrs.), on the Mental Powers of Spiders, 40
Pectination, E. B. Titchener, 103, 248, 368 ; H. R. Davies, 367
Pender (Captain Daniel), Death of, 472
Penning (W. H.), Geology of Southern Transvaal, 455
Perihelia of Comets, Dr. Holetschek, 233
Perkin (F. M.), Derivatives of Piperonyl, 478
Perkin (W. H., F.R.S.), Magnetic Rotation of Saline Solutions, 261
Perkin (Prof. W. H., Jun., F.R.S.) : Berberine, 335 ; Action of Reducing Agents on αα'-Diacetylpentane, Synthesis of Dimethyldihydroxyheptamethylene, 477 ; Acetylcarbinol, 550 ; Action of Ethyl Dichloracetate on Sodium Derivative of Ethyl Malonate, 550 ; Benzoylacetic Acid and Derivatives, 550 ; Syntheses with Aid of Ethyl Pentanetetracarboxylate, 550
Permian Vertebrata of Bohemia, Dr. Anton Fritsch, 293
Perrot (F. L.), Refraction and Dispersion in Isomorphous Series of Two-axial Crystals, 216
Perrotin (M.), on the Rotation of Venus, 22
Perry (Prof. John, F.R.S.) : Tables of Spherical Harmonics, 118 ; Spinning Tops, 365
Persia : the Earthquake of June 1890 in, Dr. Jellisew, 42 ; North-Eastern, and Western Afghanistan, J. E. T. Aitchison, F.R.S., 174
Persian Tobacco, 591
Perspective, Photographic, and the Use of Enlargement, A. Mallock, 517
Petrie (Mr.), a Pyramid Temple Discovered at Maydoom, Egypt, by, 591
Petroleum Flames in Various Azimuths, Illuminating Power of Flat, A. M. Mayer, 309
Petrology : the Granites of Leinster, Prof. W. J. Sollas, F.R.S., 412
Phenological Observations, 93
Phi Beta Kappa Society and the Four Hundredth Anniversary of the Discovery of America, 62
Philadelphia, Franklin Institute, 216
Philology : Dictionary of the Language of the Micmac Indians, Rev. Dr. S. T. Rand, 102 ; Formation of Language, W. J. Stillman, 491 ; Bistratification in Modern Greek, Prof. Blackie, 527
Philosophical Basis of Evolution, James Croll, F.R.S., 434
Philosophy, the Prevailing Types of, can they Logically reach Reality ?, Dr. James McCosh, 531
Philosophy of Surgery, Sir James Paget, 606
Phonophore, an Explanation of the, and more especially of the Simplex Phonophore Telegraph, C. Langdon-Davies, 531
Phosphograms, W. Ainslie Hollis, 532
Phosphorescent Light of Insects, the, Prof. Langley, 88
Phosphorescence of Minerals under Light and Heat, H. Becquerel, 504
Photo-electricity : Prof. G. M. Minchin, 334 ; Experiments in illustration of Prof. Minchin's paper on, 407

Photography: Handbuch der Photographie, Prof. Dr. H. W. Vogel, Prof. R. Meldola, F.R.S., 3; Geological Photographs Committee, 42; Mr. Mayall and the New Objective of 1·6 N.A., an Explanation, 47; a Photo-chronographic Apparatus to analyze all kinds of Motion, M. Marey, 48; Wilh. Kopske on Retouching, 55; Photographs of Meteorological Phenomena, Arthur W. Clayden, 55; Photographic Society's New Premises, 108; the Photography of Solar Prominences, G. E. Hale, 133; the Watkins Exposure Meter, 164; the proposed Photographic Institute, 208; Opening of Photomicrographic Laboratory at Rimini by Count R. Sernagiotto, 230; Photometric Observations of Sun and Sky, Wm. Brennand, 237; Stereoscopic Astronomy, 269; Photographic Chart of the Heavens, 276, 540, 568, 615; Photographer's Diary and Desk-book, 326; Photography of Colours, G. Lippmann, 366; Journal of the Camera Club, 426; a Simplified Photocollographic Process, Leon Warnerke, 450; Photographic Spectrum of the Sun and Elements, Prof. Rowland, 452; Photographic Chemistry, Prof. Meldola, F.R.S., 473; Photographic Perspective and the Use of Enlargement, A. Mallock, 517; the Electric Light for Photographic Self-Registering Instruments, Dr. H. Wild, 519; Photographic Conference of the Camera Club, Fifth, 519

Physics: Properties of Matter, Prof. P. G. Tait, 78; the Determination of the Work done upon the Cores of Iron in Electrical Apparatus subject to Alternating Currents, T. H. Blakesley, 116; Physical Society, 118, 190, 334, 406, 478, 501, 548, 623; the Researches of Dr. R. Kœnig on the Physical Basis of Musical Sounds, Prof. Silvanus P. Thompson, 199, 224, 249; Chemical Action and the Conservation of Energy, Geo. N. Huntly, 246; John Romanes on Various Questions of Cosmical Physics, 301; Soap-Bubbles, Prof. C. V. Boys, F.R.S., 317; Hand-Dictionary of Physicists and Chemists, Carl Schaedler, 414; Prof. Van der Waals on the Continuity of the Liquid and Gaseous States, Prof. J. T. Bottomley, F.R.S., 415; Prof. A. W. Rücker, F.R.S., 437; Robert E. Baynes, 488; Physical and Astronomical Geography for Science Students, R. A. Gregory, 459; the Flying to Pieces of a Whirling Ring, Prof. Oliver J. Lodge, F.R.S., 439, 461, 533; Prof. A. G. Greenhill, F.R.S., 461; Prof. J. A. Ewing, F.R.S., 462; Prof. C. V. Boys, F.R.S., 463; Prof. A. M. Worthington, 463; G. H. Bryan, 463; Prof. Karl Pearson, 488; Deductions from the Gaseous Theory of Solution, Prof. Spencer U. Pickering, 488; an Explanation of the Phonopore, and more especially of the Simplex Phonopore Telegraph, C. Langdon-Davies, 531; Crystals of Platinum and Palladium, J. Joly, 541; Force and Determinism, Prof. C. Lloyd Morgan, 558; the Virial, with special reference to Isothermals of Carbonic Acid, Prof. Tait, 575; Physikalische Krystallographie, Dr. M. Liebisch, 126; Experimental, Compressibility of Mixed Air and Carbon Dioxide, Ulysse Lala, 144; Determinations of Heat Capacity and Fusion Heat of Substances to test Validity of Person's Absolute Zero, S. U. Pickering, 214; Beats in Vibration of a Revolving Cylinder or Bell and their bearing on Theory of Thin Elastic Shells, G. H. Bryan, 215; Waves caused by Explosion, &c., M. Berthelot, 264; Experiments on Mechanical Action on Rocks of Gases at High Temperatures and Pressures and in Rapid Motion, M. Daubrée, 311; Apparatus for Determining Acceleration due to Gravity, Prof. F. R. Barrell, 335; Resistance of Various Gases to Movement of Pendulum, Commandant Defforges, 408; Compressibility of Mixtures of Air and Hydrogen, Ulysse Lala, 432; Surface Tension, the Right Hon. Lord Rayleigh, F.R.S., Miss Agnes Pockels, 437; Velocity of Evaporation of Boiling Liquids, P. de Heen, 456; Method of Determining Density of Liquid in Small Quantities, Dr. Haycraft, 479; Method of Determining Critical Temperatures and Pressures of Water, Cailletet and Colardeau, 504; Graphical Method of Determining Relative Values of Force of Gravity in Various Places, Alphonse Berget, 504; Wax Floats in Water above 18° C., Herr Kleinstrick, 520; Variation of Surface-tension with Temperature, Prof. A. L. Selby, 549

Physiology: the Origin of Right- or Left-handedness, Prof. J. M. Baldwin, 43; Dr. Romanes on Physiological Selection, Dr. Alfred R. Wallace, 79; Dr. Geo. J. Romanes, F.R.S., 127, 150, 197; the Homology between Genital Ducts and Nephridia in the Oligochæta, F. E. Beddard, 116; the Patterns in Thumb and Finger Marks, Fras. Galton, F.R.S., 117; Relation between Size of Animals and Size and Number of their Sense-Organs, H. H. Brindley, 119; Comparative Alkalinity of Blood of Vertebrata, René Drouin, 144; Development of Adenoid Tissue, Dr. Gulland, 239; the Minute Structure of Striped Muscle, J. B. Haycraft, 286; a Simple Mode of Demonstrating how Form of Thorax is Determined by Gravitation, Prof. T. P. A. Stuart, 406; Development of Oviduct in Frog, E. W. MacBride, 407; Physiology and Histology of Granite, Prof. W. J. Sollas, F.R.S., 412; Infectious Diseases, their Nature, Cause, and Mode of Spread, Dr. E. Klein, F.R.S., 416, 443; the Mammalian Nervous System, Francis Gotch and Victor Horsley, F.R.S., 428; Influence of Exterior Temperature on Heat-production in Warm-blooded Animals, G. Ausiaux, 432; New Form of Excretory Organ in Oligochætous Annelid, F. E. Beddard, 477; Vegetable, "Attractive Spheres" in Vegetable Cells, Léon Guignard, 480; Murché's Text-book of Physiology, 497; Dentine Physiology, J. H. Mummery, 501; Glycolytic Power of Human Blood, Lepine and Barral, 528; Transformation *in vitro* of Lymphatic Cells into Clasmatocytes, L. Ranvier, 576; the Action of the Nerves of the Batrachian Heart in Relation to Temperature and Endocardiac Pressure, G. N. Stewart, 548, 558; the Influence of Temperature on the Vagus, Dr. T. Lauder Brunton, F.R.S., 558; Co-adaptation and Free Intercrossing, Prof. George J. Romanes, 582

Pickering (Prof. Edward C.): Aids to Astronomical Research, 105; Stars having Peculiar Spectra, 280, 545

Pickering (Prof. Spencer U., F.R.S.): Chemical Action and the Conservation of Energy, 165; Determination of Heat Capacity and Fusion Heat of Substances to test Validity of Person's Absolute Zero, 214; Deductions from the Gaseous Theory of Solution, 488; the Theory of Dissociation into Ions, 548

Pigeons, Our Fancy, George Ure, 79

Pilcomayo Expedition, the, 107

Pilot Chart for North Atlantic, Monck, 592

Pinches (T. J.), the Newly-discovered Assyrian Version of the Story of the Creation, 163

Pinkerton (R. H.) Elementary Text-book of Trigonometry, 7

Pinks of Central Europe, F. N. Williams, 149

Pinsk Marshes, Canalization of, 164

Pisciculture: Loch Leven Trout in the Thames, 327; Oyster Culture in Experimental Fish-pond of Roscoff Laboratory, De Lacaze-Duthiers, 456

Pivot Bearing, Friction of a, 499

Place's (Captain de) Sciseophone, 279

Planet or New Star?, Dr. Lescarbault, 303

Plants, the Effect of Fog on, Dr. D. H. Scott, 129

Plants from Upper Burma and the Shan States, on a Collection of, Brigadier-General H. Collett and W. Botting Hemsley, F.R.S., 386

Platinum and Palladium, Crystals of, J. Joly, 541

Playfair (Sir Lyon, F.R.S.): the Scientific Work of Joule, 112; Chemical Society's Jubilee, 491

Plummer (John I.): Araucaria Cones, 29; the Distances of the Stars, 104

Pockels (Miss Agnes), Surface Tension, the Right Hon. Lord Rayleigh, F.R.S., 437

Poison Apparatus of the Heloderma, Dr. R. W. Shufeldt, 514

Poison of the Malay Peninsula, the Ipoh, 377

Political Economy, the Scope and Method of, John Neville Keynes, 387

Porpoise Fishery, the Hatteras, F. W. True, 497

Porro (Signor), Turin Observatory, 257

Porta and Rigo (MM.), Spanish Botanical Expedition, 377

Potanin (Herr), Skin of *Æluropus melanoleucus* procured by, 355

Potassium Salts in Sea-Water, Dr. T. Sterry Hunt, F.R.S., 463

Potato Disease, W. Carruthers, F.R.S., on, 474

Potato, Distillation of Alcohol from the, Aimé Girard, 120

Potts (W. A.) and W. L. Sargant, Elementary Algebra, 28

Powell (J. W.), Ninth Annual Report of the United States Geological Survey to the Secretary of the Interior, 1887–88, Prof. T. G. Bonney, F.R.S., 509

Practical Life, the Applications of Geometry to, Prof. Karl Pearson, 273

Prague Botanic Garden, Weather Damage to, 42

Prain (Dr.), Naturalized Flora of Andaman Islands since 1858, 398

Pratt (A. E.), Exploration of South-West China, 570

Prediction, on Tidal, Prof. G. H. Darwin, F.R.S., 609

Prentice (B.), Syntheses with Aid of Ethyl Pentanetetracarboxylate, 550
Pressure-Gauge, the Bursting of a, Frederick J. Smith, 318; Newton and Co., 366
Preston (Thomas), the Theory of Light, 53
Prestwich (Prof. Joseph, F.R.S.), the Darent Valley, 359
Priestley, Cavendish, and Lavoisier, Prof. T. E. Thorpe, F.R.S., 1
Primitiæ Fl. Shan, W. Botting Hemsley, F.R.S., and Brigadier-General H. Collett, 386
Primitive Folk, Elie Reclus, 580
Pringle (C. P.), Return from Mexico of, 377
Prisms for the Optical Lantern, on Erecting, Prof. S. P. Thompson, 623
Pritchard (Prof.), the Nebula near Merope, 546
Proceedings of the Royal Society of Canada, 44
Prodomus of the Zoology of Victoria, Sir Frederick McCoy, 389
Properties of Matter, Prof. P. G. Tait, 78
Protective Resemblance in an African Butterfly, an Assumed Instance of Compound, W. L. Distant, 390
Psychological Laboratory at University College, Toronto, the, Baldwin, 88
Psychology: Hand-book of, Prof. J. M. Baldwin, 100; a New Psychology, Rev. Dr. George Jamieson, 100; Suggested Records of certain Facts of Child Development, Prof. E. A. Kirkpatrick, 163; Infant, Prof. J. M. Baldwin, 256; Modern Biology and Psychology, C. Lloyd Morgan, Dr. Alfred R. Wallace, 337; Outlines of Physiological Psychology, a Text-book of Mental Science for Academies and Colleges, George Trumbull Ladd, 506; the Principles of Psychology, William James, Prof. C. Lloyd Morgan, 506; Outlines of Psychology, Prof. Harald Höffding, Translated by Mary E. Lowndes, 553
Ptolemaic Geography of Africa, 418
Public Schools, Museums for, Alexander Meek, 180
Puiseux and Lœwy (MM.), Determination of the Constant of Aberration, 498
Pullinger (Frank), Vapour Density of Ammonium Chloride, 454
Punctaria, Prof. T. Johnson, 119
Puschin (N. L.), Death of, 398
Putnam (Prof. F. W.), on Man and the Mammoth in America, 63

Quarterly Journal of Microscopical Science, 70, 333
Quaternions in the Algebra of Vectors, on the Rôle of, Prof. J. Willard Gibbs, 511; Prof. P. G. Tait, 535, 608
Quatrefages (M. de), Presentation to, 207
Queen's College, Birmingham, Medical Faculty of, Removal of, 542
Queensland: Floods from Excessive Rainfall in, 226; Proposed Marine Biological Station in, W. Saville-Kent, 255
Quincié, Fluor-spar of, 72

Races, Multiple Origin of, W. T. Thiselton Dyer, F.R.S., 535
Raffles (W. Hargreaves), the Darkness of London Air, 152
Railway Guide, an American Geological, James Macfarlane, 8
Railway, the Proposed South Kensington and Paddington Subway, 145; Electric Locomotive Power a New Peril for Scientific Teaching Institutions, 299
Railway from St. Petersburg to Archangel, Proposed Electric, 163
Rain, the Diurnal Probability of, Prof. Busin, 20
Rainbows on Scum, T. W. Backhouse, 416
Rainfall, Influence of Height of Rain-Gauges on Records of, Dr. Hellmann, 520
Rainfall of Kent and Surrey, 63
Rain-Gauges, History of, G. J. Symons, F.R.S., 504
Rain-Gauges, Influence on Rainfall Records of Height of, Dr. Hellmann, 520
Ralfs (John), Proposed Memorial to, 230
Ramsay (Prof. William, F.R.S.): Elementary Systematic Chemistry for the use of Schools and Colleges, 364; Elementary Systematic Chemistry, 391; Solutions, 589
Rand (Rev. Dr. S. T.), Dictionary of the Language of the Micmac Indians, 102
Ransome (A., M.D., F.R.S.), Conditions which Modify Virulence of Tubercle Bacillus, 286
Ranvier (L.), Transformation *in vitro* of Lymphatic Cells into Clasmatocytes, 576
Rat, the Irish, Clarke and Barrett-Hamilton, 255
Raven, the Myology of the, R. W. Shufeldt, 101
Rayleigh (Right Hon. Lord, F.R.S.): the Scientific Papers of James Clerk Maxwell, F.R.S., 26; Award of the De Morgan Medal to, 83; Surface Tension, Miss Agnes Pockels, 437
Read (Robert H.): a Swallow's Terrace, 176; Nests of the Red-backed Shrike, 176
Reale Istituto Lombardo, 501
Reale Istituto Veneto, 619
Recession of the Niagara Gorge, Prof. Edward G. Bourne, 515
Reclus (Elie), Primitive Folk, 580
Records of 1890 Meeting of Russian Naturalists and Physicians at St. Petersburg, 231
Redin (M.), Self-recording Barometer, 255
Reinwald (M. C.), Death of, 398
Relative Numbers for 1890, Wolf's, 400
Relativity of Knowledge, 531
Renard (Adolphe), Trithionyl, 264
Renton (A. Wood) and Lewis Edmunds, the Law and Practice of Letters Patent for Inventions, 53
Reptilia and Batrachia, George A. Boulenger, O. Boettger, 361
Reptiles of British New Guinea, 115
Reptiles, K. v. Zittel's Handbuch der Palæontology, 420
Respiration: Different Volumes of Air respired by Different Persons, Dr. Marcet, 451
Reusch (Dr. Hans), Glacial Striæ and Morainic Gravel in Norwegian Lapland far older than the Ice Age, 106

REVIEWS *and* OUR BOOK SHELF:—
  Priestley, Cavendish, and Lavoisier, Prof. T. E. Thorpe, F.R.S., 1
  Hand-book of Photography, Prof. R. Meldola, F.R.S., 3
  Contributions to Indian Botany, W. Botting Hemsley, 6
  Elementary Text-book of Trigonometry, R. H. Pinkerton, 7
  Higher Geometry, W. J. Macdonald, 8
  Nautical Surveying, Vice Admiral Shortland, 8
  American Geological Railway Guide, James Macfarlane, 8
  Scientific Papers of James Clerk-Maxwell, 26
  Sap: does it Rise from the Roots?, J. A. Reeves, 27
  Hand-book of Games, 28
  Elementary Algebra, W. A. Potts and W. L. Sargant, 28
  Heat and Light Problems, R. Wallace Stewart, 28
  Annalen des k.k. naturhistorischen Hofmuseums, Wien, 28
  Exercises in Practical Chemistry, A. D. Hall, 29
  Elementary Geography of India, Burma, and Ceylon, Henry F. Blanford, F.R.S., 29
  Chemistry of Iron and Steel Making, W. Mattieu Williams, John Parry, 50
  Theory of Light, Thomas Preston, 53
  Law and Practice of Letters Patent for Inventions, &c., Lewis Edmunds and A. Wood Renton, 53
  Lessons on Health, Arthur Newsholme, Dr. J. H. E. Brock, 54
  Practical Inorganic Chemistry, E. J. Cox, 54
  Notes on Trigonometry and Logarithms, Rev. J. M. Eustace, 55
  Elementary Statics, Rev. J. B. Lock, 55
  Die photographische Retouche in ihrem ganzen Umfange, Wilh. Kopske, 55
  American Spiders and their Spinning Work, Henry C. McCook, 74
  Elementary Treatise on Hydrodynamics and Sound, A. B. Basset, 75
  Anatomy, Physiology, Morphology, and Development of the Blow-fly (*Calliphora erythrocephala*), B. Thompson Lowne, 77
  Treatise on the Diseases of the Sheep, being a Manual of Ovine Pathology especially adapted for the Use of Veterinary Practitioners and Students, John Henry Steel, 78
  Wild Beasts and their Ways, Sir Samuel W. Baker, F.R.S., 78
  Properties of Matter, P. G. Tait, 78
  Our Fancy Pigeons, George Ure, 79
  Alexis and his Flowers, Beatrix F. Cresswell, 79
  Tycho Brahe: a Picture of Scientific Life and Work in the Sixteenth Century, J. L. E. Dreyer, A. M. Clerke, 98
  New Psychology: an Aim at Universal Science, Rev. George Jamieson, D.D., 100

Hand-book of Psychology: Senses and Intellect, Prof. J. M. Baldwin, 100
Myology of the Raven (*Corvus corax sinuatus*): a Guide to the Study of the Muscular System in Birds, R. W. Shufeldt, 101
Chemical Arithmetic, W. Dittmar, LL.D., 102
Dictionary of the Language of the Micmac Indians, Rev. Silas Tertius Rand, LL.D., 102
Elementary Manual of Magnetism and Electricity, Andrew Jamieson, 102
Stereoscopic Manual, W. I. Chadwick, 103
Astronomy: Sun, Moon, and Stars, &c., William Durham, 103
The Life Story of our Earth, N. D'Anvers, 103
The Story of Early Man, N. D'Anvers, 103
The Steam-engine considered as a Thermo-dynamic Engine, Prof. James H. Cotterill, F.R.S., 123
Zoological Types and Classification, W. E. Fothergill, 124
A Zoological Pocket-book, or Synopsis of Animal Classification, 124
The Hand-book of Folk-Lore, G. L. Gomme, 125
Physikalische Krystallographie, Dr. M. Liebisch, 126
Anleitung zur Darstellung chemischer Präparate: ein Leitfaden für den praktischen Unterricht in der anorganischen Chemie, Dr. Hugo Erdmann, 126
Analysis of a Simple Salt, William Briggs and R. W. Stewart, 126
Magnetism and Electricity, J. Spencer, 127
The Elements of Euclid, Books I. and II., Horace Deighton, 127
Palæozoic Fishes of North America, John Strong Newberry, 146
Hand-book for Mechanical Engineers, Prof. Henry Adams, 147
On the Coal and Plant-bearing Beds of Palæozoic and Mesozoic Age in Eastern Australia and Tasmania, with Special Reference to the Fossil Flora, B. O. Feistmantel, J. S. Gardner, 148
The Development of Africa, Arthur Silva White, 149
The Pinks of Central Europe, F. N. Williams, 149
Materials for a Flora of the Malayan Peninsula, Dr. G. King, F.R.S., 149
Colonist's Medical Hand-book, E. A. Barton, 150
The System of the Stars, Agnes M. Clerke, 169
The First Crossing of Greenland, Fridtjof Nansen, 172
Les Microbes de la Bouche, Dr. Th. David, 174
Notes on the Products of Western Afghanistan and North-Eastern Persia, J. E. T. Aitchison, F.R.S., 174
Metal-turning, 175
The Century Arithmetic (complete), 175
A Treatise on the Common Sole (*Solea vulgaris*) considered both as an Organism and as a Commodity, J. T. Cunningham, Prof. W. A. Herdman, 193
Exercises in Wood-working, with a Short Treatise on Wood, written for Manual Training Classes in Schools and Colleges, Ivin Sickels, 195
Les Bactéries et leur rôle dans l'Étiologie, l'Anatomie, et l'Histologie pathologiques des Maladies Infectieuses, A. V. Cornil and V. Babes, 195
Verslag omtrent den Staat van S' Lands Plantentuin te Buitenzorg en de daarbij behoorende Inrichtingen over het Jaar 1889, 196
The Electric Light, A. Bromley Holmes, 196
Maps and Map-drawing, William A. Elderton, 196
Krystallographisch-chemische Tabellen, Dr. A. Fock, A. E. Tutton, 197
Are the Effects of Use and Disuse Inherited, William Platt Ball, Prof. Geo. J. Romanes, F.R.S., 217
Manual Training in Education, C. M. Woodward, 220
Monographie der baltischen Bernsteinbäume: Vergleichende Untersuchungen über die Vegetationsorgane und Blüthen, sowie über das Harz und die Krankheiten der baltischen Bernsteinbäume, H. Conwentz, 221
The Birth and Growth of Worlds, A. H. Green, F.R.S., 221
Chambers's Encyclopædia, 221
Class-book of Geology, Archibald Geikie, F.R.S., Prof. A. H. Green, F.R.S., 242
Elementary Geology, Charles Bird, Prof. A. H. Green, F.R.S., 242
The Art of Electrolytic Separation of Metals, Theoretical and Practical, G. Gore, F.R.S., 244
A Treatise on Electro-Metallurgy, W. G. McMillan, 244
Leçons sur l'Électricité professeés à l'Institut Électro-technique Montefiore annexé à l'Université de Liége, Eric Gerard, 245
Fathers of Biology, Charles McRae, 245
Through Magic Mirrors, Arabella B. Buckley, 246
The Fauna of British India, including Ceylon and Burma, Eugene W. Oates, R. Bowdler Sharpe, 266
A Manual of Public Health, A. Wynter Blyth, Dr. J. H. E. Brock, 267
Lehrbuch der Zoologie für Studirende und Lehrer, Dr. J. E. V. Boas, 268
A Pocket-book of Electrical Rules and Tables, John Munro and Andrew Jamieson, 268
Three Years in Western China: a Narrative of Three Journeys in Szechuan, Kweichow, and Yunnan, Alexander Hosie, 290
Die Gattung Stelletta, F. E. Schulze, Dr. R. von Lendenfeld, 292
Die Theorieen über die Entstehung der Koralleninseln und Korallenriffe und ihre Bedeutung für geophysische Fragen, Dr. R. Langenbeck, 293
Fauna der Gaskohle und der Kalksteine der Permformation Böhmens, Dr. Anton Fritsch, 293
The Life of Ferdinand Magellan, F. H. H. Guillemard, 294
Key to Arithmetic in Theory and Practice, J. Brooksmith, 294
The Birds of Norfolk, with Remarks on their Habits, Migration, and Local Distribution, Henry Stevenson, 313
The Threshold of Science, C. B. Alder Wright, F.R.S., Prof. Wyndham R. Dunstan, 314
Zoological Record for 1889, 316
Reports on the Mining Industry in New Zealand, 316
Soap-bubbles, C. V. Boys, F.R.S., 317
Astronomical Lessons, John Ellard Gore, 317
The Canary Islands, John Whitford, 317
Gesammelte Abhandlungen, H. A. Schwartz, Prof. O. Henrici, F.R.S., 321, 349
Animal Life and Intelligence, C. Lloyd Morgan, Dr. Alfred R. Wallace, 337
The Lake-dwellings of Europe: being the Rhind Lectures in Archæology for 1888, Robert Munro, M.D., Prof. W. Boyd Dawkins, F.R.S., 341
The Fauna of British India, including Ceylon and Burma, O. Boettger, 361
Reptilia and Batrachia, George A. Boulenger, O. Boettger, 361
History of Education in Alabama, 1702–1889, Willis G. Clark, 362
Elementary Systematic Chemistry for the Use of Schools and Colleges, William Ramsay, F.R.S., 364
Applied Geography, J. Scott Keltie, 365
Autobiography of the Earth, Rev. H. N. Hutchinson, 365
Spinning Tops, Romance of Science Series, Prof. John Perry, F.R.S., 365
Wild Life on a Tidal Water, P. H. Emerson, 366
Arcana Fairfaxiana, 366
Berge's Complete Natural History, 366
Surveying and Levelling Instruments, William Ford Stanley, 374
Dogs, Jackals, Wolves, and Foxes, a Monograph of the Canidæ, St. George Mivart, F.R.S., 385
On a Collection of Plants from Upper Burma and the Shan States, Brigadier-General H. Collett and W. Botting Hemsley, F.R.S., 386
The Scope and Method of Political Economy, John Neville Keynes, 387
Mixed Metals, or Metallic Alloys, A. H. Hiorns, Prof. W. C. Roberts-Austen, F.R.S., 388
Grasses of the South-West: Plates and Descriptions of the Grasses of the Desert Region of Western Texas, New Mexico, and Southern California, Dr. Geo. Vasey, 389
Prodomus of the Zoology of Victoria, Sir Frederick McCoy, 389
Annals of a Fishing Village, 389
Solutions of the Examples in Elementary Algebra, H. S. Hall and S. R. Knight, 389
British Ferns and where Found, E. J. Lowe, F.R.S., 389

