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

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INDEX

ABASTOUMAN, a New Observatory at, 133
Abbadie (M. d'), on the Variations in the Intensity of Terrestrial Gravitation, 384
Abbe (Prof. C.), Atmospheric Electricity, Earth-Currents and Terrestrial Magnetism, 261
Abbott's (Dr. W. L.) Collections of African Mammals, F. W. True, 39
Abbott (W. J. L.), Walrus in the Thames Valley, 132
Abney (Captain W. de W., F.R.S.), Sensitiveness of the Eye to Light and Colour, 538
Abnormality in the Veins of the Rabbit, on an, Prof. W. N. Parker, 270
Absorption of our Atmosphere, Photographic, Prof. Schaeberle, 304
Abyssinia, Proposed Expedition of Mr. and Mrs. Theodore Bent to, 115
Academy, French, Science Prizes, 232
Academy of Sciences, Turin Royal, the Bressa Prizes, 233
Acoustics, on Plane and Spherical Sound-Waves of Finite Amplitude, Dr. C. V. Burton, 500
Addenbrooke (Mr.), Diffusion of Light, 191
Aëronautics: Meteorological Balloon Ascent at Berlin, A. L. Rotch, 46; Dirigible Balloon in Construction at Chalais-Meudon, 112; Exploration of the Higher Regions of the Atmosphere by Means of Free Balloons provided with Automatic Recorders, Gustave Hermite, 119; the First Aërial Voyage across the English Channel, R. de C. Ward, 143; the Longest Balloon Ascent on Record, Maurice Mallet, 182
Africa: Marine Shells of South Africa, G. B. Sowerby, 27; the Ferns of South Africa, Thos. R. Sim, J. G. Baker, F.R.S., 291; Dromedaries in German South-West Africa, Captain von François, 38; New Harbour found in German South-West, 452; Dr. W. L. Abbott's Collection of African Mammals, F. W. True, 39; Mr. D. J. Rankin's Zambesi Journey, 64; Kettler's Afrikanische Nachrichten, 115; Captain H. L. Gallwey's Travels in Benin Country, 134; E. Wilkinson's Journey in Kalabari Desert, 134; M. Alexandre Delcommune's Lomami Expedition, 209; the Uganda Commission, 210; Proposed Exploration of Africa by Telegraph, Cecil Rhodes, 210; the Bonjo, a Cannibal Tribe, M. Dybowski, 257; Territorial Nomenclature, 282; the Stanley Falls District of the Congo, M. Page, 282; African Nomenclature, 304; Stoppage of M. Mizon's Adamava Expedition, 304; Aston Chanler's Expedition to Lake Rudolf, 327; the Soil of Sakalava Plain, Madagascar, Emile Gautier, 327; the Frontier Delimitation between British South Africa Company's Territory and Portuguese Possessions, Major Leverson, 327; Death of R. H. Nelson, 353; Dr. Baumann's Journeys in the Nile-Sources Region, 377; the Orthography of African Place-Names, 400; Two Akka Girls brought to Germany by Dr. Stuhlmann, 470; the Katanga Company's Expeditions, 474; French Exploration towards Lake Chad, 519; Arrival of Mr. and Mrs. Theodore Bent at Adowa, 519; at Aksum, 547; the Partition of Africa, J. Scott Keltie, 580; Return of the Delcommune Expedition, 590
Afterglow, the, Sereno E. Bishop, 102; Prof. Grenville A. J. Cole, 127
Afterglows and Bishop's Ring, the, T. W. Backhouse, 582
Agassiz (Prof. A.), Observations in the West Indies, 608
Age, the Earth's, Bernard Hobson, Dr. Alfred Russel Wallace, 175, 226; Clarence King, 285
Ages, Ancient Ice, J. Lomas, 227
Aggressive Mimicry, the *Volucellæ* as Examples of, Edward B. Poulton, F.R.S., 28
Aggressive Mimicry, the Alleged, of *Volucellæ*, William Bateson, 77
Agriculture: the South Australian Rust in Wheat Conference, 86; the Plague of Field Voles in Scotland, 155; the Plague of Field Mice in Thessaly and Scotland, 396; Agriculture in the United States, Experiment Stations, A. Warrington, F.R.S., 157; Investigations in Soils, 157; Influence of Manure on Development of Roots, M. Dehérain, 280; Journal of the Royal Agricultural Society of England, 285; Field Experiments on the Fixation of Free Nitrogen, James Mason, 285; Yew Poisoning, E. P. Squarey, Charles Whitehead, W. Carruthers, F.R.S., and Dr. Munro, 285; Proposed Investigation of Chemical Composition of Soil in Cape Colony, 301: a Text-book of Tropical Agriculture, H. A. Alford Nicholls, 313; the Florida Phosphate Beds, T. N. Lupton, 325; the New York State Pecuniary Contribution to Agriculture, 349; Wood Ashes as a Medicine for Farm Animals, J. M. Stahl, 397; Proposed Commemoration of the Jubilee of Sir John Lawes' Rothamsted Experiments, 448; Ornithology in Relation to Agriculture and Horticulture, John Watson, 533; Manual of Dairy Work, Prof. James Muir, Walter Thorp. 555; Agricultural Chemistry; the Food of Plants, A. P. Laurie, 556
Aids to Experimental Science, Andrew Gray, 173
Aigran (M. A.), Action of Temperature upon the Rotatory Power of Liquids, 576
Ainu, Yezo and the, Prof. J. Milne, F.R.S., 330; A. H. Savage Landor, 330
Air-pumps, Automatic Mercurial, Dr. August Raps, 369
Aitken (John), on the Particles in Fogs and Clouds,
Akka Girls, Two, brought to Germany by Dr. Stuhlmann, 470
Alabama, Great Meteor in, 86
Alcohol Solutions, Temperature of Maximum Density of, L. de Coppet, 48
Alcyonaria Stolonifera, Revision of Genera of, Dr. Hickson, 215
Alexander (Peter), Treatise on Thermodynamics, 388
Algebra: Principles of the Algebra of Vectors, A. Macfarlane, 3; the Algebra of Co-planar Vectors and Trigonometry, R. Baldwin Hayward, F.R.S., 266; Quaternions and the Algebra of Vectors, Prof. J. Willard Gibbs, 463; Algebra for Beginners, H. S. Hall and S. R. Knight, 28; the Theory of Substitutions and its Applications to Algebra, Dr. Eugen Netto, 338; a new Algebra, T. B. Sprague, 527
Alkali Metals, the Colours of the, G. S. Newth, 55; Wm. L. Dudley, 175
Allen (Edgar), Anatomy of Larva of *Palæmonetes varians*, 237; on the Minute Structure of the Gills of *Palæmonetes varians*, 261
Allen (F. J.), Dust Photographs, 341
Allen (Grant), Science in Arcady, 173
Alligators' Nest, the, S. Devenish, 587
Alloys, on Iron, 58
Alloys Research Committee, Second Report of the, Prof. W. C. Roberts-Austen, F.R.S., 617
Almanac, the Nautical, for 1896, 326
Alpes Françaises, les, Albert Falsan, 76
Alpine Flowers, on the Cause of Bright Colours of, Dr. J. Joly, F.R.S., 431
Alpine Lakes, the Glacier Theory of, Dr. Alfred Russel Wallace, 437
Alsace-Lorraine, Statistics of Wine-growing in, 614
Aluminium Slate Pencils, 131
Amagat (E. H.): the Laws of Compressibility of Liquids, 48; Laws of Dilatation of Gases under Constant Pressure, 96;

the form of Isothermals of Liquids and Gases, 143; Dilatation and Compressibility of Water, 288; Expansion of Water at Constant Pressure and at Constant Volume, 623
Amazons, River, the Naturalist on the, Henry Walter Bates, F.R.S., 269
Ambronn (Dr. L.), Astronomical Instruments up to Date, 114
America: American Microscopical Society—Prizes offered for Encouragement of Research, 15; American Vine-disease; appearance of the Black Rot in Europe, 16; American Meteorological Journal, 46, 143, 261, 454, 574; a New Blind Cave Salamander from North America, L. Stejneger, 62; American Opinion of Photography in England, Xanthus Smith, 86; American Journal of Science, 188, 285, 380, 499, 596; On the American Iron Trade and its Progress during Sixteen Years, Sir Lowthian Bell, F.R.S.; John Parry, 195; American Society of Naturalists, 205; American Forestry, C. S. Sergent, Prof. W. R. Fisher, 275; American Mechanism, 241; a Correction, 281; the Destruction of Ancient Monuments in Central America, M. H. Saville, 302; American Psychological Association, 348; the U.S. Geological Survey and American Mining Industries, 350; American Journal of Mathematics, 380, 620; Observations of Atmospheric Electricity in America, J. C. Mendenhall, Prof. Oliver J. Lodge, F.R.S., 392; Archæological Work in America, Prof. Putnam, 474; the Destruction of Trees in America, Dr. W. J. Beal, 563
Amide and Imide of Sulphuric Acid, the, Dr. Traube, A. E. Tutton, 566
Aminol, a True Disinfectant, Dr. E. Klein, F.R.S., 149, 247; Hugo Wollheim, 246
Amsterdam Royal Academy of Sciences, 216, 288, 432, 504, 624
Amu-Daria, Discovery of Subterranean Town on the, 64
Analysis, Modern Advanced, G. B. Mathews, 289
Anatomy; Bell's Idea of a New Anatomy of the Brain, Jas. B. Bailey, 11; the Brain in Mudfishes, Dr. Rudolf Burckhardt, 339: the Relation of Anatomy to Art, Paul Richer, 470; Die Epiglottis, Carl Gegenbaur, 542; the Rudimentary Hind Limbs of Great Fin-whale, Humpback, and Greenland Right-whale compared, Dr. John Struthers, 588
Ancient Ice Ages, T. Mellard Reade, 174
Anchisaurus colurus, Restoration of, Prof. O. C. Marsh, 349
Ancient Dyes, Notes on some, Edward Schunck, F.R.S., 22
Ancient Ice Ages, J. Lomas, 227
Anderson (A.), Blind Animals in Caves, 439
Anderson (Dr. W.), Technical Education, 155
André (M.), on the Organic Substances constituting Vegetable Soil, 551
Andromeda, Comet in, 40
Andromedes, the, Maclair Boraston, 326
Anemometer, New Maximum, W. H. Dines, 118
Anemometry, H. W. Dines, 143
Angot (Alfred), Eiffel Tower, Experiments on Decrease of Air-Temperature with Elevation, 240
Animal Body, the Chemical Basis of the, A. Sheridan Lea, F.R.S., 340
Animal Life, the Study of, J. Arthur Thomson, 2
Animals, Blind, in Caves, Prof. E. Ray Lankester, F.R.S., 389, 486; J. T. Cunningham, 339, 537; A. Anderson, 439; G. A. Boulenger, 608
Animals' Rights, H. S. Salt, 73, 127; the Reviewer, 151
Annalen des K.K. Naturhistorischen Hofmuseums, 525
Annelid, Sponge and, a Strange Commensalism, James Hornell, 78
Anschutz (Prof.), Compounds of Salicylic and Cresotinic Acid Lactides with Chloroform, 255
Antarctic Continent, the Chatham Islands and an, H. O. Forbes, 474
Antarctic Whaling Fleet, the, 282; Letters from the, 590
Anthropology: Prof. Virchow on the Immediate Task for Anthropologists, 38; Anthropological Institute, 46, 239, 335, 455, 527, 623; Development of Buddhist Architecture and Symbolism in Burma, Major Temple, 46; Prehistoric Interments of Bahi Rossi Caves, near Mentone, A. J. Evans, 239; Mythographic Origin of Polynesian Ornament-forms, Dr. H. C. March, 239; Relics of Primitive Fashions in India, Kedarnath Basu, 301; Dr. Ten Kate on the Type-characteristics of the North American Indians, 374; the Tokelaus, 423; on Nicobar Pottery, E. H. Man, 455; on some Islands of the New Hebrides, Lieut. Boyle, T. Somerville, 455; Physical Anthropology in America, 475; Use of Chloride of Potassium instead of Salt by Soudanese, M. Dybowski, 499; the International Congress of Prehistoric Archæology and Anthropology, 523; Prehistoric Anthropology, the Quaternary Deposits in Russia and their Relations to the Finds Resulting from the Activity of Prehistoric Man, S. Nikitine, 523; Race in Anthropology, M. Topinard, 524; Which is the most Ancient Race in Russia, Prof. A. Bogdanov, 524; on the Rude Stone Implements of the Tasmanians, showing them to belong to the Palæolithic or Unground Stage of the Implement-makers Art, Dr. Tylor, 527; Anthropological Uses of the Camera, E. F. im Thurn, 548; Neolithic Village of the Roche-au-Diable, near Tesnières, Canton of Lorez-le-Bocage (Seine-et-Marne), Armaud Viré, 576; on Egyptian Mummies, Prof. Macalister, 623
Ants, the Use of, to Aphides and Coccidæ, J. D. A. Cockerell, 608
Aphides and Coccidæ, the Use of Ants to, 608
Appleyard (J. R.), Salts of Active and Inactive Glyceric Acid: the Influence of Metals on the Specific Rotatory Power of Active Acids, 405
Applied Mechanics, Elementary Manual on, Prof. Jamieson, 147
Applied Natural History, W. L. Calderwood, 492
Aqua et Terra, Quæstio de, Edmund G. Gardner, 295
Aquarium, the Boxing Kangaroo at Westminster, 111
Arabia Petræa, Geology of, Prof. Edw. Hull, F.R.S., 166
Arborescent Frost Patterns, Prof. R. Meldola, F.R.S., 125; G. J. Symons, F.R.S., 162; Rev. T. G. Bonney, F.R.S., 162; Dr. J. H. Gladstone, F.R.S., 162; Dr. Wetterhan, 162; J. T. Richards, 162; J. J. Armitage, 162; Prof. G. A. Lebour, Prof. Sollas, F.R.S., 213
Arcady, Science in, Grant Allen, 173
Archæology, the Tell-el-Hesy Excavations, F. J. Bliss, 302; the Destruction of Ancient Monuments in Central America, M. H. Saville, 302; Ancient Copper Relics discovered in the Course of M. de Sarzec's Excavations in Chaldea, M. Berthelot, 360; Depredations among the recently-discovered Phœnician Tombs in Malta, 396; Archæological Work in America, Prof. Putnam, 474; the International Congress of Prehistoric Archæology and Anthropology, 523
Architecture and Symbolism in Burma, Development in Buddhist, Major Temple, 47
Architecture, Naval: Institution of Naval Architects, 519; Annual General Meeting, 494; the Strength of Bulkheads, Dr. Elgar, 520: Experiments on the Transmission of Heat through Tube-plates, A. J. Durston, 521; Notes on Boiler-testing, J. T. Melton, 521; the Apparatus for Measuring and Registering Vibrations of Steamers, E. Otto Schlick, 521; Experiments with Engines of S. S. Iveagh, John Inglis, 521
Arctic Expedition, Dr. Nansen's, 65; Proposed Arctic Expedition of Lieut. Peary, 133; Proposed Arctic Expedition by way of Franz Josef Land, F. G. Jackson, 377
Arctic Travel: the Voyage of *La Manche* to Iceland, Jan Mayen, and Spitzbergen in 1892, M. Bienaimé, 48
Argyll (the Duke of, F.R.S.), Glacier Action, 389; Origin of Lake Basins, 485
Arizona, Central, Cliff and Cave-dwellings in, J. W. Tourney, 112
Armitage (J. J.), Arborescent Frost Patterns, 162
Armstrong (H. E.), the Origin of Colour and Fluorescence, 238; Products of Interaction of Zinc Chloride on Sulphuric Acid and Camphor, 239; Griess-Sandmeyer Interactions and Gattemann's Modification thereof, 239; Origin of Colour VII. and VIII., 551
Army, Science in the Public Schools and the Scientific Branches of the, 513
Arno (Signor), Rotation of Cylinder by Inductive Action, 374
Arons (Dr.), an Arc Light between Mercurial Electrodes in Vacuo, 24
Arsonval (M. d'), the Physiological Effects of Electric Currents of High Frequency, 517
Art, the Relation of Anatomy to, Paul Richer, 470
Art, the Evolution of Decorative, Henry Balfour, 606
Arts, Society of, Opening Meeting of, 63
Arteries in the Rabbit, Unusual Origin of, Philip J. White, 365
Artesian Boring and Irrigation in New South Wales, J. W. Boultbee, 183
Arthropoda, on the Walking of, Henry H. Dixon, 56
Arthur (J. G.), Gases in Living Plants, 427

Artificially Incubated Eggs, W. Whitman Bailey, 200
Artiodactyle, Artionyx—a Clawed, Prof. Henry S. Osborn, 610
Artiodactyle, Protocerus, the New, Prof. Henry S. Osborn, 321
Artionyx—A Clawed Artiodactyle, Prof. Henry S. Osborn, 610
Ascidians, the Mantle-cells of, Kowalevsky, 62
Asia, Central, Dr. J. Troll's Journey through, 160
Asia from East to West, the Steppe Belt Traversing, H. J. Mackinder, 353
Asiatic Loess, the Geology of the, Thos. W. Kingsmill, Prof. G. H. Darwin, F.R.S., 30
Assheton (R.), on a Supposed Law of Metazoan Development, 176; on the Development of the Optic Nerve of Vertebrates and the Choroidal Fissure of Embryonic Life, 261
Assmann (Prof.), Detailed Description of the Meteorographs set up in the "Urania-Pillars," 287
Aston (E.), Atomic Weight of Boron, 165
Astronomy: Nova Aurigæ, 399, 159; H. F. Newall, 7; Prof. Barnard, 282; Mr. Huggins, 425; Hydrogen Line Hβ in the Spectrum of, Herr Victor Schumann, 425; Motion of, Prof. W. W. Campbell, 256; the New Star in the Constellation of Auriga, W. J. Lockyer, 137; Spectra of Planetary Nebulæ and, M. Eugen Gothard, 352; Astronomical Column, 18, 40, 63, 88, 114, 132, 159, 186; 208, 235, 256, 281, 303, 326, 351, 376, 399, 425, 451, 473, 498, 518, 546, 565, 589, 616; Comet Barnard (October 12), 18, 40; Comet Brooks (August 28), 18, 41, 63; a New Comet (Brooks, November 19–20), 133, 186, 208, 235, 257, 281, 304, 326, 352, 376, 399, 425, 451; Prof. Kreuz, 159; MM. Esmiol and Fabry, 159; Tabular History of Astronomy to the Year 1500 A.D., Dr. Felix Müller, 18; a Large Telescope, 18; the Atmospheres of Planets, 18; Comet in Andromeda, 40; Motion of the Solar System, Prof. J. G. Porter, 41; Occultation of Mars and Jupiter by the Moon, Prof. Barnard, 41; the Recent Opposition of Mars, Prof. W. H. Pickering, 235; the Canals of Mars, 64; the Channels of Mars, T. W. Kingsmill, 133; the Markings on Mars, M. Schaeberle, 209; the New Comet, 63; W. F. Denning, 77; M. Bigourdan, 88; the Light of Planets, 64; John Garstang, 77: Stellar Magnitudes in Relation to the Milky Way, Prof. Kapteyn, 64; the Fifth Satellite of Jupiter, E. Royer, 71; A. A. Common, 208; E. E. Barnard, 377; the Sizes of Jupiter's Satellites, M. J. J. Lauderer, 473; Jupiter and his Satellites, Prof. Pickering, 518; Rutherfurd Measures of Stars about β Cygni, Prof. Harold Jacoby, 77: Parallax of β Cygni, Harold Jacoby, 399; Motion in the Line of Sight, M. H. Deslandres, 88; Himmel und Erde, the Heat in August 1892, Prof. W. J. van Bebber, 88; Astronomy of the Invisible, Dr. J. Scheiner, 88; Observations of Perseids, 88; Comet Holmes (November 6, 1892), 114, 132, 159, 186, 209, 235, 281, 303, 351, 376, 425, 473; M. Schulhof, 256, 451, 498; Lewis Boss, Rev. E. M. Searle, Mr. Roberts, 256; Dr. F. Cohn, 326; Dr. R. Schorr, 326; Prof. Keeler, Prof. C. A. Young, 518; W. F. Denning, 365; Prof. E. Barnard, 399; Spectrum of Comet Holmes, 235; a New Comet, 133; Astronomy and Astrophysics, 133; a New Observatory at Abastouman, 133; Astronomy at Columbia College, U.S.A., 159; Companion to the Observatory for 1893, 159; Swift's Comet, Prof. Barnard, 186; Comet Swift (*a* 1892), A. E. Douglas, 546; Ultra-Violet Spectrum in Solar Prominences, Prof. G. E. Hale, 186; Ephemeris for Bodies moving in the Biela Orbit, Dr. Chandler, 186; Madras Meridian Circle Observations, 186; a Bright Comet, W. R. Brooks, 114; Astronomical Instruments up to Date, Dr. L. Ambronn and Herr Julius Springer, 114; Motion of β Persei, 115; Proper Motions, M. Deslandres, 115; the Present Comets, T. W. Backhouse, 127; the Star of Bethlehem, J. H. Stockwell, 177; Astronomical Theory of the Ice Age, N. L. W. A. Gravelaar, 200; the Tercentenary of Galileo at Padua, 207; Burnham's Double-star Observations, 281; the Lick Observatory, Miss Milicent W. Shinn, 209; Washington Magnetic Observations, 209; an Atlas of Astronomy, Sir Robert Stawell Ball, F.R.S., 225; Measurement of Distances of Binary Stars, Prof. Arthur A. Rambaut, 226; Astronomical Discoveries in 1892, W. F. Denning, 256; the Meteor Shower of November 23, 1892, 257; Total Solar Eclipse April 15–16, 1893, 304, 376, 584; M. De la Baume Pluvinel, 281, 304; A. Taylor, 317; Astronomical Journal Prizes, 282, 425, 616; Newcomb-Engelmann's Populäre Astronomie, 291; Photographic Absorption of our Atmosphere, Prof. Schaeberle, 304; Harvard College Observatory, Prof. Pickering, 304; Solar Observations at Rome, Prof. Tacchini, 304, 399, 565; the Nautical Almanac for 1896, 326; Eclipse Photography, M. de la Baume Pluvinel, 326; the Andromedes, Maclair Boraston, 326; a New Method of Photographing the Corona, M. H. Deslandres, 327; the Milky Way from the North Pole to 10° of South Declination, drawn at the Earl of Rosse's Observatory at Birr Castle, Dr. Otto Boeddicker, 337; Sun-spots and Magnetic Perturbations in 1892, M. Ricco, 352; New Minor Planets, 352; the Lunar Surface, 352; Remarkable Comets, Mr. Lynn, 376; Relative Positions of Stars in Cluster χ Persei, Sir Robert Ball and Arthur Rambaut, 376; L'Astronomie, 377; Astronomy for Everyday Readers, B. J. Hopkins, 389; the Star Catalogue of the Astronomische Gesellschaft, 399; Coincidence of Solar and Terrestrial Phenomena, Prof. G. E. Hale, 425; Prof. Hale's Solar Photographs, 498; La Grandissima Macchia Solare del Febbrajo 1892, A. Ricco, 429; Distribution of Stars in Space, Prof. J. C. Kapteyn, 432; Théorie du Soleil, Dr. A. Brester, 433; Observational Astronomy, Arthur Mee, 434; Universal Time, 451; the Bielids, 1892, M. Bredichin, 451; the Bielids of 1872, 1885, and 1892, M. Bredichin, 498; Wolsingham Observatory, 518, 590, 616; T. E. Espin, 452; United States Naval Observatory, 452; Yale Astronomical Observatory, 452; the Evolution of Double Stars, T. J. J. See; Prof. G. H. Darwin, F.R.S., 459; Observations of the Zodiacal Light, Arthur Searle and Prof. Bailey, 473; Winck's Lunar Enlargements, 473; L'Astronomie for March, 473; Bermerside Observatory, 473; the Melbourne Observatory, 498; Natal Observatory, 498; Roche's Limit, 509; Prof. G. H. Darwin, F.R.S., 581; the Horizontal Pendulum, Dr. E. von Rebeur-Paschwitz, 519; the Rising and Setting of Stars, 519; Paris Observatory in 1892 M. Tisserand, 546; the Large Nebula near ξ Persei (N.G.C. 1499) (Dr. F. Skinner, 546; Minor Planets, 547; La Planète Mars et ses Conditions d'Habitabilité, Camille Flammarion, William J. Lockyer, 553; Parallaxes of μ and θ Cassiopeiæ, Harold Jacoby, 565; Fall of a Meteorite, 565; Jahrbuch der Astronomie und Geophysik, 566; the Observatory, 566; Centenary of Birth of Wilhelmus Struve, 585; Photographic Chart of the Heavens, M. Lœwy, 589; Catalogue of Southern Star Magnitudes, Edwin Sawyer, 589; a New Table of Standard Wave-Lengths, Prof. H. A. Rowland, 590; Meteor Showers, 590; Distance of the Stars by Döppler's Principle, G. W. Colles, jun., 596; Large Telescopes, 616; Spectrum of β Lyræ, Prof. Keeler, Société Astronomique de France, 616
Astrophysics, Astronomy and, 133
Atlantic (North), Pilot Chart of, 86, 398
Atlantic Ocean, North, Synoptic Daily Weather Charts of, 543
Atlas of Astronomy, An, Sir Robert Stawell Ball, F.R.S., 225
Atlas der Völkerkunde, Dr. Georg Gerland, Dr. Edward B. Tyler, F.R.S., 223
Atmosphere, Higher, Exploration by Means of Free Balloons with Automatic Recorders of, Gustave Hermite, 119
Atmosphere, Photographic Absorption of our, Prof. Schasberle, 304
Atmosphere, the Thermal Exchanges of the, Prof. von Bezold, 552
Atmospheres of Planets, 18
Atmospheric Electricity in America, Observations of, T. C. Mendenhall, Prof. Oliver J. Lodge, F.R.S., 392
Atmospheric Nitrogen, Researches on the Fixation by Microbes of, M. Berthelot, 23
Auriga, the New Star in the Constellation of, W. J. Lockyer, 137
Auroral Phenomena, Thunderstorms and, J. Ewen Davidson, 582
Auroras, the Height and Spectrum of, T. W. Backhouse, 151
Austen (E. E.), Description of New Species of Dipterous Insects of the Family *Syrphidæ*, in the Collection of the British Museum, with Notes on Species described by the late Francis Walker, 335
Australia: the "Bean-tree" of Central, 40; Catalogue of Eastern and Australian Lepidoptera in the Collection of the Oxford University Museum, Col. C. Swinhoe, 53; an Ancient Glacial Epoch in Australia, Dr. Alfred R. Wallace, 55; the South Australian Rust in Wheat Conference, 86; a Large Meteorite from Western Australia, James R. Gregory, 90; Edgar B. Waite appointed Assistant Curator in the Australian Museum, Sydney, 111; an Introduction to the Study of Botany, with a Special Chapter on some Australian Natural

Orders, Arthur Dendy and A. H. S. Lucas, 125; Australian Travels, R. Von Lendenfeld, 274; Tobacco-Culture in Australia, 324; Great Meteorite from Western Australia, 469; the Triangulation of North-west South Australia, 519; Ethnological Observations in Australia, R. Etheridge, 594; Palæontological Discovery in Australia, Prof. Alfred Newton, F.R.S., 606
Automatic Mercurial Air-Pumps, Dr. August Raps, 369
Autres Mondes, Amédée Guillemin, 485
Ayrton (Prof.) Science Teaching, 359
Azoimide, 136
Aztecs, the Calendar System of the Ancients, Zelia Nuttall, 156

Babes (Messrs. V. and A.), the Purification of Water by Bacteriological Methods, 588
Backhouse (T. W.), the Present Comets, 127; the Height and Spectrum of Auroras, 151; the Afterglows and Bishop's Ring, 582
Bacteriology: the Bacteriology of Tetanus, Kikasoto, 158; Bacteriology of Tobacco, and Vinous Fermentation, Suchsland, Nathan, and Kosutany, 208; Investigations on the Behaviour of Micro-organisms at Various Temperatures, 234; Methods of Examining Milk for Tubercle Bacillus, Ilkewitsch and Thörner, 254; Bacilli in Butter, Mrs. Percy Frankland, 283; on the Bacterial Investigation of the Sea and its Floor, H. L. Russell, 285; on the Germination of Seedlings in the Absence of Bacteria, H. H. Dickson, 287; the Action of Light upon certain Micro-organisms, Herr Buchner, 303; Experiments on the Action of Light on *Bacillus anthracis*, Prof. Marshall Ward, F.R.S., 331; Bacteria and Beer, 379; Micro-organisms and their Investigation, Mrs. Percy Frankland, 446; Dunbar on the Question of the Separate Identification of Typhoid and *B. Coli Communis* Bacilli, 472; Blood-Serum Inoculation for Diphtheria, Dr. Wernicke 480; Contribution à l'Etude de la Morphologie et du Développement des Bactériacées, Dr. A. Billet, Dr. Rubert Boyce, 532; the Potato as a Diagnostic Agent, Herr Krannhals, 545; the Purification of Water, Messrs. V. and A. Babes and Percy Frankland, 588; Further Experiments on the action of Light on *Bacillus anthracis*, H. Marshall Ward, 597
Baddeleyite; the Occurrence of Native Zirconia, L. Fletcher, 282
Baden-Powell (B. F. S.), In Savage Isles and Settled Lands, 122
Bagard (Henri), On Thermo-electric Phenomena between Two Electrolytes, 263
Bailey (Prof.) Observations of the Zodiacal Light, 473
Bailey (E. H. S.), the Great Spirit Spring Mound, Kansas, 87
Bailey (G. P.), Meteor of March 18, 1893, 516
Bailey (Jas. B.), Bell's Idea of a New Anatomy of the Brain, 11
Bailey (Prof. L. H.), Value of Electric Light for Lettuce and other Winter Crops, 130
Bailey (W. Whitman), Artificially Incubated Eggs, 200
Bailey (W.), Williams on the Dimensions of Physical Quantities, 116
Baker (J. G., F.R.S.), the Ferns of South Africa, Thos. R. Sim, 291
Baldwin (Prof. J. Mark), Tracery Imitation, 149
Balfour (Henry), Women and Musical Instruments, 55; the Evolution of Decorative Art, 606
Ball (Sir Robert Stawell, F.R.S.), an Atlas of Astronomy, 225; Relative Position of the Stars in Cluster χ Persei, 376
Ball (Dr. V., F.R.S.), Lion-Tiger and Tiger-Lion Hybrids, 390, 607
Ballooning: Meteorological Balloon Ascent at Berlin, October 24, 1891, A. L. Rotch, 46; Dirigible Balloon in Construction at Chalais-Meudon, 112; Exploration of Higher Atmosphere by Means of Free Balloons with Automatic Recorders, Gustave Hermite, 119; the First Aerial Voyage Across English Channel, R. de C. Ward, 143; the Longest Ascent on Record, Maurice Malet, 182; Exploration of the Higher Atmosphere, Gustave Hermite, 600
Baltic Ship Canal, C. Beseke, 579
Bangweolo, Mr. Joseph Thompson's Journey to Lake, 115
Barbary States, Bibliography of the, Lieut.-Col. Sir R. Lambert Playfair and Dr. Robert Brown, 298
Barber (Samuel), Beneath Helvellyn's Shade, 364
Baring-Gould (S.), Strange Survivals: Some Chapters in the History of Man, 53
Barnaby (Mr.), the Screw Propeller, 21
Barnard, Comet (October 12), 18, 40
Barnard (Prof.), Occultation of Mars and Jupiter by the Moon, 41; Nova Aurigæ, 282; Swift's Comet, 186; Jupiter's Fifth Satellite, 377; Comet Holmes (1892, III.), 399
Barometer, a Highly Sensitive Mercury, Dr. Carlo di Lungo, 586
Barometry, Standard, Dr. Frank Waldo, 511
Barrow (Geo.), on an Intrusion of Muscovite-biotite-gneiss in the South-Eastern Highlands, and its Accompanying Thermo-Metamorphism, 575
Barry (John Warren), Studies in Corsica, 462
Bartholomew (John), Death of, 547
Barus (C.), Preliminary Note on the Colours of Cloudy Condensation, 380; Isothermals, Isopiestics and Isometrics relative to Viscosity, 380
Basset (A. B., F.R.S.), Stability and Instability of Viscous Liquids, 94; Théorie Mathématique de la Lumière, H. Poincaré, 386; Motion of a Solid Body in a Viscous Liquid, 512
Basset (L.), the New Triangulation of France, 71
Bastelaer's (D. A. van), Observations on Ozone, 373
Bates (Henry Walter, F.R.S.), the Naturalist on the River Amazons, 269
Bateson (William), the Alleged "Aggressive Mimicry" of *Volucellæ*, 77
Bateson (W.), on Numerical Variation in Digits in Illustration of a Principle of Symmetry, 503
Baudin (L. C.), Depression of Zero in Boiled Thermometers, 143
Bauerman (H.), Prof. Wadsworth on the Geology of the Iron, Gold, and Copper Districts of Michigan, 118
Baumann's (Dr.), Journeys in Nile Sources Region, 377
Bayard (F. C.), the Direction of the Wind over the British Isles, 1876–80, 623
Beal (Dr. W. J.), the Destruction of Trees in America, 563
Bean-Tree of Central Australia, 40
Beard (J.), on a Supposed Law of Metazoan Development, 79
Beauties of Nature, the, and the Wonders of the World we Live in, Hon. Sir John Lubbock, F.R.S. 28
Beaver, Castorologia, or the History and Traditions of the Canadian, Horace Martin, 224
Bebber (Prof. W. J. van), Heat in August, 1892, 88
Beck (C. R.): the Preparation of Phosphoric Oxide free from the Lower Oxide, 430; Note on the Preparation of Platinous Chloride and on the Interaction of Chlorine and Mercury, 479
Beer, Bacteria and, 379
Bees, The Death's Head Moth, J. R. S. Clifford, 234
Bees in New South Wales, Plants most visited by, 614
Beetle Tamed, a, 280
Beetles, Butterflies, Moths, and other Insects, A. W. Kapple and W. Egmont Kirby, 148
Behring (Dr.): the Blood-serum Therapeutists, 336; Experiments with Preventive Serum, 600
Bell (F. Jeffrey), Catalogue of the British Echinoderms in the British Museum, 508
Bell (H. H. J.), Notes on a Spider, 557
Bell (Sir Lowthian, F.R.S.), on the American Iron Trade, and its Progress during Sixteen Years, John Parry, 195
Bell's Idea of a New Anatomy of the Brain, Jas. B. Bailey, 11
Beneath Helvellyn's Shade, Samuel Barber, 364
Beneden (F. J. van), Fossil Fauna of the Black Sea, 544
Bengal Census, Facts Significant of Progress, 617
Benham (William Blaxland), British Earthworms, 102; on a New Genus and Species of Aquatic Oligochæta belonging to the Family Rhinodrilidæ found in England by, 261
Bennett (Mr.), Fungus Internally Parasitic in Diatoms, 118
Bent (Mr. and Mrs. Theodore): Proposed Expedition to Abyssinia, 115; Arrival at Adowa and Aksam of, 519, 547
Berget (M.), Dilatation of Iron on Magnetization, 71
Berlin: Berlin Method of Cleaning Mercury, 16; Berlin Meteorological Society, 24, 168, 287, 480, 552; Berlin Physical Society, 24, 168, 216, 312, 336, 480, 503, 624; Berlin Physiological Society, 72, 144, 216, 287, 480, 504, 552, 600; Meteorological Balloon Ascent at Berlin, October 29, 1891, A. L. Rotch, 46; Large Male Gorilla acquired by Berlin Aquarium, 86; Berlin Physikalisch-Technische Reichsanstalt, Construction of Copies of Permanent Mercury Resistance, 233; Berlin Academy Grants in aid of Research, 586; Berlin Imperial Physico-Technical Institute, Researches on