Sculptured Anthropoid Ape Heads found in or near the Valley of the John Day River, a Tributary of the Columbia River, Oregon, James Terry, Dr. Alfred R. Wallace, 396
Œuvres Complètes de Christiaan Huygens, A. M. Clerke, 433
Philosophical Basis of Evolution, James Croll, F.R.S., 434
Natural History of the Animal Kingdom for the Use of Young People, 435
Commercial Botany of the Nineteenth Century, John R. Jackson, 436
Fresenius's Quantitative Analysis, 436
The Design of Structures, S. Auglin, 436
Biological Lectures delivered at the Marine Biological Laboratory of Wood's Holl in the Summer Session of 1890, 457
Elementary Physical and Astronomical Geography, R. A. Gregory, 459
Whences comes Man, from Nature or from God?, Arthur John Bell, 460
Why does Man Exist?, Arthur John Bell, 460
Elementary Botany, J. W. Oliver, 461
Household Hygiene, Mary Taylor Bissell, H. Brock, 461
Lessons in Applied Mechanics, J. H. Cotterill, F.R.S., and J. H. Slade, 461
Lake Bonneville, Grove Karl Gilbert, Prof. T. G. Bonney, F.R.S., 485
On the Morphology of the Duck and Auk Tribes, W. Kitchen Parker, 486
A Dictionary of Metric and other Useful Measures, Latimer Clark, 487
A Text-book of Geometrical Deduction, James Blaikie and W. Thomson, 487
Elementary Algebra, W. W. Rouse Ball, 487
A Ride through Asia Minor and Armenia, Henry C. Barkley 487
Outlines of Physiological Psychology, a Text-book of Mental Science for Academies and Colleges, Geo. Trumbull Ladd, Prof. C. Lloyd Morgan, 506
The Principles of Psychology, William James, Prof. C. Lloyd Morgan, 506
Ninth Annual Report of the United States Geological Survey to the Secretary of the Interior, 1887–88, J. W. Powell, Prof. T. G. Bonney, F.R.S., 509
On the Eggs and Larvæ of some Teleosteans, Ernest W. L. Holt, 510
The Hand-book of Games, 510
Object-Lessons from Nature, a First Book of Science, L. C. Miall, 511
On the Modification of Organisms, David Syme, Dr. Alfred R. Wallace, 529
An Introduction to the Study of Metallurgy, Prof. W. C. Roberts-Austen, F.R.S., Thomas Gibb, 530
The Prevailing Types of Philosophy: Can they Logically reach Reality?, James McCosh, 531
An Explanation of the Phonopore, and more especially of the Simplex Phonopore Telegraph, C. Langdon-Davies, 531
The Naturalist of Cumbrae, being the Life of David Robertson, Rev. T. R. R. Stebbing, 532
By Track and Trail, a Journey through Canada, Edward Roper, 532
An Etymological Dictionary of the German Language, Frederick Kluge, 532
Nature's Wonder-Workers, Kate Lovell, 532
Outlines of Psychology, Prof. Harald Höffding. Translated by Mary E. Lowndes, 553
The Foundations of Geometry, by E. T. Dixon, 554
Optical Projection, by Lewis Wright, 555
Die Denudation in der Wüste und ihre geologische Bedeutung, Untersuchungen über die Bildung der Sediment in den Ägyptischen Wüsten, Johannes Walther, 550
The American Race, by Daniel G. Brinton, 556
Charts of the Constellations, Arthur Cottam, 557
Catalogue of the Fossil Fishes in the British Museum, Natural History, Part II., Arthur Smith Woodward, 577
The Honey-Bee, its Natural History, Anatomy, and Physiology, T. W. Cowan, 578
The Electro-plater's Hand-book, G. E. Bonney, 579
Primitive Folk, Studies in Comparative Ethnology, Élie Reclus, 580
Tongues in Trees and Sermons in Stones, Rev. W. Tuckwell, 581
Supplement to Euclid Revised, R. C. J. Nixon, 581
Life of Philip Henry Gosse, F.R.S., by his Son, Edmund Gosse, 603
A Class-Book on Light, R. E. Steel, 606
Studies of Old Case-Books, Sir James Paget, Bart., 606
Euclid's Elements of Geometry, Books III. and IV., edited by H. M. Taylor, 607
Zoological Articles contributed to the Encyclopædia Britannica by E. Ray Lankester, F.R.S., 607
The British Empire, the Colonies and Dependencies, W. G. Baker, 607

Revillagigedo Islands, the Flora of, W. Botting Hemsley, 471
Reynier (Émile), Death of, 325
Rhind Lectures in Archæology for 1888, Lake-Dwellings of Europe, being the, Robert Munro, Prof. W. Boyd Dawkins, F.R.S., 341
Rhodium, Redetermination of Atomic Weight of, Prof. Seubert and Dr. Kobbé, 356
Richard (Jules), hitherto Unknown Crustaceans in Bois de Boulogne Lakes, 280
Richardson (Dr. A.), Action of Light on Ether in Presence of Oxygen and Water, 261
Richet (Charles), Toxicity of Soluble Products of Tuberculous Cultures, 504
Ridley (H. N.): the Coco-nut Beetle, 476; the Red Ant (Caringa) of the Malay Peninsula, 620
Riecke, Pyro-electricity of Tourmaline, 120
Rigo and Porta's (MM.) Spanish Botanical Expedition, 377
Rigollot (H.), Absorption Spectra of Iodine Solutions, 264
Ring, the Flying to Pieces of a Whirling, Prof. Oliver J. Lodge, 439, 461, 533; Prof. A. G. Greenhill, F.R.S., 461; Prof. J. A. Ewing, F.R.S., 462; Prof. C. V. Boys, F.R.S., 463; Prof. A. M. Worthington, 463; G. H. Bryan, 463
Roberts (Isaac), Variability of the Andromeda Nebula, 379
Roberts-Austen (Prof. W. C., F.R.S.): Mixed Metals or Metallic Alloys, A. H. Hiorns, 388; an Introduction to the Study of Metallurgy, Thomas Gibb, 530
Robertson (David), the Naturalist of Cumbrae, the Life of, Rev. T. R. R. Stebbing, 532
Rock Drills, 499
Rôle of Quarternions in the Algebra of Vectors, Prof. P. G. Tait, 608
Rollers, Prof. Cleveland Abbe on the Atlantic, by Ascension Island, 563
Romanes (Dr. G. J., F.R.S.): on Physiological Selection, Dr. Alfred R. Wallace, 79; Dr. Wallace on Physiological Selection, 127, 150, 197; Are the Effects of Use and Disuse Inherited?, William Platt Ball, 217; Co-adaptation, 490; Co-adaptation and Free Intercrossing, 582; Cackling of Hens, 516; Elected a Member of the Athenæum Club, 619
Romanes (John), on Questions of Cosmical Physics, 301
Rome, the New Commercial Museum at, 497
Roper (Edward), By Track and Trail, a Journey Through Canada, 532
Roscoe's (Sir Henry, F.R.S.) Technical Education Bill, 397; a Treatise on Chemistry, New Edition, 474
Rosse (Earl of, F.R.S.), Measures of Lunar Radiation, 104
Rossiter (E. C.), Chloro- and Bromo-derivatives of Naphthol and Naphthylamine, 503
Rothamsted, Field Experiments at, 189
Rothrock's (Dr.) Biological Expedition to West Indies and Yucatan, 426
Round Games with Cards, Baxter-Wray, 414
Round Island, the Geology of, W. T. Thiselton-Dyer, F.R.S., 253; Prof. John W. Judd, F.R.S., 253
Roux (M.), Relations between Optical Dispersive Power of Alcohols, Ethers, and Fatty Acids, and their Molecular Weight and Constitution, 133; Indicator of Charge of Electrical Accumulators, 356
Rowland (Prof.), Photographic Spectrum of the Sun and Elements, 452
Royal Botanic Society, 88, 162, 450
Royal Geographical Society, 278; Awards of Medals, &c., 566
Royal Institution Lectures, 108, 182, 496, 520, 590
Royal Meteorological Society, 94, 215, 311, 431, 504, 599; Proposed Exhibition of Rain Gauges, &c., 254

Royal Microscopical Society, 47, 254, 360, 526
Royal Society, 42, 116, 142, 189, 214, 237, 261, 358, 406, 430, 454, 477, 501, 525, 572 ; Anniversary Meeting, 61, 107 ; the Anniversary of the, Presidential Address, Sir Geo. Stokes, P.R.S., 133 ; Proposed Presentation to Sir George Gabriel Stokes, 162 ; the Croonian Lecturers, 299
Royal Society of Canada, the Coming Annual Meeting of, 619
Royal Society of New South Wales, 239
Rubies, Artificial, M. Fremy, 432
Rubies and Sapphires in Siam, 164
Rubies, Synthesis of, 72
Rücker (Prof. A. W., F.R.S.) : Prof. Van der Waals on the Continuity of the Liquid and Gaseous States, Prof. J. T. Bottomley, F.R.S., 415, 437 ; Magnetic Anomalies in France, M. Mascart, 617
Rudler (F. W.), Source of Jade used for Ancient Implements in Europe and America, 310
Rugby School Natural History Society, 619
Russell (Captain A.H.), Mechanical Trisection of any Angle, 547
Russell (H. C.), Meteorological Observations in New South Wales during 1888, 377
Russell (T.), Prediction of Cold Wave from Signal Service Weather Maps, 260
Russell's (Dr. W. J., F.R.S.) Address at the Chemical Society's Jubilee, 440
Russia : Proposed Electric Railway from St. Petersburg to Archangel, 163 ; Canalization of Pinsk Marshes, 164 ; Russian Meteorological Review, 183 ; Records of the 1890 Meeting of Russian Naturalists and Physicians at St. Petersburg, 231 ; Krilof's Portraits of Typical Representatives of Russian Races, 255 ; Russian Geographical Society, 354 ; Exploration of Tundras of North-east, Th. H. Tchernysheff, 397 ; a New Russian Scientific Expedition to the Pamir, 591
Rutherford (Prof. W.), the Sense of Hearing, 431

Sabatier (Prof. Armand), Proposed Marine Zoological Station at Cette, 397
Sahara : the Conchological Fauna of the, Dr. P. Fischer, 312 ; Discovery of a Large Body of Water in, 566
St. Louis, Electric Lighting in, 497
St. Pancras Vestry Hall, Electrical Exhibition at, 472
St. Petersburg : Remarkably Warm Winter at, 42 ; Foundation of an Institute for Experimental Medicine at, 208 ; Records of the 1890 Meeting of Russian Naturalists and Physicians at, 231 ; St. Petersburg Academy of Science, 404
Salamanders, Microscopical Study of the Skin of Salamanders, Herr Schultz, 209
Salisbury (Marquis of, F. R. S.), Speech at the Chemical Society's Jubilee, 491
Salt, Analysis of a Simple, William Briggs and R. W. Stewart, 126
Salt Lake (Utah), Past History of the Great, Grove Karl Gilbert, Prof. T. G. Bonney, F.R.S., 485
Samos, New Fossil Mammalia discovered at, 85
Samotherium at the British Museum, 85
Sampson (R. A.), on Stokes's Current Function, 261
Sand, Squeaking *versus* Musical, Prof. H. Carrington Bolton, 30
Sanitary Assurance Association, Retirement of Sir Joseph Fayrer, 354
Sanitary Institute, Transactions of the, 89 ; Lectures of the, 254
Sanitary Science, Sir Edwin Chadwick's Bequests for Advancement of, 130
Sanitation, a Manual of Public Health, A. Wynter Blyth, Dr. J. H. E. Brock, 267
Santillan's (Señor R. A.), Meteorological Bibliography of Mexico, 398
Sap, does it Rise from the Roots?, 27
Sapphires and Rubies in Siam, 164
Sarawak Museum, Appointment of Mr. Edw. Bartlett as Curator of, 472
Sargant (W. L.) and W. A. Potts, Elementary Algebra, 28
Saturn, a New Theory of Jupiter and, G. W. Hill, 357
Saturn and its Ring, Asaph Hall, 65
Savélief (M.), Actinometric Observations at Kief, 1890, 456
Saville-Kent (W.), proposed Marine Biological Station in Queensland, 255
Scacchi (Prof. Arcangelo), Celebration of Fiftieth Anniversary of, 376
Scent-farming in Victoria, 544
Schaeberle on the Solar Corona, 568
Schaedler (Carl), Hand-dictionary of Physicists and Chemists, 414
Schliemann (Dr. Henry) : Obituary Notice of, 227 ; Meeting at Berlin in Honour of his Memory, 425
Schnyver (S. B.), Asymmetry of Nitrogen in Substituted Ammonium Compounds, 550
Schorlemmer (C.), a Treatise on Chemistry, New Edition, 474
Schönrock (Herr), Study of Mean Velocities of Thunderstorms in Russia, 520
Schulz (Herr), Microscopic Study of Skin of Toads and Salamanders, 209
Schwartz (Dr. H. A.), Theory of Functions, Prof. O. Henrici, F.R.S., 321, 349
Science and Art Department Buildings in Exhibition Road, the, 354
Science Collections, the, 590
Science, Durham College of, Newcastle-upon-Tyne, 519
Science Lessons, Elementary, W. Hewett, 414
Science Museum, the, 495
Science Museum and Gallery of British Art at South Kensington, 590, 601
Science, New Year's Honours to Men of, 230
Science in New Zealand, Sir James Hector, F.R.S., 521
Science, St. Petersburg Academy of, 404
Science, Shaking the Foundations of, 145 ; Major-General C. E. Webber, 222
Science, the Threshold of, C. R. Alder Wright, F.R.S., Prof. Wyndham R. Dunstan, 314
Scientific Instruments, Effect of the McKinley Bill on Cost of, 108
SCIENTIFIC WORTHIES : Louis Pasteur, Sir James Paget, V.P.R.S., 481
Sciseophone, Captain de Place's, 279
Scotch Sea Fisheries, Dr. T. W. Fulton on, 108
Scotland, Earthquakes in, 62
Scotland, Secondary Education in, 325
Scott (Dr. D. H.), the Effect of Fog on Plants, 129
Scott (R. H., F.R.S.), Peculiar Effect of Lightning on Eggs, 215
Scott (Thomas), the Scientific Investigations of the Fisheries Board for Scotland, 56
Scott (Prof. W. B.) and Prof. H. F. Osborn, on the Fossil Mammals of North America, 177
Scottish Meteorological Society, 496
Sculptures, Remarkable Ancient, from North-West America, James Terry, Dr. Alfred R. Wallace, 396
Scum, Rainbows on, T. W. Backhouse, 416
Sea : the Use of Oil at, 451 ; Manganese Nodules in Deep Sea, Dr. John Morgan, 287 ; Oceanic and Littoral Manganese Nodules, J. Y. Buchanan, 287 ; Thousands of Sea-Gulls in the Edinburgh Botanic Garden, 278 ; on the Relationships of Sea-Spiders, Prof. T. H. Morgan, 459 ; the Composition of Sea-Water, 199 ; Potassium Salts in Sae-Water, Dr. T. Sterry Hunt, F.R.S., 463
Seaweed : Punctaria, Prof. T. Johnson, 119
Sebastopol, Proposed Marine Station at, 543
See (Dr. T. J. J.), Eccentricities of Stellar Orbits, 379
Seismology : Earthquakes of May 1890, Chili, A. F. Noguès, 24 ; Earthquakes in the North-East of Scotland, 62 ; Vibration-Recorders, Prof. John Milne, F.R.S., and John Macdonald, 154 ; Photography applied to Mechanical Registration of Motions of Seismo-microphone, Signor Baratta, 209 ; Relation of Earth-Tremors to Seasons, M. de Montessus, 456 ; Charleston Earthquake, Captain C. E. Dutton, 509
Selby (Prof. A. L.), Variation of Surface-Tension with Temperature, 549
Seler (Dr.), the Name Anahuac applied by mistake to Plateau of Mexico, 208
Selenium-Cell and Electric Lamp, an Automatic Lamp-Lighter, Shelford Bidwell, F.R.S., 395
Selenka (Emil) and J. R. A. Davis, a Zoological Pocket-book or Synopsis of Animal Classification, 124
Sella (Alfonso), Native Nickel in Sands of Elvo Torrent, Piedmont, 312
Sella (Signor), Distribution of Magnetism in Alpine Regions, 378
Setchell (W. A.), on *Tuomeya fluviatilis*, Harvey, 65
Seubert (Prof.), Redetermination of Atomic Weight of Rhodium, 356 ; Redeterminaion of Atomic Weight of Osmium, 427

Shaking the Foundations of Science, 145 ; Major-General C. E. Webber, 222
Shaler (Prof. N. S.), Glacial Climate, 155
Shan States, Ethnology of, 62
Sharp (D.), Araucaria Cones, 57
Sharpe (R. Bowdler), Fauna of British India, including Ceylon and Burma, 266 ; American Testimonial to, 300
Shaw (J.), Honeycomb Appearance of Water, 30
Shaw (James), Araucaria Cones, 81
Sheep, a Treatise on the Diseases of the, John Henry Steel, 78
Shells, Freshwater, Dispersal of, H. Wallis Kew, 56
Sherwood (William), Mud Glaciers of Cromer, 515
Shipley (A. E.), *Bipalium kewense* in a New Locality, 407
Shortland (Vice-Admiral), Nautical Surveying, 8
Shrike, Red-backed, Nests of the, Robert H. Read, 176
Shrubsole (O. A.), a Rare Visitor, Water-Rail, 319
Shufeldt (Dr. R. W.) : the Myology of the Raven, 101 ; on the Affinities of Hesperornis, 176 ; the Poison Apparatus of the Heloderma, 514
Shunts, a Property of Magnetic, Prof. S. P. Thompson, 623
Siam, Rubies and Sapphires in, 164
Siberia : Successful Opening of Commercial Communication between England and, 88 ; Natural History Museums in, 163 ; Proposed Zoological and Botanical Expedition to, Herr H. Leder, 451
Sibley (Walter K.), on the Incubation of Snakes' Eggs, 68
Sickels (Ivin, M.D.), Wood-working, 195
Sidgreaves (Rev. Walter), Kœnig's Superior Beats, 9
Silver, Allotropic, M. C. Lea, 598
Silver, Gold-coloured Allotropic, M. C. Lea, 500
Sirodot (Mount), the Mount Dol Elephants, 408
Slade (J. N., R.N.) and J. H. Cotterill, F.R.S., Lessons in Applied Mechanics, 461
Smith (B. A.), Newton's Rings, 55
Smith (Prof. C. Michie), the Spectrum of the Zodiacal Light, 22
Smith (Frederick J.), the Bursting of a Pressure-Gauge, 318
Smith (Worthington G.) : Attractive Characters in Fungi, 224 ; Skeleton of Brachycephalic Celt, 319 ; Notable Palæolithic Implement, 345
Smithsonian Institution, 450, 497, 619
Smoke Annihilation, 377
Smoke Nuisance, City Commission of Sewers and the, 182, 426
Snakes, the Fauna of British India, including Ceylon and Burma, 361 ; Reptilia and Batrachia, George A. Boulenger, O. Boettger, 361
Snakes' Eggs, on the Incubation of, Walter K. Sibley, 68
Snellen (Dr. Maurice), appointed Director of Royal Meteorological Institute of Netherlands, 542
Snow on the Branches of Trees, Prof. Samuel Hart, 391
Soap-bubbles, C. V. Boys, F.R.S., 317
Soaring of Birds, 30 ; Prof. F. Guthrie, 8
Société Botanique de France, 473
Society of Arts : Sessional Arrangements, 42 ; Benjamin Shaw Trust, 542
Solar Activity (July–December 1890), Prof. Tacchini, 302
Solar Corona, F. H. Bigelow, 71
Solar Phenomena, Latitudinal Distribution of, P. Tacchini, 360
Solar Prominences, the Photography of, G. E. Hale, 133
Solar Spectrum at Medium and Low Altitudes, Dr. Ludwig Becker, 399
Solar Spectrum, Ultra-violet Limit of, A. Cornu, 216
Sole, the Common, Rev. W. Spotswood Green, 56 ; J. T. Cunningham, 104 ; Considered both as an Organism and a Commodity, J. T. Cunningham, 193
Sollas (Prof. W. J., F.R.S.) : a Method of Determining Specific Gravity, 404 ; the Granites of Leinster, 412
Solution, a Deduction from the Gaseous Theory of, Prof. Orme Masson, 345 ; Prof. Spencer U. Pickering, 488
Solutions, Prof. William Ramsay, F.R.S., 589
Somatic Modifications, the Inheritance of, and the Principle of Lamarck, Prof. Giard, 328
Soot, the Darkness of London Air, George White, 318
Souls, South African Doctrine of, Rev. James Macdonald, 307
Sound and Hydrodynamics, an Elementary Treatise on, A. B. Basset, 75
Sounds, Musical, the Researches of Dr. R. Kœnig on the Physical Basis of, Prof. Silvanus P. Thompson, 199, 224, 249
South African Doctrine of Souls, Rev. James Macdonald, 307
South Indian Ocean, Dr. Meldrum's Atlas of Cyclone Tracks of, 620
South Kensington Natural History Museum, Additions to, 378
South Kensington, the Proposed Underground Railway to, 145, 217 ; Prof. W. H. Flower, F.R.S., 246 ; Alfred W. Bennett, 246
South Kensington, the Science and Art Department Buildings in Exhibition Road, 354
South Kensington, the Science Collections and the Proposed Gallery of British Art at, 590, 601
Sowerby (W.), India-rubber and Gutta-percha, 355
Sparrow Courtship, 63
Spears (John R.) : on the Flight of Oceanic Birds, 319 ; Eskimo Art Works, 464
Specialization and Organization, Prof. C. O. Whitman, 457
Specific Gravity, a Method of Determining, Prof. W. J. Sollas, F.R.S., 404
Spectro-Photometry : Behaviour of Water as to the Passage of Different Light-rays, Hüfner and Albrecht, 520
Spectrum Analysis : the Spectrum of the Zodiacal Light, Prof. C. Michie Smith, 22 ; Variations of Certain Stellar Spectra, Rev. T. E. Espin, 165 ; Greenwich Spectroscopic Observations, 210 ; Spectroscopic Note (D_3), Rev. A. L. Cortie, 210 ; Ultra-violet Limit of Solar Spectrum, A. Cornu, 216 ; on the Orbit of α Virginis, Prof. H. C. Vogel, 235 ; Spectroscopic Observations of Sun-spots, Rev. A. L. Cortie, 256 ; Spectra of Blue and Yellow Chlorophyll, 264 ; Absorption Spectra of Iodine Solutions, H. Rigollot, 264 ; Stars having Peculiar Spectra, Prof. Pickering, 280 ; Solar Spectrum at Medium and Low Altitudes, Dr. Ludwig Becker, 399 ; Change in Absorption Spectrum of Cobalt Glass produced by Heat, Sir John Conroy, Bart., 406 ; Spectrum of α Lyræ, H. Deslandres, 432 ; Photographic Spectrum of the Sun and Elements, Prof. Rowland, 452 ; Colour Absorption Spectrum of Liquid Oxygen, M. Olszewski, 498 ; Phosphorescence of Minerals under Light and Heat, H. Becquerel, 504 ; Bisulphite Compounds of Alizarin-blue and Cœrulin as Sensitizers for Rays of Low Refrangibility, George Higgs, 525 ; Phosphograms, W. Ainslie Hollis, 532 ; Stars having Peculiar Spectra, Prof. E. C. Pickering, Mrs. Fleming, 545 ; Cause of Double Lines in Spectra of Gases, Dr. G. J. Stoney, F.R.S., 551 ; Application of New Method of Investigating Feeble Bands in Banded Spectra to Hydrocarbon Spectra, H. Deslandres, 551
Spencer (J.), Magnetism and Electricity, 127
Spencer (J. W.), Deformation of Iroquois Beach and Birth of Lake Ontario, 260
Spicer (Rev. E. C.), a Remarkable Flight of Birds, 222
Spider, Some Habits of the, 55, 129 ; W. H. Hudson, 151 ; Notes on the Habits of Some Common English Spiders, Prof. C. V. Boys, F.R.S., 40 ; American Spiders and their Spinning Work, Henry C. McCook, D.D., 74 ; *Agelena labyrinthica*, Oviposition of Spider, C. Warburton, 119
Spinning Disks, J. T. Nicolson, 514
Spinning Tops, Prof. John Perry, F.R.S., 365
Sponge, a Boring (*Alectona millari*), Vaughan Jennings, 119
Sponges, Comparative Anatomy of, Arthur Dendy, 333
Sponges : Die Gattung Stelletta, 292
Sprung (Dr.), Effect of Difference of Exposure on Reading of Thermometers, 592
Squeaking Sand *versus* Musical Sand, Prof. H. Carrington Bolton, 30
Squirrels, 451
Squirrels in Cold Weather, P. B. Mason, 544
Squirrels and Corn, C. P. Gillette, 620
Stampe (Oscar), Apex of the Sun's Way, 90
Stanley (A.), Fermentations induced by Friedländer's Pneumococcus, 503
Stanley (William Ford), Surveying and Levelling Instruments, 374
Stapf (Dr. Otto), Obituary Notice of Carl Johann Maximowicz, 449
Starlings Eaten by Rooks, 326
Stars : the Duplicity of α Lyræ, A. Fowler, 64 ; Stars 121 and 483 Birm., 90 ; Orbits of 61 Cygni, Castor, and 70 Ophiuchi, N. M. Mann, 90 ; the System of the Stars, Agnes M. Clerke, 169 ; the Distances of the Stars, John I. Plummer, 104 ; New Asteroids, 111 ; Variations of Certain Stellar Spectra, Rev. T. E. Espin, 165 ; Stars having Peculiar Spectra, Prof. E. C. Pickering, 280, 545 ; Mrs. Fleming, 545 ; Planet or New Star?, Dr. Lescarbault, 303 ; Argelander-Oeltzen Star Catalogue, Dr. E. Weiss, 357 ; Eccentricities of Stellar Orbits, Dr. T. J. J. See, 379 ; New Variable Stars, 428,

590; Photographic Chart of the Stars, 568; the Planet Mercury, 569
Stas (M. Jean Servais), Coming Jubilee of, 397
Statics, Elementary, J. B. Lock, 55
Steam-engine, the, considered as a Thermodynamic Engine, James H. Cotterill, F.R.S., J. Larmor, 123
Steam-yacht, Prince of Monaco's New, 519
Stebbing (Rev. T. R. R.), the Naturalist of Cumbrae, being the Life of David Robertson, 532
Steel (John Henry), a Treatise on the Diseases of the Sheep, 78
Steel (R. E.), a Class-book on Light, 606
Steel and Iron Making, Chemistry of, W. Mattieu Williams, John Parry, 50
Stelletta, Die Gattung, 292
Stenhouse (J.), Benzoylacetic Acid and Derivatives, 550
Stephens (Edward) and the Rev. John Mathew on Australian Aborigines, 185
Stephens (Prof. W. J.), Death of, 230
Stereom, F. A. Bather, 345
Stereoscopic Astronomy, 269; W. Le Conte Stevens, 344
Stereoscopic Manual, W. I. Chadwick, 103
Sternum, the Morphology of the, Prof. G. B. Howes, 269
Sternum, on the Presence of a, in *Notidanus indicus*, Prof. T. Jeffery Parker, F.R.S., 516
Stevens (W. Le Conte), Stereoscopic Astronomy, 344
Stevenson (Henry), the Birds of Norfolk, 313
Stewart (Colonel), the Lake System, &c., of Tabreez, 232
Stewart (Prof. C.), Poisonous Lizards, 383
Stewart (G. N.), the Action of the Nerves of the Batrachian Heart in Relation to Temperature and Endocardiac Pressure, 548, 558
Stewart (R. Wallace): Heat and Light Problems, 28; Doppler's Principle, 80; and William Briggs, Analysis of a Simple Salt, 126
Stillman (W. J.), Formation of Language, 491
Stockholm Royal Academy of Sciences, 144, 240, 360, 408, 576, 600
Stoehr (Dr.), a New Class of Organic Bases, 301
Stoel (L. M. J.), Influence of Temperature on Viscosity of Fluid Methyl Chloride, 528
Stokes (Sir George Gabriel): Proposed Presentation to, 162; the Anniversary of the Royal Society, Presidential Address, 133
Stoliczka's Collection of Yarkand Birds, 327
Stoney (Dr. G. Johnstone, F.R.S.), Cause of Double Lines in Spectra of Gases, 551
Stoppani (Cav. Ab. Antonio), Death of, 230
Storms, on the Determination of the Path taken by, Nils Ekholm, 63
Strahan (A.), a Phosphatic Chalk with *Belemnitella quadrata* at Taplow, 551
Strassmaier (Rev. J. H.) and Rev. Joseph Epping, on Babylonian Astronomy and Chronology, 369
Straton (Charles R.), the Value of Attractive Characters to Fungi, 9
Streamers of White Vapour, N. F. Dupuis, 175
Stresses in a Whirling Disk, Prof. J. A. Ewing, F.R.S., 514
Structures, the Design of, S. Auglin, 436
Stuart (Prof. T. P. A.): a Membrane lining the Fossa Patellaris of Corpus Vitreum, 406; Connection between Suspensory Ligament of Crystalline Lens and Lens Capsule, 406; a Simple Mode of Demonstrating how Form of Thorax partly determined by Gravitation, 406
Studies from Johns Hopkins University Biological Laboratory, 477
Studies of Old Case-books, Sir James Paget, 606
Subway, the Proposed South Kensington and Paddington, Prof. W. H. Flower, F.R.S., 246; Alfred W. Bennett, 246; Prof. W. E. Ayrton, F.R.S., 278
Sudborough (J. J.), Action of Heat on Nitrosyl Chloride, 262
Sugar-Cane, Seed-Production of, W. T. Thiselton Dyer, F.R.S., 300
Sumpner (Dr. W. E.), Measurement of Power supplied by any Electric Current to any Circuit, 572
Sun and Elements, Photographic Spectrum of the, Prof. Rowland, 452
Sun, a Green, Chas. T. Whitmell, 440
Sun, Mock, T. Mann Jones, 269
Sun, Moon, and Stars, &c., William Durham, 103
Sun Record in the Botanic Gardens, 450
Sun, the Solar Corona, 568; Prof. Charroppin on the, 568; Prof. Frank H. Bigelow on the, 568; Mr. Schaeberle on the, 568
Sun's Way, Apex of the, 90
Sun-spot Cycle, the Paradox of the, Henry F. Blanford, F.R.S., 583
Sun-spots in 1889, Observations of, Em. Marchand, 432
Sun-spots, Spectroscopic Observations of, Rev. A. L. Cortie, 256
Sunday Lecture Society, 108
Sunshine, February, 453
Sunshine of London, Fredk. J. Brodie, 424
Surface Tension, the Right Hon. Lord Rayleigh, F.R.S., Miss Agnes Pockels, 437
Surface Tension with Temperature, Variation of, Prof. A. L Selby, 549
Surgery, Philosophy of, Sir James Paget, 606
Surrey and Kent, Rainfall of, 63
Survey, the Burmese, 399
Surveying and Levelling Instruments, William Ford Stanley, 374
Swallow's Terrace, a, W. Warde Fowler, 80, 318; Robert H. Read, 176
Swift and Son's Improved Student's Microscope, 47
Swinburne (James): Alternate Current Condensers, 262; Electrostatic Wattmeters, 501; some Points in Electrolysis, 549
Swine, Wild, of Palawan and the Philippines, A. H. Everett, 416
Switzerland: Technical Education in, 21; Scientific Guide-books to, 43
Sydney, Australian Museum, 132
Syme (David), on the Modification of Organisms, Dr. Alfred R. Wallace, 529
Symington (William), Unveiling of Bust of, 87
Symons (G. J., F.R.S.): Extraordinary Dryness of February 1891, 426; History of Rain-Gauges, 504
Systematic Chemistry, Elementary, Prof. William Ramsay, F.R.S., 391

Ta-tsien-lu, A. E. Pratt's Exploration of, 570
Table Games, 28
Tabreez, the Lake-system, &c., of, Colonel Stewart, 232
Tacchini (Prof.): Solar Activity, July–December 1890, 302; Latitudinal Distribution of Solar Phenomena, 360
Tacheometer, William Ford Stanley, 374
Tait (Prof. P. G.): Properties of Matter, 78; on Impact, 287; Quaternions and the Algebra of Vectors, 535; the Rôle of Quaternions in the Algebra of Vectors, 608; the Virial, with Special Reference to Isothermals of Carbonic Acid, 575
Tanaka (Dr. Shohei), a New Musical Instrument, 521
Tanner (Colonel H. C. B.), our Present Knowledge of the Himalayas, 621
Taylor (J. F.), Proof of Generality of Mr. Blakesley's Formulæ, Tests of a Transformer, 478
Tchernysheff (Th.), Exploration of Tundras of North-East Russia, 397
Tchihatchef (Pierre de), Death and Obituary Notice of, 19
Tea, Gift by Messrs. Dakin to the Botanic Society's Collection of Samples of Curious Kinds of, 398
Tea at £10 12*s*. 6*d*. a Pound, 451
Technical Education and the County Councils, 97, 602; the City and Guilds of London Institute and County Councils, 429
Technical Education: Manual Training in Education, C. M. Woodward, 220
Technical Education: Sir H. Roscoe's Bill, 397
Technical Instruction Act, Essex and the, 337, 357
Technical Instruction Act and the Local Taxation Act, the Working of the, 167
Telegraph Poles, Woodpeckers and Bears and, J. D. Pasteur, 184
Telegraphy: an Explanation of the Phonopore, and more especially of the Simplex Phonopore Telegraph, C. Langdon-Davies, 531
Teleosteans, on the Eggs and Larvæ of some, Ernest W. L. Holt, 510
Telephone, the London-Paris, 475
Telephonic Effects, Intensity of, E. Mercadier, 288
Tellurium, Volumetric Estimate of, II., B. Brauner, 503
Temperature in the Glacial Epoch, Prof. T. G. Bonney, F.R.S., 373

Temperature, the Influence of, on the Vagus, Dr. T. Lauder Brunton, F.R.S., 558
Temperature, Nocturnal, of the Air, 63
Tennessee, &c., Physical Geology of, Prof. Edw. Hull, F.R.S., 215
Tension of a Girdle of the Earth, Prof. A. S. Herschel, F.R.S., 513
Tension, Surface, Right Hon. Lord Rayleigh, F.R.S., Miss Agnes Pockels, 437
Terby (Dr.): and M. Van Lint, the Frequency of Meteors, 133; the Permanence of Markings on Venus, 427
Terminology, Three Suggestions in Biological, Prof. T. Jeffery Parker, F.R.S., 141
Tern, Arctic, the Flight of, John R. Spears, 319
Ternary Series of Weights, Weighing by a, J. Willis, 30
Terry (James), Remarkable Ancient Sculptures from North-West America, Dr. Alfred R. Wallace, 396
Tetanus and Diphtheria, a Cure for, E. H. Hankin, 121
Texas, Earthquake in, 254
Thames, Trout Culture in the, 327
Thebes, Egypt, Discovery of a Vault Full of Mummies, &c., near, 398
Theory of Functions, Dr. H. A. Schwartz, Prof. O. Henrici, F.R.S., 321, 349
Thermodynamics: the Steam Engine Considered as a Thermodynamic Engine, James H. Cotterill, F.R.S., J. Larmor, 123
Thermo-electric Researches, Chassagny and Abraham, 24
Thermometer: Practical Solution of Problem of Emergent Liquid Column of Thermometer by Employment of Connecting Stem, C. E. Guillaume, 288; Effect of Difference of Exposure on Reading of, Dr. Sprung, 592; Thermometer-Scale, Principle of Construction of Fahrenheit, Arthur Gamgee, F.R.S., 119
Thibetan Plants, Dried, Added to Kew Herbarium, 299
Thomas (R. Haig): Extraordinary Flight of Leaves, 9; Attractive Characters in Fungi, 79
Thompson (Prof. Silvanus P.): Illustration of Ewing's Theory of Magnetism, 190; Magnetic Proof Pieces and Proof Planes, 549; a Property of Magnetic Shunts, 623; on Erecting Prisms for the Optical Lantern, 623; the Researches of Dr. R. Kœnig on the Physical Basis of Musical Sounds, 199, 224, 249
Thomson (Prof. J. J.), Electric Discharge Through Rarefied Gases without Electrodes, 384
Thomson (W.) and James Blaikie, a Text-book of Geometrical Deduction, 487
Thorpe (Prof. T. E., F.R.S.), Priestley, Cavendish, and Lavoisier, 1
Three Years in Western China, a Narrative of Three Journeys in Szechuan, Kweichow, and Yunnan, Alexander Hosie, 290
Through Magic Mirrors, Arabella B. Buckley, 246
Throwing-sticks and Canoes in New Guinea, Prof. Henry O. Forbes, 248; Prof. A. C. Haddon, 295
Thrush, the Intelligence of the, Rev. John Hoskyns-Abrahall, 583
Thumb and Finger Marks, the Patterns in, Francis Galton, F.R.S., 117
Thunderstorms in Russia, Study of Mean Velocities of 197, Herr Schönrock, 520
Tidal Prediction, Prof. G. H. Darwin, F.R.S., 320, 609
Tidal Water, Wild Life on a, P. H. Emerson, 366
Tides off the Dutch Coast, 450
Tillo (General de), Distribution of Atmospheric Pressure in Russia and Asia, 208
Titchener (E. B.), Pectination, 248, 368
Toads, Microscopical Study of Skin of, Herr Schultz, 209
Toads and Wasps, Hiron-Royer, 232
Tobacco, Persian, 591
Tobacco-growing in Victoria, 355
Todd (Prof. David P.) and Prof. Cleveland Abbe, Additional Results of the United States Scientific Expedition to West Africa, 563
Tokio Botanical Magazine, 382
Tomlinson (C., F.R.S.), Formation of Language, 534
Tonge (Mr.), Coal-mining in 1850 and 1890, 544
Tongues in Trees and Sermons in Stones, Rev. W. Tuckwell, 581
Topographical Survey of Alaska, Proposed United States, 279
Tops, Spinning, Prof. John Perry, F.R.S., 365
Tornado, the, Prof. H. A. Hazen, 128
Tourists' Guides, Messrs. Wesley's, 63
Tourmaline, Pyro-electricity of, Riecke, 120
Tower (Beauchamp), Friction of a Pivot Bearing, 499
Track and Trail, by, a Journey through Canada, Edward Roper, 532
Travertine and Siliceous Sinter, on the Formation of, by the Vegetation of Hot Springs, W. H. Weed, 510
Trees, Snow on the Branches of, Prof. Samuel Hart, 391
Treub (Dr.), Report on the Condition of the State Gardens at Buitenzorg, 196
Triana (Dr. J. J.), Death and Obituary Notice of, 87
Trigonometry, Elementary Text-book of, R. H. Pinkerton, 7
Trigonometry and Logarithms, J. M. Eustace, 55
Trisection, Mechanical, of any Angle, Captain A. H. Russell, 547
Tromometer-indications Vitiated by Wind, M. Carcani, 520
Trout in the Thames, Loch Leven, 327
True (F. W.), the Hatteras Porpoise Fishery, 497
Tube-frame Goods Waggons, M. R. Jefferds, 22
Tubercle Bacillus, conditions which Modify Virulence of, A. Ransome, M.D., F.R.S., 286
Tuberculosis, Koch's Cure for Consumption, 49, 66, 265, 281
Tuckwell (Rev. W.): the Honey Bee, by T. W. Cowan, 578; Tongues in Trees and Sermons in Stones, 581
Tuhlin (T.), on the Nocturnal Temperature of the Air, 63
Tuning-forks and Spiders, Prof. C. V. Boys, F.R.S., 40
Tuomeya fluviatilis, Harvey, 65
Turin Observatory, Signor Porro, 257
Turkey in Asia, Showers of "Manna," Edible Lichen, in, 255
Turle (Dr. James): Fire-ball Meteor of December 14, 176; Frozen Fish, 464
Turner (Sir William, F.R.S.), the Cell Theory, Past and Present, 10, 31
Tutton (A. E.): Krystallographisch-chemischen, Dr. A. Fock, 197; the Isolation of Hydrazine, $\overset{NH_2}{\underset{NH_2}{|}}$, 205; Crystalline Form of Calcium Salt of Optically Active Glyceric Acid, 503; Crystallographical Characters of Aconitine from *Aconitum napellus*, 550

Underground Railway, the Proposed, to South Kensington 145
United States: Census of, 73; Report on the Meteorology of, for Year ending June 30, 1890, 131; Geographical Teaching in, 131; the Origin of the Great Lakes of North America, Prof. T. G. Bonney, F.R.S., 203; American Blast-furnace Work, 211; a Remarkable Ice-storm at Hartford, Prof. Samuel Hart, 317; Naval Observatory, 357; Ninth Annual Report of the United States Geological Survey to the Secretary of the Interior, 1887–88, J. W. Powell, Prof. T. G. Bonney, F.R.S., 509; Scientific Expedition to West Africa, Profs. Cleveland Abbe and David P. Todd, 563; National Museum Report, 620
University College, London, Augmentation of the Botanical Department of, 20; Free Evening Lectures, 278; Appeal for Funds, 289
University College of North Wales, Purchase of Mr. E. Watkin's Library by, 87
University Colleges, Annual Conference of Heads of, 208
University Colleges of England, Right Hon. G. J. Goschen and the Proposed Increase in the Annual Grant to the, 425
University and Educational Intelligence, 23, 47, 94, 116, 142, 285, 309, 358, 382, 405, 430, 476, 548
University Extension Delegacy, Reports of Oxford, 355
University and King's Colleges Extension Funds, Mansion House Meeting in aid of, 397
Ure (George), Our Fancy Pigeons, 79
Use and Disuse, Are the Effects of, Inherited, William Platt Ball, Prof. Geo. J. Romanes, F.R.S., 217

Vaccination by Minimum Doses, Ch. Bouchard, 576
Vagus, the Influence of Temperature on the, Dr. T. Lauder Brunton, F.R.S., 558
Van Lint (M.) and M. Terby, the Frequency of Meteors, 133
Vapour, Streamers of White, N. F. Dupuis, 175
Varet (R.), Fermentation of Starch by Action of Butyric Ferment, 480
Variability of the Andromeda Nebula, Isaac Roberts, 379

Variable Nebula, a, 453
Variable Star, New, 428
Variable Stars: Stars 121 and 483 Birm., 90
Variot (Dr.), Embalming Dead Bodies by Galvano-plastic Method, 163
Vasey (Dr. Geo.), Grasses of the South-West, 389
Vatican Observatory, the, 496
Vaucluse, Discovery of Prehistoric Workshop by M. Rousset in, 183
Vectors, Quaternions and the Algebra of, Prof. P. G. Tait, 535, 608; Prof. J. Willard Gibbs, 511
Veeder (M. A.), the Aurora, Forces concerned in Development of Storms, 20
Vegetable Biology, the Laboratory of, at Fontainebleau, 37
Vegetation of Lord Howe Island, W. Botting Hemsley, F.R.S., 565
Veley (V. H.): Condition of Chemical Change between Nitric Acid and certain Metals, 118; Variations of Electromotive Force of Cells consisting of certain Metals, Platinum, and Nitric Acid, 142
Vénukoff (M.), Measurement of 52nd Parallel in Europe, 480
Venus: the Permanence of Markings on, Dr. Terby, 427; Rotation of, M. Perrotin on the, 22; Transits of, in 1761 and 1769, 328
Verneuil (M.), Relation of Gangrenous Septicæmia to Lockjaw, 48
Vibration-Recorders, Prof. John Milne, F.R.S., and John Macdonald, 154
Victoria Hall Lectures, 520
Victoria: Tobacco-growing in, 355; Scent-farming in, 544; Prodomus of the Zoology of, Sir Frederick McCoy, 389
Vienna Museum, Annales des k.k. naturhistorischen Hofmuseums, 28
Vienna, Remarkable Cavern Discovered at Baden, near, 567
Ville (J.), Action of Urea on Sulphanilic Acid, 624
Virchow (Prof. R.): Seventieth Birthday of, 230; on the Consumption Cure, E. H. Hankin, 248; Virchow Testimonial Fund, 324
Virginis, on the Orbit of α, Prof. H. C. Vogel, 235
Virial, the, with Special Reference to Isothermals of Carbonic Acid, Prof. P. G. Tait, 575
Vogel (Prof. H. C.), on the Orbit of α Virginis, 235
Vogel (Prof. Dr. H. W.), Handbuch der Photographie, Prof. R. Meldola, F.R.S., 3
Volcanic Eruption in Behring Sea, 279
Volcanic Eruption at Macao, 182
Volta's so-called Contact Force, Modern Views of Electricity, S. H. Burbury, 268; Prof. Oliver J. Lodge, F.R.S., 268
Volta's Force, Modern Views of Electricity, Prof. Oliver J. Lodge, F.R.S., 463
Vulcanite, Physical Properties of, 309

Waals (Prof. Van der): on the Continuity of the Liquid and Gaseous States, Prof. A. W. Rücker, F.R.S., 437; Prof. J. T. Bottomley, F.R.S., 415, 437; Robert E. Baynes, 487; Co-existent Phases of a Mixture, 600
Waga (Herr Anton), Death of, 131
Waggons, Tube-Frame Goods, M. R. Jefferds, 22
Wahl (W. H.), Alloys of Sodium and Lead, 216
Walcott (C. D.), Discovery of Vertebrate Life in Lower Silurian (Ordovician) Strata, 425
Walker (Dr. James), Synthesis of Dibasic Acid by Electrolysis, 575
Wallace (Dr. Alfred R.): Dr. Romanes, F.R.S., on Physiological Selection, 79, 127, 150, 197; Animal Life and Intelligence, C. Lloyd Morgan, 337; Remarkable Ancient Sculptures from North-West America, James Terry, 396; David Syme on the Modification of Organisms, 529
Wallis (H. S.), Rainfall of February 1891, 599
Walther (Prof. Johannes), Dry Denudation, 556
Walton-on-Naze, Cliff Subsidence at, 131
Warburton (C.), Oviposition of Spider (*Agelena labyrinthica*), 119
Warnerke (Leon), a Simplified Photo-collographic Process, 450
Warwickshire, the Flora of, J. G. Baker, F.R.S., 413
Washington Observations: Appendix I., 188; Appendix II., Asaph Hall on Saturn, 65
Wasps, Toads and, Hiron-Royer, 232
Water, Honeycomb Appearance of, J. Shaw, 30
Water as to Passage of Different Light-rays, Behaviour of, Hüfner and Albrecht, 520
Water, the Relation to Disease of Ground, Baldwin Latham, 94
Water in Sahara, Discovery of Large Body of, 566
Water, Sea, the Composition of, 199
Water, Surface Tension of, the Right Hon. Lord Rayleigh, F.R.S., Miss Agnes Pockets, 437
Water, Volumetric Composition of, E. W. Morley, 600
Waterall (Nath.), Robin's Nest in Flat Handbasket, 163
Waterfalls, Great, Arthur G. Guillemard, 105, 129
Water-rail, a Rare Visitor, O. A. Shrubsole, 319
Waterspout at Newhaven, Connecticut, H. J. Cox, 285
Watkin's (Mr. E.), Library, 87
Watson (William), the Coco-de-Mer in Cultivation, 19
Wax, Chinese Insect, Industry, 291
Wax floats in Water above 18° C., Herr Kleinstrick, 520
Weather, the Wheat Harvest in Relation to, 569
Webber (Major-General, C.E.), Shaking the Foundations of Science, 222
Webster (A. D.), Araucaria Cones, 57
Webster (Clement L.), Ancient Mounds at Floyd, Iowa, 213
Weed (W. H.), on the Formation of Travertine and Siliceous Sinter by the Vegetation of Hot Springs, 510
Weighing by a Ternary Series of Weights, J. Willis, 30, 198; Prof. J. D. Everett, F.R.S., 198
Weights, Proceeding by Powers of 3, Prof. J. D. Everett, F.R.S., 104
Weights, Weighing by a Series of, Major P. A. MacMahon, F.R.S., 113
Weihrauch (Dr. Karl), Death and Obituary Notice of, 325
Weismann and Maupas on the Origin of Death, E. G. Gardiner, 458
Weiss (Dr. E.), Argelander-Oeltzen Star Catalogue, 357
Weld-Blundell (Adrian), Araucaria Cones, 128
Wesley's Tourists' Guides, 63
West (Tuffen), Death and Obituary Notice of, 543
West Indies, Botanical Enterprise in the, 129
West Indies, Coral Rocks of, Jukes-Brown and Harrison, 383
West Indies, the Zoology and Botany of, 212
Whaling Expedition, a Novel, 109
Wheat Harvest in Relation to Weather, 569
Whirling Disk, the Stresses in a, Prof. J. A. Ewing, F.R.S., 514
Whirling Ring and Disk, Prof. Oliver J. Lodge, F.R.S., 533
Whirling Ring, the Flying to Pieces of a, Prof. Oliver J. Lodge, F.R.S., 439, 461; Prof. A. G. Greenhill, F.R.S., 461; Prof. J. A. Ewing, F.R.S., 462; Prof. C. V. Boys, F.R.S., 463; Prof. A. M. Worthington, 463, 583; G. H. Bryan, 463; Prof. Karl Pearson, 488
White (Arthur Silva), the Development of Africa, 149
White (George), the Darkness of London Air, 318
White (William), Antipathy (?) of Birds for Colour, 608
White Vapour, Streamers of, N. F. Dupuis, 175
Whitford (John), the Canary Islands, 317
Whitman (Prof. C. O.), Specialization and Organization, the Naturalist's Occupation, 457
Whitmell (Chas. T.), a Green Sun, 440
Widgeon with Hanging Feather Tassel, J. E. Harting, 311
Wife, Husband and, Resemblances between, Hermann Fol, 255
Wild (Dr. H.), the Electric Light for Photographic Self-Registering Instruments, 519
Wild Beasts and their Ways, Sir Samuel W. Baker, F.R.S., 78
Wild Life on a Tidal Water, P. H. Emerson, 366
Wilks (Rev. W.), Peach Trees injured by Contact with Galvanized Wire during Hard Frost, 377
Williams (F. N.), the Pinks of Central Europe, 149
Williams (G. J.), Manod and the Moelwyns, 526
Williams (W. Mattieu), the Chemistry of Iron and Steel Making, John Parry, 50
Williamson (Prof. W. C., F.R.S.): Plants Represented in Coal-measures, 355; Organization of Fossil Plants of Coal-measures, 454
Willis (J.), Weighing by a Ternary Series of Weights, 30, 198
Wilsmore (U. T. M.), does Magnesium combine with Hydrocarbon Radicles, 454
Wilson (Prof. E. B.), some Problems of Annelid Morphology, 458
Wilson-Barker (Captain David), on the Flight of Oceanic Birds, 222

Wimshurst (James), an Alternating Current Influence Machine, 623
Wind, Tromometer Indications Vitiated by, M. Carcani, 520
Wind-pressures and Measurement of Wind Velocities, Prof. Marvin, 548
Wolf's Relative Numbers for 1890, 400
Wolves, Dogs, Jackals, and Foxes, a Monograph of the Canidæ, St. George Mivart, F.R.S., 385
Wolves in France, the Destruction of, 356
Women only, Bedford College and Scientific Education for, 425
Women, Scientific Education of, Opening of Shaen Wing of Bedford College, 496
Wonder-workers, Nature's, Kate Lovell, 532
Wood (R. W., Jun.), Effects of Ice on Pressure, 309
Wood's Holl, Biological Lectures delivered at the Marine Biological Laboratory of, in the Summer Session of 1890, 457, 591; Prof. E. Ray Lankester, F.R.S., 516
Wood-working, Ivin Sickels, M.D., 195
Woodpeckers and Telegraph Poles, J. D. Pasteur, 184
Woodward (Arthur Smith), Catalogue of the Fossil Fishes in the British Museum, Part II., Prof. E. Ray Lankester, F.R.S., 577
Woodward (C. M.), Manual Training in Education, 220
Worlds, the Birth and Growth of, A. H. Green, 221
Worthington (Prof. A. M.), the Flying to Pieces of a Whirling Ring, 583
Woven Design, Colour in, Roberts Beaumont, 343
Wragge (Clement L.), Establishment of Meteorological Station at Noumea, 326
Wrath Silver, Payment of, 42
Wrens, Fight between Cock, Aubrey Edwards, 209
Wright (C. R. Alder, F.R.S.), the Threshold of Science, Prof. Wyndham R. Dunstan, 314
Wright (Lewis), Optical Projection, 555
Würtemberg: Discovery of Stalactite Cave in, 43; Injury from Hail in, from 1828–87, Herr Bühler, 473; Agricultural Education in, Lord Vaux of Harrowden, 567
Wynne (W. B.), Andresen's β-Naphthylaminedisulphonic Acid, 480

Yang-tse-Kiang, the Monsoons of the, 232
Yankowsky's (Captain) Collection of Chinese Butterflies, Sale of, 543
Yarkand Birds, Stoliczka's Collection of, 327
Yarrow (A. F.), the Science of Boiler Construction, 500
Year-book of Pharmacy, 256
Yorkshire College, Leeds, Mr. James Muir appointed Professor of Agriculture at, 397
Young (Prof. S.), Method of Determining Specific Volumes of Liquids, &c., 261

Zarudnoi (M.), Ornithological Fauna of the Amu-Daria Region, 334
Zenger (C. V.), Electro-dynamic Origin of Planetary Motion, 48
Zittel (Prof. K. von) Handbuch der Palæontologie, Vol. III. Reptiles, 420, 440
Zodiacal Light, the Spectrum of the, Prof. C. Michie Smith, 22
Zona's Comet, 111; Elements and Ephemeris of, 165
Zoology: Additions to the Zoological Gardens, 21, 44, 64, 109, 133, 164, 184, 210, 256, 280, 302, 328, 356, 379, 399, 427, 452, 474, 498, 521, 545, 568, 593, 621; Death of an Aged Crane, 472; Acquisition of a Lesser Orang (*Simia morio*), 618; Zoological Society, 71, 95, 143, 310, 382, 407, 431, 479, 496, 526, 575, 618; the Myology of the Raven, R. W. Shufeldt, 101; Zoological Types and Classification, W. E. Fothergill, 124; a Zoological Pocket-book, or Synopsis of Animal Classification, Emil Selenka and J. R. A. Davis, 124; the Antelopes of Nyassaland, Richard Crawshay, 143; on the Affinities of Hesperornis, R. W. Shufeldt, 176; Zoology and Botany of the West Indies, 212; Lehrbuch der Zoologie für Studirende und Lehrer, Dr. J. E. V. Boas, 268; Zoological Record for 1889, 316; Skin of *Æluropus melanoleucus*, procured by Herren Potanin and Beresowski, 355; the Fauna of British India, including Ceylon and Burma, O. Boettger, 361; Reptilia and Batrachia, George A. Boulenger, O. Boettger, 361; Scientific Zoology, Hunter and Aristotle, Jonathan Hutchinson, F.R.S., 377; Dogs, Jackals, Wolves, and Foxes, a Monograph of the Canidæ, St. George Mivart, F.R.S., 385; Prodomus of the Zoology of Victoria, Sir Frederick McCoy, 389; Zoological Station of Naples, 392; Dr. Anton Dohrn, 465; Proposed Marine Zoological Station at Cette, Prof. A. Sabatier, 397; Wild Swine of Palawan and the Philippines, A. H. Everett, 416; the Zoologist, 451; Dr. Abbott's East African Collections, 497; Extermination, 505; the Poison Apparatus of the Heloderma, Dr. R. W. Shufeldt, 514; the Naturalist of Cumbrae, being the Life of David Robertson, by the Rev. T. R. R. Stebbing, 532; Zoological Articles contributed to the Encyclopædia Britannica, Prof. E. Ray Lankester, F.R.S., 607

NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH.

THURSDAY, NOVEMBER 6, 1890.

PRIESTLEY, CAVENDISH, AND LAVOISIER.

THE *Revue Scientifique* of the 25th ult. contains a translation of the address which I had the honour of delivering to the members of the Chemical Section of the British Association at the recent meeting in Leeds, to which, on the invitation of the editor, M. Charles Richet, M. Berthelot prefixes a letter, of which the following is a translation:—

"I have no direct concern in the republication of Mr. Thorpe's address which you purpose making in the *Revue.* Personally, I have not any reason to complain of his courtesy, and I should have been silent so far as he is concerned, holding that one is not bound to enter into a controversy which is purely critical, where no new fact is alleged, and where the judgment of public opinion suffices to set things in their true place; however, I comply with your request to let your readers know what my opinion is.

"To my mind, nothing is more opposed to truth and justice than the introduction of national prejudices into the history of science. All civilized nations are at one in proclaiming the glory of Newton, the greatest of astromers, and yet the majority of English men of science, refusing to treat his rivals with equity, are not agreed to recognize Leibnitz's rights to the invention of the differential calculus: they are as prejudiced in this respect as was Newton himself. Something analogous occurs in regard to the discoveries which created modern chemistry a hundred years ago.

"Unquestionably, Priestley and Cavendish are recognized by all as great discoverers. I have myself taken pains to describe Priestley's discovery of the principal gases in terms of admiration ('La Révolution Chimique,' p. 39), and especially that of oxygen, which I unreservedly attribute to him (pp. 61–62). I have also detailed, with the encomiums which they merit, the investigations of Cavendish, 'one of the most powerful scientific minds of the last century,' and particularly his fruitful research on (to use Blagden's phrase) the artificial generation of water. But the well-merited praise accorded to these English *savants* does not prevent some of their countrymen from persistently denying the right of Lavoisier to the discovery and co-ordination of those general ideas on which rest our actual conception of matter, more especially in relation to the composition of air and water. This, I venture to repeat, is an incident in the long-standing feud, continually being renewed in the history of science, between the sagacious discoverers of particular facts and the men of genius who frame general theories. The opinion of most Continental men of science seems, however, to be decided on this special point, as may be seen from the judgment given, not only by Dumas, but by Hoefer, in his 'History of Chemistry,' by H. Kopp, in his careful account of the discovery of the composition of water, and by many others. I have merely concurred with them.