the Siemens Platinum Foil Unit as a Standard for the Intensity of a Source of Light, 615
Bermerside Observatory, 473
Berson (Dr.), Relationship between Insolation and Temperature, 24
Berthelot (M.), Researches on the Fixation of Atmospheric Nitrogen by Microbes, 23; Ancient Copper Relics discovered in the course of M. de Sarzec's Excavations in Chaldæa, 360; High Temperature and Carbon Vaporization, 240; on the Organic Substances Constituting Vegetable Soil, 551
Beseke (C.), Der Nord-Ostsee-Kanal, 579
Bethlehem, the Star of, J. H. Stockwell, 177
Bezold (Prof. von), the Thermal Changes of the Atmosphere, 552
Bianco (Ottavio Zanotti), Discovery of the Potential, 510
Bibliographia Medica Italiana, Prof. P. Giacosa, 606
Bickerton (Dr. T. H.), the Association of Shipping Disasters with Defective Vision in Sailors, 16
Bidgood (John), A Course of Practical Elementary Biology, 434
Bidwell (Shelford, F.R.S.), on some Meteorological Problems, 502
Biela Orbit, Ephemeris for Bodies Moving in the, Dr. Chandler, 186
Bielids, the, 1892, M. Bredichin, 451
Bielids of 1872, 1885, and 1892, the, M. Bredichin, 498
Bienaimé (M.), the Voyage of *La Manche* to Iceland, Jan Mayer and Spitzbergen in 1892, 48
Bigourdan (M.), the New Comet (Holmes), 88
Billett (Dr. A.), Contribution à l'Etude de la Morphologie et du Développement des Bacteriacées, Dr. Rubert Boyce, 532
Binary Stars: Measurement of Distances of, C. E. Stromeyer, 199; Prof. Arthur A. Rambaut, 226
Biology: Experimental Evolution, Henry de Varigny, 25; Biological Nomenclature: the Rule "Once a Synonym, always a Synonym," Elliott Coues, 39; on the Reproduction of Orbitolites, H. B. Brady, 119; on the Occurrence of Embryonic Fission in Cyclostomatous Polyzoa, Sidney F. Harmer, 524; Text-book of Elementary Biology, H. J. Campbell, 530; Text-book of Biology, H. G. Wells, 605; a Course of Practical Elementary Biology, John Bidgood, 434; Forschungsberichte aus der Biologischen Station zur Plön, Dr. Otto Zacharias, 461; Marine Laboratories in the United States, Prof. J. P. Campbell, 66; Marine Biology, the Destruction of Immature Fish, Ernest W. L. Holt, 160; Dredging Work at Plymouth, 375; the Rising and Sinking Process in the Radiolaria, Herr Verworn, 397; the Week's Work of the Plymouth Station, 398, 424, 451, 472, 497, 518, 546, 565, 589, 616; Port Erin (Isle of Man) Station, 515
Birds, the Flight of, Herbert Withington, 414
Birds, the Migration of, an Attempt to reduce Avian Season-Flight to Law, Charles Dixon, 169
Birds of New Zealand, the Preservation of the Native, 394
Birkeland (Kr.), Electric Oscillations in Wires, Direct Measurement of the Moving Wave, 286
Birmingham, Technical Education in, Sir Henry Roscoe, 301
Bishop (Sereno E.), the Afterglow, 102
Bishop's Ring, the Afterglows and, T. W. Backhouse, 582
Bismuth, Further Researches in Connection with the Metallurgy of, Edward Mathey, F.R.S., 358
Black (W. G.), Ozone, 390
Black Sea, Fossil Fauna of the, T. J. van Beneden, 544
Blake (Prof. J. F.), the Landslip at Sandgate, 467
Blakesley (Mr.), Diffusion of Light, 191
Blakesley (J. H.), on the Differential Equation of Electric Flow, 574
Blandford (W. F. H.), Insects Injurious to Conifers, 620
Blanford (Henry F., F.R.S.), a Palæozoic Ice-Age, 101, 152; Death of, 300; Obituary Notice of, 322
Blanford (Dr. W. T., F.R.S.), a Palæozoic Ice-Age, 101, 152
Blass (E.), Experiments to Determine Temperature of Flame of Water Gas, 113
Blind Animals in Caves: Prof. E. Ray Lankester, F.R.S., 389, 486; J. T. Cunningham, 439, 537; A. Anderson, 439; G. A. Boulenger, 608
Bliss (F. J.), the Excavations at Tell-el-Hesy, 302
Blomefield (Rev. Leonard), Address of Congratulation to, 85
Blondel (M. A.), General Conditions to be fulfilled by Registering Instruments or Indicators, Problem of Integral Synchronisation, 599
Blood-Vessels of the Skin in Different Parts, Signor Minervini, 254
Bodies Moving in the Biela Orbit, Ephemeris for, Dr. Chandler, 186
Boeddicker (Dr. Otto), the Milky Way from the North Pole to 10° of South Declination, drawn at the Earl of Rosse's Observatory at Birr Castle, 337
Bois (Dr. Du), Method of Producing Intense Monochromatic Light, 255
Bois (H. E. J. G. du), a Modified Astatic Galvanometer, 455
Bogdanov (Prof. A.), Which is the most Ancient Race in Russia? 524
Böhm (Dr. J.), Observations on the Potato Disease, 254
Bolletino de la Società Botanica Italiana, 23
Bombay, Magnetical and Meteorological Observations made at the Government Observatory, 1890, 379
Bonney (Prof. T. G., F.R.S.): "the Lake of Geneva," Prof. F. A. Forel, 5; Arborescent Frost Patterns, 162; on some Schistose "Greenstones" from the Pennine Alps, 263; Note on the Nufenen-Stock (Lepontine Alps), 263; on a Secondary Development of Biolite and of Hornblende in Crystalline Schists from the Binnenthal, 263; some Lake Basins in France, 341, 414; Action of Glaciers on the Land, 521
Boraston (Maclair), the Andromedes, 326
Borneo, Travels in, Charles Hose, 282
Boss (Lewis), Comet Holmes, 256
Botany: Fossil, the Genus Sphenophyllum, Prof. Wm. Crawford Williamson, 11; Bolletino della Società Botanica Italiano, 23; Journal of Botany, 23, 261, 596; the Cultivation of Diatoms, Signor Macchiati, 23; Biological Relations between Plants and Snails, 23; the Botanical Gazette, 23, 285, 596; the Dischidia Rafflesiana established at Kew, 38; the "Bean-tree" of Central Australia, 40: Botanical Nomenclature, W. T. Thiselton Dyer, F.R.S., 53; Sereno Watson, 53; Gynodiæcism in the Labiatæ, II; Observations on Origanum (continued), J. C. Willis, 119; Influence of Moisture on Vegetation, E. Gain, 119; Mode of Production of Perfume in Flowers, E. Mesnard, 120; An Introduction to the Study of Botany, with a Special Chapter on Some Australian Natural Orders, Arthur Dendy and A. H. S. Lucas, 125; Teaching of Botany, Dr. D. H. Scott, 228; The Teaching of Botany, 151; a New Irish Alga, Prof. T. Johnson, 167; Grasses of the Pacific Slope, including Alaska and the Adjacent Islands, Dr. Geo. Vasey, 173; Naked-Eye Botany, F. E. Kitchener, 198; Botanical Explorations in Idaho, D. T. Macdougal, 206; a Botanist's Vacation in the Hawaiian Islands, Prof. D. H. Campbell, 236, 355; A Contribution to our Knowledge of Seedlings, Sir John Lubbock, F.R.S., Dr. Maxwell T. Masters, F.R.S., 243; Electric Currents in Plants, Prof. Burdon Sanderson, 255; Botanical Laboratory Established at Eustis, Florida, 278; H. L. Russell on the Bacterial Investigation of the Sea and its Floor, 285; What is the True Shamrock? Nathaniel Colgan, R. L. Praeger, 302; Sisal Hemp Grown in Botanic Society', Gardens, 324; the Jamaica Botanical Department, W. Fawcett, 348; Proposed Establishment of a New Order (Myxobacteriaceæ) of Schizomycetes, R. Thaxter, 373; Flowering of *Fourcroya Selloa* in Botanic Society's Conservatory, 373; Botanische Zeitung, 398; Engler's Botanische Jahrbücher für Systematik, Pflanzen-geschichte und Pflanzengeographie, 413; Death of Cav. G. A. Pasquale, 421; Dionæa, Dr. Macfarlane, Bashford Dean, 423; on the Cause of the Bright Colours of Alpine Flowers, Dr. J. Joly, F.R.S., 431; Government Stations in United States, 450; Deaths and Obituary Notices of Dr. G. Vasey, Rev. W. Woolls and Dr. Karl Prantl, 495; Climbing Plants, Dr. H. Schenck, W. Botting Hemsley, F.R.S., 514; the Process of Transference of Material in Plants, Herr Brasse, 544; Deaths of Alphonse de Candolle and Rev. T. Wolle, 561; a Graft Hybrid between Red and White Geraniums, H. L. Jones, 563; the Radiation and Absorption of Heat by Leaves, Alfred Goldsborough Mayer, 596; Studies in the Morphology of Spore-producing Members, F. O. Bower, F.R.S., 598; Plants most visited by Bees in New South Wales, 614; Report of the Conifer Conference, Dr. M. T. Masters, F.R.S., and Prof. Carl Hauser, 619; List of Conifers and Texads, Dr. Masters, 619; Prof. Carl Hauser's *Pinetum Danicum*, 619; Coniferæ of Japan, H. J. Veitch, 619; Conifers for Economic Planting, A. D. Webster, 619; the Timber of Exotic Conifers, D. F. Mackenzie, 619; Insects Injurious to Conifers, W. G. H. Blandford, 620;

Bottles designed to Collect Specimens of Deep Waters, on a Modification to be Applied to the Construction of, J. Thoulet, 408
Boulenger (G. A.), Blind Animals in Caves, 608
Boultbee (J. W.), Artesian Boring and Irrigation in New South Wales, 183
Bouty (M.), on Initial Capacities of Polarisation, 552
Bower (F. O., F.R.S.), Studies in the Morphology of Spore-producing Members, 598
Bowers (Captain), Journey in Tibet, 490
Boyce (Dr. Rubert), Contribution à l'Étude de la Morphologie et du Développement des Bacteriacées, Dr. A. Billet, 532
Boys (Mr.), Williams on the Dimensions of Physical Quantities, 116
Boys (C. V., F.R.S.), on Electric Spark Photographs, or Photography of Flying Bullets, &c., by the Light of the Electric Spark, 415, 440
Boxing Kangaroo at Westminster Aquarium, the, 111
Boyd (R. Nelson), Coal Pits and Pitmen, 481
Boyle (Robert), a Sanitarian's Travels, 105
Braddon (Sir Edward), Tasmania the Paradise of Horticulturists, 587
Brady (H. B.), on the Reproduction of Orbitolites, 119
Brain, Bell's Idea of a New Anatomy of the, Jas. B. Bailey, 11
Brain of George Grote, Examination of, Prof. John Marshall, 15
Brain in Mud-fishes, the, Dr. Rudolf Burckhardt, 339
Brandis (Sir D.), American Forestry, Prof. W. R. Fisher, 275
Branly (Edouard), Experiments on Loss of Electrical Charge of Bodies in Diffuse Light and in Darkness, 586
Brasse (Herr), the Process of Transference of Material in Plants, 544
Brazil, the Proposed Transference of the Capital of, 64
Breath Figures, W. B. Croft, 187
Breath Figures, Dust Photographs and, W. B. Croft, 364
Bredichin (M.), the Bielids, 1892, 451; the Bielids of 1872, 1885, and 1892, 498
Bressa Prize, the, 233
Bressa Prize, the Ninth, Turin Academy of Sciences, 543
Brester (Dr. A.), Théorie du Soleil, 433
Brewers, a Handy Book for, Herbert Edwards Wright, 75
Brightwen (Mrs.), More about Wild Nature, 125
Brinton (Dr.), the Alleged Increase of Nervous Diseases with Growth of Civilisation, 280, 374
British Association, the Coming Nottingham Meeting of the, 612
British Columbia, Lizard Superstition of Shuswap Indians, Dr. Geo. Dawson, F.R.S., 184; Superstitions of the Shuswaps of, Colonel C. Bushe, 199
British Earthworms, William Flaxland Benham, 102; Frank J. Cole, 255
British Fungus-Flora, a Classified Text-Book of Mycology, George Massee, 26
British History, some Geographical Aspects of, H. G. Mackinder, 519
British Islands, the Hemiptera Heteroptera of the, Edward Saunders, 292
British Journal Photographic Almanac for 1893, 462
British Jurassic Gasteropoda, a Catalogue of, W. H. Hudleston, F.R.S., and Edward Wilson, H. Woods, 363
British Marine Fauna, Proposed Handbook to the, Prof. W. A. Herdman, F.R.S., 231; Prof. D'Arcy W. Thompson, 269; Prof. W. A. Herdman, F.R.S., 293; W. Garstang, 293
British Museum, Catalogue of the British Echinoderms in the, F. Jeffrey Bell, 508
British New Guinea, J. P. Thompson, Henry O. Forbes, 345; Prof. Alfred C. Haddon, 414; Henry O. Forbes, 414
Brock (Dr. H.), a Treatise on Public Health and its Applications in Different European Countries, Dr. Albert Palmberg, 507
Brodie (Rev. P. B.), on some Additional Remains of Cestraciont and other Fishes in the Green Gritty Marls immediately overlying the Red Marls of the Upper Keuper in Warwickshire, 286
Brodmann (C.), on a Modification of the Transpiration Method Suitable for the Investigation of very Viscous Liquids, 357
Brooks, Comet, August 28), 18, 41
Brooks's Comet, the New, MM. Esmiol and Fabry, 159
Brooks, Comet (November 19-20, 1892), 159, 208, 235, 257, 281, 304, 325, 352, 376, 399, 425, 451; Berberich, 186
Brothers (A.), a Manual of Photography, 98
Brown (Arthur E.), Photographic Dry Plates, 11
Brown (Dr. Robert), Morocco, 298
Brown (W. Piffe), Winter Temperatures on Mountain Summits, 431
Browning (R. E.), Quantitative Separation of Barium from Strontium, and of Strontium from Calcium by Action of Amyl Alcohol on Bromides and Nitrates respectively, 189
Brayn (Lobry de), Hydroxylamine, 185
Bryan (G. H.), on a Hydrodynamical Proof of the Equations of Motion of a Perforated Solid with Applications to the Motion of a fine Frame-work on Circulating Liquids, 500
Bryant (Thomas), John Hunter (the Hunterian Lecture), 372
Bubbles, Permanent Soap, formed with a Resinous Soap, M. Izarn, 119
Buchner (Herr), the Action of Light upon certain Micro-Organisms, 303
Buck (Sir E. C.), the Debra Dun Forest School, 614
Buck (Walter J.), Wild Spain, 583
Buckton (G. B., F.R.S.), the Reflector with the Projection Microscope, 54
Buddhist Architecture and Symbolism in Burma, Developments in, Major Temple, 46
Bulgaria, Earthquake in, 562
Bulletin de l'Académie Royal de Belgique, 309, 428, 500, 525, 621
Bulletin of the New York Mathematical Society, 23, 286, 428
Bullets, Photography of Flying, C. V. Boys, F.R.S., 415, 440
Burbury (S. H.), Mr. Sutherland's Paper on the Laws of Molecular Force, 117
Burckhardt (Dr. Rudolf), Das Centralnervensystem von Protopterus Annectens; eine Vergleichend Anatomische Studie, 339
Burmah; Snakes in Thatch, 113
Burma Mines, Valuable Ruby Discovered at, 586
Burnham's Double-Star Observations, 281
Burton (Dr.), Williams on the Dimensions of Physical Quantities, 116
Burton (Dr. C. S.), Diffusion of Light, 191; on Plane and Spherical Sound-Waves of Finite Amplitude, 500; on the Applicability of Lagrange's Equations of Motion to a General Class of Problems, with Special Reference to the Motion of a Perforated Solid in a Liquid, 500
Bushe (Col. C.), Superstitions of the Shuswaps of British Columbia, 199
Butte (M. L.), on the Urea contained in the Blood in Cases of Eclampsia, 456
Butter, Bacilli in, Mrs. Percy Frankland, 283
Butterflies: on the Mimetic Forms of certain Butterflies of the genus *Hypolimnas*, Col. C. Swinhoe, 429

Cables, Electric, the Teredo and, Sir Henry Mance, 450
Cage-bird Club, Formation of a, 495
Calcutta, the Proposed Snake Laboratory at, 253
Calderwood (Henry), Evolution and Man's Place in Nature, 385
Calderwood (W. L.), Applied Natural History, 492
Calendar System of the Ancient Aztecs, the, Zelia Nuttall, 156
Caliche (Nitrate of Soda), the Origin of, G. M. Hunter, 254
California: Geology of Taylorville Region in Sierra Nevada, California, J. S. Diller, 39
California (Southern), the Fishes of, C. H. Eigenmann, 61
California, Geographical Society of, 134
Calvert (G. A.), the Measurement of Wake Currents, 520
Cambridge: Election of Honorary Fellows of Gonville and Caius College, 130
Cambridge University Extension Movement, the; Summer Meeting Programme, 183, 586
Cambridge Philosophical Society, 95, 119, 360, 502
Camel, the, as a Defertilising Influence in Egypt, E. A. Floyer, 156
Camera, Anthropological Uses of the, E. F. im Thurn, 548
Campbell (Prof. D. H.), a Botanist's Vacation in the Hawaiian Islands, 236, 355
Campbell (H. J.), Text-book of Elementary Biology, 530
Campbell (Prof. J. P.), Marine Laboratories in the United States, 66
Campbell (Prof. W. W.), Motion of Nova Aurigæ, 256
Camphor, Japanese, 142

Canada: Prof. Edward Prince appointed Commissioner of Fisheries for, 37; the Inspection of Canadian Meteorological Stations, Charles Carpmael, 61; Castorologia; or the History and Traditions of the Canadian Beaver, Horace Martin, 224
Canals of Mars, the, 64
Candolle (Alphonse de), Death of, 561
Caoutchouc, Vulcanic, Rules for Estimating Quality of, M. Vladimiroff, 563
Cape Colony, Government Encouragement of the Fruit Export Trade, 234; Proposed Investigations of Chemical Composition of Soils in, 301; Water-boring in, 349; the Dairy Industry in, A. C. Macdonald, 471
Capraja, on the Petrography of the Island of, Hamilton Emmons, 334
Carbon, Crystallised, 370
Carburation of Iron, on the, John Parry, 560
Carey (E. G.), the Value of Annealing Steel, 396
Carpmael (Charles), the Inspection of Canadian Meteorological Stations, 61
Carr (F. H.), the Composition of some Commercial Specimens of Aconitine, 430
Carrington (Dr. Benjamin), Death of, 348
Carruthers (W., F.R.S.), Yew Poisoning, 285
Carus-Wilson (Chas. A.), the Niagara Spray Clouds, 414
Cassie (W.), Printing Mathematics, 8
Cassiopeiæ, Parallaxes of μ and θ, Harold Jacoby, 565
Castorologia; or, the History and Traditions of the Canadian Beaver, Horace Martin, 224
Cat, Electricity in, Remarkable Case of, 17
Catalogue of the British Echinoderms in the British Museum, F. Jeffrey Bell, 508
Catania, Earthquake at, 543
Caucasus, Silk Culture in the, G. Sedlaczek, 397
Cave and Cliff Dwellings in Central Arizona, J. W. Tourney, 112
Cave-burial, the Prehistoric Interments of the Bahi Rossi Caves, near Mentone, A. J. Evans, 239
Caves, Blind Animals in, Prof. E. Ray Lankester, F.R.S., 389, 486; J. T. Cunningham, 439, 587; A. Anderson, 439; G. A. Boulenger, 608
Caves, Yorkshire, Relics found in, Rev. Edward Jones, 112
Ceilings, Soot-figures on, E. B. Poulton, F.R.S., 608; Prof. Oliver Lodge, F.R.S., 608
Cells: their Structure and Functions, Prof. Dr. Oscar Hertwig, 314
Census, Bengal, Facts significant of Progress, 617
Centipedes, Phosphorescence in, R. I. Pocock, 545
Cetacean Genus *Mesoplodon*, Observations on the Development of the Rostrum in the, Henry O. Forbes, 455
Cetology; Sowerby's Whale on the Norfolk Coast, T. Southwell, 349
Ceylon's Contribution to the Chicago Exhibition, 156
Ceylon, the Industrial Population of, 470
Ceylon Tea Industry, the Development of the, Dr. Trimen, 613
Chalais-Meudon, Dirigible Balloon in Construction at, 112
Chaldæa, Ancient Copper Relics discovered in the course of M. de Sarzec's Excavations in, M. Berthelot, 360
Chanler's (Aston) Expedition to Lake Rudolf, 327
Chambers's Encyclopædia, 340
Champagne Trade since 1844, Statistics of, 157
Chandler (Dr.), Ephemeris for Bodies Moving in the Biela Orbit, 186
Channels of Mars, J. W. Kingsmill, 133
Chapeaux (Marcelin) on the Digestion of the Cœlenterata, 621
Chapman (Abel), Wild Spain, 585
Chatham Islands and an Antarctic Continent, the, H. O. Forbes, 474
Chauveau (A.), the Pancreas and the Nervous Centres Controlling the Glycemic Function, 479; the Pancreas and the Nerve Centres regulating the Glycemic Function; Experimental Demonstrations Derived from a Comparison of the Effects of a Removal of the Pancreas with those of Bulbary Section, 528; on the Pathogeny of Diabetes, 384; Existence of Distinct Nervous Centres for Perception of Fundamental Colours of Spectrum, 143
Chemistry: Memorial Celebration for, A. W. von Hofmann, 14; Berlin Method of Cleaning Mercury, 16; New Method of Preparing Glycol Aldehyde, Drs. Marckwald and Ellinger, 17; the Framework of Chemistry, W. M. Williams, 28; Projected Memorial to Carl Wilhelm Scheele, 37; Isolation of Gold and Cadmium Compound, Heycock and Neville, 40; Power of Hydrogen Absorption of Various Metals, Herren Neumann and Streintz, 63; Further Researches on Nucleinic Acid, Prof. Kessel, 72; Isolation of Fluosulphuric Acid, Thorpe and Kermian, 87; Chemical Lecture Experiments, G. S. Newth, Sir Henry E. Roscoe, F.R.S., 97; Matriculation Chemistry, Temple Orme, 99; Notes on Qualitative Chemical Analysis, Lakshmi Narasu Nayudu, 100; Isolation of Penta-Iodide and Bromide of Cæsium, Wells and Wheeler, 113; Outlines of Organic Chemistry, Clement J. Leaper, 124; a Lilac Colour produced from Extract of Chestnuts, Mr. Palmer, 132; Azoimide, 136; Preparation of Metallic Chromium by Electrolysis, Em. Placet, 144; Carl Wilhelm Scheele, Prof. T. E. Thorpe, F.R.S., 152; Preparation of Chloraurates and Bromaurates of Cæsium and Rubidium, Wells and Wheeler, 158; Interaction of Iodine and Potassium Chlorate, Thorpe and Perry, 165; Magnetic Rotation of Sulphuric and Nitric Acids, W. H. Perkins, Sen., 165; Refractive Indices and Magnetic Rotations of Sulphuric Acid Solutions, S. U. Pickering, 165; Some Alkylamine Hydrates, S. U. Pickering, 165; Experiment on Triethylamine Hydrate, Prof. Thorpe, 165; Atomic Weight of Boron, E. Aston and W. Ramsay, 165; Methoxyamido-1:3-dimethylbenzene, W. R. Hodgkinson and L. Limbach, 165; Chemical Society, 165, 238, 311, 405, 430, 479, 551; Chemical Society's Memorial Lectures, 248; Necessity of Water in Chemical Reactions, V. H. Veley, 167; Chemical Study of Opium Smoke, Henri Moissan, 168; Oxygen for Limelight, T. C. Hepworth, 176; Proposed Memorial to Jean Servais Stas, 182; Hydroxylamine, Lobry de Bruyn, 185; Notes on Silver Chlorides, M. C. Lea, 189; Quantitative Separation of Barium from Strontium and of Strontium from Calcium by Action of Amyl Alcohol on Bromides and Nitrates respectively, P. E. Browning, 189; Action of High Temperature on Metallic Acids, H. Moissan, 192; the Chemistry of Life and Health, C. W. Kimmins, 198; Isolation of two Predicted Hydrates of Nitric Acid, S. W. Pickering, 238; Anhydrous Oxalic Acid, W. W. Fisher, 238; Production of Orcinol, &c., from Dehydracetic Acid, W. Collie and W. S. Myers, 238; the Origin of Colour and Fluorescence, W. N. Hartley, 238; H. E. Armstrong, 238; the Reduction Products of Dimethyldiacetylpentane, F. S. Kipping, 238; Products of Interaction of Zinc Chloride on Sulphuric Acid and Camphor, H. E. Armstrong and F. S. Kipping, 239; Griess-Sandmeyer Interactions and Cattermann's Modification thereof, H. E. Armstrong and W. P. Wynne, 239; Methods of Observing and Separating Spectra of easily Volatile Metals and their Salts, W. N. Hartley, 239; Manganese Borate, W. N. Hartley and H. Ramage, 239; Compounds of Salicylic and Cresotinic Acid Lactides with Chloroform, Prof. Anschütz, 255; on the Purification of Arsenical Zinc, M. H. Lescoeur, 288; on Some Isoimides of Camphoric Acid, MM. S. Hoogewerff and W. A. Van Dorp, 288; on a New Fluorine-Derivative of Carbon, Frédéric Swarts, 309; on a Process of Sterilisation of Albumin Solutions at 100° C., Émile Marchal, 310; the Identity of Caffeine and Theine and the Interactions of Caffeine and Auric Chloride, W. R. Dunsten and W. F. J. Shepheard, 311; Studies on Isomeric Change II and III, G. T. Moody, 311; Formation and Nitration of Phenyldiazoimide, W. A. Tilden and J. H. Millar, 311; the Production of Naphthalene Derivatives from Dehydracetic Acid, J. N. Collie, 311; a New Synthesis of Hydrindone, F. S. Kipping, 311; the Resolution of Methoxysuccinic Acid into its Optically Active Components, T. Purdie and W. Marshall, 311; Optically Active Ethoxysuccinic Acid, T. Purdie and J. W. Walker, 311; the Formation of Benzyldihydroxypyridine from Benzylglutaconic Acid, S. Ruhemann, 311; Note on the Action of Phenylhydrazine on Mono- and Di-Carboxylic Acids at Elevated Temperatures, W. R. Hodgkinson and A. H. Coote, 311; the Chemical Basis of the Animal Body, A. Sheridan Lea, F.R.S., 340; a Crystallised Compound of Iron and Tungsten, Drs. Poleck and Grützner, 351; the value of Tungsten in Improving Hardness of Steel, 350; on Urobilin, A. Eichholz, 360; Qualitative Analysis Tables and the Reactions of Certain Organic Substances, E. A. Letts Chapman Jones, 361; the Volatility of Manganese, Prof. Lorenz and Dr. Hensler, 375; on the Mode of Elimination

of Carbonic Oxide, M. L. de Saint-Martin, 384; Chemistry of Osmium, A. E. Tutton, 400; the Preparation of Glucina from Beryl, J. Gibson, 405; the Hydrocarbons derived from Dipentene Dihydrochloride, W. A. Tilden and S. Williamson, 405; Sulphonic Derivatives of Camphor, F. S. Kipping and W. J. Pope, 405; Thionyl Bromide, J. Hartog and W. E. Sims, 405; Desulphurisation of the Substituted Thioureas, A. E. Dixon, 405; Salts of Active and Inactive Glyceric Acid: the Influence of Metals on the Specific Rotatory Power of Active Acids, P. F. Frankland and J. R. Appleyard, 405; Dibromo-β-lapachone, S. C. Hooker and A. D. Gray, 405; the Conversion of Para- into Ortho-Quinone Derivatives, S. C. Hooker, 405; a Method for the Preparation of Acetylene, M. W. Travers, 406; Gases in Living Plants, J. G. Arthur, 427; on Some Recent Determinations of Molecular Refraction and Dispersion, Dr. J. H. Gladstone, F.R.S., 429; the Action of Nitrosyl Chloride and of Nitric Peroxide on some Members of the Olefine Series, W. A. Tilden and J. J. Sudborough, 430; Piperazine, W. Majert and A. Schmidt, 430; the Connection between the Atomic Weight of the Contained Metals and the Magnitude of the Angles of Crystals of Isomorphous Series, A. E. Tutton, 430; the Preparation of Phosphoric Oxide Free from the Lower Oxide, W. A. Shenstone and C. R. Beck, 430; on Isaconitine (Napelline), W. R. Dunstan and E. F. Harrison, 430; the Composition of some Commercial Specimens of Aconitine, W. R. Dunstan and F. H. Carr, 430; Synthesis of Oxazoles from Benzoin and Nitriles, F. R. Japp and T. S. Murray, 430; Stéréochimie, J. H. Van't Hoff, 436; Prof. Percy Frankland, F.R.S., 510; Prof. F. R. Japp, F.R.S., 510: Ruthenium, M. Joly, 451; on Stas's Determination of the Atomic Weight of Lead, M. G. Hinrichs, 456; the Chemical Properties of the Diamond, M. Moissan, 472; on the Industrial Preparation of Aluminium, M. A. Ditte, 479; Note on the Preparation of Platinous Chloride, and on the Interaction of Chlorine and Mercury, W. A. Shenstone and C. R. Beck, 479; the Action of Phosphoric Anhydride on Fatty Acids, F. S. Kipping, 479; Regularities in the Melting Points of Certain Paraffinoid Compounds of Similar Constitutions, F. S. Kipping, 479; some Relations between Constitution and Physical Constants in the case of Benzenoid Amines, W. R. Hodgkinson and L. Limpach, 479; Electrolysis of Sodic Ethylic Camphorate, J. Walker, 479; the Hydrates of Hydrogen Chloride, S. U. Pickering, 479; a New Base from *Corydalis Cava*, J. A. Dobbie and H. Lander, 479; Metallic Osmium, MM. Joly and Vèzes, 497; Further Studies on Hydrazine, A. E. Tutton. 522; on the Preparation of a Variety of Swelling Graphite, M. Henri Moissan, 527; Origin of Colour, VII. VIII. and IX., H. E. Armstrong, 551; Formation of the Ketone 2:6-Dimethyl-1-Ketohexaphane, F. S. Kipping, 551; Note on the Interactions of Alkali Metal Haloids and Lead Haloids, and of Alkali Metal Haloids and Bismuth Haloids, Eleanor Field, 551; an Isomeric Form of Benzylphenylbenzylthiourea, A. E. Dixon, 551; on the Carburization of Iron, John Parry, 560; the Amide and Imide of Sulphuric Acid, Dr. Traube, A. E. Tutton, 566; the Densities of the Principal Gases, Lord Rayleigh, F.R.S., 567; New Mode of Preparing Hyponitrous Acid, Dr. Wilhelm Wisticenus, 588; on Nitrogenised Copper, MM. Paul Sabatier and J. B. Senderens, 600; Isolation of Amidophosphoric Acid, H. N. Stokes, 615, 616; Cryohydrates in Systems of two Salts, Bakhius Roozeboom, 624; the Peptone of Kühne, Mr. Pekelharing, 624; Agricultural Chemistry, Drainage Waters of Cultivated Lands, M. P. P. Dehérain, 287; Proposed Investigation of Chemical Composition of Soils in Cape Colony, 301; on the Organic Substances constituting Vegetable Soil, MM. Berthélot and André, 551