"It was in this spirit that I had sought to trace the history of the discoveries which constituted the doctrine of modern chemistry, by faithfully reproducing all its phases, whilst at the same time indicating the continuity of sequence in the facts and the paternity of the ideas. I did this with an impartiality which has brought upon me the reproach that I have been indifferent to the reputation of my countryman—the very opposite to the accusations which are now directed against me.

"As regards the composition of air, it is easy to separate facts from ideas. It is certain that the discovery of oxygen is due to Priestley. But, said Lavoisier: 'If I am reproached for having borrowed my proofs from the works of this celebrated philosopher, at least none will contest my right to the conclusions, which are often diametrically opposed to his.'

"Priestley, obstinately adhering to the theory of phlogiston, regarded his new gas as consisting of the very substance of air deprived simply of its phlogiston; whilst nitrogen, according to him, was formed also of this same substance combined with a complementary portion of phlogiston. He remained faithful to this doctrine, which obscured the true nature of the greater number of chemical phenomena, until the moment when, like Lavoisier, persecuted by his countrymen, who now proclaim his fame, driven from home, his laboratory burnt by a mob, and threatened with death, he fled to America, where he died in sadness and in solitude. Even more unfortunate was Lavoisier!

"But whatever may have been the personal fate of these two great men, if it is true that Priestley discovered oxygen, it is not the less certain that the true theory of the nature of air is due to Lavoisier.

"The history of the composition of water is more complicated. In reality, the discovery of the facts belongs neither wholly to Cavendish—who undoubtedly played a most important part, inasmuch as he gave the impetus towards the definitive solution—nor to Lavoisier, who first established a knowledge of the facts by his public experiments and his published writings—nor even to the two combined. They had predecessors, and at the moment even when the light came, Monge played an essential part in the rigorous demonstration of which Mr. Thorpe apparently has no suspicion. Thus each man's share in this history cannot be settled by a word: we require to follow exactly the gradual progress of experiment and publication. But here, again, if Lavoisier is not the principal discoverer of the facts, it is he who has the incontestable merit of having furnished the exact interpretation of the phenomena, freed from the mists of phlogiston, to which Cavendish seems to have remained faithful to the day of his death.

"I have elsewhere laid bare all these facts, and I have no intention of reproducing here the details of a controversy exhausted even in Lavoisier's time, and in which Mr. Thorpe does no more than reproduce the unjustifiable imputations of Blagden, who, impelled by passion, went so far as to interpolate and falsify, with his own hand, the manuscript memoirs of Cavendish, in order to gain arguments in support of his accusations.

"Moreover, nothing more decisively establishes the part played by Lavoisier, and his right to the institution of our modern theories, than the letter of a contemporary English *savant*, Black, as celebrated for his discoveries in physics as in chemistry, and who might have put forward claims on his own account. In 1791 he wrote to Lavoisier, in a letter equally honourable to both :—'The numerous experiments which you have made on a large scale, and which you have so well devised, have been pursued with so much care and with such scrupulous attention to details that nothing can be more satisfactory than the proofs you have obtained. The system which you have based on the facts is so intimately connected with them, is so simple and so intelligible, that it must become more and more generally approved and adopted by a great number of chemists who have long been accustomed to the old system. . . . Having for thirty years believed and taught the doctrine of phlogiston as it was understood before the discovery of your system, I, for a long time, felt inimical to the new system, which represented as absurd that which I had hitherto regarded as sound doctrine; but this enmity, which sprang only from the force of habit, has gradually diminished, subdued by the clearness of your proofs and the soundness of your plan.'

"We can but hope to see the day when the scientific men of England will conform to the opinion of one of the most illustrious of their countrymen.

"M. BERTHELOT,
"of the Institute."

I quite agree with M. Berthelot that nothing is more opposed to truth and justice than the introduction of national prejudices into the history of science. It was for that reason that I felt compelled, in the Leeds address, to protest against the spirit and bias of the accounts of the discovery of the facts relating to oxygen and the composition of water given in "La Révolution Chimique." Although M. Berthelot's letter somewhat confuses the issues, there is, in reality, but small difference between us. What I ventured to criticize was the general tone and tendency of M. Berthelot's argument, which seems to palliate, and even to justify, Lavoisier's pretensions to a discovery in which he has no right even to be considered as a participator. M. Berthelot now tells us, in his letter that he attributes the discovery of oxygen unreservedly to Priestley. So far so good. It is something gained to have thus secured such an unqualified statement from one who occupies the position of authority in the world of chemistry in France that is enjoyed by the present Perpetual Secretary of the Academy. We may well hope, therefore, that this particular question has been finally set at rest.

M. Berthelot need not ask British men of science to conform to the opinion of Black. They already do so. That to Lavoisier, and to Lavoisier alone, belongs the merit of having effected the overthrow of the theory of phlogiston, and of having to that extent laid the foundation of modern chemistry, is not questioned on this side of the Channel. So far as I know, it has only been among Lavoisier's own countrymen that any doubt on this point has been raised. We all remember the passionate scorn with which Lavoisier repudiated and protested against the attempts of his compatriots to rob him of his rights: "Cette théorie n'est donc pas comme je l'entends dire—la théorie des chimistes français; elle est la *mienne*, et c'est une propriété que je réclaime auprès de mes contemporains et de la postérité." It is true, as M. Berthelot implies, that Black has claims. Lavoisier himself admits as much. It would be easy, if it were not beside the points at issue, to match the letter which M. Berthelot quotes, by others from Lavoisier in which he ascribes to Black the germs of his doctrine. M. Berthelot, I repeat, confuses the issues. This particular point was never raised by me in the address. What I said was:—"Two cardinal facts made the downfall of phlogiston complete—the discovery of oxygen, and the determination of the compound nature of water. M. Berthelot's contention is, that not only did Lavoisier effect the overthrow, but he also discovered the facts." I, in common, I venture to assert, with every British chemist, admit unreservedly that Lavoisier effected the overthrow, but we deny that he discovered the facts. It is altogether beside the question for M. Berthelot now to say in effect:—"Have I not praised your men of science, and thereby drawn down upon myself the wrath of my countrymen? And yet you are not satisfied!' We are sorry for M. Berthelot: he is in the position of the man with many friends, and his friends for the moment are a little angry. He has either not the courage of his convictions, or he has halted between two opinions—with the usual consequences.

With respect to the discovery of the compound nature of water, M. Berthelot now takes up a different position from that which he occupies in "La Révolution Chimique." His contention there was that by every legitimate canon the experiment of June 24, 1783, gives to Lavoisier the priority of discovery. He now admits that Cavendish played "un rôle capital—car il donna le branle aux esprits vers la solution définitive." But how was this possible when Cavendish's memoir was not published until January 1784? There is really only one answer—it was given simply by the intervention of Blagden. I repeat that Blagden told Lavoisier of Cavendish's researches and of his conclusions, and that it was in the light of that knowledge that the experiment of June 24, 1783, was made. There can be no question of this. Blagden's testimony, as given in the letter to Crell, is as direct and decisive as it is damning. It was never contradicted by Lavoisier, nor by Laplace, Vandermonde, Fourcroy, Meusnier, or Legendre, who were present on the occasion when Lavoisier himself admits that he received the information.

M. Berthelot does not contradict it, but, instead, he asperses the moral character of Blagden. This method of treating a witness whose evidence cannot be rebutted is apt, when unsuccessful, to recoil on him who attempts it. It is perfectly true that Blagden interpolated the famous passage in Cavendish's memoir :—

> "During the last summer, also, a friend of mine gave some account of them [the experiments] to M. Lavoisier, as well as of the conclusion drawn from them. . . . But at that time so far was M. Lavoisier from thinking any such opinion warranted that, till he was prevailed upon to repeat the experiment himself, he found some difficulty in believing that nearly the whole of the two airs could be converted into water."

This passage, however, was inserted with Cavendish's knowledge and consent, and by his assistant and amanuensis, who happened to be the very man who had a personal knowledge of the facts. Assuming the statement to be true, where is the immorality of the proceeding?

Everything that we can learn authoritatively concerning Blagden goes to show that he was an upright and honourable man. Sir Joseph Banks has testified to his abilities and integrity. Dr. Johnson spoke of his copiousness and precision of communication, with the characteristic addition : "Blagden, sir, is a delightful fellow." Laplace, Cuvier, Berthollet, and Benjamin Delessert, were among his friends.[1] He was rich, and was understood to have speculated to profit in the French funds. For thirteen years he was a Secretary of the Royal Society, and in 1792 he was knighted for his services to science. Every year he spent a considerable time on the Continent, and was frequently in Paris. The gossip of the period states that he aspired to the hand of Madame Lavoisier, who preferred Count Rumford. He died in Berthollet's house at Arcueil, on March 26, 1820. In an obituary notice in the *Moniteur* of September 22, 1820, M. Jomard testifies to his benevolence and generosity, and states that "none of his countrymen have done more justice to the labours and discoveries of the French, or have contributed more than he to the happy relations which have subsisted for six years (1814–20) between the *savans* of the two countries." By his will he provided liberally for his scientific friends : Berthollet, the daughter of Madame Cuvier, and the daughter of Count Rumford, each received £1000; and Laplace £100, "to purchase a ring." M. Berthelot asperses the character, not only of Blagden, but also of his countrymen by his insinuations. Would he have us believe that men like Berthollet, Cuvier, and Laplace, would extend their friendship to, and receive pecuniary benefits from, one whom they believed had foully stabbed their compatriot in the back? It is surely incumbent on M. Berthelot, on every ground, either to substantiate his implications or to withdraw them.

M. Berthelot makes the gratuitous assumption that I am ignorant of the work of Monge. Whether I am or not is altogether beside the mark. There is, indeed, no question of Monge. Monge distinctly disclaims priority to Cavendish, nor did he attempt to establish a right to be considered an independent discoverer of the true nature of water. In his memoir, "Sur le Résultat de l'Inflammation du Gas inflammable et de l'Air dephlogistiqué dans les Vaisseaux Clos," he tells us that the experiments recorded in it were made in June and July 1783, and repeated in October of the same year. "I did not then know," he adds, "that Mr. Cavendish had made them several months before in England, though on a smaller scale; nor that MM. Lavoisier and Laplace had made them about the same time at Paris in an apparatus which did not admit of as much precision as the one which I employed." I fail to see what M. Berthelot gains by his reference to Monge.

M. Berthelot reproaches Priestley and Cavendish for their adherence to phlogistonism. I say it with all respect, but is it seemly for M. Berthelot, of all men, to cast this stone? Is not he himself an exemplification of that conservatism which he deplores? A generation ago the doctrine of Avogadro became the corner-stone of that edifice of which M. Berthelot asserts that Lavoisier laid the foundations. Indeed, the introduction of that doctrine effected a revolution hardly less momentous than that of which Lavoisier was the leader. But what has been M. Berthelot's consistent attitude towards this teaching? We can illustrate it by a single example. He is the sole teacher in Europe of any position who continues to symbolize the constitution of that very substance of which he claims that Lavoisier discovered the composition by a formula which is as obsolete as any conception of phlogistonism.

T. E. Thorpe.

A HAND-BOOK OF PHOTOGRAPHY.

Handbuch der Photographie. Part I. Fourth Edition. By Prof. Dr. H. W. Vogel. (Berlin: Robert Oppenheim, 1890.)

THIS is the latest edition of a work which has been known in Germany for ten years, and of which the author is the Director of the Photochemical Laboratory of the Imperial Technical High School in Berlin. The existence of such a post as that occupied by Dr. Vogel in one of the foremost technical schools of Germany is as much an indication of the advanced state of technical education in that country as the non-existence of such specialists in the technical schools of this country is a sign of our comparatively backward condition in the field of chemical technology. The subjects comprised under this heading are so wide in their range and so difficult to grasp, excepting by actual personal contact with the chemical industries, that no instruction likely to be of any great value to those preparing for, or engaged in, the latter can be given, unless the instructor has this qualification. Nor can the student properly avail himself of the instruction thus offered, unless he on his part is well grounded in the general principles of the science which underlies his subject. When such a ground-work has been laid, and the student thus equipped is passed on to the specialist, the result is a chemical technologist who is likely to be of real use to his country. The Germans have realized this long ago—the machinery exists both for laying the foundation and for raising the superstructure of specialized knowledge. In this country, so far as

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[1] Many of the letters of Berthollet to Blagden are still in existence. In one of these, dated "19 Mars, 1785," he writes from Paris:—"L'on s'est beaucoup occupé ici ces derniers tems de la belle découverte de Mr. Cavendish, sur la composition de l'eau: Mr. Lavoisier a tâché de porter sur cet objet toute l'exactitude dont il est susceptible. . . . Mr. Lavoisier veut repéter l'expérience en faisant brûler l'air dephlogistiqué dans le gas inflammable, et il y a apparence qu'alors on n'aura point d'acide nitreux, selon les belles observations de Mr. Cavendish." Is this language consistent with the belief that Berthollet, who must have known the facts, regarded Lavoisier as the real discoverer of the compound nature of water?

chemical technology is concerned, we have not yet advanced very much beyond the stage of furnishing the appliances for the general training—the real technical or special training has been allowed to take care of itself, and the student is supposed to have finished his education at the time when he ought really to be beginning it.

These ideas naturally suggest themselves in having brought under one's notice the various special works on applied science which reach us from time to time from the German press, and of which Dr. Vogel's book is a not unfavourable specimen. The present volume, which is the first part of the work only, contains some 350 pages, and the advancement of the subject since the last edition is indicated by the fact, which the author states in the preface, that the subject of photochemistry alone has been increased from 8½ to 22 pages. The whole volume consists of an introductory portion, three chapters, and an appendix. We propose to give, in the first place, a brief analysis of its contents.

The introduction consists of a history of photography, followed by some remarks on the study of the subject. The scientific aspect of photography forms the subject of the three chapters which constitute the main portion of the work. The first chapter deals with the physical action of light, and comprises such subjects as phosphorescence, phosphorography, the photophone, telephotography, the action of light on polished surfaces, Crookes's radiometer, and the action of light on ebonite, including an account of Edison's tasimeter, which, the author thinks, may become serviceable as a chemical photometer.

The second chapter occupies over 200 pages, and is divided into two sections, dealing with the action of light on non-metals and metals respectively. The subdivisions of this portion of the subject are well planned, but are not carried out with logical consistency. The action of light on inorganic compounds brings in allotropy and the photochemical combination of hydrogen and chlorine, &c. The action of light on organic compounds begins with the remarkable subject of the photopolymerisation of such compounds as vinyl bromide, anthracene, quinine, chloral, and asphalte, the latter, of course, leading to the original heliographic process of the elder Niepce. The author gives good reasons for the belief that the change in this last case is due to polymerisation, and not to oxidation. Instances of combination between organic compounds under the influence of light are then discussed, the formation of a compound from phenanthrenequinone and acetaldehyde being compared with the synthetical processes which go on in plants by the action of this same agency.

The photochemical decomposition of organic substances is dealt with at considerable length. The remarks concerning the action of light on cellulose in the form of wood and paper should be carefully studied by those interested in the technology of paper-making. The action of light on colouring-matters, both organic and mineral, is also treated of very completely, and the tables given will be found valuable to dyers, colourists, and painters. This portion of the subject is covered to some extent by Russell and Abney's Report on the fading of water-colours, of which the author has evidently availed himself. With respect to the fading of organic colouring-matters, it is of interest to note that all the artificial yellows are faster than the natural vegetable yellows. The section treating of the action of light on the vital processes of plants and animals brings the author into contact with such physiological subjects as germination, the formation of chlorophyll and other plant colouring-matters, the respiration of plants, and Aimé Girard's observations on the formation of sugar in the beetroot. Under the action of light on animals, the author gives an account of Engelmann's experiments on *Bacterium photometricum*, but Lubbock's analogous experiments with the Daphnidæ are not alluded to.

Passing to the second section, we find 167 pages devoted to the action of light on metallic compounds. The logical sequence is here broken by the introduction of a large amount of ordinary chemistry, *i.e.* the formulæ and properties of the most important compounds of the metals used in photography. This is very well in its way, and it is essential that the scientific photographer should be familiar with this portion of his subject, but it may be suggested that in future editions these paragraphs should be relegated to the third chapter, which deals with the chemistry of photographic materials. The author's use of formulæ, we may here point out, is somewhat antiquated, capricious, and inconsistent. Thus in some places chloracetic acid is written $C_2\left.\begin{matrix}H_3\\Cl\end{matrix}\right\}O_2$, ferrous tartrate, $C_4\left.\begin{matrix}H_4\\Fe\end{matrix}\right\}O_6$, ferric tartrate, $C_{12}\left(\begin{matrix}H_{12}\\Fe_2\end{matrix}\right)O_{18}$, ferrous acetate, $C_4\left.\begin{matrix}H_6\\Fe\end{matrix}\right\}O_4$; while on the same page, or in other parts of the book, we find ferric citrate written, $(C_6H_5O_7)_2Fe_2$, silver tartrate, $C_4H_4O_6Ag_2$, and silver citrate, $C_6H_6O_7Ag_2$. In the same equation, on p. 152, sodium thiosulphate is written $Na_2S_2O_3$, and the double silver salt, $\left.\begin{matrix}Na_4\\Ag_2\end{matrix}\right\}3S_2O_3$. Then, again, some compounds of very definite composition, such as sodium nitroprusside, Prussian blue, &c., are not favoured with formulæ at all; while in other cases, such as under the salts of ammonia, the alkalies, and the alkaline earths (third chapter), the formulæ suddenly rise to the dignity of thick type.

Under the action of light on iron salts, we have a description of the various printing processes depending on the use of these compounds, such as Willis's platinotype, the negative and positive blue processes and other less widely-known methods. Under chromium compounds we have an account of the chromatized gelatine processes, including that of Pretzsch (relief process), Fox Talbot (steel etching), pigment printing, Woodburytype, photogalvanography, lithography, and zincography, and a multitude of other processes, which form quite a special feature among the recent developments of applied photography. The salts of uranium and copper are dealt with in due order, but the chief interest, of course, centres in the compounds of silver. After a brief description of the pulverulent metal, the use of silver as a developing agent in acid and alkaline developers is discussed. Speaking of the black compound which is precipitated when ammonia is added to a solution containing silver nitrate and a ferrous salt, the author uncompromisingly gives the formula, $Ag_4O + Fe_3O_4$, which at least may be said to require confirmation. The chemical principles of intensification and toning, and the transformations of silver pictures by substitution, are well dealt with, and a long account of Carey Lea's researches on the allotropic modifications of silver is given. When treating of silver nitrate, Dr. Vogel gives way to patriotic bias in the form of a footnote:—

> "Als Thatsache erwähnen wir, dass das in Deutschland fabricirte Silbernitrat das reinste ist, welches geliefert wird. Ueberhaupt übertreffen ALLE deutschen Chemikalien die ausländischen weit und werden daher von allen Pharmazeuten des Auslandes hoch geschätzt. Selbst in China, Japan, Indien, Nordamerika werden deutsche Chemikalien allen übrigen vorgezogen. Wenn zuweilen in ausländischen Zeitungen Verdächtigungen derselben versucht werden, so laufen diese auf Concurrenzneid hinaus" (p. 147).

Our chemical manufacturers had better see to this!

Of fundamental importance in photography are the silver haloids, and to these we naturally turn with the greatest interest. The author admits the existence of a subchloride, but justly expresses reserve as to its formula. The "photosalts" of Carey Lea are described, and the oxychloride theory referred to, but the author omits to mention that these coloured compounds, produced by chemical methods, were discovered by the British Association Committee of 1859. Photochromy is treated of briefly, and in an earlier portion of the work (p. 9) the author distinctly asserts that permanent photography in natural colours has never been accomplished. Of course we knew this before, but it is well that the public should have the statement from such a recognized expert as Dr. Vogel. Under silver bromide will be found an account of Stas's modifications of this haloid, and after discussing the ripening of the salt in emulsions, and the action of ammonia thereon, the author arrives at the conclusion that these modifications represent different states of physical aggregation—a conclusion which most scientific photographers will endorse. The discussion of this part of the subject is, we may add, very thorough. Silver iodide is similarly treated of, and then comes a section on the influence of different substances on the sensitiveness of the silver haloids.

This last subject leads to the action of sensitizers, and some very useful tables, giving the results of experiments with all kinds of sensitizers, are here given. The subject of development, which the author divides into chemical and physical, is dealt with under this same section, and the different compounds which have been used for this purpose are enumerated. Sensitizers are again discussed after development has been disposed of, and we are sorry to see that Dr. Vogel still classifies these as chemical and optical sensitizers. The latter comprise those colouring-matters which confer on the silver haloid film a special sensitiveness for certain rays of low refrangibility, and the author gives a long account of this discovery, with which his name will always be associated. We confess to being somewhat disappointed with this section. The theory of orthochromatic photography is in a very unsatisfactory state, and we should have liked to know the author's views on this subject. By his still retaining the term "optical sensitizer," he leads us to infer that he has not abandoned the physical theory. In this case a discussion of Abney's results and a repetition of his experiments are most urgently needed. We shall, however, perhaps, be more enlightened in the practical portion of the work which is to follow the present part.

In reviewing the history of orthochromatic photography, the results of Eder are given almost *in extenso*, but the experiments of Abney and Bothamley in connection with this subject are not referred to. Perhaps these, again, are being reserved for the succeeding parts of the book. Dr. Vogel can certainly score against those who discredited his discovery in 1874, but he is hardly correct in stating that the Berlin Academy of Sciences alone recognized its importance. In NATURE, vol. x. p. 281, the value of the discovery was pointed out, and an account, of the early experiments was given. The treatment of the subject of solarisation, which follows that of "optical sensitizers," is somewhat meagre, and from the scientific point of view we should have been glad to have a more complete discussion of a topic of such fundamental importance to the theory of the photographic image. The author accepts the explanation of reversed chemical action, but he does not, we venture to think, lay sufficient stress on the important part played by the vehicle or sensitizer in this phenomenon.

After dealing with the salts of silver, we are led in due order to the compounds of mercury, lead, gold, platinum, and allied metals. The only comment we have to make is that the ordinary chemical properties of these different salts would be more in place if described in the third chapter, so that this portion of the work might be restricted to what its title indicates, viz. the chemical action of light. Nor do we understand why the author should retain the old name "platina," as his own countrymen have now generally dropped the terminal letter, while here and in America the names of all the metals of the group are made to end in "um." It certainly seems strange to a chemist of the present time to read: "hierher gehören Platina, Iridium, Palladium, Osmium, &c."

The third chapter is devoted to a description of photographic chemicals, and is more or less of the nature of an ordinary descriptive manual of chemistry, having special reference to the elements and compounds used in photography. Among the metalloids, oxygen, hydrogen, and the halogens are alone treated of. Under solvents, we have the compounds water, ethyl and methyl alcohols and some of their homologues, glycerin, ether, chloroform, benzene, &c. Then follow the acids, inorganic and organic, and bases and salts, beginning with the compounds of potassium, sodium, and ammonium, and ending with those of the earthy metals. The salts of the heavy metals, having been taken (out of order) in the preceding chapter, are not dealt with here. The author, in explaining the theory of salts, clings to the old water type, *e.g.* $\left.\begin{matrix}SO_2\\H_2\end{matrix}\right\}O_2$, $\left.\begin{matrix}NO_2\\H\end{matrix}\right\}O$, &c. Under ammonia (p. 269) we are told that in the salts of this base ammonium (NH_4) is present, "das mit Sauerstoff das Ammoniumoxyd (NH_4O) bildet." On p. 261 potash alum is written $K_2SO_4 . Al_2(SO_4)_3 + 24H_2O$, and on p. 269 ammonia alum is formulated $(NH_4)Al(SO_4)_2 + 12H_2O$. The section on reducing agents and developers treats of hydroxylamine, tannin, gallic acid, pyrogallol, hydroquinone, pyrocatechol and resorcinol, paraphenylene-diamine, phenylhydrazine, and eikonogen.

Following this section we again have some ten pages devoted to optical sensitizers, the compounds described being eosin and allied colouring-matters, cyanin, quinoline red, and chlorophyll. There are some amusing footnotes attached to this section, one of which we cannot refrain from quoting, as illustrating the author's method of dealing with the sceptics at home and abroad. After attempting once more to make clear his definition of an optical sensitizer, he admits that this definition can only be made intelligible to those

> "welche von farbigen Strahlen, d. h. Spectralfarben, und von optischer Absorption derselben, also Absorptionsspectralanalyse, eine klare Vorstellung besitzen, die leider bei sehr vielen Empirikern (und auch Wissenschaftern) die in dieser Sphäre arbeiten, vermisst wird."

Then comes the note :—

> "Wie übel es in dieser Hinsicht bestellt ist, geht daraus hervor, dass sogar ein Professor der Chemie und Physik in Berlin das Spectrum von Eosin und Eosinsilber, welche total von einander verschieden sind, als gleich erklärte, dass der gerichtliche Sachverständige Prof. Spiller in England die Behauptung aufstellte, Eosin und Chinolinroth seien identisch, und dass sogar Carey Lea meinte, ein Sonnenspectrum lasse sich durch eine Anzahl farbiger Glasstreifen ersetzen."

We do not regard it as "good form" in this country to make horrid examples of our co-workers in a book intended for the use of students. If any remarks of a polemical character had to be brought forward, there were other arenas, both here and in Germany, where Dr. Vogel might have broken a lance with his adversaries.

Under the heading "Bildträger" (image-carriers or film-producing materials), we have an account of cellulose, starch, pyroxylin, albumin, gelatine, and paper. The description of the various cellulose nitrates and their preparation is fairly complete. Under gelatine we do not find, either in this chapter or in the historical portion, any reference to the name of Maddox, who first made the use of this vehicle practicable, and laid the foundation of our modern gelatino-bromide emulsion processes. Some miscellaneous subjects which are not included in the text are added in an appendix. It must be mentioned that there are numerous prints and thirteen plates inserted in the work, some of them very beautifully executed, and introduced with the object of illustrating the various photo-etching, engraving, and printing processes, the difference between orthochromatic and ordinary plates, spectrum photography on dyed films, &c.

With respect to the work as a whole, it will be seen that it covers to a large extent the same ground as the "Ausführliches Handbuch der Photographie" of Eder, which is also a recognized standard work. In some respects Dr. Vogel's book offers advantages over the latter, but in other respects it is inferior. We miss the splendidly complete lists of references with which Eder's work abounds, and in neglecting to supply this information the author often unconsciously does himself injustice, for there is much original work included in the volume which many readers would desire to refer to in the original papers. Dr. Vogel has contributed so largely to the advancement of photography that any observation or experiment of his is entitled to the fullest consideration.

The criticisms which have been offered in the course of this notice are on minor points, but taken in their *ensemble* they indicate certain weaknesses which are to be regretted. It is obvious from the examples given that pure chemistry is not the author's strong point, and it would have been better, seeing how largely the subject is connected with this science, if he had consulted some of his chemical colleagues. The inconsistencies of formulation and classification which have been pointed out might thus have been avoided, and the work made more logically coherent. Another defect is the retention here and there of passages which look like survivals from an earlier edition. For example, we read on p. 7: "Der Collodium process verbreitete sich allgemein, wurde im Laufe der Zeit immer mehr und mehr vervollkommnet und ist jetzt der ausschliesslich angewendete." Again, on the same page: "Collodium für den Negativprocess, Albuminpapier für den Positivprocess bildeten die wichtigsten Grundlagen unserer photographischen Bilder." This may have been true at the time of the last edition (1878), but is certainly not the case now.

In concluding this notice we can only express regret that the author should have fallen into the habit, now, unfortunately, becoming only too common on the Continent and across the Atlantic, of allowing insufficient credit for, or, worse still, of ignoring altogether, work done outside his own country. The historical portion of the book hardly does justice to the labours of Fox Talbot when it is stated that "nach dem Bekanntwerden der Daguerre'schen Entdeckung suchte Talbot auch Camerabilder auf Papier aufzunehmen." If the introduction of the collodion process by Archer and Fry is considered worthy of historical record, surely the gelatino-bromide emulsion process of Maddox is at least of equal importance. When dealing with the action of light on selenium (p. 23), the author gives a description of Shelford Bidwell's experiments on telephotography, but the experiment of Ayrton and Perry having for its object the electrical transmission of moving images (*telopy*) is not referred to. When treating of optical sensitizers, he tells us that von Baeyer discovered fluoresceïn, that Caro discovered eosin, and that Jacobsen discovered quinoline red, but the reader is not informed that cyanin, one of the best special sensitizers, was given to science by Greville Williams as long ago as 1860. These and the other blemishes which we have felt bound to indicate have only to be remedied in future editions to make Dr. Vogel's book take that high position to which it is justly entitled, both on account of the vast body of useful and often original information which it contains, and the deserved reputation of the author as one of the foremost of German scientific photographers.