Chersky (I. D.), Death of, 232

Chicago Exhibition, Ceylon's Contribution to, 156

Chicago Exhibition, Instruments for the Earthquake Laboratory at the, Prof. John Milne, F.R.S., F. Omori, 356

Chicago, the University of, 278

Child (Theodore), Death of, 65

China: Railways in, 400; Remarkable Cold Wave over, B. J. Skertchly, 516

Cholera, a New Method of Treatment for, 83

Chromatic Curves of Microscope Objectives, on the, Dr. W. H. Dallinger, 501

Chronology; the Calendar System of the Ancient Aztecs, Zelia Nuttall, 156

Cider, the Improvement by Wine-Yeast of, Nathan, 208

City and Guilds of London Institute; Improvements in Technological Examinations, 612

Clapham Junction and Paddington Railway, 515

Clark (G. M.), Determination of Low Temperatures by Platinum Thermometers, 95

Clark (H. L.), the Flight-Speed of Wild Ducks, 374

Clark (Prof. W. B.), the Surface Configuration of Maryland from the Meteorological point of view, 585

Clayton (H. H.), Cloud Observations at Blue Hill (Mass.) Observatory, 183

Cleveland (D.), the Trap-door Spider, 375

Cliff and Cave Dwellings in Central Arizona, J. W. Tourney, 112

Climate of New South Wales, Physical Geography and, H. C. Russell, F.R.S., 258

Climate, Fossil Floras and, Sir William Dawson, F.R.S., 556, J. Starkie Gardner, 582

Climate, Fossil Plants as Tests of, A. C. Seward, 267, 364; J. Starkie Gardner, 267, 364; Chas. E. De Rance, 294, 342

Climatology: Indications of a Rainy Period in Southern Peru, A. E. Douglass, 38; Recent Researches on the Influence of Forests, Dr. Schubert, 480

Climbing Plants, Dr. H. Schenck, W. Botting Hemsley, F.R.S., 514

Cline (I. M.), Hot Winds in Texas, May 29 and 30, 1892, 454

Clouds, the Niagara Spray, Chas. A. Carus-Wilson, 414

Clowes (Prof. Frank), a New Portable Miner's Safety-Lamp, 596

Co-planar Vectors and Trigonometry, the Algebra of, R. Baldwin Hayward, F.R.S., 266

Coal Bricks, Anthracite, the Manufacture of, 396

Coal Pits and Pitmen, R. Nelson Boyd, 481

Coccidæ, the Use of Ants to Aphides and, T. D. A. Cockerell, 608

Cockerell (T. D. A.), the Use of Ants to Aphides and Coccidæ, 608

Codman (J. E.), Notes on the Use of Automatic Rain Gauges, 261

Cohn (Dr. F.), Comet Holmes, 326

Coincidence of Solar and Terrestrial Phenomena, Prof. G. E. Hale, 425

Cold, Interesting Results in Application of, 184

Cole (Frank J.), British Earthworms, 295

Cole (Prof. Grenville A. J.), Geology of Scotland, 101; the Afterglow, 127; Glacial Drift of the Irish Channel, 464

Colenso (Rev. W., F.R.S.), Some Reminiscences of the Maoris, 41

Colgan (Nathaniel), What is the True Shamrock? 302

College of Science, Durham, Appeal for Relief from Financial Difficulties, 585

College of Science, Newcastle, Laying Foundation Stone of, 129

Colles (G. W., jun.), Distance of the Stars by Doppler's Principle, 596

Collett (Sir H.), Super-Abundant Rain, 247

Collie (J. N.), the Production of Naphthalene Derivatives from Dehydracetic Acid, 311

Collie (N.), Production of Orcinol, &c., from Dehydracetic Acid, 238

Colliery Explosions, Colliers and, R. Nelson Boyd, 481

Colonial Meteorology, C. J. Symons, F.R.S., 390

Colorado, on the High Altitudes of, and their Climates, Dr. C. J. Williams, 333

Colorimeter for Comparing Intensity of Colour in Solution, Papasogli, 131

Colour: Helmholtz on Hering's Theory of, Prof. J. D. Everett, F.R.S., 365; the Cause of the Sexual Differences of Colour in Eclectus, Prof. A. B. Meyer, 486; Sensitiveness of the Eye to Light and Colour, Captain W. de W. Abney, F.R.S., 538; Origin of Colour, VII. VIII. and IX, H. E. Armstrong, 551; Colour Blindness, Dr. W. Pole, 335; Note on the Colours of the Alkali Metals, G. S. Newth, 55; Wm. L. Dudley, 175; Iridescent Colours, Alex. Hodgkinson, 92; Baron C. B. Osten-Sacken, 102

Colson (R.), Demonstration by means of Telephone of existence of Interference of Electric Waves in Closed Circuit, 96

Columbia, British, Superstitions of the Shuswaps of, Colonel C. Bushe, 199
Columbia, British, a New Coaly Mineral from, 280
Columbia College, U.S.A., Astronomy at, 159
Columbia College, New York, the Loubat Prizes, 496
Comets: Comet in Andromeda, 40; Comet Barnard (October 12), 18, 40; Comet Brooks (August 28), 18, 41, 63; a Bright Comet discovered by W. R. Brooks, 114; Comet Brooks (November 19-20, 1892), 133, 208, 235, 257, 281, 304, 326, 352, 376, 399, 425, 451; Prof. Kreutz, 159; Berberich, 186; Comet Holmes (November 6, 1892), 114, 132, 159, 186, 209, 235, 281, 303, 351, 376, 425, 473; in Bigourdan, 88; Spectrum of, 235; Lewis Boss, 256; Rev. E. M. Searle, 257; Mr. Roberts, 257; M. Schulhof, 257, 451, 498; Dr. F. Cohn, 326; Dr. R. Schorr, 326; W. F. Denning, 365; Prof. E. Barnard, 399; Prof. Keeler, Prof. C. A. Young, 518; Swift's Comet, Prof. Barnard, 186; Comet Swift (a 1892), A. E. Douglas, 546; a New Comet, 133; the New Comet, 63; W. F. Denning, 77; the Present Comets, T. W. Backhouse, 127; Remarkable Comets, Mr. Lynn, 376
Commensalism, a Strange; Sponge and Annelid, James Hornell, 78
Commission, University, 1
Common (A. A.), Jupiter's Fifth Satellite, 208
Comparative Sunshine, Bishop Reginald Courtenay, 150
Conchology: Marine Shells of South Africa, G. E. Sowerby, 27; Hints for Collectors of Mollusks, William H. Dall, 140
Congo Free States: Progress of the Matadi-Stanley Pool Railway, Major Thys, 189
Congress at Moscow, International Zoological, 236
Conifers: List of Conifers and Texads, Dr. Masters, 619; Prof. Carl Hansen's Pinetum Danicum, 619; Coniferæ of Japan, H. J. Veitch, 619; Conifers for Economic Planting, A. D. Webster, 619; the Timber of Exotic Conifers, D. G. Mackenzie, 619; Insects Injurious to Conifers, W. F. H. Blandford, 620
Conjugate Angles; on the Need of a New Geometrical Term, Prof. A. M. Worthington, 8
Continuity, Optical, Francis Galton, F.R.S., 342
Convention signed, International Sanitary, 585
Conway's Karakoram Range Expedition, 19; Crossing of the Hispar Pass, 327
Cook (C. H.), the Protection of Sea-Fish, 396
Cooke (J. H.), Discovery of Ursus Arctos in Malta Pleistocene, 62
Cooke (M. C.), Vegetable Wasps and Plant Worms, 99
Coote (A. H.), Note on the Action of Phenylhydrazine on Mono- and di-carboxylic Acids at Elevated Temperatures, 311
Copper Resources of the United States, the, James Douglas, 132
Coppet (L. de), Temperature of Maximum Density of Alcohol Solutions, 48
Corn-Cockle, Dangers of Adulteration of Food Seeds with, 185
Corncrake caught in Wales, December 8, 1892, 157
Corona, a New Method of Photographing the, M. H. Deslandres, 327
Corsica, Studies in, John Warren Barry, 462
Costa Rica and Nicaragua, the Boundaries of, Dr. H. Polakowsky, 257
Coues (Elliott), the Rule "Once a Synonym, Always a Synonym," 39
Courtenay (Bishop Reginald), Comparative Sunshine, 150
Couttwell (G. E. W.), Foundations of Two River Piers of Tower Bridge, 545
Craters, Lunar, Mr. H. Maw, 31
Cremation in England, the Progress of, 396
Criticism of the Royal Society, 145
Croft (W. B.), Optical Illusions, 78; Breath Figures, 187; Spectra of Various Orders of Colours in Newton's Scale, 190; Science Teaching, 359; Dust Photographs and Breath Figures, 364
Croonian Lecture, the, Prof. Virchow, 487
Cross-Striping of Muscle, the, Prof. Richard Ewald and Prof. Haycraft, 92
Crystallised Carbon, 370
Crystallites, Ice, Rev. Dr. A. Irving, 126
Crystals; Dendritic Forms, Sydney Lupton, 13
Crystals, Growth of, Prof. Sollas, 213
Crystals, Two Experimental Verifications Relative to Refraction in, J. Verschaffelt, 428
Crystals, Ice, C. M. Irvine, 31; B. Woodd Smith, 79
Cumming (L.), Science Teaching, 359
Cuneiform Tablet, the Tell-el-Hesy, F. J. Bliss, 302
Cunningham (J. T.), Blind Animals in Caves, 439, 537
Curie (P.), Magnetic Properties of Bodies at Different Temperatures, 96; Magnetic Properties of Oxygen, 240
Cuverville (Rear Admiral Cavelier de), Experiments in Use of Oil in Calming Waves, 279
Curzon's (Hon. E. M.) Journey in Indo-China, 617
Cygni, Parallax of β, Harold Jacoby, 399
Cyprus, the Vineyards of, M. Mouillefert, 517

Dairy Industry in Cape Colony, the, A. C. Macdonald, 471
Dairy Work, Manual of, Prof. James Muir, Walter Thorp, 555
Dall (William H.), Hints for Collectors of Mollusks, 140
Dallinger (Dr. W. H.), on the Chromatic Curves of Microscope Objectives, 501; Prof. Bütschli's experiments on the so-called Artificial Protoplasms, 526
Dallmeyer (J. R.), the new Telephotographic Lens, 161
Dante's "Quæstio de Aqua et Terra," Edmund G. Gardner, 295
Danzig Naturforschende Gesellschaft, 150th anniversary of, 37
Darwin, a Criticism on, Dr. Geo. J. Romanes, F.R.S., 127
Darwin and After Darwin, Geo. John Romanes, F.R.S., 290
Darwin (Charles): His Life told in an Autobiographical Chapter and in a Selected Series of his Published Letters, 53
Darwin (Prof. G. H., F.R.S.), the Geology of the Asiatic Loess, 30; Reduction of Tidal Observations, 402; Die Entwickelung der Doppelstern-Systeme, T. J. J. See, 459; Roche's Limit, 581
Daubrée (M.), Observations on the conditions which appear to have obtained during the formation of Meteorites, 432
Davidson (J. Ewen), Thunderstorms and Auroral Phenomena, 582
Davies (Thomas), Obituary Notice of, L. Fletcher, F.R.S., 371
Davis (Prof. W. M.), the General Winds of the Atlantic Ocean, 574
Davos Platz, Record of Medical Experience at, Dr. Spengler, 517
Dawson (Dr. Geo., F.R.S.), Lizard-Superstition of Shuswap Indians, British Columbia, 184
Dawson (Sir William, F.R.S.), Fossil Floras and Climate, J. Starkie Gardner, 582, 556
Day of the Week, a Simple Rule for finding the, corresponding to any given day of the Month and Year, 509
Day (Mr.), Experiments on the value of the Steam-jacket, 20
De Morgan (W.), Earthenware Manufacture in Egypt, 613
Dean (Bashford), Dionea, 423
Decapods: on the Minute Structure of the Gills of *Palaemonetes varians*, Edgar J. Allen, 261
December Meteors (Geminids), W. F. Denning, 226
Decharme (C.), Displacements of a Magnet on Mercury under action of Electric Current, 48
Decimal System, the, S. Montagu, M.P., Sir William Harcourt, J. H. Yoxall, 323
Decorative Art, the Evolution of, Henry Balfour, 606
Deduction, Induction and, Edward T. Dixon, 10, 127, E. E. Constance Jones, 78
Defence, Remarkable Weapons of, G. F. Hampson, E. Ernest Green, 199
Dehérain (M.), Influence of Manure on Development of Roots, 280; Drainage Waters of Cultivated Lands, 287
Delcommune's (M. Alexandre) Lomami Expedition, 209; Return of the, 590
Demavend (Mt.), Sven Hedin's Ascent of, 19
Dendritic Forms, Sydney Lupton, 13
Dendy (Arthur), an Introduction to the Study of Botany, with a special chapter on some Australian Natural Orders, 125; the Hatching of a Peripatus Egg, 508
Denning (W. F.), the New Comet, 77; Holmes's Comet, 365; December Meteors (Geminids), 226; Astronomical Discoveries in 1892, 256
Densities of the Principal Gases, the, Lord Rayleigh, F.R.S., 567
Deslandres (M. H.) Motion in the Line of Sight, 88; Proper Motions, 115; a new Method of Photographing the Corona, 327

Destruction of Immature Fish, the, Ernest W. L. Holt, 160
Development, on a Supposed Law of Metazoan, R. Assheton, 176
Devenish (S.), the Alligator's Nest, 587
Dew, Herr Wollny, 398
Dew and Frost, Hon. R. Russell, 210
Diamond in Meteoric Iron, the, C. Friedel, 192
Diamond, the Chemical Properties of the, M. Moissan, 472
Diatoms, the Cultivation of, Signor Macchiati, 23
Diatoms, Fungus internally parasitic in, C. H. Gill, A. W. Bennett, 118
Dickson (H. N.), the Characteristic Form of the Coast Line as Affecting the Physical Conditions of the Waters of the English Channel, 235
Diener's (Dr. Karl), Geological Expedition in Himalayas, 133
Digestion, Influence of Bodily Exertion on Process of, Herr Rosenberg, 62
Diller (J. S.), Geology of Taylorville Region in Sierra Nevada, California, 39
Dimensions of Physical Quantities to Directions in Space, Williams on the relation of the, Profs. Fitzgerald, Henrici and Rucker and Dr. Sumpner, 69
Dines (H. W.), Anemometry, 143; Measurement of Maximum Wind Pressure, 118
Dinornithidæ, on the Cranial Osteology, Classification, and Phylogeny of the, Prof. T. Jeffrey Parker, F.R.S., 431
Dionea, Dr. Macfarlane, Bashford Smith, 423
Dipnoi, Dr. Rudolf Burckhardt, 339
Disease Germs, Flies and, 499
Diseases, Nervous, the Alleged Increase with Growth of Civilisation of, Dr. Brinton, 280, 374
Disinfectant, Aminol a True, Dr. E. Klein, F.R.S., 149, 247; Hugo Wollheim, 246
Ditte (M. A.), on the Industrial Preparation of Aluminium, 479
Dixey (F. A.), Epidemic Influenza, 244
Dixon (A. E.), Desulphurisation of the Substituted Thioureas, 405; an Isomeric Form of Benzylphenylbenzylthiourea, 551
Dixon (Charles), The Migration of Birds: an Attempt to Reduce Avian Season-flight to Law, 169
Dixon (Edward T.), Induction and Deduction, 10, 127
Dixon (Prof. Harold B.), the Rate of Explosion in Gases, 299
Dixon (Henry H.), on the Walking of Arthropoda, 56; on the Germination of Seedlings in the Absence of Bacteria, 287
Dobbie (J. A.) a New base from Corydalis Cava, 479
Doberck (W), Severe Frost at Hongkong, 536
Dodge (Frank S.), Kilauea in August 1892, 499
Dokoutchaiev (Prof. W. W.), Russian Steppes Past and Present, 523
Domestic Electric Lighting treated from the Consumer's Point of View, E. C. De Segundo, 172
Dominica, Earthquake in, W. R. Elliott, 562
Donkin (Bryan, jun.), Experiments on the Value of the Steam-jacket, 20
Dorp (W. A. van), on some Isoimides of Camphoric Acid, 288
Double-Star Observations, Burnham's, 281
Double Stars, the Evolution of, T. J. J. See, Prof. G. H. Darwin, F.R.S., 459
Douglas (James), the Copper Resources of the United States, 132
Douglass (A. E.), Indications of a Rainy Period in Southern Peru, 38; Comet Swift (a 1892), 546
Douglass (G. M.), Assumption of the Male Plumage by a Peahen, 71
Dowson (J. Emerson), Gas Power for Electric Lighting, 284
Dromedaries in German South-west Africa, Capt. von François, 38
Dry Plates, Photographic, Arthur E. Brown, 11
Dublin Royal Society, 167, 287, 431
Dudley (Wm. L.), Colours of the Alkali Metals, 175
Dunbar on the Question of the Separate Identification of Typhoid and Coli Communis Bacilli, 472
Duncan (Mr.), Fishes and Water-oxygenation, 280
Dundas (Commander F. G.), the Juba River, 186
Dundas (Robert), Improvements in Railway Rolling Stock, 131
Dundee Whaling Fleet, Return of the, 473
Dunell (Mr.), the Screw Propeller, 21
Duner (Dr. Nils C.), Lord Kelvin, 110
Dunn (E. J.), Notes on the Glacial Conglomerate, Wild Duck Creek, 55
Dunstan (W. R.), the Identity of Caffeine and Theine and the Interactions of Caffeine and Auric Chloride, 311; on Isaconitine (Napelline), 430; the Composition of some Commercial Specimens of Aconitine, 430
Durham College of Science: Appeal for Relief from Financial Difficulties, 585
Durham (James), the Ordnance Survey and Geological Faults, 510
Durston (A. J.), Experiments on the Transmission of Heat through Tube-plates, 521
Dust Photographs, W. T. Thiselton-Dyer, F.R.S., 341; F. J. Allen, 341
Dust Photographs and Breath Figures, W. B. Croft, 364
Duthie (Col.), Egg Collecting, 253
Dwarfs, African; two Akka Girls brought to Germany by Dr. Stuhlmann, 470
Dwarfs, Racial, in the Pyrenees, R. G. Haliburton, 294; Wm. McPherson, 294
Dybowski (M.), the Bonjos, a Cannibal African Tribe, 257; Use of Chloride of Potassium instead of Salt by Soudanese, 499
Dyer (W. T. Thiselton, F.R.S.), Botanical Nomenclature, 53
Dyes, Notes on some Ancient, Edward Schunck, F.R.S., 22
Dynamics in Nubibus, "Waterdale," 601

Earth Oscillations, Observations of, P. Plantamour, 254
Earth's Age, the, Bernard Hobson, 175, 226; Dr. Alfred Russel Wallace, 175, 226; Clarence King, 285
Earth's History, the, R. D. Roberts, 412
Earth-Currents, the Recording of, 586
Earthenware Manufacture in Egypt, W. de Morgan, 613
Earthquakes: Earthquake in Ponza, 86; Earthquake Shocks, E. J. Lowe, F.R.S., 247, 270; Earthquake in Zante, 323, 348, 394, 585, 620; Instruments for the Earthquake Laboratory at the Chicago Exhibition, Prof. John Milne, F.R.S., F. Omori, 356; Earthquake in Samothrace and New Zealand, 372; Stromboli Earthquakes, A. Ricco and G. Mercalli, Dr. H. J. Johnston-Lavis, 453; Earthquake at Quetta, 470; Greater Frequency of Earthquakes in Cold Weather, 517; Earthquake at Catania, 543; in Servia, Bulgaria, Hungary, and Dominica, 562
Earthworms: On a Supposed New Species of Earthworms and on the Nomenclature of Earthworms, Dr. C. Herbert Hurst, 31; British Earthworms, William Blaxland Benham, 102; Frank J. Cole, 295; "Hare-Lip" in Earthworms, Rev. Hilderic Friend, 316; Luminous Earthworms, Rev. Hilderic Friend, 462
Eastern and Australian Lepidoptera in the Collection of the Oxford University Museum, Catalogue of, Col. C. Swinhoe, 53
Ebert (H.), An Automatic Interruptor for Accumulators, 69
Echinoderms, British, Catalogue of the, in the British Museum, F. Jeffrey Bell, 508
Eclectus, the Cause of the Sexual Differences of Colour in, Prof. A. B. Meyer, 486
Eclipse Photography, M. de la Baume Pluvinel, 326
Eclipse, Total Solar, of April 15–16 1893, 304, 376, 584, 611; M. de la Baume Pluvinel, 281, 304; A. Taylor, 317
Edinburgh Royal Society, 239, 287, 335, 431, 527
Education: Meeting of Association for Improvement of Geometrical Teaching, 277; Appointment of Committee on Organisation of Secondary Education, 277; the University of Chicago, 278; Richard Mulcaster, Foster Watson, 279; Changes recommended by Association of New England College Officers in Grammar School Curriculum, 279; Physical Education, Frederick Treves, 292; Scientific Education, Lord Playfair, 301; Higher Education in the United States, Dr. Low, 325; Work among the North American Indians, 350; Science Teaching, F. W. Sanderson, Prof. A. M. Worthington, L. Cumming, Dr. Stoney, W. B. Croft, Prof. Ayrton, E. J. Smith, Dr. Gladstone, 358; the Origin and Progress of the Educational Movement in Wales, O. M. Edwards, 421; the Proposed New Building for the Royal College of Science, Mr. Shaw-Lefevre, 448; Medical Education at Oxford, Lord Salisbury, 449; Formation of School

Gradation Committee, 613; Technical Education, Dr. W. Anderson, 155; Industrial School opened at Lucknow, 111; Technical Education Conference, 130; Technical Education in London, Report of the London County Council Committee, 300; Technical Education in Birmingham, Sir Henry Roscoe, 301; the Slöjd Association of Great Britain, 324; the London County Council and Technical Education, 348; Report of the Scottish Technical Education Committee, 543; the Universities and the County Councils, 586; the Cambridge University Extension Movement, 586; Improvement in City Guilds and London Institute Technological Examinations, 612
Egg Collecting, Col. Duthie, 253
Eggs, Artificially Incubated, W. Whitman Bailey, 200
Eggs, Study of the Form of, Dr. Nicolsky, 253
Egypt: Appointment of W. Flinders Petrie to Chair of Egyptology, University College, London, 111; Prof. Flinders Petrie's First Lecture, 278; Egyptian Figs, Rev. George Henslow, 102, 152; the Causes of the Defertilisation of, E. A. Floyer, 156; Ancient Egypt, Prof. Flinders Petrie, 301; Earthenware Manufacture in, W. de Morgan, 613; Egyptian Mummies, Prof. Macalister on, 623
Eichholz (A.) on Urobilin, 360
Eigenmann (C. H.), The Fishes of Southern California, 61
Elasticity, a Treatise on the Mathematical Theory of, A. E. H. Love, Prof. A. G. Greenhill, F.R.S., 529
Electricity: Remarkable Case of Electricity in a Cat, 17; an Arc-Light between Mercurial Electrodes in Vacuo, Dr. Arons, 24; Displacements of a Magnet on Mercury under Action of Electric Current, D. Decharme, 48; Cost of Electric Supply, Dr. John Hopkinson, F.R.S., 38; Power of Hydrogen-absorption of Various Metals, Herren Neumann and Streintz, 63; an Automatic Interruptor for Accumulators, H. Ebert, 69; Institution of Electrical Engineers Annual Dinner, 85; Demonstration by Means of Telephone of Existence of Interference of Electric Waves in Closed Circuits, R. Colson, 96; New Mirror Electrometer for High Potentials, Dr. Heydweiler, 112; Electric Oscillations, Pierre Janet, 119; Electrical Standards, 128; Value of Electric Light for Lettuce and other Winter Crops, Prof. L. H. Bailey, 130; Domestic Electric Lighting Treated from the Consumer's Point of View, E. C. De Segundo, 172; Electric Lighting and Power Distribution, W. Perren Maycock, 269; Gas Power for Electric Lighting, J. Emerson Dowson, 284; the Distribution of Power by Electricity from a Central Generating Station, A. Siemens, 378; Ionic Velocities, W. C. D. Whetham, 164; the Velocity of Crooke's Cathode Stream, Lord Kelvin, F.R.S., 164; Experiments in Electric and Magnetic Fields, Constant and Varying, Rimington and Wythe Smith, 165, Mr. Swinburne, 166; Prof. S. P. Thompson, 166; the Utilisation of Niagara Falls for Generating Electricity, 182; Improvement on the Herz Oscillator, MM. Sarasin and De la Rive, 184; a New Electric Furnace, H. Moissan, 192; Apparatus for Demonstrating Difference of Potential at Poles of Galvanic Cell, Messrs. Elster and Geitel, 233; Construction of Copies of Permanent Standard Mercury Resistances, 233; the Temperature of the Electric Arc, J. Violle, 240; Electric Currents in Plants, Prof. Burdon Sanderson, 255; on Thermo-electric Phenomena between two Electrolytes, Henri Bagard, 263; on the Origin of the Electric Nerves in the Torpedo, Gymnotus, Mormyrus, and Malapterurus, Gustav Fritsch, 271; Pure Gases Incapable of Producing Electrification by Friction, Mr. Wesendorck, 280; on the Temperature Coefficient of the Electrical Resistance of Mercury and on the Mercury Resistances of the Imperial Institution, Dr. Kreichgauer and W. Jaeger, 286; Electric Oscillations in Wires, Direct Measurement of the Moving Wave, Kr. Birkeland, 286; Magnetism and Electricity, R. W. Stewart, 315; the Growth of Electrical Industry, W. H. Preece, F.R.S., 327; Sarasin and De la Rive's Experiments in Measurement of Rate of Hertz Electric Waves, Prof. Raoul Pictet, 336; Rotation of Cylinder by Inductive Action, Signor Arno, 374; Observations of Atmospheric Electricity in America, T. C. Mendenhall, Prof. Oliver J. Lodge, F.R.S., 392; on the Electric Figures produced at the Surface of Crystallised Bodies, Paul Jannetaz, 408; on Electric Spark Photographs, or Photography of Flying Bullets, &c., by the Light of the Electric Spark, C. V. Boys, F.R.S., 415, 440; Hysteresis and Dielectric Viscosity of Mica for Rapid Oscillations, M. P. Janet, 432; a Magnetic Screen, Frederick J. Smith, 439; the Effects of Mechanical Stress on the Electrical Resistance of Metals, James H. Gray and James B. Henderson, 478; the Use of the Electric Current in producing High Temperatures, MM. Moissan and Violle, MM. Lagrange and Hoho, 497; Electrical Actinometer used by Messrs. Elster and Geitel in Measurement of Sun's Ultra-violet Radiation, 422; Ready Preparation of Large Quantities of the more Refractory Metals by means of the Electric Furnace, M. Moissan, 424; the Teredo and Electric Cables, Sir Henry Mance, 450; Intense and Rapid Heating Process by means of the Electric Current, MM. Lagrange and Hoho, 503; Photographic Registration Apparatus, Dr. Raps, 503; Electromagnetic Waves, 505; Electrical Papers, Oliver Heaviside, 505; Apparent Attraction of Closed Circuits by Alternating Magnetic Pole, Prof. Elihu Thomson, 517; the Physiological Effects of Electric Currents of High Frequency, M. d'Arsonval, 517; Penetration of thin Metallic Plates by Cathode Rays causing Phosphorescence, Herr Lenard, 518; a New Electrical Process permitting the Production of Temperatures Superior to those Actually Realisable, Eug. Lagrange and P. Hoho, 525; Equipotential Lines due to Current Flowing through Conducting Sheet fixed Photographically, E. Lommel, 544; on Initial Capacities of Polarisation, M. E. Bouty, 552; Experiments on Phosphorescence-producing Kathode Rays of a Geissler Tube, Dr. P. Lenard, 564; Electrical Railways, Dr. Edward Hopkinson, 570; on the Differential Equation of Electric Flow, T. H. Blakesley, Prof. Perry, Prof. O. J. Lodge, Dr. Sumpner, Mr. Swinburne, 574; a Method of obtaining Alternating Currents of Constant and easily-determined Frequency, I. Pupin, 586; Experiments on Loss of Electrical Charge of Bodies in diffuse Light and in Darkness, Edouard Branly, 586; the Recording of Earth-currents, 586; Dynamo-electric Machinery with Compound Excitation, M. Paul Hoho, 599; Experiments on Electric Oscillations of Medium Frequency, M. Janet, 615; Researches into the Study of Hall's Phenomenon, Prof. Kundt, 624
Elgar (Dr.), the Strength of Bulkheads, 520
Ellinger (Dr.), New Method of Preparing Glycol Aldehyde, 17
Elliott (W. R.), Earthquake in Dominica, 562
Ellis (W.), Map Showing Lines of Equal Magnetic Declination for Jan. 1, 1893, in England and Wales, 323; Relation between the Duration of Sunshine, the Amount of Cloud, and the Height of the Barometer, 431
Elster and Geitel (Messrs.), Apparatus for Demonstrating Difference of Potential at Poles of Galvanic Cell, 233; Electrical Actinometer used in Measurement of Sun's Ultra-violet Radiation by, 422
Embryology; on a Supposed Law of Metazoan Development, J. Beard, 79
Emmons (Hamilton) on the Petrography of the Island of Capraja, 334
Energy, the Identity of, Prof. Oliver Lodge, F.R.S., 293
Energy and Vision, Prof. S. P. Langley, 252
Engineering: Institution of Mechanical Engineers, 19, 300, 353, 617; Mechanical Engineering, Report on the Value of the Steam Jacket, J. G. Mair-Burnley, Col. English, Mr. Day, Bryan Donkin, Prof. Unwin, Bryan Donkin, Jun., Mr. Morrison, and Mr. Schonheyder, 20; the Screw Propeller, Messrs. Walker, Kennedy, Barnaby, Dunell, and Shield, 21; Junior Engineering Society, 38; Institution of Electrical Engineers, Annual Dinner, 85; Modern Mechanism, 241; a Correction, 281; Mechanical Engineering, the Apparatus at the Haslar Experimental Works, R. E. Froude, 353; the Southampton Water-softening Plant, William Matthews, 354; the Foundations of the Two River Piers of the Tower Bridge, E. E. W. Crettwell, 545; the Value of Annealing Steel, E. G. Carey, 397; Der Nord-Ostsee-Kanal, C. Beseke, 579; Steam Engine Trials, 594; the Alloys Research Committee, Second Report, Prof. W. C. Roberts-Austen, F.R.S., 617; the Action of Bismuth on Copper, Prof. W. C. Roberts-Austen, F.R.S., 618
England, American Opinion of Photography in, Xanthus Smith, 86; the Progress of Cremation in, 396; the English Flower Gardens, W. Robinson, 508
Engler's (Dr. A.) "Botanische Jahrbücher für Systematik, Pflanzengeschichte und Pflanzengeographie," 413
English (Col.) Experiments on the value of the Steam-Jacket, 20
Entomology; Remarkable Hornet's Nest presented to Madras Museum by Lord Wenlock, 16; a new species of Belytidæ from