R. MELDOLA.

CONTRIBUTIONS TO INDIAN BOTANY.

Annals of the Royal Botanic Garden, Calcutta. Vol. II. Pp. 110; with 104 Lithographed Plates. (Calcutta: Bengal Secretariat Press, 1889.)

THE whole of this volume, like the first, with the exception of a part of the Appendix, is the work of Dr. G. King, the Director of the Calcutta Botanic Garden. It contains a monograph of the species of *Artocarpus* indigenous to British India, and a monograph of the Indo-Malayan species of *Quercus* and *Castanopsis*, both fully and excellently illustrated. The genus *Artocarpus* was founded by the Forsters (father and son, who accompanied Captain Cook on his second voyage, not brothers, as inadvertently stated by Dr. King) for the bread-fruit tree, with which they became familiar in the Pacific Islands. This they called *Artocarpus communis*, though most subsequent botanists have adopted the later Linnean name, *A. incisa;* and the younger Forster published a separate illustrated memoir on it, in German, entitled "The History and Description of the Bread-fruit Tree." Dampier, however, appears to have been the first to make this valuable tree known to Europeans. The only other familiar species of the genus is the Jak fruit (*Artocarpus integrifolia*), a prominent cultivated tree in the Malay peninsula and archipelago, and recently collected by Colonel Beddome in a wild state in the forests of the Western Ghats in the Deccan Peninsula, South India. Exclusive of this, Dr. King now describes and figures seventeen species found within the limits of British India, seven of which are described for the first time. Many of them are very handsome trees, but their wood is of little value, and, as far as their history goes, none yields an edible fruit.

The Indo-Malayan species of *Quercus* and *Castanopsis* number 82 and 22 respectively, besides some imperfectly-known species. As Dr. King remarks, there is no reason, except convenience and the desirability of not adding to the already overloaded synonymy, why all the species described under *Castanopsis* should not be placed in the section *Chlamydobalanus* of *Quercus*. Generally speaking, *Castanopsis* differs from *Quercus* in the involucre, which answers to the cup of the acorn, being prickly or tubercular, and completely inclosing the nut, and when ripe splitting irregularly to free the nut. But this distinction completely breaks down in the long series of species illustrated in the present monograph, the cup or involucre varying, in the species referred to *Quercus*, from two or three series of scales, or a discoid form, to an ovoid or globose receptacle completely enveloping the nut, and sometimes more or less prickly. In *Castanopsis*, on the other hand, the involucre is sometimes quite smooth. In foliage there is nothing to distinguish them, yet, taking the whole series of species, these Asiatic oaks exhibit a wonderful and beautiful variety in foliage and fruit, especially in the latter, being in many of them exceedingly elegant in shape and structure. There are about half-a-dozen species of the same group (*Lepidobalanus*) as the British oak, but none has quite the kind of foliage characteristic of this, and some have leaves more like an apple-tree, others almost exactly the same as the sweet chestnut. In other groups the leaves are often very large, thick, and leathery, having entire margins. Like some of the oaks of Central America, some of the Indian species have acorns of enormous size. One of the handsomest trees of the Eastern Himalayas, at elevations of 5000 to 8000 feet, is *Quercus lamellosa*. It grows from eighty to a hundred feet high, and in young vigorous specimens the conspicuously veined leaves are as much as a foot long, and the depressed spheroidal cups or involucres are 2½ inches in diameter, enveloping all but the apex of the nut, and built up of broad concentric plates, thin towards the usually fimbriate edge. The figures illustrating this species in Dr. King's monograph are partly copied from Hooker's "Illustrations of Himalayan Plants," though this fact is not mentioned, which is apparently an oversight, as all other copies that we have noticed are acknowledged. Running through the plates from the beginning, we will indicate a few of the more striking. Thus, *Quercus semecarpifolia* (Plate 15), has globose acorns with the cup reduced to a small disk at the base; *Q. serrata* (Plate 16) is remarkable for having the acorn almost buried in a cup of long narrow scales; *Q. oidocarpa* has an ovoid acorn with a closely-fitting cup, at least two-thirds of its length, and consisting of a few elegantly notched broad plates; *Q. Kunstleri* (Plate 31) has the long narrow acorns in spikes, and seated in shallow cups similar to those of the common oak; *Q. grandifrons* (Plate 35), has broad leaves sometimes 15 to 18 inches long, and *Q. Scortechinii*, on the same plate, has a mossy cup containing a huge obovoid acorn; *Q. platycarpa* (Plate 65) has very flat acorns half immersed in the thin cup, and about an inch and a half across; *Q. cyclophora* (Plate 67) is somewhat similar, but the cup is very thick, and composed of very numerous rounded scales; in *Q. reflexa* (Plate 72) the cone-shaped acorns are borne in clusters, and entirely inclosed in a thin cup completely beset with short recurved prickles; *Q. Junghunii* (Plate 73) is so like a *Castanopsis* in its unsymmetrical prickly fruit as to be undistinguishable from it; and, finally, *Q. Beccariana* (Plate 78) presents a fruit more resembling the nest of a solitary wasp than an acorn, being an oblong body about three inches long by two broad, the cup entirely inclosing the acorn, and consisting of about four very broad overlapping layers of scales with thin edges.

Apart from the practical use of Dr. King's monograph, this long series of figures of the Indian oaks offers a most interesting opportunity to the student of evolution, especially if it be remembered that the figures are portraits of individual specimens, not embodiments of "species," and that another series of specimens of the same "species" would probably exhibit many intermediate modifications.

It is fortunate for science that Dr. King, supported by the Indian Government, should devote his time and talents to the elucidation of such large and difficult arboreous genera as *Ficus* and *Quercus*. These monographs are of the greatest value to botanists generally, and one would say specially valuable to the officers of the Forest Department of India. The drawings by native artists, are, as already stated, with few exceptions, original, very faithfully executed, and mostly sufficient for purposes of identification. The lithography, too, by the Government School of Art, Calcutta, deserves praise, comparing favourably with much of the work done in this country. We may perhaps be permitted to call attention to the fact that the copies we have seen of the present volume are printed on smaller paper than the first, which detracts from their appearance on the shelves.

W. BOTTING HEMSLEY.

OUR BOOK SHELF.

Elementary Text-book of Trigonometry. By R. H. Pinkerton, M.A. New Edition. (London: Blackie and Son, Limited, 1890.)

THIS is a new and enlarged edition of this excellent elementary text-book. An account of the method of proportional parts and of its application to logarithmic and trigonometrical tables has been added, together with a collection of questions in trigonometry which have been set during the last ten years in examination papers of the Science and Art Department in mathematics, second stage, and of the London University Intermediate Examination in Arts.

Instead of each paper being given separately, the questions in them are arranged under headings, and the source from which each is taken is indicated; and, to avoid the necessity of a book of tables, the logarithms required for their solution are given in a table at the end.

Throughout the work the author has explained most clearly and fully every part that might in any way prove difficult to the beginner, and he has added numerous well-chosen examples at the conclusion of each chapter.

Higher Geometry. Containing an Introduction to Modern Geometry and Elementary Geometrical Conics. By W. J. Macdonald, M.A. (Edinburgh: James Thin, 1890.)

Many of the more advanced theorems in geometry, which are not very often treated to any extent in elementary books, are here dealt with. The author's idea seems to have been to connect the theorems together as much as possible in a continuous and graduated series; and this, together with the fact that they are worked out in a neat and concise form, will greatly add to the utility of the book.

The latter part of the work treats of geometrical conics. Although it does not contain so many propositions as many of the elementary works on the subject, yet the author has included in it all the most important propositions, thus making it a brief course for those who are about to attack the subject for the first time. Many problems have been put in here and there among the propositions, and an index to definitions, which has been added at the end, ought to prove handy for reference.

Nautical Surveying. By the late Vice-Admiral Shortland, LL.D. (London: Macmillan and Co., 1890.)

This volume, which is published by the late Admiral's widow and children, relates chiefly to the errors to which surveyors and their instruments are liable. It shows how these errors are to be found and corrected. The book is not one for beginners, but appears to have been written rather for surveyors themselves, after they have become thoroughly acquainted with the more practical and simple surveying. Every branch of surveying is thoroughly discussed, but at such length that the work would be of little practical use to a beginner. An index would greatly improve the book.

An American Geological Railway Guide. By James Macfarlane, Ph.D. Second Edition. (New York: D. Appleton and Co., 1890.)

The first edition of this book appeared in 1878, and the object of the compiler was to provide travellers with a hand-book from which they might learn the geological structure of every district in America intersected by railways. Many changes and additions have, of course, become necessary since 1878; and at the time of his death, in 1885, Dr. Macfarlane had made extensive preparations for a new edition. His work has been completed by his son, Mr. James R. Macfarlane, who has had the advantage of being aided by various competent contributors and advisers. The idea of the book is excellent, and has been carried out with great care and intelligence. It relates to the Dominion of Canada, as well as to the United States, and anyone travelling in these countries may find out at once, by turning to the proper page of this volume, the geological significance of phenomena that may happen to attract attention during the journey. The work ought to do much to encourage a liking for geology in the New World; and even professional geologists may find it useful for occasional reference.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* Nature. *No notice is taken of anonymous communications.*]

Araucaria Cones.

I should be glad to know through any of your correspondents whether the Araucaria is often known to bear cones in the British Islands?

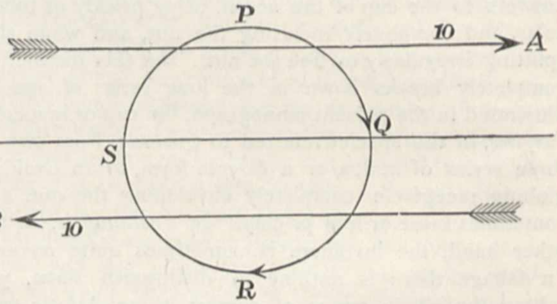
A plant of the common Araucaria in my garden here was blown down in a severe gale two years ago. It was a well-grown plant about 20 feet high, and very healthy. I replaced it on the spot, supporting it by ropes well pinned down.

This autumn it has come out covered with cones all over the top branches. I have never seen them before, and I think they must be rare. They are terminal on the branches which bear them—sit upright upon them—and are of a very handsome ovate form. No scales are visible—the actual seed-vessels being covered and concealed by a thick coating of modified leaves or needles, narrowed, elongated, and terminating in hooked bristles. Argyll.

Inveraray.

On the Soaring of Birds.

It is a pity that so many of your correspondents on this subject fail to grasp the elementary and self-evident fact that no common horizontal movement, relatively to the surface of the earth, of the air in which a bird is immersed can by any possibility enable it to soar. Upward convection-currents and upward slants may have something to do with the question, as may also the existence of different horizontal currents.



Thus, let there be two horizontal currents, A and B, in opposite directions, of 10 miles an hour each, and let a bird arrive at Q, down the path PQ, moving through the air at Q with a velocity of 5 miles an hour. On passing into the current B, it has a velocity relative to B of 20 miles an hour in the direction of current A, and of 5 miles an hour in a perpendicular direction. By proper adjustment of the wings, this relative velocity can be converted into work, and spent in lifting the bird to a higher altitude, so that, on arriving at S, its velocity relatively to the A current is again reduced to 5 miles an hour, and the circumstances at Q are exactly reproduced. When Mr. Magnus Blix began his communication (August 21, vol. xlii. p. 397) I expected that he was going to suggest this explanation; but though he commences with the supposition of the bird passing from one current to another, he goes on as if the bird afterwards remained constantly in one current.

An upward convection-current, as suggested by Mr. O. Fisher (September 4, p. 457), is, no doubt, a *vera causa* for a bird's being assisted in floating; but has Mr. Fisher reflected or calculated whether it is an adequate cause for actual soaring? Natural convection-currents can seldom have a rate of more than a few feet per second, whereas a velocity of 20 feet per second would be required to support a bird which weighed 1 pound for every square foot of supporting surface—wing, tail, and body. It is true that, in soaring, the rapid horizontal motion probably increases the horizontal support of the air, just as the transverse motion of the sails of a windmill through the air-current propelling them increases the horizontal pressure, as is proved by the fact that windmills evolve more work than they would be calculated to evolve if the pressure were the same as when they are at rest. The great difficulty, however, in all these explanations is that birds soar under circumstances which render these explanations inapplicable. In the open ocean, where a steady wind is blowing, where there are certainly no upward convection-currents, and where, equally certainly, there are no cross currents or diverse horizontal currents, where from the smoke of the steamer it is obvious that the air is moving *quasi* rigidly—that is, without perceptible internal motion—birds nevertheless soar to perfection.

As definite instances, however, are of more value than abstract statements, I subjoin copy of an actual observation made by myself and a friend some years ago. As the eyes become tired and dazzled in following the same object intently, we took it in turns to watch the bird, exchanging watch when the bird was so situated as to be easily identified.

"*Walmer Castle*, May 4, 1876" (on voyage home from the Cape).—"Observed a common gull for 11 minutes by the watch, following the ship without flapping a wing. The wings were used occasionally, but with a slow motion, as if for balancing or changing direction. The tail was occasionally altered in position when the bird ascended or descended.

"During the time of observation, the bird sometimes followed the ship with a steady motion, without apparently changing its height or velocity. Occasionally it veered to right or left a distance of a hundred yards or so, it rose and fell in altitude, changed its direction, fell back or overtook the vessel, without any apparent muscular effort. In overtaking the ship it seemed rather to rise than to fall" (though this is very difficult to judge of, because one cannot allow with any exactness for perspective or parallactic change of position).

"The direction of the wind, relatively to the ship, was E. by N., its velocity, as estimated by Captain Webster, was 7. The course of the ship was N.E. ½ N., its rate 8 knots.

"While following the ship, the bird was often within 15 yards, at which time there was no perceptible inequality in the horizontal angles of its wings, and no perceptible angle made by the axis of its body with the ship's keel.

"In moving to right or left out of the ship's course, there was generally a change of level." F. GUTHRIE.

South African College, Cape Town.

The Value of Attractive Characters to Fungi.

THE importance of attractive colours and odours and of modifications of form to flowering plants is now perfectly understood; but the value of attractive characters to fungi has received comparatively little recognition. At first sight it would seem unnecessary that a plant, insusceptible of fertilization, should possess characters apparently designed to enlist living creatures in its service: there is no pollen for them to carry, and no ripe seeds for them to distribute, and yet attractive characters, such as colour, taste, and odour, are extremely well marked.

The colours which fungi exhibit include almost every hue from white to black. We have the brilliant red of the Peziza cups; the orange-scarlet of the *Amanita muscarius*, with its cap gaily speckled with white; the crimson of the *Russula emetica;* the rich yellow of the *Cantharellus cibarius;* the blue of the bruised *Boletus luridus;* the amethyst of the *Agaricus laccatus;* and the dark green of the bruised *Lactarius deliciosus*, with every possible shade to the deepest jet. But not only have fungi colours that are attractive by day; some, like the *Agaricus olearius*, are phosphorescent by night. Many tropical species light up the jungle in the hours of darkness; and in this country the coal-mines are often found illuminated by one of the polypores which propagates itself on the timbers of the workings.

The tastes and odours of fungi are equally varied and attractive. Many Agarics have an odour of fresh meal; the *Hydnum repandum* rejoices in the flavour of oysters; the *Armillaria mucidus* in that of nuts; the yellow Chanterelle in that of apricots; others have the scents of various flowers, such as the violet and woodruff; or of aromatics like anise; while a large number have an indescribable damp cellar or fungus smell, such as slugs delight in. Many, like the shameless stinkhorn, *Phallus impudicus*, emit an intolerable stench which so strongly resembles

"the carrion of some woodland thing"

that blow-flies and ravens quickly find it out.

There can be little doubt that these are attractive characters. What, then, can be the service which these characters induce animals to perform for fungi? To answer this let us review briefly the life-history of any fungus possessing characters of an attractive kind.

The common mushroom, *Psalliota campestris*, is particularly agreeable to sheep and oxen, and is abundant in autumn in rich pastures. Although there is still much in our knowledge of its life-history that is incomplete, yet it is evidently composed of two main periods: first, a parasitic period passed in the body of an animal host; and secondly, a saprophytic period passed on some suitable organic soil. Let us sow the spores of a ripe mushroom as carefully as we may, none of them will grow: the first stage of the mushroom's existence must be passed in the body of an animal host; and as horses, sheep, and oxen are all readily attracted by its taste and mealy smell, it has never any difficulty in finding a host to take it in.

When once the spores have passed from the body of the host, they produce a mycelium, from which the future mushroom is formed. The connection between fungi and animal droppings is a matter of very early observation, and our forefathers were wont to believe that certain evil species came from the body of the Wicked One, and familiarly called them Tode's-stools, or Devil's droppings.

In this division of the life-history of fungi I believe we have the key to the value of attractive characters. Horses, oxen, sheep, foxes, squirrels, moles, birds, snails, and insects are all attracted by appropriate scents, tastes, and colours; and the forms and habitats of fungi are those which have best succeeded in attracting their particular hosts. There is no living being either great enough or small enough to escape the attentions of these plants in their ceaseless endeavours to attract; and among fungi, just as among flowering plants, every variation of form, scent, and colour has been perpetuated and developed, because it has been successful in attracting and in thus securing the multiplication of the species.

The subject is one, I think, that requires the gathering together of much individual observation in all parts of the world; and it would be well if those who have the opportunity would note at the time the name of the fungus and its observed host, and if students of biology who possess facilities for laboratory work would follow the matter still further by artificial cultures, and so determine the changes that take place in the body of the host, and the course of the alternating sexual and agamous generations. CHARLES R. STRATON.

Wilton, Salisbury.

Extraordinary Flight of Leaves.

MR. SHAW'S letter (NATURE, vol. xlii. p. 637) is a curiously corroborative fact in support of Mr. Wallace's theory of the wind being an agent for the dispersion of seeds, which he so strongly urges in his book, "Darwinism," to account for the universal distribution of many plants. For if, as the letter intimates, such weighty objects as oak-leaves can be conveyed through the air in such vast numbers as to cover an area two miles long by one in width, then it would not require a wide stretch of imagination to conceive that miniature objects like seeds, delicately winged for flight by Nature as so many are, might travel by thousands for hundreds of miles in favourable winds.

Speaking of leaves, on the morning of that severe frost last week, I observed a horse-chestnut, in full foliage, showering down its leaves with extraordinary rapidity, so that in three hours the tree was bare—half the leaves were yellow, the rest still quite green. The gardener gathered twelve large barrow-loads from beneath it. R. HAIG THOMAS.

November 1.

Kœnig's Superior Beats.

THE interesting experiments of Dr. Kœnig at the meeting of the Physical Society on May 16, and described in NATURE (vol. xlii. p. 190), have induced me to offer your readers the following view of Dr. Kœnig's superior beats and beat-tones.

If we look for the physical cause of the superior beats, we find nothing but the inferior beats themselves to build upon. But if we can admit that inferior beats may be in an ascending and descending scale of intensities, we have at once the structure of a beat which may be appropriately termed a superior beat, viz. the beat resulting from the periodic recurrence of a maximum and minimum inferior beat, in the same manner as the inferior beat is the result of the periodic recurrence of a maximum and minimum vibration.

Now, all inferior beats can be divided into two classes—similar and dissimilar beats. The four beats given by 128 vibrations, with 124 vibrations in the second, are perfectly similar beats, because the ratio $\frac{128}{124}$ is $\frac{4 \times 32}{4 \times 31}$, *i.e.* the ratio in its simplest form is $\frac{32}{31}$, giving one beat under precisely the same conditions of phase coincidence, at each quarter of a second. But the four beats given by 127 vibrations, with 123 in the second, are dissimilar beats; because the two S.H.M.'s, though together four times in the same phase in the second, these coincidences are at different parts of the wave-length; just as the hands of a watch are twelve times together in one complete revolution of the hour hand, but at different parts of the dial.

Another example will complete my meaning. The example is an experiment described by Dr. Kœnig in his valuable work, "Expériences d'Acoustique," with the two tuning-forks making 75 and 40 vibrations in the second, English measure. Five distinct beats were heard along with the beat-sound of 35 beats in the second. Here we have the interval-ratio $\frac{75}{40} = \frac{5 \times 15}{5 \times 8}$ showing (15 - 8) or 7 dissimilar beats recurring five times in the second. And we have apparently no other physical quantities to deal with. So that it appears a necessary conclusion that the seven dissimilar beats are in an undulatory order of intensity, in order to account for the five superior beats of Dr. Kœnig. The construction is then simple. Each recurrence of the 7 dissimilar beats is marked by a beat, and there are 5 of these in the second. These are the 5 superior beats formed out of the 5 × 7, or 35 inferior beats.

For this reason I have introduced the term cycle for the resultant generating curve formed by two S.H.M.'s of unequal periodic times. The curve or cycle is traced by the extremity of one generating radius revolving about the moving extremity of the other as a centre, and is complete when the two radii return to their original position. The cycle may be of long or short period. In the octave interval it is very short. With a nearly perfect unison it is very long. In every cycle there is at least one beat. There may be many dissimilar beats, but there cannot be any similar beats in the same cycle. The superior beat, number is, then, the number of cycles in the second; and the inferior beat-number is the product of the number of cycles into the number of dissimilar beats in the cycle.

According to this theory, therefore, the superior beats heard on the occasion referred to should be accounted for as follows:—

(1) When the two tuning-forks were executing 120 and 64 vibrations respectively in the second, eight superior beats were heard, because $\frac{120}{64} = \frac{8 \times 15}{8 \times 8}$ gives 8 cycles of 7 dissimilar beats, in each second of time.

(2) When the forks were making 96 and 64 vibrations, or the fifth interval, 3/2, it would be more correct to say that there were no superior beats than to say that "the inferior and superior beats agree in frequency." Because the ratio $\frac{96}{64} = \frac{32 \times 3}{32 \times 2}$ shows 32 cycles of only one beat in each.

In like manner all the other experiments given in detail in the text of June 19 (p. 190) can be accounted for. But there is an explanatory example given by Dr. S. P. Thompson, which, if verified by experiment, must be fatal to my theory of Kœnig's superior beats. This illustration gives 92 inferior beats and 8 superior beats in 492 and 100 vibrations of the primaries; while, according to the theory just sketched, $\frac{492}{100} = \frac{4 \times 123}{4 \times 25}$, shows only 4 superior beats with 392 inferior beats. Only Dr. Kœnig's valuable instruments could satisfactorily decide between them, and I should be glad to know if my building cannot stand. On the other hand, Dr. Molloy's elegant experiment, described in NATURE (vol. xlii. p. 246), is in favour of the cycles of dissimilar beats, inasmuch as these account for his three beats directly from the primaries without the aid of secondaries. In this experiment the primaries were 384 and 255 vibrations in the second; and the ratio $\frac{384}{255} = \frac{3 \times 128}{3 \times 85}$ shows 3 superior beats with 43 dissimilar beats in 129 inferior beats.

Stonyhurst College. WALTER SIDGREAVES.

THE CELL THEORY, PAST AND PRESENT.[1]

I.

IN taking the chair at the first general meeting of the Scottish Microscopical Society, I would offer to the members my hearty thanks for having done me the honour to choose me as the President under whom the work of the Society is to be inaugurated, and during whose incumbency the Society is to begin to substantiate its claim to have an existence amongst the scientific Societies in Scotland.

As myself engaged in biological studies, it is only natural that my attention should have been more particularly directed to the use of the microscope in connection with them, and to the influence which it has exercised on their advancement. Since the time of Hooke, Grew, Malpighi, and Leeuwenhoek, this influence has been continuous and progressive. The improvements in the instrument during the present century have led to discoveries of the utmost value in the structure of plants and animals, and to generalizations of a wide-reaching importance.

One of, if not the most fundamental of these discoveries, was the recognition of the anatomical unit, which we call a *cell*, as a common element in the structure of organisms. Our conceptions of the structure of cells, of the relative function of their constituent parts, and the mode in which cells are developed and multiply, has varied very materially from time to time. I purpose to pass in review those aspects of the subject which have attained prominence, and have influenced the course of investigation.

Dr. Robert Hooke was one of the first men of science to employ the microscope in the study of the structure of plants and animals. A chapter in his "Micrographia" (London, 1665) is entitled "Of the Schematisme or Texture of Cork and of the Cells and Pores of some other such frothy Bodies." This is probably the first use of the word cell in histological description. In the course of this chapter he refers to the lightness of cork, which he compares with froth, or an empty honeycomb. Its substance, he says, is wholly filled with air, which "is perfectly enclosed in little Boxes or Cells distinct from one another." Further, he gives an idea of the dimensions of these cells by stating that about sixty could be placed endways in the $\frac{1}{18}$th part of an inch, and that 1,166,400 could be placed in a square inch. He thinks that they are the channels through which the juices of the plant are conveyed.

The term cell was also employed to express a definite morphological unit by Dr. Nehemiah Grew,[2] who shares with Malpighi the glory of being one of the fathers of vegetable physiology. When describing in his "Anatomy of Plants" the skin of the root (p. 62), he says the parenchymous material is "frequently constructed of exceeding little *Cells* or *Bladders*, which, in some Roots, as of Asparagus, cut traverse, and, viewed through a Microscope, are plainly visible. These Bladders are of different sizes; in Buglos larger, in Asparagus less, and sometimes they coincide and disappear."

In his account of the parenchyma of the bark he again uses the word cells (p. 64), and says that "each is bounded within itself, so that the *Parenchyma* of the *Barque* is much the same thing as to its conformation, which the Froth of *Beer* or *Eggs* is as a fluid, or a piece of fine *Manchet* as a fixed body." These cells are so small as "scarcely, without the microscope, to be discerned;" more usually, however, Grew applies to them the term bladders or vesicles. In the chapter on the vegetation of roots he speaks of the sap swelling and dilating the bladders, and

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[1] The Inaugural Address delivered to the Scottish Microscopical Society, by Sir William Turner, F.R.SS. L. and E., President of the Society.

[2] "The Anatomy of Plants," London, 2nd ed., 1682. The several books into which Grew divided his treatise were presented to the Royal Society of London at various dates between 1671 and 1675.

as being fermented therein, as transmitted from bladder to bladder, and leaving certain of its principles adhering to them. He thus recognized that the cells or bladders played an important part in the nutrition of the plant. Almost, indeed, he seemed to have grasped the idea that they exercised a selective or secreting influence; for, in describing the parenchyma of the fruit of the lemon, he speaks (p. 180) of "those little *Cells* which contain the essential Oyl of the fruit," whilst, he says, in other bladders "lies the acid juyce of the limon."

Malpighi, whose work on the anatomy of plants ("Anatome Plantarum," London, 1675) was almost cotemporaneous with the treatise of Grew, had also seen the structures which Grew named cells or bladders, and had designated them *utriculi*, and believed that they could be separated from each other. In a subsequent treatise ("Opera," vol. ii. p. 41, 1686) he described the lobules of fat in animals as consisting of adipose vesicles.

Leeuwenhoek, in the course of his microscopic inquiries into the structure of plants, gave the name of *globules* to many of the objects which we now term cells, though he expressly states that they were not perfect spheres.[1]

Clopton Havers, in his treatise on the skeleton, described ("Osteologia nova," p. 167, 1691) the vesicular structure of the marrow, and compared it, when seen under the microscope, to a heap of pearls.

Alex. Monro, *primus*, in his work on the bones ("Anatomy of the Humane Bones," Edinburgh, 1st ed., 1726; 2nd ed., 1732), when writing on the medullary structure, stated that it is subdivided "into communicating vesicular Cells, in which the Marrow is contained. Hence it is that the Marrow, when hardned and viewed with a Microscope, appears like a Cluster of small Pearls. This Texture is much the same as what obtains in the other cellular parts of the Body where Fat is collected, only that the Cells containing the Marrow are smaller than those of the *Tunica adiposa* or *cellulosa* elsewhere."

Caspar F. Wolff[2] also recognized that fat was contained in small vesicles, surrounded by a fine membrane. He conceived also that the developing organs, both of plants and animals, consisted of a viscous substance which contained cavities, cells, or bladders which communicated with each other.

Fontana figured the fat vesicles, both free and surrounded by the fibres of the areolar tissue.[3]

Mirbel, in his botanical writings,[4] published at the beginning of the present century, stated that vegetables were composed largely of cells. He described *le tissu cellulaire* as composed of *les cellules*, which were contiguous with each other, so that the walls were in common. These walls were extremely thin and translucent, and sometimes riddled with pores. The term cells was also used both by his contemporaries and immediate successors in their writings on the anatomy of plants.

But anatomists experienced much greater difficulty in distinguishing the presence of cells in the textures of animals. It is true that from the time of Malpighi and Leeuwenhoek, the globules or particles had been recognized in the blood, but it is only within a comparatively recent period that their cellular structure was determined. Both Bichat ("Anatomie générale," Paris, 1812) and Béclard ("Élémens d'Anatomie générale," Paris, 1823), in their important treatises on general anatomy, made no reference to cells as elements of the tissues. Both these authors had chapters *du tissu cellulaire* or *du système cellulaire*, a term which had been in use from the early part of the last century. But by the tela cellulosa or cellular tissue, anatomists meant that form of tissue which we now more appropriately call areolar tissue; the so-called cells of which are not microscopic closed vesicles, but areolæ or spaces bounded by the fibres or laminæ of which the tissue is chiefly composed.[1] Béclard, in his description of the adipose tissue, stated that the lobules of fat consisted of microscopic vesicles $\frac{1}{100}$ to $\frac{1}{800}$ of an inch in diameter. The vesicles, he says, have walls, but they are so thin as to be indistinguishable. The presence of organized vesicles or globules in the tissues of animals had thus been recognized, but it needed further observations and facts in order to bring them into association with the cells of vegetable tissue.

This was supplied by the discovery in 1831 by the great English botanist, Robert Brown, of the "nucleus" or "areola" in the cells of the epidermis, and other tissues in Orchideæ and many other families of plants.[2] Following closely upon this discovery were the observations of Schleiden, published in 1838 ("Beiträge zur Phytogenesis," *Müller's Archiv*, p. 137, 1838), that the nucleus was a universal elementary organ in vegetables. Schleiden also came to the conclusion that the nucleus must hold some close relation to the development of the cell itself, and he consequently called the nucleus a "cytoblast."[3] Schleiden further discovered that the cytoblasts contained one or more minute circumscribed "spots," or "rings," or "points," which he considered to be formed earlier than the cytoblasts, and which were regarded by him as hollow globules, and were subsequently named by Schwann "nucleoli."

The cellular structure of some of the animal tissues had also begun to be recognized. Turpin had noticed the resemblance between the epithelium corpuscles found in vaginal discharges and the cells of plants. Johannes Müller had discovered that the chorda dorsalis of fishes was composed of separate cells provided with distinct walls, though he did not detect a nucleus in them. Purkinje, Von Baer, Rudolph Wagner, Coste, and Wharton Jones had seen the germinal vesicle within the animal ovum. E. H. Schultz had observed the nucleus in the blood globules, and Valentin and Henle had seen it in the cells of the epidermis. The way was thus prepared for a fuller recognition of the essential correspondence between the elementary tissues of plants and animals and for a wider generalization. Science had not long to wait for an observer who could take a comprehensive grasp of the whole subject; and in 1839 Theodore Schwann published[4] his famous researches into the structure of animals and plants, in which he announced the important generalization that the tissues of the animal body are composed of cells, or of materials derived from cells:—

"That there is one universal principle of development for the elementary part of organisms, however different, and that this principle is the formation of cells."

Both Schleiden and Schwann entertained the idea, which had long before been present in the mind of Grew, that a cell was a microscopic bladder or vesicle. In its typical shape they regarded it as globular or ovoid, though capable of undergoing many changes of form. This vesicle possessed a cell-membrane or wall, which enclosed

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[1] Samuel Hoole, who translated many of Leeuwenhoek's writings (London, 1799, part 2, p. 178), when describing Fig. 11, on Pl. vi., says that the globules of meal are enclosed as it were in cells, and that some of those cells are represented at H in the figure. Leeuwenhoek, himself, however, in his description of the same figure ("Epistolæ physiologicæ," Delphis, 1719, p. 25), does not use the word cellula.

[2] "Theoria Generationis," editio nova, 1774; Commentary "Ueber die Nutritionskraft," by Blumenbach and Born, St. Petersburg, 1789.

[3] See his essay "Sur la structure primitive du corps animal" in his "Traité sur le vénin de la Vipère," Florence, 1781 (Pl. viii., Figs. 19, 20).

[4] "Traité d'Anatomie et de Physiologie végétale," t. i., Paris, An. x.; "Exposition de la Théorie de l'organisation végétale," Paris, 1809. Ch. Robin, in the article "Cellule," "Dict. Encyclop. des Sciences médicales," Paris, 1873, credits Mirbel with having introduced the term "cellules," but the extracts given in the text show that its English equivalent, cells, had been in use for upwards of a century before Mirbel wrote.

[1] The term cellular tissue was originally applied to this texture from a fancied resemblance to the proper cell tissue of plants; the walls of the cells of which were believed to be formed of a framework of fine fibres.

[2] "Organs and Mode of Fecundation in Orchideæ and Asclepiadeæ," Trans Linn. Soc., vol. xvi., 1833; reprinted in "Miscellaneous Botanical Works," vol. i. p. 511. Ray Society edition.

[3] Fontana (*op. cit.*) figured the "globules" or scales of the epidermis, in which he recognized the nucleus, but he neither gave it a special name, nor knew its importance (Plate i., Figs. 8, 9, 10.)

[4] "Mikroskopische Untersuchungen," 1839; and preliminary notices in Froriep's *Notizen*, 1838.

contents that were either fluid or somewhat more consistent. Either attached to the wall or embedded in it was the nucleus, which in its turn contained the nucleolus. Schwann, however, recognized (p. 176 of Sydenham Society's translation of Schwann's memoir) that many cells did not exhibit any appearance of a cell-membrane, but seemed to be solid, and had their external layer somewhat more compact. As showing, however, the importance which Schwann attached to the cell-wall, I should state that he regarded the chemical changes or metabolic phenomena as he termed them, as being chiefly produced by the cell-membrane, though the nucleus might participate. He explained the distinction between the character of the cell contents and the cytoblastema external to the cell, to the power exercised by the cell-membrane of chemically altering the substances which it is either in contact with or has imbibed, and also of separating them so that certain substances appear on the inner and others on the outer surface of that membrane. In this way, he accounted for the secretion of urea by the cells lining the uriniferous tubes, and for the changes which not unfrequently take place in the cell-membrane itself by thickening or deposition of layers on or within it.

Schwann described the nucleus as either solid or hollow and vesicular, in the latter case being surrounded by a smooth structureless membrane; whilst the contents of the nucleus, other than the nucleoli, were in his view either pellucid or very minutely granulous.

Both Schleiden and Schwann conceived that in the formation of a nucleus a nucleolus was first produced, that around it new molecules were deposited for a certain distance, and then a nucleus was formed. When the nucleus had reached a certain stage of development, new molecules were deposited upon its exterior so as to form a stratum, which when thin was developed into a cell-membrane, but when thick only its outer portion became consolidated into a cell-membrane. Immediately the membrane became consolidated its expansion proceeded by the progressive reception of new molecules; the cell-wall separated from the cell nucleus, and a vesicle was formed; the intermediate space at the same time became filled with fluid, which constituted the cell contents.

Schleiden contented himself with little more than a simple statement of what he conceived to be the process of cell formation in plants; but Schwann entered into an elaborate survey of cell-life both in animals and plants, and founded on it a theory of cells applicable to all organisms.

Schwann conceived that there existed in organized bodies a solid amorphous or fluid substance to which he gave the name *cytoblastema;* this substance might be contained either within cells already existing, or else be situated in the interspaces between cells; and he believed that the cytoblastema for the lymph and blood corpuscles is the fluid lymph-plasma and liquor sanguinis in which these corpuscles float. He held that in the cytoblastema new cells are formed in the manner just described. In animals he says it is rare for cells to arise within pre-existing cells; more usually they arise in a cytoblastema external to the cells already present. Schleiden, on the other hand, maintained that in plants new cells were never formed in the intercellular substance, but only within pre-existing cells. The idea obviously present in the mind of Schwann was that the process of cell formation in a cytoblastema had some affinity with that of crystallization. He figuratively compares the cytoblastema to a mother-liquid in which crystals are formed. He speaks of molecules being deposited around a nucleolus to form a nucleus; of a nucleus growing by a continuous deposition of new molecules between those already existing; and of the cell being formed around the nucleus by a progressive deposition of new molecules; and in more than one passage he indicated that this deposition is a precipitation. He obviously considered the principle of formation of the cell around the nucleus as the same as that of the nucleus around the nucleolus, a process which Valentin subsequently described as heterogeneous circum-position.

But Schwann at the same time showed that, with reference to the plastic phenomena, cells differed from crystals in form, structure, and mode of growth; for whilst a crystal increases only by the external apposition of new particles, a cell grows both by that method and by the intussusception of new matter between the particles already deposited. The difference, he says, is yet more marked in the metabolic phenomena, which he conceived to be quite peculiar to cells. Cells and crystals, however, he considered resembled each other in this point, that solid bodies of a definite and regular shape are formed in a fluid at the expense of a substance held in solution by that fluid, for both attract the substance dissolved in the fluid. Schwann concluded his memoir by advancing, as a possible hypothesis, the view that organisms are nothing but the form under which substances capable of imbibition crystallize; and although this hypothesis involved very much that is uncertain and paradoxical, yet he considered it to be compatible with the most important phenomena of organic life. Schwann inclined, therefore, to a physico-chemical explanation of cell-formation and cell-growth.

Shortly after the publication of Schwann's famous memoir, Henle, who had for some years been engaged in microscopic investigations on the tissues, published his well-known treatise on general anatomy.[1] He attached great importance in cell formation to extremely minute particles, $\frac{1}{6000}$ to $\frac{1}{12000}$ of an inch in diameter, which he called *elementary granules*. He conceived that these appeared in a blastema, that several aggregated together to form a nucleus, in connection with which he thought it not improbable that a cell subsequently formed. He looked upon the elementary granules as the first and most general morphological elements of the animal tissues, and he regarded them as vesicles consisting of excessively minute particles of oil coated with a film of albumen. It should be stated that Henle's observations on cell formation were conducted to a large extent on the products of inflammation, and on the lymph and chyle, in all of which fatty and granular particles abound.

As regards the part which the nucleus plays in the process of cell formation, both Schleiden and Schwann regarded it as of prime importance, though in the subsequent life of the cell they considered that its function terminated. Schleiden stated that, subject to certain exceptions which he enumerated, it is rare for the cytoblast to accompany the cell through its entire vital process—that it is often absorbed either in its original place, or cast off as a useless member, and dissolved in the cavity of the cell. Schwann, whilst contending for the exceedingly frequent, if not absolutely universal, presence of the nucleus, yet held that in the course of time it usually became absorbed and disappeared, so that it had no permanent influence either on the life of the cell or the reproduction of young cells, though he recognized that it remained in the blood corpuscles of some animals. Henle, again, maintained that, as there are nuclei without nucleoli, so also cells exist without nuclei, and that new cells may arise without the least trace of cytoblasts.

At about the same time, and also immediately after the publication of the important investigations by these eminent German observers, a young graduate of medicine of the University of Edinburgh, Dr. Martin Barry, stimulated, he says, by the researches and encouraged by the friendship of Johannes Müller, Ehrenberg, Rudolph Wagner, and Schwann, undertook elaborate researches into the structure of the ovum, more especially in mammals. His results were published in a series of memoirs printed in the Transactions of the Royal Society

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[1] "Allgemeine Anatomie," Leipzig, 1841; also French translation by Jourdan in "Encyclopédie Anatomique," vols. vi., vii., Paris, 1843.

of London from 1838 to 1841.[1] In these embryological memoirs, Barry announced several important discoveries. In his first memoir (1838) he pointed out that the germinal vesicle which had been discovered in the mammalian ovum by M. Coste and Mr. Wharton Jones was the first part of the ovum to be formed both in mammals and birds, and he thought that this was probably the case throughout the animal kingdom. In his second memoir (1839) he extended to the mammalian ovum an observation which had been made by Prevost and Dumas on the ovum of the frog, and by Rusconi on the ovum in osseous fish. He described the formation within the rabbit's ovum of the body which he named, and which has been known since his time as the mulberry-like structure. This body arose at first as two vesicles, then as four, and so on in multiple progression, so that Barry was the first to recognize in the ovum of mammals the process which we now know as the segmentation of the yelk. He showed that the vesicles of the mulberry body were cells, and that each contained a pellucid nucleus, and that each nucleus presented a nucleolus. Further, these vesicles arranged themselves as a layer within the zona pellucida.

Barry's third memoir was published in 1840, and as he gave it the subsidiary title of "A Contribution to the Physiology of Cells," it is clear that he regarded his embryological inquiries as having an important bearing on the facts of cell-formation and function. He repeated his observations on the formation of the mulberry-like body, and now recognized that its component cells had been derived from the germinal vesicle, the contents of which entered at first into the formation of two cells, each of which presented a nucleus which resolved itself into other cells, and by a repetition of this process, the cells within the ovum became greatly augmented in number. Further, he stated that the whole embryo at a subsequent period is composed of cells, filled with the foundations of other cells. Although we may not agree with all the details given by Barry in his account of these observations, yet there can be no doubt that he had early recognized the important fact, that in animals new cells arose within pre-existing cells, as Schleiden had affirmed to be the case in plants, and that the nucleus acted as an important centre for the production of young cells. In recognizing the endogenous reproduction of young cells in animals, Barry made an important advance on the view entertained by Schwann, who regarded the endogenous production of cells as quite exceptional amongst animals.

In this same memoir Barry incidentally mentioned that he saw in the ovum of the rabbit a cleft or orifice in the zona pellucida, and that on one occasion he observed what he believed to be the head of a spermatozoon within the orifice. Two years afterwards he read to the Royal Society (Phil. Trans., vol. cxxxiii.; read December 8, 1842) a short paper, in which he announced that he had seen a number of spermatozoa within the ova of the rabbit, and in October 1843 he published a figure of an ovum with spermatozoa in its interior ("On Fissiparous Generation," *Edin. New Phil. Journ.*, October 1843).

In a memoir on the corpuscles of the blood, published in 1841, Barry announced a still more definite conception of the function of the nucleus. He directly traversed the statement of Schleiden, that the nucleus, after having given origin to the cell-membrane, has performed its chief office, and is usually cast off and absorbed; as well as that of Schwann, who had never, except in some instances in fat-cells, observed anything to be produced by the nucleus of the cell. Barry stated that the nucleus is a centre for the origin, "not only of the transitory contents of its own cell, but also of the two or three principal and last formed cells destined to succeed that cell; and in fact, that by far the greater portion of the nucleus, instead of existing anterior to the formation of the cell, arises within the cavity." Further, he says, "young cells originate through division of the nucleus of the parent cell, instead of arising as a sort of product of crystallization in the fluid cytoblastema of the parent cell." He regarded the division of the nucleus in pus corpuscles as not artificially produced by the agency of acetic acid, as was held by Henle and Schwann, but as a part of the process by which cells were produced, and apparently universal in its operation.

In a paper published in 1847, Dr. Barry summarized his observations on the nucleus of animal and vegetable cells, and whilst expressing certain opinions on the mode of formation of the nucleolus and nucleus and the growth of cells which cannot now be accepted, he continued to maintain that cells are descended from an original mother cell by cleavage of the nucleus, and all subsequent nuclei are propagated in the same way by fissiparous generation. Every nucleus, therefore, was a sort of centre, inheriting more or less the properties of the original nucleus of the fecundated ovum, which he conceived to be the germinal spot, and exercising an assimilative power. Dr. Barry's contributions to a correct conception of the development of cells are of the highest importance when viewed in the light of modern observations.

But another Edinburgh inquirer, Mr. John Goodsir, afterwards as Prof. Goodsir the distinguished occupant of the Chair of Anatomy in the University of Edinburgh, was engaged between the years 1842 and 1845 in studying the processes of cell-life, both in healthy tissues and in certain pathological conditions.[1] In his important memoir on secreting structures, published in 1842, he demonstrated from a variety of examples that secretion is a function of the nucleated cell, and he gave, as one of his many illustrations, the cells of the testis containing spermatozoa which were derived from the nuclei of these cells. In the original memoir he was inclined to believe that the cell wall was the structure engaged in forming the secretion; but in a reprint of it in 1845, he modified that view, and gave as his opinion that the secretion would appear to be a product of the nucleus. Goodsir also stated in the memoir of 1842 "that the nucleus is the reproductive organ of the cell, that it is from it, as from a germinal spot, that new cells are formed," and he cited cases in which it became developed into young cells. He subsequently, in a short paper on centres of nutrition, extended this view to the tissues generally. He defined the nutritive centres as minute cellular parts, existing, for a certain period at least, in all the tissues and organs. They drew from the capillary vessels or other sources nutritive material, which they distributed to the tissues and organs to which they belonged. He regarded a nutritive centre as a cell, the nucleus of which is the permanent source of successive broods of young cells, which from time to time fill the cavity of their parent. He called this central or capital cell the mother of all those within its own territory or department. Goodsir also showed that cells were important agents in absorption, ulceration, and inflammation. In inflammation of cartilage, for example, he described and figured the cells in the area affected as increased in size, modified in shape, and crowded with a mass of nucleated cells in their interior, through the agency of which the walls of the corpuscles and the hyaline matrix became absorbed. He also gave illustrations of the multiplication of nuclei within cells in the course of formation of cysts. Corroborative observations on endogenous formation within

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[1] Phil. Trans., vols. cxxviii.-cxxxi. The value which was attached to these memoirs at the time may be estimated by the fact that the Royal Society of London awarded to their author in 1839 one of the Royal Medals. The neglect into which Dr. Barry's writings have since fallen is largely due to the disbelief in his subsequent descriptions of the spiral structure of muscular fibre, of blood-corpuscles, and indeed of the elements of the tissues generally.

[1] "On Secreting Structures," Trans. Roy. Soc. Edin., 1842; "On Peyer's Glands," *London and Edinburgh Monthly Journal*, April 1842; "On Structure of Human Kidney," *ibid.*, May 1842; "Anatomical and Pathological Observations," Edinburgh, 1845; also, his collected papers in "Anatomical Memoirs," Edinburgh, 1868, edited by W. Turner.

animal cells were also given by Mr. H. D. S. Goodsir, as confirmatory of the doctrine propounded by his brother on the cell as a centre of nutrition, secretion, and production of young cells. In a research into the structure of the testis in Decapodous Crustacea, Henry Goodsir observed that the head of the spermatozoon corresponded with the nucleus.

The conception entertained both by Martin Barry and John Goodsir of the process of cell-formation and of the function of the nucleus was in the main very different from that propounded by Schleiden and Schwann. Whilst agreeing with Schleiden in holding that new cells were formed within parent cells, they did not look upon the process as one of deposition, in the first instance around a nucleolus and then around a nucleus, but they regarded the nucleus as the prime factor by the division of which new cells were formed. With regard to the free formation of cells, as it was not unfrequently called, by deposition in a cytoblastema situated externally to existing cells, to which Schwann and Henle attached so much importance in animals, they gave no concurrence. Both Barry and John Goodsir had grasped and advocated the fundamental principle, both of the endogenous development of cells from a parent centre and of an organic continuity between a mother cell and its descendants through the nucleus; and the brothers Goodsir had applied this principle in their anatomical, pathological, and zoological researches.

As regards the physiological action of cells, Mr. (now Sir William) Bowman had expressed the opinion[1] that there was a strong presumption that the epithelium of glands assimilated the secretion from the blood; that the secretion might be separated, either by the passage of its elements through the cells, or by the cells undergoing solution or deliquescence, or by the cells being cast off entire with their contents. Mr. (now Sir John) Simon also expressed, in 1845, some important general conclusions on the physiological action of cells ("Essay on the Thymus Gland," London, 1845). He looked upon the cell wall as of secondary importance and of inessential formation, and he regarded the nucleus with the material developed around it as constituting the sole physical evidence of activity in the part. He saw bile and other secretions within cells, and stated that when the products of secretion can be seen within a cell, they are accumulated in the portion which corresponds to the nucleus as though it were the true centre of attraction. Simon also observed the development of spermatozoa within cells, and had seen one end adhering to the relique of a cell, probably its nucleus.

Histologists elsewhere had made isolated observations on the development in the animal body of young cells within parent cells. Even before the publication of Schwann's immortal treatise, Turpin had stated that the corpuscles which he found in vaginal discharges contained a new generation in their interior, and Dumortier had described secondary cells as formed in the ova of snails. These observations exercised, however, no influence on the progress of thought; and Schwann, though referring to them in the preface to his treatise, yet appeared to question their accuracy.

In 1841, Robert Remak published (*Medicinische Zeitung*, p. 127, July 7, 1841) an account of what he saw in the blood corpuscles of the chick, some of which were biscuit-shaped. At each end was a nucleus, and the two nuclei were connected together by a thin stalk which traversed the intermediate part of the corpuscle. He thought it probable from these observations that a multiplication of blood corpuscles through division occurred. He obtained also similar evidence in the blood of the embryo pig, and saw, both in the blood of the horse and of man, red blood-cells formed in the interior of large mother cells. It is customary in Germany to credit Remak with being the first to recognize the division of the nucleus within the cell as a stage antecedent to, and associated with, the division of the cell itself; but from what has already been stated, it will be seen that Martin Barry had preceded him by some months[1] in the recognition of the importance of division of the nucleus in the production of young cells.

In 1843, Albert von Kölliker published (*Müller's Archiv*, 1843) an interesting memoir on the changes which take place in the fertilized ova of various parasitic worms. He described and figured the production in regular progression of young cells within the ovum, and observed that in some cells the nucleus was elongated; in others constricted in the middle, as if about to divide; in others two nuclei were present, each smaller than the single nucleus of adjoining cells, as if they had arisen from the division of a larger nucleus. A legitimate inference from these observations was that, in the formation of young cells, the nucleus of the parent cell divided into two, and that each of these gave origin to a new cell.

The endogenous multiplication of animal cells by division of the nucleus now began to be more widely recognized. It was described by Kölliker and by Mr. (now Sir James) Paget in the blood corpuscles of the embryo, by Kölliker in cartilage and in the giant cells of the marrow of bones, and by various observers in the fertilized ovum. It acquired, therefore, much more importance as a mode of origin of animal cells than was accorded to it by Schwann.

At the time when I began the study of anatomy and physiology in 1850, the current teaching of the schools embraced two methods of cell-formation—the one through the intermediation of existing cells, which might be either by endogenous production within a mother cell through division of the nucleus, or by fissiparous division, or by budding off of a part of a cell; the other by a process of free cell-formation outside existing cells and within a blastema. When I came to Edinburgh in 1854 to act as Demonstrator of Anatomy, I found that the biologists were divided into two hostile forces—the one was presided over by Prof. John Goodsir, whose views on the intracellular origin of new cells I have already explained, and which he systematically expounded in his lectures; the other was led by the then Professor of the Institutes of Medicine, Dr. Hughes Bennett. Dr. Bennett, whose investigations into cell-formation and cell-life had been largely based, like those of Henle, on the study of pathological processes, was led to attach great importance to the granules or molecules which abound in the so-called inflammatory exudations and in purulent fluids. Bennett held that molecules arose in an organic fluid, and that an aggregation of molecules produced nuclei, upon which cell walls may be formed; that the molecule was the primary, elementary, and most simple form of organized matter, and that an aggregation of molecules might even form fibres and membranes without the agency of cells. His views were almost a reproduction of those of Henle, and he advocated them with great vigour and persistency, especially in regard to the production of pus and other products of inflammation.

Pathologists had indeed very generally supported the

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[1] Article "Mucous Membrane," in Todd's "Cyclopædia," date probably 1842 or 1843.

[1] Barry's later memoirs were read to the Royal Society of London, May 7, 1840; January 7, 1841; June 17 and 23, 1841. They are illustrated with numerous beautiful figures, in which the division of the nucleus and the endogenous production of young cells are shown. Further, it should be kept in mind that Remak's observation was on a single tissue, the embryonic blood corpuscle; whilst Barry's was a generalization based on a large series of researches on the ovum, blood and mucous corpuscles, epithelium and other cells. John Goodsir, in a footnote to his important paper "On Centres of Nutrition," already referred to, says:—"For the first consistent account of the development of cells from a parent centre, and more especially of the appearance of new centres within the original sphere, we are indebted to the researches of Dr. Martin Barry." Remak subsequently extended his observations, on the multiplication of cells through division of the nuclei, to the ovum, and the cells of the tissues generally. See *Müller's Archiv*, 1852, p. 47, and "Untersuchungen über die Entwicklung der Wirbelthiere," 1855.

theory of the free formation of cells in exudations; but this view, however, was not universally entertained by them. Prof. Goodsir (*op. cit.*, 1845), and Dr. Redfern[1] had shown its inapplicability in inflammation and ulceration of articular cartilages. Prof. Virchow, in a series of papers in his *Archiv*, commencing with vol. i. in 1847, had described the endogenous formation of young cells in pathological structures. In his "Lectures on Cellular Pathology," published in 1858, Virchow, like Goodsir, announced his belief in the mapping out of the body into cell territories. Virchow's conception of the territory was the intercellular substance immediately surrounding a cell, and subject to its influence.[2] He maintained that in pathological structures there was no instance of development *de novo*, but that where a cell existed, there one must have been before. He called it the law of continuous development, which could be formulated in the expression *omnis cellula e cellula*. He adduced a great variety of specific instances to show the diffusion throughout the tissues and organs of nucleated cells, and he established, by a variety of proofs, the important part played by the cell elements, more especially those of the connective tissue, in the inflammatory process and in the production of new formations. He advanced, indeed, such a mass of evidence in support of this position, that the theory of free cell-formation was shortly after abandoned in connection with pathological processes, as it had been some time previously by most observers in normal histogenesis.[3]

(*To be continued.*)

THE CAUSES OF ANTICYCLONES AND CYCLONES.

A MEMOIR presented to the Vienna Academy of Sciences on April 17 last by Prof. J. Hann, giving the results of his study of an anticyclone which lay over Central Europe from November 12 to 24, 1889,[4] brings to a climax one of those investigations that rank as landmarks in the advance of science, and compels us to modify in some important particulars the views now generally current on some of the leading phenomena in meteorology. Next to the facts of the general circulation of the atmosphere, which, in recent years, have been treated of more particularly by Ferrel, Hann, Siemens, Sprung, Oberbeck, and Pernter, the relations between areas of high and low pressure, or anticyclones and cyclones, have played a chief part in the science of atmospheric movements; and indeed in that large and popular department that deals with the weather and its vicissitudes, they may be said almost to monopolize the field. Hitherto, however, excepting in so far as the movements of the clouds afford us any information of the changes in progress in the higher atmosphere, our experiential knowledge of cyclones and anticyclones has been almost restricted to what can be observed within a small distance of the general land-surface. As a rule, a region of high barometer, especially in the winter, is one of low surface temperature, while cyclones, which originate in regions of low pressure, are fed by warm southerly winds. Interpreting these facts by the light of well-known physical laws, it has become the common teaching of our text-books that the former are due to the low mean temperature and therefore increased density of the superincumbent air column, while the latter are brought about by the opposite conditions. The correctness of these views, in so far, at least, as regards anticyclones, was challenged by Dr. Hann as long ago as 1875, in a paper published in the Vienna *Zeitschrift* (vol. x. p. 210), wherein he showed that, as a result both of theory and observation, the cold that prevails in a region of high barometer in winter is really due to terrestrial radiation under the clear skies that are characteristic of such an area, that it is restricted to a stratum of very moderate thickness, and that above this the compression of the sinking atmosphere must induce a high temperature, and consequently greatly reduce its density.

In a subsequent, very suggestive paper, published in 1879 in the fourteenth volume of the same periodical, he discussed more fully the causes of anticyclones, and concluded that they are essentially the same as those which give rise to the two sub-tropical zones of high barometer—viz. the congestion of the upper or anti-trade currents, directed polewards and eastwards, which, owing to the rapid contraction of the circumpolar zones in high latitudes, are partially arrested and forced to return in a lower stratum of the atmosphere. He also expressed the opinion that these areas of congested currents determine the formation of travelling cyclones in the intervals of relatively low pressure, instead of being themselves caused by the overflow of the upper currents from the latter (which is Ferrel's view). Hence that both anticyclones and cyclones have their origin in the circumstances of the general atmospheric circulation, and are, in neither case, primarily due to the heating or cooling of that part of the earth's surface which they temporarily occupy. Some further consequences of high importance were pointed out in this essay. Since the general circulation of the atmosphere is determined by the expansion of the air over the equatorial zone, and the consequent tilting of the planes of equal pressure to form a gradient between the equator and the two poles, the greater frequency of stormy weather in the higher latitudes in winter was shown to follow from the increased activity of the higher or anti-trade currents; the difference of temperature between equatorial and polar and sub-polar regions being at that season at their maximum; and not merely to the contrasted conditions of continents and oceans. Also that any cause tending to increase the heating and expansion of the equatorial atmosphere must intensify both the anticyclonic and cyclonic movements of the temperate and sub-polar zones. In another paper, published in the fifteenth volume of the *Zeitschrift*, he pointed out that this last view received confirmation from the fact, then recently ascertained, that those years in which the barometer ranged below the average in the Indo-Malayan region were years of excessive barometric pressure in winter (but not in summer) in Western Siberia and Russia.