New Zealand, Rev. T. A. Marshall, 17; Entomological Society, 47, 191, 334, 383, 455, 501, 575, 599; Vegetable Wasps and Plant Worms, M. C. Cooke, 99; Hints on Sugaring for Moths, W. Holland, 131; Beetles, Butterflies, and other Insects, A. W. Kapple and W. Egmont Kirby, 148; Death of H. T. Stainton, 155; the Bite of the Tarantula, C. W. Meaden, 184; Coleopterous Larvæ voided by a Child, J. E. Harting, 190; Remarkable Varieties of Telchinia Encedon, Lycæna adonis, Noctua xanthographa and Acronycta rumicis 191; Remarkable Weapons of Defence, G. F. Hampson, E. Ernest Green, 199; the Death's Head Moth and Bees, J. R. S. Clifford, 234; on the Anatomy of *Pentastomum teretiusculum*, Prof. W. Baldwin Spencer, 260; a Beetle tamed, 280; Description of New Species of Dipterous Insects of the Family *Syrphidæ*, in the Collection of the British Museum, with Notes on Species described by the late Francis Walker, E. E. Austen, 335; the Trap-door Spider, D. Cleveland, 375; The Fauna of British India, including Ceylon and Burma, G. F. Hampson, 387; On Stridulating Ants, Dr. Sharp, 501; a New Species (and genus) of *Acarus* found in Cornwall, A. D. Michael, 502; on the Anatomy of the *Eurypteridæ*, Malcolm Laurie, 527, Phosphorescence in Centipedes, R. J. Pocock, 545; Notes on a Spider, H. H. J. Bell, 557; Vanessa Polychloros in London, L. J. Tremayne, 563; the Use of Ants to Aphides and Coccidæ, T. D. A. Cockerell, 608

Ephemeris for Bodies Moving in the Biela Orbit, Dr. Chandler, 186

Ephemeris of Comet Brooks (November 20, 1892), 257, 281

Epidemic Influenza, F. A. Dixey, 244

Epiglottis, Die, Carl Gegenbaur, 542

Equivalent of Heat, the Value of the Mechanical, E. H. Griffiths, 537

Eritrea, Agriculture in the Italian Red Sea Colony of, 19

Eschenhagen (Herr), Improvement in Registration of Needle's Variations, 544

Esmoil (M.), the New Brooks' Comet, 159

Espin (T. E.), the Wolsingham Observatory, 452

Etheridge (R.), Ethnological Observations in Australia, 594

Ethnography, Prehistoric, of Central and North-East Russia, J. Smirnov, 524

Ethnology: the Mixed Character of the Population of Morocco, A. Le Châtelier, 61; Atlas der Völkerkunde, Dr. Georg Gerland, Dr. Edward B. Tylor, F.R.S., 223: Ethnological Observations in Australia, R. Etheridge, 594

Etna, on the Age of the Most Ancient Eruptions of, M. Wallerant, 264

"Eudiometer," the Author of the Word, Prof. Herbert McLeod, F.R.S., 536

Europe, Appearance of the American Vine-disease, Black-rot, in, 16

Evans (A. J.), Prehistoric Interments of the Bahi Rossi Caves near Mentone, 239

Everett (Prof. J. D., F.R.S.), Helmholtz on Hering's Theory of Colour, 365; a New and Handy Focometer, 500

Evolution, the Probable Physiognomy of the Cretaceous Plant Population, Conway McMillan, 587

Evolution of Decorative Art, the, Henry Balfour, 606

Evolution of Double Stars, the, T. J. J. See, Prof. G. H. Darwin, F.R.S., 459

Evolution, Experimental, Henry de Varigny, 25

Evolution and Man's Place in Nature, Henry Calderwood, 385

Ewald (Prof. Richard), the Cross-Striping of Muscle, 92

Ewing (J. A., F.R.S.), Magnetic Induction in Iron and other Metals, E. Wilson, 460

Examinations, Technological, 35

Exhibition, Chicago, Ceylon's Contribution to, 156

Exner (Prof.) on the Innervation of the Cricothyroid Muscle in Rabbits and Dogs, 287

Explosion in Gases, the Rate of, Prof. Harold B. Dixon, 299

Extinct Monsters, Rev. H. N. Hutchinson, 250

Eye, the Alleged Sexual Difference in the, Herr Greef, 325

Eye, Seven Images of the Human, M. Tcherning, 354

Eye, Sensitiveness of the, to Light and Colour, Capt. W. de W. Abney, F.R.S., 538

Fabre (Charles), Traité Encyclopédique de Photographie, 6

Fabry (M.), the New Brooks' Comet, 159

Falsan (Albert), Les Alpes Françaises, 76

Fasting Men, Experiments on the Nutrition of, D. J. Munk; Respiratory Interchange in the Fasting Body, Prof. Zuntz; the Construction of Carbohydrates in the Fasting Body, Dr. Vogelius, 552

Fauna of British India, including Ceylon and Burma, G. F. Hampson, 387

Fauna, British Marine, Proposed Handbook to the, Prof. W. A. Herdman, F.R.S., 231, 293; Prof. D'Arcy, W. Thompson, 269; W. Garstang, 293

Fauna and Flora of Gloucestershire, Charles A. Witchell and W. Bishop Strugnell, 197

Fauna of Lakeland, a Vertebrate, Rev. A. Macpherson, 457

Favaro (Prof. Antonio), Galileo Galilei and the Approaching Celebration at Padua, 82, 180

Fawcett (W.), the Jamaica Botanical Department, 378

Faye (H.), Sunspots, 167; on the True Theory of Waterspouts and Tornadoes, with Special Reference to that of Lawrence Massachusetts, 503

Fayrer (Sir Joseph), Speech at Tercentenary of Galileo, 207

Ferns of South Africa, the, Thos. R. Sim, J. G. Baker, F.R.S., 291

Field Naturalists' Club, Victoria, 62

Field (Eleanor), Note on the Interactions of Alkali-Metal Haloids and Lead Haloids, and of Alkali-Metal Haloids and Bismuth Haloids, 551

Field (Dr. Herbert H.), an International Zoological Record, 606

Fifth Satellite, Jupiter's, E. Roger, 71; A. A. Common, 208; E. E. Barnard, 377

Figs, Egyptian, Rev. George Henslow, 102, 152

Figures, Breath, W. B. Croft, 186

Figures, Breath, Dust Photographs and, W. B. Croft, 364

Finland, Opening of Wiborg-Imatra Railway, 160

Fischer (Prof. Theobald), the Geography and Social Conditions of the Iberian Peninsula, 547

Fish, the Destruction of Immature, Ernest W. L. Holt, 160; the Protection of Sea Fish, C. H. Cook, 396; Electric Fishes, Gustav Fritsch, 271; Fishes and Oxygenation of Water, Duncan and Hoppe-Seyler, 280; Prof. Edward Prince appointed Commissioner for Canada Fisheries, 37; Fishery Board for Scotland, the, 85; Japan and the Korean Fishery, 324

Fisher (Prof. W. R.), American Forestry, C. S. Sergent, 275

Fisher (W. W.), Anhydrous Oxalic Acid, 238

Fitzgerald (Prof.), Williams on the Relation of the Dimensions of Physical Quantities to Directions in Space, 69; Mr. Sutherland's Paper on the Laws of Molecular Force, 117

Fitzgerald (Prof. George Francis), Universities and Research, 100

Flammarion (Camille), La Planète Mars et ses Conditions d'Habitabilité, William J. S. Lockyer, 553

Fletcher (L., F.R.S), Baddeleyite, 70; the Occurrence of Native Zirconia (Baddeleyite), 283; Obituary Notice of Thomas Davies, 371

Flies and Disease Germs, 499

Flight of Birds, the, Herbert Withington, 414

Flora and Fauna of Gloucestershire, Charles A. Witchell and W. Bishop Strugnell, 197

Florida, Botanical Laboratory Established at Eustis, 278

Florida Phosphate Beds, the, T. N. Lupton, 325

Flower (Major L.), Water and Water Supply, 183

Flower Garden, the English, W. Robinson, 508

Floyer (E. A.), the Causes of the Defertilisation of Egypt, 156

Fluids, Elementary Mechanics of Solids and, A. L. Selby, 315

Flying Bullets, the Photography of, by the Light of the Electric Spark, C. V. Boys, F.R.S., 415, 440

Folding, Experiments on, and on the Genesis of Mountain Ranges, Prof. E. Reyer, 81

Folie (F.), Remarkable Optical Phenomenon near Zermatt, 303

Folk-Lore: Australian Rain-Maker's Leather Boots, 62; the Were-Wolf in Latin Literature, 423; Lizard Superstition of Shuswap (British Columbia) Indians, Dr. George Dawson, F.R.S., 184

Food of Plants, the, A. P. Laurie, 556

Food, the Principal Starches Used as, W. Griffiths, 76

Foraminifer or Sponge? R. Hanitsch, 365, 439; F. G. Pearcey, 390

Forbes (Henry O.), British New Guinea, 345, 414; Observations on the Development of the Rostrum in the Cetacean Genus *Mesoplodon*, 455; the Chatham Islands and an Antarctic Continent, 474

Force, the Laws of Molecular, Mr. Sutherland's Paper on, Prof.

Fitzgerald, Dr. Gladstone, S. H. Burbury, Prof. Ramsay, Macfarlane Gray, Prof. Herschel, 117
Ford (Charles), Severe Frost at Hongkong, 535
Ford (W. V.), Snow Rollers, 422
Forel (Prof. F. A.), Le Léman, Monographie Limnologique, Prof. T. G. Bonney, F.R.S., 5
Force (M.), the Resistance of Ice, 564
Forest Tithes and other Studies from Nature, 580
Forestry, the Destruction of Trees in America, Dr. W. J. Beal, 563; Forestry in India, the Dehra Dun Forest School, Sir E. C. Buck, 613; Recent Researches on the Influence of Forests, Dr. Schubert, 480
Fossil Botany, the Genus Sphenophyllum, Prof. Wm. Crawford Williamson, 11; on a New Fern from the Coal Measures, A. C. Seward, 360
Fossil Fauna of the Black Sea, T. J. van Beneden, 544
Fossil Floras and Climate, Sir William Dawson, F.R.S., 556; J. Starkie Gardner, 582
Fossil Plants as Tests of Climate, A. C. Seward, 267; J. Starkie Gardner, 267, 364; Chas. E. De Rance, 294, 342
Fossil Reptiles from the Elgin Sandstone, Some New, E. T. Newton, 189
Fossile Flora der Höttinger Breccie, Die, R. von Wettstein, 436
Fossils, A Catalogue of British Jurassic Gasteropoda, W. H. Hudlestone, F.R.S., and Edward Wilson, H. Woods, 363
Fourcroya Selloa, Flowering in Botanic Society's Conservatory of, 373
Fox (Howard), Notes on Some Coast Sections at the Lizard, 407; on a Radiolarian Chert from Mullion Island, 407
Framework of Chemistry, the, W. M. Williams, 28
France: the New Triangulation of, L. Bassot, 71; Dirigible Balloon in Construction at Chalais-Meudon, 112; French Academy, Science Prizes, 232; Statistics of Average Life in France, M. Turqun, 255; Some Lake Basins in, Prof. T. G. Bonney, F.R.S., 341, 414; Société Astronomique de, 616
François (Capt. von), Dromedaries in German S.W. Africa, 38
Frankland (Prof. P. F., F.R.S.), Salts of Active and Inactive Glyceric Acid: the Influence of Metals on the Specific Rotatory Power of Active Acids, 405; Van't Hoff's Stereochemistry, 510; the Purification of Water by Bacteriological Methods, 588
Frankland (Mrs. Percy), Bacilli in Butter, 283
Friedel (C.), Diamond in Meteoric Iron, 192; on the Meteoric Iron of the Cañon Diablo, 408
Friend (Rev. Hilderic), "Hare-Lip" in Earthworms, 316; Luminous Earthworms, 462
Fritsch (Gustav), on the Origin of the Electric Nerves in the Torpedo, Gymnosus, Mormyrus, and Malapterurus, 271
Frost, Dew and, Hon. R. Russell, 210
Frost, severe, at Hongkong, W. T. Thiselton-Dyer, F.R.S., 535; Charles Ford, 535; W. Doberck, 536
Frost Patterns, Arborescent, 213; Prof. R. Meldola, F.R.S., 125; G. J. Symons, F.R.S., 162; Rev. T. G. Bonney, F.R.S., 162; Dr. J. H. Gladstone, F.R.S., 162; D. Wetterhan, 162; J. T. Richards, 162; J. J. Armitage, 162
Froude (R. E.), the Apparatus at the Haslar Experimental Works, 353
Fruit Export Trade, Cape Colony Government Encouragement of, 234
Fujisan, John Milne, F.R.S., 178
Fulcher (L. W.), Stromboli in 1891, 89

Gad (Prof.), the Respiratory Centre, 144
Gadolin (General A.W.), Death of, 232
Gain (E.), Influence of Moisture on Vegetation, 119
Galileo Galilei and the Approaching Celebration at Padua, Prof. Antonio Favaro, 82, 180, 207
Galitzine (B.), Method for Determining Density by Saturated Vapours and Expansion of Liquids at Higher Temperatures, 189
Gallwey's (Capt. H. L.), Travels in Benin Country, 134
Galton (Francis, F.R.S.), Measure of the Imagination, 319; Optical Continuity, 342
Galvanometer, A Modified Astatic, H. E. J. G. du Bois and H. Rubens, 455
Garden, the English Flower, W. Robinson, 508
Gardner (Edmund G.), Quæstio de Aqua et Terra, 295
Gardner (J. Starkie), Fossil Plants as Tests of Climate, being the Sedgwick Prize Essay for the year 1892, A. C. Seward, 267
Fossil Plants as Tests of Climate, 364
Garstang (John), the Light of Planets, 77
Garstang (W.), a Proposed Handbook of the British Marine Fauna, 293
Gas Power for Electric Lighting, J. Emerson Dowson, 284
Gases, the Densities of the Principal, Lord Rayleigh, F.R.S., 567
Gases in Living Plants, J. G. Arthur, 427
Gases, the Rate of Explosion in, Prof. Harold B. Dixon, 299
Gasteropoda, a Catalogue of British Jurassic, W. H Hudleston, F.R.S., and Edward Wilson H. Woods, 363
Gauss (Charles Frederick), Proposed Monument to, 106
Gaye (Selina), the Great World's Farm, 198
Gegenbaur (Carl), Die Epiglottis, 542
Geikie (Sir Archibald, F.R.S.), Geological Map of Scotland, Prof. A. H. Green, F.R.S., 49; Prof. Wadsworth on the Geology of the Iron, Gold, and Copper Districts of Michigan, 117; Prof. A. de Lapparent, 217; Geology of the North-West Highlands, 292
Geitel and Elster (Messrs.), Apparatus for Demonstrating Difference of Potential at Poles of Galvanic Cells, 233; Electrical Actinometer used in Measurement of Sun's Ultra-Violet Radiation by, 422
Geminids, December Meteors, W. F. Denning, 226
Geneva, the Lake of, Prof. F. A. Forel, Prof. T. G. Bonney, F.R.S., 5
Genoa, Munificent Bequest by Admiral Marquis Ricci for Founding Scientific Institutions in, 613
Genus Sphenophyllum, the, Prof. Wm. Crawford Williamson, 11
Geodesy: the New Triangulation of France, L. Bassot, 71; French Measurement of Arc of Meridian, 115; on the Progress of the Art of Surveying with the Aid of Photography in Europe and America, M. A. Lauseolat, 384; the Trianguiation of N.W. South Australia, 519; Measurement of the Parallel of 47° 30′ in Russia, M. Venukoff, 576
Geography: Le Léman: Monographie Limnologique, Prof. F. A. Forel, Prof. T. G. Bonney, F.R.S., 5; Sven Hedin's Ascent of Mount Demavend, 19; Agriculture in the Italian Red Sea Colony of Eritrea, 19; Mr. Conway's Karakoram Range Expedition, 19; His Crossing of the Hispar Pass, 327; Geographical Notes, 19, 64, 89, 115, 133, 159, 209, 235, 257, 282, 304, 327, 352, 377, 399, 426, 452, 473, 498, 519, 547, 566, 590, 617; Uganda, Capt. F. D. Lugard, 45; the Uganda Commission, 210; the Voyage of *La Manche* to Iceland, Jan Mayen and Spitzbergen in 1892, M. Bienaimé, 48; Discovery of Subterranean Town on the Amu-Daria, 64; the Proposed Transference of the Capital of Brazil, 64; Mr. D. J. Rankin's Zambesi Journey, 1890–91, 64; Death of Theodore Child, 65; Supposed Suicide of Lieut. Frederick Schwatka, 65; the Cause of Lieut. Schwatka's Death, 89; Royal Geographical Society, 65, 115, 89, 209, 617; Experiments on Folding and on the Genesis of Mountain Ranges, Prof. E. Reyer, 81; Official Rule for Spelling of Place-names of German Protectorates, 89; Completion of Capt. Monteil's Mission, 89; Mr. Joseph Thomson's Journey to Lake Bangweolo, 115; French Measurement of Arc of Meridian, 115; Kettler's Afrikanische Nachrichten, 115; Proposed Expedition of Mr. and Mrs. Theodore Bent to Abyssinia, 115; Arrival of Mr. and Mrs. Theodore Bent at Adowa, 519; Arrival of Mr. and Mrs. Theodore Bent at Aksum, 547; In Savage Isles and Settled Lands, B. F. S. Baden-Powell, 122; Dr. Karl Diener's Himalaya Expedition, 133; Proposed Arctic Expedition of Lieut. Peary, 133, 452; Death of F. H. von Hellwald, 133; Capt. H. L. Gallwey's Travels in Benin Country, 134; E. Wilkinson's Journey in the Kalahari Desert, 134; Geographical Society of California, 134; Progress of Congo Railway, Major Thys, 159; Manchester Geographical Society, 159; Liverpool Geographical Society, 159, 428; Scottish Geographical Society, 159; Dr. J. Troll's Journey through Central Asia, 160; Opening of Wiborg and Imatra Railway, Finland, 160; the Juba River, Commander F. G. Dundas, 186; M Alex. Delcommune's Lomami Expedition, 209, 590; the Death of Cardinal Lavigerie, 210; Proposed Exploration of Africa by Telegraph, Cecil Rhodes, 210; the Characteristic Form of the Coast Line as affecting the Physical Conditions of the Waters of the English Channel, H. N. Dickson, 235; Geographical Names, Colonel H. H. Godwin-Austen, F.R.S.,