From time to time, as occasion has served, Prof. Hann has continued to verify these views, by investigating the temperature conditions of anticyclones, on the evidence afforded more particularly by the high Alpine and other mountain observatories. He has thus shown that the relatively high temperature prevailing at high levels during periods of intense winter cold at low levels is no exceptional occurrence, but a constant and characteristic feature of anticyclonic conditions. Moreover, that, as a general fact, the temperature at mountain observatories in winter rises and falls directly with the barometric pressure at those elevations, while the reverse holds good at the general ground surface. In summer, the lowest temperatures at mountain observatories coincide also with the lowest pressures at the ground surface; and this he explains partly by the fact that a low barometer is accompanied with stormy and rainy weather, and with snow at the greater elevations, and partly by the dynamic cooling of the ascending air over the region of minimum pressure. The Alpine observatories are, however, less favourably situated for

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[1] "Abnormal Nutrition in Articular Cartilages," *Edinburgh Monthly Medical Journal*, August 1849; and separate memoir, Edinburgh, 1850.

[2] He first used the term "Zellen Territorien" in his *Archiv*, Bd. iv., 1852, p. 383.

[3] In a lecture which I delivered before the Royal College of Surgeons, Edinburgh, in 1863 (*Edinburgh Medical Journal*, April 1863), I summarized the evidence of the derivation of pathological cell formations from pre-existing cells, and adduced additional examples from my own observations.

[4] "Das Luftdruck Maximum von November 1889 in Mittel Europa, nebst Bemerkungen ueber die Barometer-maxima in Allgemeinen," von J. Hann, W.M.K. Akad.

observing the conditions of cyclones than of anticyclones, since the Eastern Alps lie away from the ordinary tracks of these storms, and are but seldom traversed by their vortices.

In his latest memoir, Dr. Hann describes and compares two striking instances: one of a prolonged period of barometric maximum (from November 12 to 17, 1889), and one of a barometric minimum, on October 1 of the same year. Both of these included the Eastern Alps, and thus afforded an unusually favourable opportunity for contrasting their accompanying temperature conditions up to an elevation of over 3000 metres. The result is that, notwithstanding that the anticyclone occurred six weeks later in the year than the barometric minimum of October 1, the mean temperature of the anticyclonic air-column up to a height of 3 kilometres, was certainly more than 2° C. higher than that of the antecedent minimum. The further conclusions may be given in the words of the memoir.

"That this result holds good, not only for the barometric minimum of October 1, 1889, but as a general fact (the temperatures in both cases being regarded as deviations from the normal of the season), is in itself probable, and has been fully established in my investigation of the temperature of the summit of the Sonnblick during periods of high and low pressure at the earth's surface. One result of this was that the cyclones of the summer half-year bring about a great cooling of the air-column of at least considerably over 3000 metres in height, and cause the greatest depressions of temperature that occur in the summer generally. The mean temperature of the whole air-column in a summer cyclone, from the ground up to a height of certainly over 5000 metres, is lower than in an anticyclone. It is probable that this holds good for winter cyclones also, if their temperature be compared with that in the centre of an anticyclone. The warmth which accompanies winter cyclones at the earth's surface, and which was assumed to characterize the whole air-column, is restricted chiefly to the lower atmospheric layers; the observations on high mountains show that the greatest warmth is always brought by anticyclones; the higher the mountain, the more pronounced is this result.

"At very great elevations, above the cirrus level for instance, the temperature differences of cyclones and anticyclones may again be reversed; possibly, however, not so. For either of these alternative views plausible grounds may be assigned. But this much is certain, that the theory of the causes of cyclonic and anticyclonic movements of the atmosphere must take count of the fact that up to heights of at least 4 or 5 kilometres, the air temperature in the heart of an anticyclone may be (and perhaps always is) higher than that in the centre of a cyclone.

"Thus fall to the ground the views of those who have sought for the cause of these movements in the different specific gravities of the air in cyclones and anticyclones; in the 'upcast' to which the air must be subject in a cyclone. . . .

"So long as the observed temperatures were those only of the earth's surface, one fell almost necessarily into this error, which was so natural and apparently explanatory. Where cold air lay on the earth's surface, there we found high pressures, and *vice versâ;* what could be more self-evident than that the temperature of the air-column was the determining cause of the pressure? It was the observations of mountain observatories, those of peak-summits, that first set us free from this error; and we must now conclude that the temperature conditions of wandering cyclones and anticyclones are the effect and not the cause, that they are the consequence of the movements of the air-masses, of the ascents and descents of the vertical circulation of the atmosphere. There can no longer be any doubt that the pressures in barometric maxima and minima generally are to be explained mainly through these movements of the air. The forces which set up the atmospheric circulation of the higher latitudes, especially in winter, have their origin in the warmth of the tropics—that is to say, in the difference of temperature between the polar regions and the equatorial zone. Cyclones and anticyclones are but partial phases in the general circulation of the atmosphere. The air-currents that set towards the poles as a consequence of the upper gradients are partially resolved in vortices in the higher latitudes, and their progressive movement is chiefly determined by the prevailing westerly direction of the wind currents. The influences of variations of the terrestrial surface, of the heating and cooling of continents and oceans, as well as of the local influx of water-vapour and its condensation, are but of secondary importance. They may however strengthen or destroy the ascending or descending eddies, and modify their paths and their rate of progression.

"These views are such as I have always enunciated (for a long time, indeed, without any apparent result) in opposition to the then prevalent theories of the local origin of barometric minima through the agency of condensing water-vapour (as contended by Mohn, Reye, Loomis, and Blanford). They now begin to make way and to prevail. Most clearly is this seen in the case of Loomis, who, in the course of his own persistent study of the behaviour of barometric minima and maxima, has been compelled by degrees to give up the 'condensation theory' to which he formerly adhered so strongly, and to ascribe the origin as well as the progressive movement of cyclones to the general circulation of the atmosphere."

After a cursory recapitulation of some of the leading demonstrations of his previous writings, to which brief reference has been made above, Prof. Hann concludes:—

"This theory is not merely deductive, nor is it put forward simply as a speculation. On numerous occasions I have demonstrated step by step how it agrees with observation in all its details, so that I may fairly claim the right of priority for its establishment."

This claim will doubtless be readily admitted, and it adds one more to the many great services which its author has already rendered to the cause of physical meteorology, and which have long since won for him his universally acknowledged place in the forefront of modern meteorological science. As regards the genesis of anticyclones, for the study of which the Sonnblick, Hoch Obir, and Santis Observatories have afforded him numerous opportunities, which he has turned to the best account, Prof. Hann's conclusions appear to be unassailable. And in respect of the cyclones or barometric minima of the temperate and sub-Arctic zones, although the evidence is perhaps less decisive, and its conclusiveness may possibly yet be challenged in some particulars, it must at least be conceded that his arguments are entitled to much weight; and the facts adduced greatly weaken, if indeed they do not altogether destroy, the validity of the views hitherto prevalent. For my own part, I am quite prepared to admit the probability that these barometric minima are, as he contends, in their origin, great eddies in the higher atmosphere, and are not determined by the high mean temperature of the air-column over the spot in which they first appear.

But I cannot admit that these conclusions can be extended to the case of tropical cyclones. Prof. Hann does not indeed expressly claim such extension; but, on the other hand, he does not expressly limit their application to the storms of extra-tropical latitudes, and from the fact that, in a paper recently published in the *Meteorologische Zeitschrift*,[1] he discusses the conditions of both these classes of cyclones without insisting on any fundamental distinction between them, it must, I think, be in-

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[1] "Bemerkungen über die Temperatur in den Cyclonen und Anticyclonen," *Met. Zeitschr.*, Heft 9, September 1890.

ferred that he contemplates as at least a high probability that they originate from like causes. Moreover, in the paragraph quoted above, he refers to myself as an upholder of the condensation theory, in terms that seem to imply that he regards me as an opponent, the fact being that I have never contended that this theory is applicable to the case of other than tropical cyclones, or, to be exact, of other than those of Indian seas. To this contention I must still adhere; but as the discussion of the question would unduly extend the limits of this notice, I reserve it for another occasion. HENRY F. BLANFORD.

ON THE ANATOMY AND DEVELOPMENT OF APTERYX.

IN a paper read before the Royal Society on April 17 of this year, Prof. T. Jeffery Parker, F.R.S., of Otago University, gives an account of his researches on the anatomy and development of the Kiwi (*Apteryx*) which are of especial interest, as so few detailed observations have been recorded on the development of any of the flightless birds (*Ratitæ*). Moreover, the comparisons which are given of the different species of *Apteryx;* the account of the sexual differences, and the variations seen within the same species; and the tables showing the relative proportions of the various regions of the body in different stages of development, illustrating the "law of growth," add greatly to our knowledge of this remarkable genus. A number of new terms are proposed in the description of the skeleton, and a new method of writing the vertebral formula of birds is adopted. Notes are given with regard to the presence of uncinate processes and to the structure of the foot in *Dinornis*.

The chief materials on which the investigation is based consist of a number of embryos of the three common species of *Apteryx*, which naturally group themselves into ten stages (A–K); an eleventh stage (L) is furnished by a bird a few weeks old, a twelfth (M) by the skeleton of an adolescent specimen, and a thirteenth (N) and fourteenth (O) by odd bones of young birds; the adult may be considered as constituting a fifteenth stage. The embryos were, for the most part, well preserved, but not sufficiently well for the purposes of exact histological study. The single embryo belonging to stage A corresponds in most respects to a chick of the fourth day.

The paper is illustrated with over 300 figures, and gives so many technical details as to the structure and development of the skeleton, and as to the muscles of the wing, the brain, and the eye, that it is impossible here to give anything approaching a satisfactory abstract of the whole, which will appear shortly in the Philosophical Transactions. The chief results of more general interest, as bearing on the phylogeny of *Apteryx* and of the Ratitæ generally, may be briefly summarized as follows.

In stage A, the limbs have already attained their permanent position, so that, if the backward shifting of the appendages so noticeable in the chick occurs in *Apteryx*, it must take place at an unusually early period. In stage C, corresponding with a sixth-day chick, there is a well-marked operculum growing backwards from the hyoidean fold, and covering the third (? and fourth) visceral cleft. A rudiment of this structure is seen in the preceding stage.

From the first appearance of the feather papillæ there are well-marked pterylæ and apteria, most of which can be made out with tolerable distinctness in the adult.

The wing of the adult has a well-marked pre- and postpatagium, and amongst its feathers may be distinguished nine or ten cubitals, two or three metacarpals, one mid-digital, and a row of tectrices majores. The barbicels of the feathers are slightly curved. The fore-limb passes through a stage in which it is a tridactyle paw with subequal digits, followed by one (stage F) in which it is a typical wing with hypertrophied second and partially atrophied first and third digits. The variability of the muscles of the wing is noteworthy, and the evidences of degeneration are very clear; a number of wing-muscles, not mentioned by Owen, are described.

The nostril has acquired its final position at the end of the beak in stage E; up to the middle of incubation the whole respiratory region of the olfactory chamber, from the anterior nares to the commencement of the turbinals, is filled with a solid mass of epithelial cells, through which a passage is formed at a later period. The turbinals are unusually well developed. A pecten is present in the eye during late embryonic life. At no stage is there any trace of the caruncle or "egg-breaker" at the end of the beak.

As regards the skull, it may be mentioned that the head of the quadrate is provided with two articular facets; no intertrabecula could be observed; there is no interorbital septum; Jacobson's cartilages are present; and the hyoidean portion of the tongue-bone ossifies late, and is obviously degraded.

The vertebral column and hind-limb are typically avian, both as regards structure and development, and these typical characters appear early in the pelvis. There is a pygostyle. The sternum and shoulder-girdle, as well as the wing, are very variable, indicating degeneration; their position in stage E resembles that seen in Carinate birds. Vestigial acromial, procoracoid, and acrocoracoid processes are present, the procoracoid being well marked in comparatively late embryonic life. There is no trace of clavicles. A vestigial keel is occasionally present in the sternum; but before considering the peculiarities in the development of the sternum as of fundamental importance, it will be necessary to study that of the flightless Carinatæ, and especially of *Stringops*.

The brain passes through a typical avian stage with lateral optic lobes. The mesencephal is unusually small from the first; in stages D—F the optic lobes are dorsal; in G they become lateral by the transverse extension of the optic commissure or median portion of the roof of the mesocœle; in H they are already ventral, although larger proportionally than in the adult. The diencephal becomes tilted backwards in later stages, its dorsal wall becoming posterior, and the foramen of Monro postero-dorsal instead of antero-dorsal. The anterior commissure and corpus callosum are large. The cerebral hemispheres are of unusual proportional length, and partly cover the cerebellum.

The greater number of the characters enumerated support the view that *Apteryx* is derived from a typical avian form capable of flight;[1] but on the other hand, the total absence of rectrices tells against this theory. Many characters, again, indicate derivation from a more generalized type than existing birds; while in other points *Apteryx* exhibits greater specialization than other birds. The general balance of evidence seems to point to the derivation of both Ratitæ and Carinatæ from an early group of typical flying birds or *Proto-Carinatæ*.

CAPTIVE BALLOONS.

SOME important experiments with captive balloons have lately been made in the Mediterranean squadron of the French navy. By the courtesy of the editor of *La Nature*, we are enabled to give, on the next page, a representation of the most interesting of these experiments, which was made on board the ironclad *Le Formidable*. All the officers who mounted in the car declare that it afforded an excellent point of observation. In clear weather they could distinguish, from Lagoubran, all the details of the coast from the entrance to Mar-

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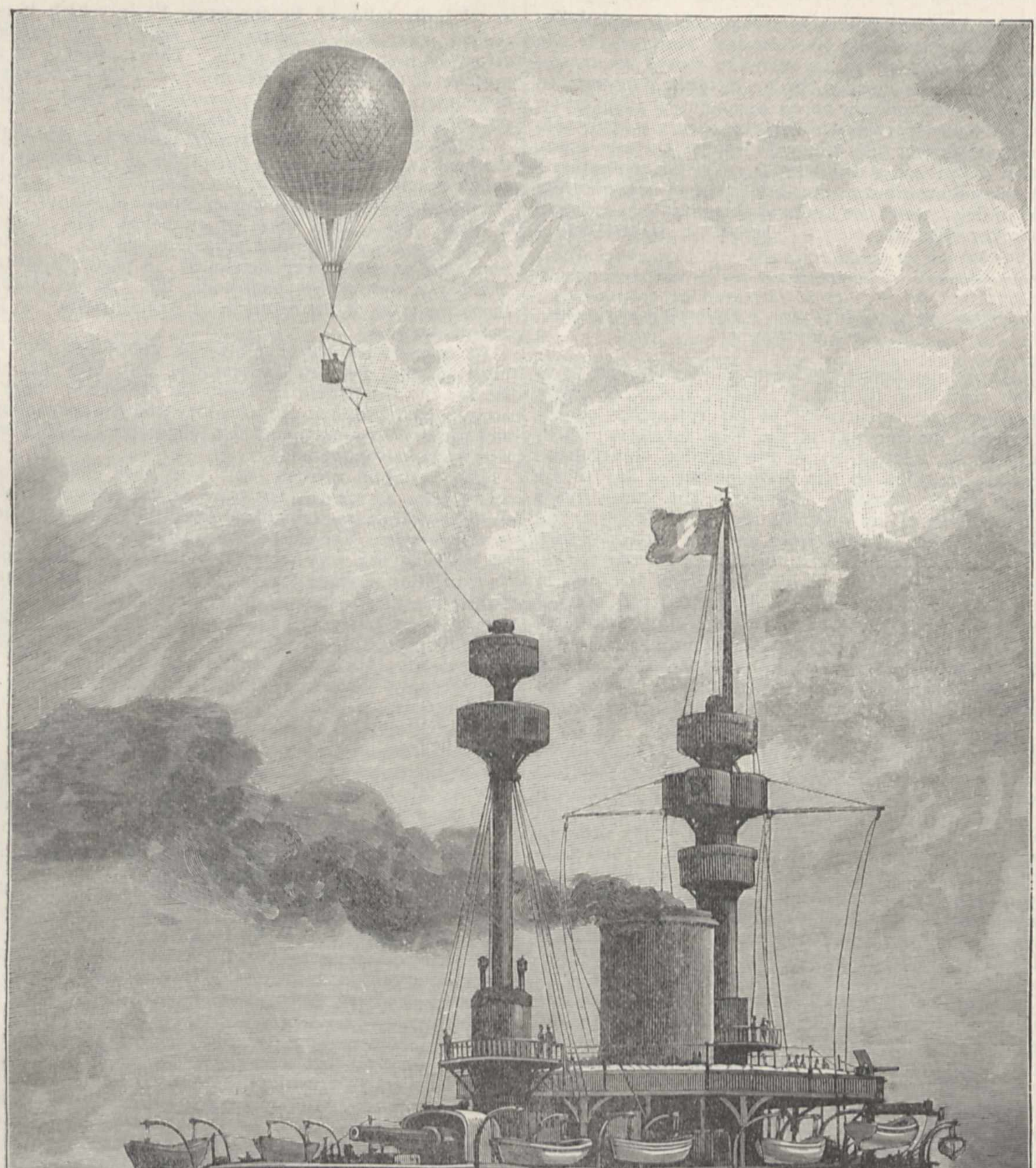
[1] The attitude assumed during sleep also supports this view.



Experiment with a captive balloon on board the French ironclad *Le Formidable.*

seilles to the eastern extremity of the Islands of Hyères. No ship within a radius of from 30 to 40 kilometres could have escaped observation. With a cable of silk, the balloon could rise in calm weather to a height of 400 metres.

It is evident from the success of these experiments that captive balloons may be a most important aid to those who hereafter make use of them in naval warfare. The subject has attracted the attention of the naval authorities in Germany, and at Wilhelmshaven a captive balloon was sent up recently from the *Mars*. We are glad to learn that the English Admiralty has taken up the question.

THE COCO-DE-MER IN CULTIVATION.

WITH only one exception, the palms of the Seychelles have long since proved amenable to cultivation in our tropical plant-houses. The genera *Stevensonia*, *Verschaffeltia*, *Roscheria*, *Latania*, *Dictyosperma*, *Acanthophœnix*, *Hyophorbe*, and *Chrysalidocarpus*, which are peculiar to this small group of islands, and which rank amongst the noblest of a noble family, are all well known in European collections of palms, their cultivation presenting no more difficulty than that of tropical plants generally. The coco-de-mer or double cocoa-nut (*Lodoicea seychellarum*) has, however, so far proved unmanageable under artificial treatment, notwithstanding that many attempts have been made to establish it at Kew and elsewhere. So long ago as the year 1827, Sir William Hooker published a series of figures and a description of the coco-de-mer in the *Botanical Magazine*, and recorded the arrival of living nuts of it at Kew, where, he says, "we cannot doubt of soon seeing them flourishing in our stoves." But they failed to grow, and although dozens of nuts have since been tried at Kew, not one ever got beyond the first stage of germination.

The absence from our collections of living examples of this most remarkable palm is most disappointing to all students of the order. At Kew we have lately been successful in establishing living plants of the Ita (*Mauritia flexuosa*) and Bussu (*Manicaria saccifera*) palms of the Demerara swamps, and the Doum (*Hyphœne thebaica*) and Palmyra (*Borassus flabelliformis*) palms of Africa. These successes stimulated the desire once more to obtain a living plant of the coco-de-mer.

Application was therefore made in January last year, through the Secretary of State for the Colonies, for a supply of fresh nuts from the Seychelles, and at the same time directions for packing and forwarding the nuts were sent to Mr. C. Button, the Conservator of Forests at those islands. The Administrator, Mr. T. Risely Griffith, took a warm interest in the matter, and through his kind exertions several consignments of nuts were received, of which four germinated. Two of these are probably too weak to live, but the other two are in a most promising condition. The strongest has a radicle 3 feet 8 inches long, and 12 inches in circumference at the end where the plumule is developed. This is now a foot long, and is pushing a perfect leaf.

In a note by the late General Gordon on the germination of the double cocoa-nut, it is stated that the nut is planted horizontally, without the husk, when it sends out a sprout some 12 feet long, which pushes up the young plant at a distance of 12 feet from the nut. The longest "sprout" we have had at Kew has not exceeded 4 feet. Nor can it be made to grow horizontally, the point turning down perpendicularly however often its position may be altered. At Kew the nuts were planted in a bed of cocoa-nut fibre, and kept at a temperature of 80°–85° F. They were planted in June 1889.

Mr. Button had kindly undertaken to plant a nut in a Wardian case, and treat it according to our instructions until it had germinated and developed the plumule before despatching it to Kew. A nut thus treated arrived in July last in the most promising condition. The radicle is 1 foot 10 inches long, and the plumule is 7 inches in circumference at the base. It has a stout sheath-leaf, and a normal leaf 3 feet 2 inches long, 3 feet wide, with thirty-six folds. The midrib is curved, and the blade at present folded double. The texture is exceptionally firm, and the colour a deep green.

Full-sized trees of the coco-de-mer attain as much as 150 feet in height, with a smooth trunk about a foot in diameter. The leaves form an immense crown on the top, and each leaf is 20 feet long and 10 or 12 feet wide. The male and female flowers are on separate plants: the male inflorescence is shaped like a huge willow catkin, its length being 5 to 6 feet by 4 inches in diameter; the female is from 2 to 4 feet long, and it bears from six to ten fruits, each of which weighs from 25 to 30 pounds. They take seven years to mature, and sometimes hang two years on the tree after they are ripe. The process of germination extends over about two years. According to General Gordon, the trees begin to fruit when about forty years old, and attain maturity in 120 years.

Royal Gardens, Kew. WILLIAM WATSON.

[The coco-de-mer is at present confined to Praslin and Curieuse, two of the islands of the northern group of the Seychelles Archipelago. It undoubtedly runs some risk of extinction from the long period which the nuts take to germinate, and from the fact that, the trees being of different sexes, isolated females may easily escape fertilization. Its cultivation in the Botanic Gardens of the tropics is therefore of considerable importance.

Plants have long flourished in the Royal Botanic Gardens at Peradeniya, and the following extract from a letter from the Director, Dr. Trimen, F.R.S., to Kew, records the interesting circumstance of a male plant having flowered:—

"*Peradeniya*, August 12, 1890.

"You will be interested to hear that one of our Lodoicea palms put out a ♂ inflorescence last month. The tree is thirty-nine years old. To my great disgust, when the spike was about 6 inches long, some visitor cut it off with a blunt knife, and I found it on the ground. The flowers were all formed, and the structure exactly as described by Sir W. Hooker in the *Botanical Magazine*. I hope my other tree will prove ♀, but that is much younger."

Sir John Kirk also succeeded in establishing the palm in his garden at Zanzibar.

The Government of the Seychelles has long watched with care the preservation of the existing groves of the palm, and pains are now taken to fertilize the female plants artificially, and to plant the seeds.—W. T. T. D.]

NOTES.

WE have to announce the death of Pierre de Tchihatchef, which took place at Florence on the 13th ultimo. This gentleman was perhaps best known as a botanist, though his principal literary work, "Asie Mineure: Description Physique, Statistique, et Archéologique de cette Contrée," took a much wider range. Prior to 1857, he travelled ten years in Asia Minor and Armenia, and, besides the work named, he published a large number of separate papers on a variety of subjects, chiefly however on botany and geology, commencing in 1840. Like so many Russians, he appears to have been an accomplished linguist, and wrote German and French with equal facility. He resided some years in France, and was one of the original members of the Botanical Society of France, founded in 1854. His "Botany of Asia Minor" forms the third part of the work named above, and consists of two volumes of letterpress, and a volume of plates by Riocreux. Pierre de Tchihatchef was also the author of an admirable French translation of Grisebach's "Vegetation der Erde." But this was something more than a translation, for it was cast in a better mould than the original, and contained much new matter, including an essay on the geological formation of oceanic islands.

We regret to have to record the death of Dr. Alexander John Ellis, F.R.S. We reprint from the *Times* the following notice of his career:—Dr. Ellis, whose original name was Sharpe, died at his residence in Auriol Road, West Kensington, on October 28. He was born in Hoxton in 1814, and educated at Shrewsbury, Eton, and Trinity College, Cambridge, of which he was elected a scholar in 1835, and graduated B.A., being sixth wrangler and first in the second class in classics, in 1837. He was elected a Fellow of the Cambridge Philosophical Society in 1837, of the Royal Society in 1864 (being a member of the Council for 1880–82), of the Society of Antiquaries in 1870, of the College of Preceptors in 1873, and a life governor of University College, London, in 1886. He was President of the Philological Society during 1872–74, and also 1880–82. He was also a member of the Mathematical Society of London, of the Royal Institution, of the Society of Arts, and honorary member of the Tonic Sol-Fa College. Dr. Ellis was a voluminous author, his works including "The Alphabet of Nature," 1845; "Essentials of Phonetics," 1848; "Plea for Phonetic Spelling," 1848; "Universal Writing and Printing," 1856; "Early English Pronunciation, with special reference to Chaucer and Shakespeare," 1869–86; "Glossic," 1870; "Practical Hints on the Quantitative Pronunciation of Latin," 1874; "On the English, Dionysian, and Hellenic Pronunciation of Greek," 1877; "Pronunciation for Singers," 1877; "Speech in Song," 1878; together with numerous other works and tracts on music and phonetics. He received the silver medal of the Society of Arts for three papers in connection with the "Musical Pitch" at home and abroad.

Dr. F. R. Japp, F. R.S., Assistant Professor of Chemistry in the Normal School of Science, South Kensington, has been elected Professor of Chemistry at the University of Aberdeen.

At the Royal Institution of Great Britain, on Monday, Mr. Victor Horsley, F.R.S., was elected Fullerian Professor of Physiology for three years.

The Secretary of State for India has appointed Mr. Arthur W. Thomson, C.E., B. Sc., of the Glasgow and West of Scotland Technical College, to be Professor of Mechanism and Applied Science in the College of Science, Poona.

We are glad to learn that the accommodation at the disposal of the Botanical Department in the University College, London, has been greatly augmented. Hitherto, all the botanical work, other than lectures, has been confined to the single general laboratory in the north cloister. In the adjacent Birkbeck building, from which the school of technological chemistry has been transferred to another portion of the College, several rooms have now been set apart for the various branches of botanical teaching; and the room in the north cloister has been fitted as a museum and general botanical laboratory. During the building operations, just concluded, the workmen found three large chests in which Prof. Lindley (who died in 1865) had stowed away a series of fossil types, representing the chief genera of plants occurring in the Coal-measures. This collection is, of course, a valuable accession to the botanical museum.

On Monday the Corporation of Brighton obtained formal possession of the museum in Dyke Road, containing the collect tion of British birds formed by the late Mr. E. T. Booth, and bequeathed by him to the town. A large assembly, including some specially invited men of science from London, gathered on the occasion. The key of the building having been handed to the Mayor of Brighton, Alderman Manwaring, he said that he trusted the collection was the beginning of such a natural history museum as no other town in the kingdom could boast of possessing. The gathering was addressed by Prof. Flower, of the British Museum, who said the collection was in many respects unrivalled in the kingdom. The homes in which the birds dwell were carefully and accurately reproduced in a manner that had never before been achieved. In that museum some of the specimens of taxidermy were very fine. All were above the average, and he did not believe there was a single bad one among them. It would have been a national calamity for such a collection to be dispersed or destroyed, and when it was offered to the British Museum he should have advised the Trustees to take it over had it not been intimated to him that the Corporation of Brighton were willing to take it and maintain it for the future benefit of mankind. Though it would have been a great privilege to him to be its official guardian, he rejoiced to find it was going to remain in Brighton, where it was formed, and in the neighbourhood of which many of the specimens were obtained.

The following have been elected as officers of the Cambridge Philosophical Society for the ensuing year: President:—Prof. G. H. Darwin, F.R.S. Vice-Presidents: J. W. Clark, Trinity; Prof. Babington, F.R.S., St. John's; Prof. Liveing, F.R.S., St. John's. Treasurer: R. T. Glazebrook, F.R.S., Trinity. Secretaries: J. Larmor, St. John's; S. F. Harmer, King's; E. W. Hobson, Christ's. New Members of Council: Dr. Alex. Hill, Downing; Dr. A. S. Lea, Caius; A. Harker, St. John's; L. R. Wilberforce, Trinity.

The Meteorological Council have published the meteorological observations made at stations of the second order (*i.e.* observations taken at 9h. a.m. and 9h. p.m. each day) for the year 1886. The present volume differs in several important particulars from those of previous years: the distribution of stations is much more complete, for, although the number for which observations in detail are published has been reduced, on the other hand the summarized observations have been considerably increased, and include the records from the observatories in connection with the Office. Some alterations will also be found in the tables, both as regards the information given and the form in which it appears: the barometer observations are no longer reduced to sea-level, owing to the uncertainty which attaches to the formula for reduction when the height of the station is considerable. In the hygrometrical values, the differences between dry- and wet-bulb readings are given under the heading of "Depression of Wet Bulb." In other respects, the general plan of the publication is the same as in previous years, and includes observations made at some selected stations of the Royal and Scottish Meteorological Societies.

In the *Bollettino Mensuale* of the Italian Meteorological Society for September, Prof. P. Busin publishes the results of his discussion of the diurnal probability of rain, calculated from long series of observations for three of the principal Italian cities, obtained by dividing the number of days of rain in a given period by the number of years of observation for the same period. He concludes that the tables show that barometric depressions do not bring rainy weather, or anticyclones fine weather, so frequently as generally supposed, and that such tables are more to be relied on by agriculturists than telegraphic forecasts of rain, owing to the variability of this element in adjacent localities. Although we cannot entirely agree with this view, there can be little doubt that such investigations may be valuable aids to the study of weather changes, if used in connection with telegraphic information of existing conditions.

We have received two pamphlets on the "Aurora" and "Forces concerned in the Development of Storms," by Mr. M. A. Veeder. In the former he finds that the phenomena depend on the rotation of the sun, and he mentions the remarkable coincidence in time of their sudden appearance and gradual fading with the solar disturbances that appear on the sun's eastern limb. He also suggests that thunderstorms "may be a reciprocal or alternative method of manifestation of forces, which, under other conditions, find their expression in the aurora." In the latter pamphlet, the working hypothesis adopted is that the distribution of atmospheric pressure as a whole may be determined to an important extent by the fact that the earth is a magnet, and that its magnetic properties are variable. In fact, convection-currents are of secondary importance, for he says that "the bringing of warm air from the tropics, or the bringing of cold air from the polar regions, is the effect, and not the cause, of the redistribution of pressure." Thus, if these magnetic forces, associated with magnetic induction from the sun, influence the atmosphere and mass it together in any particular way, equilibrium is maintained as long as these forces do not vary, but, as soon as they do, readjustment sets in, and eddies, storms, &c., are the result.

THE latest Report on the economical condition of Switzerland, from the British Legation at Berne, refers to the subject of technical education in that country. Subventions to the amount of £12,854 were granted by the Federal Government during the past year to the various technical schools existing in the different cantons. Among the more important of these schools is the Technikum at Winterthur, the various silk and cotton weaving schools in the cantons of Zürich, Basle, &c., and the schools of horology in the cantons of Geneva, Berne, and Neuchâtel. The Federal Government have, moreover, on more than one occasion been invited to consider the question of subsidizing schools of commerce, and applications for financial assistance have been addressed to them by the cantons of Geneva and Zürich, in which schools of this nature are already established. The Federal Council, while admitting that it is their duty to encourage in every way the establishment of schools in which youths may be trained for the various branches of commercial life by an education especially adapted to that end, have nevertheless decided that they would not be justified in thus adding to the expenses of the confederation until the equilibrium has been restored in the Federal budget.

WITH reference to our note, last week, about scientific guide-books, a correspondent writes:—"For Switzerland I should like to recommend the 'Botanist's Vade-Mecum,' published by the 'Librairie Saudoz' of Neuchâtel. It was compiled by Mr. Paul Morthier, Professor of Botany at the Neuchâtel Academy, and President of the International Society of Botanists. It has already passed through two or more editions, and the price is about 2*s.* 6*d.*"

MESSRS. CASSELL have issued, for the National Association for the Promotion of Technical and Secondary Education, a "Guide to Evening Classes in London." This compilation ought to be of great service to what is now, happily, a large class of students. Full information is given as to all the chief classes, elementary and advanced, that are to be held during the coming winter in the capital.

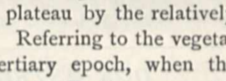
A SUPPLEMENT to the third volume of the *Internationales Archiv für Ethnographie* has been issued. It consists of some interesting ethnographical notes, by Dr. Max Weber, on Flores and Celebes. The paper is admirably illustrated.

A WEEKLY review, entitled *L'Université de Montpellier*, is about to be issued at Montpellier. It will contain information concerning the University; an abstract of the most interesting lectures in science, literature, and art; and original papers by men of science in the town and neighbourhood.

THE following is a list of the edible fungi exhibited for sale in the market of Modena during 1889:—*Amanita cæsarea, A. ovoidea, A. strobiliformis, Lepiota excoriata, L. naucina, Armillaria mellea, Pleurotus ulmarius, P. glandulosus, Entoloma Rhodopolius, Pholiota mutabilis, P. Aegerita, Psalliota campestris, Morchella esculenta, M. conica, M. rimosipes, Helvella monachella, H. crispa, Peziza vesiculosa, P. cerea, P. Acetabulum, Tuber magnatum, T. æstivum, Balsamia vulgaris.* By far the greater number of these species are also natives of Britain.

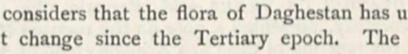
M. KUZNETSOFF, who has spent several years in the study of the flora of the Caucasus, sets forth in the last issue of the *Izvestia* of the Russian Geographical Society the following interesting conclusions:—The flora of the Kutais and Tchernomorsk regions, on the eastern coast of the Black Sea, belongs, as already known, to the Mediterranean region of evergreen trees. Next comes the region of West European flora, characterized by the extension of the beech-tree, and offering on the slopes of the mountains the very same subdivisions as we are accustomed to see in the Alps. That region extends over the provinces of Kuban and Terek as far east as the water-parting between the Terek and Sulak rivers. The territory to the east of it was formerly thought to have a flora more akin to that of Asia, but a distinctly European flora appears again on the eastern slopes of the Daghestan plateau turned towards the Caspian Sea; while the dry Daghestan plateau itself has a flora decidedly recalling that of the highlands of Central Asia. M. Kuznetsoff explains these differences by the moister climate of the Caucasus highlands, due to the proximity both of the Black and of the Caspian Sea. But it may also have a deeper cause. In fact, the plateaus of Daghestan cannot but appear to the orographer as a continuation of the geologically oldest plateaus of Asia Minor, now separated from the main plateau by the relatively much younger chain of the Caucasus. Referring to the vegetation of the Caucasus during the Tertiary epoch, when the Caucasus was a vast island surrounded by Tertiary seas, M. Kuznetsoff considers that the flora of Daghestan has undergone the greatest change since the Tertiary epoch. The floras of both the Western and the Eastern Caucasus have maintained more of their old characters, owing to less change having gone on in their climate, which has remained moist; and the vegetation of the Black Sea coast, which has a climate very much like that of the Japan archipelago, has retained still more of the aspects it had during the Tertiary epoch. Further exploration will be necessary to show how far climate alone can account for the present characters of the flora of Caucasus.

FURTHER details are given by Prof. Curtius in the current number of the *Berichte* concerning his new gas, hydrazoic acid, N_3H. Since the first announcement, a better and much readier mode of preparing the gas has been discovered. Instead of reacting with hydrazine hydrate upon benzoylglycollic acid, it is found much more convenient to commence by preparing the hydrazine derivative of hippuric acid,



$$\begin{array}{l}C_6H_5\\ |\\ CO\\ |\\ NH\\ |\\ CH_2{-}CO{-}NH{-}NH_2\end{array},$$

a substance much more readily obtained. This compound is converted into its nitroso-derivative,



$$C_6H_5{-}CO{-}NH{-}CH_2{-}CO{-}N\begin{array}{l}\diagup NO\\ \diagdown NH_2\end{array},$$

by treatment with sodium nitrite and acetic acid at a temperature about 0° C. Nitroso-hippurylhydrazine is much more permanent than the corresponding nitroso-compound of benzoylhydrazine, used in the earlier experiments, and the yield is 90 per cent. of the theoretical. The well-washed crystals of this nitroso-compound are next dissolved in dilute caustic soda, and the solution warmed for a short time upon the water-bath. The alkaline solution is afterwards placed in a flask connected with a condenser and fitted with a dropping funnel. Dilute sulphuric acid is now allowed to slowly drop into the liquid, which is maintained at the boiling temperature. Under these circumstances an aqueous solution of hydrazoic acid distils over. The distillate is allowed to flow into a solution of silver nitrate, when the silver salt, silver azoate, N_3Ag, is precipitated. The silver salt is afterwards dried at 60°–70°, at which temperature no danger attends the operation, and decomposed by sulphuric acid diluted with eight times its volume of water, when hydrazoic acid gas is liberated, contaminated with only a trace of moisture. It appears that the aqueous solution of the free acid is almost as explosive as the silver and mercury salts. Upon one occasion, when attempting to fuse the drawn out end of a tube containing about 2 c.c. of a 27 per cent. solution, Dr. Curtius had a very narrow escape of serious injury, the whole exploding with a fearful detonation, and shattering the glass tube into dust. Several of the azoates explode when a beam of coloured light is thrown upon them; thus barium azoate, BaN_6, explodes when exposed to a strong green light, as does also the still more explosive silver azoate. A concentrated solution of hydrazoic acid appears to be able to dissolve gold, with formation of a red solution of gold azoate.

The additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mr. Charles E. Flower; an Azara's Fox (*Canis azaræ* ♂) from South America, presented by Mr. H. M. Dodington; an Alligator (*Alligator mississippiensis*) from the Mississippi, presented by Mr. A. Schafer; two Black-faced Spider Monkeys (*Ateles ater* ♀ ♀) from Peru, deposited.

OUR ASTRONOMICAL COLUMN.

The Rotation of Venus.—M. Perrotin, the Director of Nice Observatory, presented a note on the rotation of the planet Venus, at the meeting of the Paris Academy held on October 27. The observations described in the note were undertaken for the purpose of testing the conclusions recently arrived at by Signor Schiaparelli. They extend from May 15 to October 4. In the interval the planet has been observed on 74 days, and 61 maps made of its appearance. The whole of the observed facts leads M. Perrotin to the following conclusions:—

(1) The rotation of the planet is very slow, and is made in such a way that the relative position of the spots and terminator do not experience any notable change during many days.

(2) The time of rotation of the planet does not differ from its sidereal period of revolution (about 225 days) more than thirty days. My observations will easily accommodate themselves, however, to a rotation of which the period is from 195 to 225 days.

(3) The axis of rotation of the planet is almost perpendicular to the plane of its orbit. The displacement of the white region observed at the northern edge indicates that the difference does not exceed 15°, as was admitted by Schiaparelli.

These conclusions, therefore, support those deduced by Schiaparelli from an extended discussion of all the observations of the planet.

Spectrum of the Zodiacal Light.—Prof. C. Michie Smith has published a series of observations made at Madras of the spectrum of the zodiacal light (Proc. Roy. Soc. Edinburgh, April 7, 1890). He used a spectroscope specially designed for observing and photographing this spectrum, and records:—"In all my observations, which have been carried on at intervals since 1875, the spectrum has appeared continuous and free from bright lines except during the spring of 1883, and even then the lines were not seen with sufficient distinctness to make their existence certain. The estimated position of the supposed line, wave-length 558, differs but little from that of the auroral line (wave-length 556·7) which was observed by Angström in the zodiacal light spectrum in 1867. He was, however, observing at Upsala, where the auroral spectrum can often be seen in almost all parts of the sky, even when the aurora itself cannot be detected. . . . There would seem to be very little risk of obtaining the auroral spectrum in Madras, and I think that if the bright line seen was real, and not imaginary, it must have been due to the zodiacal light"

These observations indicate a periodic appearance of the 558 line in the zodiacal light spectrum. They also support the idea that the origin of the line is the first fluting of manganese at λ 5576.

D'Arrest's Comet.—This faint comet (*d* 1890), re-discovered by Mr. Barnard on the 6th ult., may be observed near the following positions:—

Ephemeris for Greenwich Midnight.

1890.		R.A.		Decl.
		h. m. s.		° ′
Nov. 8	...	21 24 31	...	−27 13·0
12	...	39 15	...	26 40·3
16	...	53 32	...	26 1·4

THE INSTITUTION OF MECHANICAL ENGINEERS.

On Wednesday and Thursday evenings of last week, the 29th and 30 ult., a general meeting of the Institution of Mechanical Engineers was held. The chief business was the reading and discussion of the following two papers: on tube-frame goods waggons of light weight and large capacity, and their effect upon the working expenses of railways, by Mr. M. R. Jefferds, of London; and on milling cutters, by Mr. George Addy, of Sheffield.

Mr. Jefferds is an American engineer who has come over to this country with a view to introduce the tube-frame waggon into England. It should be explained that the tube-frame differs from the ordinary frame of an English railway truck chiefly in the fact that, in place of the timber sole-bars with which we are acquainted, there are used eight wrought-iron tubes, 2⅞ inches in diameter, each pair forming one sole-bar, and suitably connected and supported by malleable cast-iron parts. The boldness with which these castings are used in a structure upon which so much depends bears testimony to the superiority of American foundry practice and to the courage of American designers in perhaps about equal proportions. Certainly no Great George Street engineer would venture upon putting annealed castings in such a position; and, if he did, he would, no doubt, meet with disaster. The tube-frame waggons have, however, been largely built and extensively used in America, and we understand from Mr. Jefferds that there is no reason to think that the castings are not suitable for the work.

The interest in the paper, to judge by the channel into which the discussion was turned, did not centre so much in the constructive details of the waggon described as it did upon the general policy of the American as against the English methods of handling railway freight. In the United States, as most people know, they go upon the principle of having large goods waggons, some even as long as 40 feet and capable of carrying 40 tons. These, however, would be of extreme dimensions, the more usual length being 32 to 34 feet, with a carrying capacity of 30 to 32 tons—that is, American tons of 2000 pounds to the ton. These waggons are mounted on a pair of bogies, each having four wheels. Our own goods trucks are something about 20 feet long, and are mounted on wheels with axles which are fixed with their axes parallel to the ends of the trucks. The English truck will carry 10 tons, and weighs, according to Mr. Jefferds, 8 tons. Mr. Jefferds is, however, a little out here. No doubt some 10-ton trucks weigh 8 tons, but Mr. Williamson, of the Great Western Railway, and Mr. T. Hurrey Riches, of the Taff Vale Railway, state the average weight of their 10-ton trucks to be 5 tons 5 hundredweight and 4 tons 17 hundredweight respectively. Still, making every allowance for errors of this nature, and the possibility of Mr. Jefferds having placed his case in the most favourable light, there is no doubt but that the Americans carry their merchandise over their railways with a far lower proportion of tare to paying load than is the case in England. It has been notorious for years that American railway rates are far below those of this country. We will, however, let Mr. Jefferds speak for himself by making selections from his paper, merely first pointing out the great importance of this question upon our national well-being.

The supremacy of Great Britain—indeed her existence as a Power—is founded upon cheap ocean carriage. We can carry goods across the sea at a lower price than any other people.

Were we to lose that advantage to-morrow, a large part of our population would be in want of bread within a few months, and there would hardly be an individual in the country whose wealth and comfort would not be lessened. Railway carriage is of next importance, and it is only our insular position and the small size of the country which renders it secondary. There is an impression, well or ill founded, that railway goods carriage might be conducted with more economy in England; and when an American expert comes to us to show how, in his opinion, an improvement may be made, he is worthy of our best attention.

Mr. Jefferds begins his paper by pointing out that the present build of goods trucks on English railways differs nothing in principle, and but little in construction, from the truck made by George Stevenson to carry the barrel of water required for replenishing the *Rocket's* boiler. This, perhaps, is rather an exaggerated statement, but there is more truth in it than we find it pleasant to acknowledge. In America, however, such vehicles, as we have already said, are no longer seen. "Since 1865, the railway rates of the United States have," the paper says, "been reduced fully 79 per cent.; so that the railways are now rendering for £21 the same service for which in 1865 they charged £100. The reason they have been able to make so great a reduction is that they have gradually improved their goods waggons, which would formerly carry loads of their own weight only, but will now carry three or four times as much. . . . In 1889 the average rate charged for all descriptions of goods on all the railways of the United States, including terminal charges, was only 0·488*d.* per ton mile, while the average cost to the railways was only 0·311*d.* The average dividend on highly inflated shares was 3·3 per cent." Turning to individual instances, Mr. Jefferds selects three prominent American lines—the New York Central, the Pennsylvania, and the Philadelphia and Erie. The working expenses per ton mile on these were 0·28*d.*, 0·201*d.*, and 0·176*d.* respectively. The working expenses per ton mile on our London and North-Western Railway are 0·65*d.* per ton mile, or three times as much as the great American line, the Pennsylvania. When one thinks how many millions two-thirds of the cost of goods carriage in this country amounts to, one begins to grasp the magnitude of the question. According to Mr. Jefferds, all this vast sum may be saved by the use of his carriage, although he only claims a modest 9 per cent. for his particular tube-frames.

The average Englishman often wonders how it is American farmers can send wheat right across the Atlantic and undersell British growers comparatively on the spot, and that more especially since agricultural rents have so gone down that farms can be got at purely nominal rents. Here, however, is a fact, according to Mr. Jefferds's paper, which may help to throw some light on the question:—"At the present time, for every hundred tons of grain he sends to London, a farmer living 1000 miles inland in the United States has an advantage of £30, after paying both land and ocean transit, over a farmer living at Stirling in Scotland, only 420 miles from London."

The benefits promised by Mr. Jefferds, if we use his big bogie waggons, are, indeed, immense, but the price we shall have undoubtedly to pay for these benefits is immense also. In the first place, it would be very difficult—practically, we think, impossible—to run these long waggons in mixed trains with the English trucks. The difficulties are mechanical—the principal one being the system of buffing—but we have not space to enter upon them here. Therefore these long bogies could only be brought in by a very sweeping change. What would this involve? Nearly the whole of the usual appliances on the permanent way would have to be entirely reconstructed. Sidings and platforms would be too short, points would have to be altered, locking bars and switching apparatus would have to be replaced, turntables would be too small, hydraulic hoists not sufficiently powerful, even if large enough, and weighbridges would have to be replaced, coal-tips rebuilt—in fact, English railway lines would want reconstructing so far as the appliances for dealing with goods traffic are concerned.

There is, however, another salient feature to consider before we can take Mr. Jefferds and his big bogies to our bosom.

The goods traffic of America is more in bulk than that of England, as might be expected in comparing a comparatively new and sparsely peopled country with one older and more crowded. A 30- or 40-ton waggon can be loaded at St. Louis, Chicago, or elsewhere, and sent through to New York. The journey is long enough to make a big car worth filling. In Britain the conditions are different. During the discussion, one English railway manager said the average lading of general merchandise on his line was not much above 2 tons; another, Mr. Williamson, of the Great Western Railway, gave 2½ tons as a fair average. Mr. Jefferds retorts to this that no one expects a truck to go with only one parcel; the trucks can be filled even if it takes the goods of twenty consignees to make a load. Here again another question arises—Do the Americans pay for cheapness by delay? In England a merchant or manufacturer expects goods given over to the Company one afternoon to be delivered the next day (perhaps his expectation is not always fulfilled); but in America, we are told, no such expedition is observed. May not this be due to the fact that a big waggon is often waiting for the last hundredweight or two to make up its load?

The fact is, the question wants treating quantitatively, and for this purpose a vast mass of statistics must be accumulated; for Mr. Jefferds has only touched the fringe of the question. The arguments he has advanced are, however, sufficiently powerful to have made out a very strong *primâ facie* case—most distinctly a case for inquiry. The railway authorities of this country are the only persons who can supply the details by means of which the problem can be adequately discussed.

The discussion on Mr. Jefferd's paper occupied the greater part of the two evenings of the meeting. Mr. Addy's paper on milling cutters was, however, read and discussed. The author gave analyses of the steel used for the purpose, which appeared to approximate closely to razor steel, and by means of wall diagrams explained the mechanical principles which he considered should govern the construction of milling tools, and the machines in which they are used. Without the aid of these diagrams it would be impossible to make the subject clear, and for these we must refer our readers to the volume of the Transactions. The discussion which followed the reading of the paper turned chiefly on the speeds of cutting by milling in use respectively in this country and America; the fact that the American machinists are in advance of us in this respect being fully acknowledged by those present.

The next meeting of the Institution will be held in London early next year.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Vice-Chancellor, the Marquis of Hartington, LL.D., of Trinity College, Lord Walsingham, M.A., of Trinity College, Dr. Morgan, Master of Jesus College, Dr. A. S. Lea, Prof. Browne, Prof. Liveing, Prof. Foster, Albert Pell, M.A., of Trinity College, J. D. Dent, M.A., of Trinity College, W. Aldis Wright, M.A., of Trinity College, L. Ewbank, M.A., of Clare College, F. Whitting, M.A., of King's College, R. F. Scott, M.A., of St. John's College, J. R. Green, M.A., of Trinity College, have been appointed a Syndicate to consider the subject of the letter, dated July 25, 1890, addressed by the President of the Board of Agriculture to His Grace the Chancellor, on the subject of Agricultural Education in the University, and to report to the Senate before the end of the Lent Term, 1891.

At the annual election, on November 3, three Fellowships out of five were awarded to students of Natural Science:—Mr. R. A. Sampson, B.A. (Third Wrangler, 1888, First Smith's Prizeman, 1890), Lecturer in Mathematics at King's College, London, for researches in Hydrodynamics; Mr. L. E. Shore, M.A., M.B., B.C. (First Class Natural Sciences Tripos, 1884–85), (Senior Demonstrator of Physiology in the University, for researches in Physiology; E. H. Hankin, B.A. (First Class Natural Sciences Tripos, 1888–89), Junior George Henry Lewes Student in Physiology, for researches in Bacteriology.

Mr. Walter Heape, M.A., of Trinity College, has been elected to the Balfour Studentship in Animal Morphology, in succession to Mr. William Bateson, Fellow of St. John's College.

Mr. E. E. Sikes, Scholar of St. John's College, has been appointed by the Vice-Chancellor to hold the Newton Studentship at the British School of Archæology in Athens.

The Board for Biology and Geology propose to take power to appoint to the University Table of the Marine Biological

Laboratory at Plymouth, a student of either sex not a member of the University, failing a suitable University applicant.

The proposed new statute affecting the contributions to the University of financially depressed Colleges, passed the Senate on October 30, by 72 votes to 30. The opposition was headed by Prof. Humphry, Prof. Liveing, and Mr. W. N. Shaw. Under the new statute, which has yet to receive the consent of the Queen in Council, depressed Colleges may withhold part of their contribution to the University, and, instead thereof, elect University teachers to Fellowships.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 27.—M. Hermite in the chair.—Observations of the planet Venus at Nice Observatory, by M. Perrotin. (See Our Astronomical Column.)—On the reduction to the canonical form of differential equations for the variation of arbitrary constants in the theory of movements of rotation, by M. O. Callandreau.—The neutral meridian of Jerusalem-Nyanza, proposed by Italy to fix the universal hour, determined by its horary distance from 120 observatories, by M. Tondini. A list is given of the time-intervals of sixteen important observatories from the Jerusalem meridian, which cuts the equator about 75 kilometres east of Lake Nyanza. This list is part of a larger one giving the latitudes and time-intervals of 120 observatories from the same meridian. —On the developments in series of the integrals of certain differential equations, by M. R. Liouville.—Periodic visibility of interference phenomena when the light source is limited, by M. Ch. Fabry.—Thermo-electric researches, by MM. Chassagny and Abraham. It is well known that if thermo-electric couples be formed from three metals, A, B, and C, the electromotive forces obtained at a given temperature in each case may be expressed by the following equation :—

$$E(AC) = E(AB) + E(BC).$$

The authors have found the following results in some researches on this relation :—

	Electromotive Forces.	
Couples.	Calculated.	Observed.
Iron-Copper	0·0010925 volt	0·0010926 volt.
Iron-Platinum	0·0016842 ,,	0·0016842 ,,
Copper-Platinum	0·0005917 ,,	0·0005917 ,,

—Electrolysis of aluminium fluoride by igneous fusion, by M. Adolphe Minet. The author has previously shown that he had produced aluminium by electrolyzing its fluoride. He now describes the composition and properties of the bath used, and the relation between the constants of the current and those of the electrolyte—(1) when the salts which make up the bath are chemically pure; (2) when the electrolyte is mixed with other salts.—On amylamines, by M. A. Berg.—On the arteries and veins of nerves, by MM. Quénu and Lejars.—On the changes of colour of the common frog (*Rana esculenta*), by M. Abel Dutartre.—On the anatomy of the grasshopper and lizard, by M. Ch. Contejean.—The rot of the heart of the beetroot, by M. Prillieux.—Seismic motions at Chili: earthquakes of May 23, 1890, by M. A. F. Noguès. Of the eighteen movements recorded, five took place during the spring in the southern hemisphere, one in the summer, four in autumn, and eight in winter. Of the six of which the direction of motion has been exactly determined, three had an east to west direction, one south-west to north-east, one from north to south, and one from south to north.—Experiments on sedimentation, by M. J. Thoulet.—Theory of sedimentation, by M. A. Badoureau.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 6.

LINNEAN SOCIETY, at 8.—A Contribution to the Study of the Relative Effects of different parts of the Solar Spectrum on the Assimilation of Plants: Rev. Prof. Henslow.

CHEMICAL SOCIETY, at 8.—The Magnetic Rotation of Saline Solutions: Dr. W. H. Perkin.—Note on Normal and Iso-propylparatoluidine: E. Hori and H. F. Mosley.—The Action of Ammonia and Methylamine on the Oxylepideus: Dr. F. Klingemann and Dr. W. F. Laycock.—Condensation of Acetone Phenanthraquinone: G. H. Wadsworth.

FRIDAY, NOVEMBER 7.

GEOLOGISTS' ASSOCIATION, at 8.—*Conversazione.*

SATURDAY, NOVEMBER 8.

ROYAL BOTANIC SOCIETY, at 3.45.

ESSEX FIELD CLUB (at Loughton), at 7.—Essex Meteorological Records: Rev. T. A. Preston, (Communicated, with some Notes on Dr. Derham's Early Records, by Prof. G. S. Boulger.—Some Notes on Dipsacus sylvestris and D. pilosus, and their Natural Relationship: J. French.

SUNDAY, NOVEMBER 9.

SUNDAY LECTURE SOCIETY, at 4.—Why and how we Eat our Dinner (with Oxy-hydrogen Lantern Illustrations): Dr. Andrew Wilson.

TUESDAY, NOVEMBER 11.

MINERALOGICAL SOCIETY, at 8.—Anniversary Meeting.—Election of Officers.—Twins of Marcasite in Regular Disposition upon Cubes of Pyrites: Dr. C. O. Trechmann.—Tetartohedrism of Ullmannite: H. A. Miers.—Notes on Cassiterite: R. H. Solly.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Steam on Common Roads: John McLaren.

WEDNESDAY, NOVEMBER 12.

GEOLOGICAL SOCIETY, at 8.—On the Porphyritic Rocks of the Island of Jersey: Prof. A. De Lapparent. (Communicated by the President.)—On a New Species of Trionyx from the Miocene of Malta, and a Chelonian Scapula from the London Clay: R. Lydekker.—Notes on Specimens collected by W. Gowland in the Korea: T. H. Holland. (Communicated by Prof. J. W. Judd, F.R.S.).—Further Notes on the Stratigraphy of the Bagshot Beds of the London Basin (North Side): Rev. A. Irving.

THURSDAY, NOVEMBER 13.

MATHEMATICAL SOCIETY, at 8.—The Influence of Applied on the Progress of Pure Mathematics: the President.—Spherical Harmonics of Fractional Order: R. A. Sampson.—Proofs of Steiner's Theorem relating to Circumscribed and Inscribed Conics: Prof. G. B. Mathews.—On an Algebraic Integral of Two Differential Equations: R. A. Roberts.—Some Geometrical Theorems: Osher Ber.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.

FRIDAY, NOVEMBER 14.

ROYAL ASTRONOMICAL SOCIETY, at 8.

CONTENTS.

PAGE
Priestley, Cavendish, and Lavoisier. By **Prof. T. E. Thorpe, F.R.S.** . . . 1
A Hand-book of Photography. By **Prof. R. Meldola, F.R.S.** . . . 3
Contributions to Indian Botany. By **W. Botting Hemsley, F.R.S.** . . . 6
Our Book Shelf:—
  Pinkerton: "Elementary Text-book of Trigonometry" 7
  Macdonald: "Higher Geometry" . . . 8
  Shortland: "Nautical Surveying" . . . 8
  Macfarlane: "An American Geological Railway Guide" . . . 8
Letters to the Editor:—
  Araucaria Cones.—**The Duke of Argyll, F.R.S.** . . 8
  On the Soaring of Birds. (*Illustrated.*)—**Prof. F. Guthrie** . . . 8
  The Value of Attractive Characters to Fungi.—**Charles R. Straton** . . . 9
  Extraordinary Flight of Leaves.—**R. Haig Thomas** . 9
  Kœnig's Superior Beats.—**Rev. Walter Sidgreaves, S.J.** . . . 9
The Cell Theory, Past and Present. I. By **Sir William Turner, F.R.S.** . . . 10
The Causes of Cyclones and Anticyclones. By **Henry F. Blanford, F.R.S.** . . . 15
On the Anatomy and Development of *Apteryx* . . . 17
Captive Balloons. (*Illustrated.*) . . . 17
The Coco-de-Mer in Cultivation. By **William Watson** and **W. T. T. D.** . . . 19
Notes . . . 19
Our Astronomical Column:—
  The Rotation of Venus . . . 22
  Spectrum of the Zodiacal Light . . . 22
  D'Arrest's Comet . . . 22
The Institution of Mechanical Engineers . . . 22
University and Educational Intelligence . . . 23
Societies and Academies . . . 24
Diary of Societies . . . 24