245 ; M. Obrutcheff's Further Researches in Siberia, 255 ; the Bonjos, an African Cannibal Tribe, M. Dybowski, 257 ; the Boundaries of Costa Rica and Nicaragua, Dr. H. Polakowsky, 257 ; the Stranding of H.M.S. *Howe*, the Defective Chart used, 257 ; Physical Geography and Climate of New South Wales, H. C. Russell, F.R.S., 258 ; African Nomenclature, 282 ; the Stanley Falls District of the Congo, M. Page, 282 ; the Antarctic Whaling Fleet, 282, 590 ; the Pinsk Marshes and Non-Russian Atlases, M. Venukoff, 282 ; the Relation of Geography to History, H. J. Mackinder, 304 ; African Nomenclature, 304 ; Stoppage of M. Mizon's Adamawa Expedition, 304 ; French Flag Hoisted on St. Paul and New Amsterdam Islands, 304 ; Geography, 327 ; Aston Chanler's Expedition to Lake Rudolf, 327 ; the Soil of Sakalava Plain, Madagascar, Emile Gautier, 327 ; the Frontier Delimitation between British South Africa Company's Territory and Portuguese Possessions, Major Leverson, 327 ; Yezo and the Ainu, Prof. J. Milne, F.R.S., 330 ; A. H. Savage Landor, 330 ; British New Guinea, J. P. Thompson, Henry O. Forbes, 345 ; the Steppe-belt traversing Asia from East to West, H. J. Mackinder, 353 ; Death of R. H. Nelson, 353 ; Twenty Years in Zambesia, F. C. Selous, 377 ; Dr. Baumann's Journeys in the Nile-sources Region, 377 ; Proposed North Pole Expedition by way of Franz Josef Land, F. G. Jackson, 377 ; the Regulation of Swiss Torrents, M. de Salis, 377 ; the Chief Lines of Communication between Asia and Europe, H. J. Mackinder, 400 ; Captain Bower's Journey in Tibet, 400 ; the Orthography of African Place-names, 400; Railways in China, 400 ; Prof. Penck's Scheme for a Map of the World on Uniform Scale, 426 ; Mongolia and Central Tibet, C. Woodville Rockhill, 426 ; New Harbour found in German South-West Africa, 452 ; Return of the Dundee Whaling Fleet, 473 ; The Katanga Company's Expeditions, 474 ; Prof. Mohn on the Climate of Greenland, 474 ; the Chatham Islands and an Antarctic Continent, H. O. Forbes, 474 ; Political Divisions of the Earth, Dr. A. Oppel, 499 ; use of Chloride of Potassium instead of Salt by the Soudanese, M. Dybowski, 499 ; Distribution of Population of Schleswig-Holstein, Dr. A. Gloy, 499 ; French Explorations towards Lake Chad, 519 ; some Geographical Aspects of British History, 519 ; the Triangulation of North-west South Australia, 519 ; Death of John Bartholomew, 547 ; the Siberian Peninsula, Prof. Theobald Fisher, 547 ; the Object of Map-colouring, 566 ; the Form of the Geoid, M. Venukoff, 566 ; Map of Salinity of Surface Water of North Pacific, Prof. Kümmel, 590 ; the Native Papuans, T. H. Hatton-Richards, 590 ; the Hon. G. N. Curzon's Journey in Indo-China, 617 ; Facts from the Bengal Census Significant of Progress, 617 ; a Curious Mountain Group in Podolia, 617

Geoid, The Form of the, M. Venukoff, 566

Geology : Der Peloponnes, Dr. Alfred Philippson, 6 ; the Geology of the Asiatic Loess, Thos. W. Kingsmill, Prof. G. H. Darwin, F.R.S., 30 ; Geology of Taylorville Region in Sierra Nevada, California, J. S. Diller, 39 ; Geological Map of Scotland, Sir Archibald Geikie, F.R.S., Prof. A. H. Green, F.R.S., 49 ; an Ancient Glacial Epoch in Australia, Dr. Alfred R. Wallace, 55 ; Death of James Kant, 60 ; Geological Collection, presented by Mr. Evan Roberts to University College of North Wales, 60 ; the Glacial Nightmare and the Flood, Sir H. H. Howorth, 61 ; a Palæozoic Ice-Age, W. T. Blanford, F.R.S., and Henry F. Blanford, F.R.S., 101 ; Geology of Scotland, Prof. Grenville, A. J. Cole, 101 ; Geological Society, 117, 166, 263, 286, 334, 407, 501, 575, 623 ; Medal Awards, 277 ; the Iron, Gold, and Copper Districts of Michigan, Prof. M. E. Wordsworth, Sir Archibald Geikie, Dr. Hicks, H. Bauerman, 117 ; Man and the Glacial Period, Dr. G. Frederick Wright, 148 ; Difficulties of Pliocene Geology, Sir Henry H. Howarth, 150, 270 ; Geological Features of Arabia Petræa and Palestine, Prof. Edward Hull, F.R.S., 166 : Flexible Sandstone, E. M. Hamilton, Mr. Hornby, Prof. Green, 167 ; Macculloch's Geological Map of Scotland, Prof. J. W. Judd, F.R.S., 173 ; Ancient Ice Ages, T. Mellard Reade, 174 ; J. Lomas, 227 ; The Earth's Age, Dr. Alfred Wallace, 175, 226 ; Bernard Hobson, 175, 226; Clarence King, 285 ; Death of I. D. Chersky, 232 ; Further Researches in Siberia, M. Obrutcheff, 255 ; on some Schistose Greenstones from the Pennine Alps, Prof. T. G. Bonney, F.R.S., 263 ; Note on the Nufenen-stock (Lepontine Alps), Prof. T. G. Bonney, F.R.S., 263 ; on a Secondary Development of Biotite and of Hornblende in Crystalline Schists from the Binnenthal, Prof. T. G. Bonney, F.R.S., 263 ; on a Sauropodous Dinosaurian Vertebra from the Wealden of Hastings, R. Lydekker, 286 ; on some Additional Remains of Cestraciont and other Fishes in the Green Gritty Marls immediately overlying the Red Marls of the Upper Keuper in Warwickshire, Rev. P. B. Brodie, 286 ; Scandinavian Boulders at Cromer, Herr Victor Madsen, 287 ; Pitchstone and Andesite from Tertiary Dykes in Donegal, Prof. Solles, F.R.S., 287 ; on the Variolite and Associated Igneous Rocks of Roundwood, co. Wicklow, 287 ; the Geology of the North-west Highlands, Sir Archibald Geikie, F.R.S., 292 ; Variolite of the Lleyn and Associated Volcanic Rocks, Catherine A. Raisin, 334 ; on the Petrography of the Island of Caraja, Hamilton Emmons, 334 ; Some Lake Basins in France, Prof. T. G. Bonney, F.R.S., 341, 414 ; Origin of Lake Basins, the Duke of Argyll, F.R.S., 485 ; J. C. Hawkshaw, 558 ; the U.S. Survey and American Mining Industries, 350 ; Glacier Action, the Duke of Argyll, 389 ; Notes on some Coast Sections at the Lizard, Howard Fox and J. J. H. Teall, F.R.S., 407 ; on a Radiolarian Chert from Mullion Island, Howard Fox and J. J. H. Teall, F.R.S., 407 ; Note on a Radiolarian Rock from Fanny Bay, Port Darwin, Australia, G. J. Hinde, 407 ; Notes on the Geology of the District west of Caermarthen, T. Roberts, 407 ; Presidential Address at the Geological Society's Anniversary Meeting, 407 ; the Earth's History, R. D. Roberts, 412 ; Die Fossile Flora der Höttinger Breccie, R. Von Wettstein, 436 ; the Glacier Theory of Alpine Lakes, Dr. Alfred Russel Wallace, 437 ; Death of Prof. K. A. Lossen, 421 ; Glacial Drift of the Irish Channel, Prof. Grenville, A. J. Cole, 464 ; the Landslip at Sandgate, Prof. J. F. Blake, 467 ; on the Occurrence of Boulders and Pebbles from the Glacial Drift in Gravels south of the Thames, Horace W. Monckton, 501 ; a Fossiliferous Pleistocene Deposit at Stone, on the Hampshire Coast, Clement Reid, 502 ; the Ordnance Survey and Geological Faults, James Durham, 510 ; Action of Glaciers on the Land, Prof. T. G. Bonney, F.R.S., 521 ; the Quaternary Deposits of Russia and their Relations to the Finds resulting from the Activity of Prehistoric Man, S. Nikitine, 523 ; Constitution of the Quaternary Deposits in Russia and their Relations to the Finds resulting from the Activity of Prehistoric Man, S. Nikitine, 523 ; Russian Steppes Past and Present, Prof. W. W. Dokoutchaiev, 523 ; on an Intrusion of Muscovite-biotite-gneiss in the South-Eastern Highlands, and its accompanying Thermo-Metamorphism, Geo. Barrow, 575 ; Text-Book of Comparative Geology, Dr. E. Kayser, 578 ; the Probable Physiognomy of the Cretaceous Plant Population, Conway McMillan, 587 ; Geological Society of Washington founded, 613 ; on the Dwindling and Disappearance of Limestones, Frank Rutley, 623

Geometry : on the Need of a New Geometrical Term—Conjugate Angles, Prof. A. M. Worthington, 8 ; a Remarkable Case of Geometrical Isomerism, A. E. Tutton, 65 ; Meeting of Association for Improvement of Geometrical Teaching, 277 ; on the non-Euclidian Geometry, Dr. Emory McClintock, 286 ; Descriptive Geometry Models for the Use of Students in Schools and Colleges, T. Jones, 413 ; Proposed Celebration of Centenary of Birth of Lobatcheffsky, 469 ; Introductory Modern Geometry of Point, Ray, and Circle, 532

Geraniums, Red and White, a Graft-Hybrid between, H. L. Jones, 563

Gerland (Dr. George), Atlas der Völkerkunde, Dr. Edward B. Tylor, F.R.S., 223

German Science Reader, A, Francis Jones, 125

German South-west Africa : Official Rules for Spelling of Place-Names of German Protectorates, 89

German South-west Africa, New Harbour found in, 452

Germs, Flies, and Disease, 499

Giacosa (Prof. P.), Bibliographia Medica Italiana, 606

Gibbs (Dr. Morris), the Food of Humming-birds, 63

Gibbs (Prof. J. Willard), Quaternions and the Algebra of Vectors, 463

Gibson (J.), the Preparation of Glucina from Beryl, 405

Gilbert (William) of Colchester, Physician of London, on the Loadstone and Magnetic Bodies, and on the great Magnet the Earth : a New Physiology, demonstrated with many Arguments and Experiments, 556

Gill (C. H.) Fungus internally Parasitic in Diatoms, 118

Gill (T. H.), the Use of Monochromatic Yellow Light in Photomicrography, 47
Giorgis (G.), Volumetric Method for determining amount of Chromium in Steel, 397
Glacial Drift of the Irish Channel, Prof. Grenville, A. J. Cole, 464
Glacial Epoch in Australia, an Ancient, Dr. Alfred R. Wallace, 55
Glacial Nightmare and the Flood, The, Sir A. H. Howarth, 61
Glacial Period, Man and the, Dr. G. Frederick Wright, 148
Glacier Action, the Duke of Argyll, F.R.S., 389
Glacier Theory of Alpine Lakes, The, Dr. Alfred Russel Wallace, 437
Glaciers of Val d'Herens, William Sherwood, 174
Glaciers, Action of, on the Land, Prof. T. G. Bonney, F.R.S., 521
Gladstone (Dr.), Mr. Sutherland's Paper on the Laws of Molecular Force, 117
Gladstone (Dr. J. H., F.R.S.), Arborescent Frost Patterns, 162; Science Teaching, 359; on some recent determinations of Molecular Refraction and Dispersion, 429
Glan (Paul), the Fundamental Law of Complementary Colour, 455
Glazebrook (R. T., F.R.S.), Laws and Properties of Matter, 580
Glazes, Pottery, W. P. Rex, 396
Gloucestershire, Fauna and Flora of, Charles A. Witchell and W. Bishop Strugnell, 197
Gloy (Dr. A.), Distribution of Population of Schleswig-Holstein, 499
Godwin-Austen (Colonel H. H., F.R.S.), Geographical Names, 245
Goldstein (Prof.) Experiments of, 312
Gore (J. Ellard), The Visible Universe, A. Taylor, 193
Gorilla acquired by Berlin Aquarium, Large Male, 86
Gothard (M. Eugen), Spectra of Planetary Nebulæ and Nova Aurigæ, 352
Gottingen Royal Society of Sciences, 456; Prize Subject for 1893-4, 516
Gottsche (Dr. C. M.) Death of, 130
Grafting, on the Physiology of, Dr. Hermann Vöchting, 128
Grant (Robert, F.R.S.), Death of, 14; Obituary Notice of, 36
Graphical Solutions of Problems in Navigation, 547
Grasses of the Pacific Slope, including Alaska and the adjacent Islands, Dr. Geo. Vasey, 173
Gratings, Photography of, Engraved upon Metal, M. Izarn, 623
Gravelaar (N. L. W. A.), the Great Ice Age, 200
Gravitation, on the Variations in the Intensity of Terrestrial, M. d'Abbadie, 384
Gravitation, Description of an Instrument to show the small Variations in the Intensity of, M. Bouquet de la Grye, 431
Gray (Andrew), Aids to Experimental Science, 173
Gray (A. D.), Dibromo-β-Lapachone, 405
Gray (James H.), the Effects of Mechanical Stress on the Electrical Resistance of Metals, 478
Gray (Macfarlane), Mr. Sutherland's Paper on the Laws of Molecular Force, 117
Great Ice Age, The, N. L. W. A. Gravelaar, 200
Great Spirit Spring Mound, Kansas, E. H. S. Bailey, 87
Great World's Farm, The, Selina Gaye, 198
Greef (Herr), the Alleged Sexual Difference in the Eye, 325
Green (Prof. A. H., F.R.S.), Geological Map of Scotland, Sir Archibald Geikie, F.R.S., 49; Flexible Sandstone, 167
Green (E. Ernest), Remarkable Weapons of Defence, 199
Greenhill (Prof. A. G., F.R.S.), a Treatise on the Mathematical Theory of Elasticity, A. E. H. Love, 529
Greenland, the Climate of, Prof. Mohn, 474
Gregory (James R.), a Large Meteorite from Western Australia, 90
Gregory (Richard A.), a Description of the Laws and Wonders of Nature, 74
Gréhant (G.) Physiological Study of Opium Smoke, 168
Griffiths (E. H.), Determination of Low Temperatures by Platinum Thermometers, 95; the Value of the Mechanical Equivalent of Heat, 476, 537
Griffiths (J.), Note on Secondary Tucker Circles, 71
Griffiths (W.), the Principal Starches Used as Food, 76
Grote (George), Examination of Brain of, Prof. John Marshall, 15
Ground-water, The Movements of, F. H. King, 206
Grye (M. Bouquet de la), Description of an Instrument to show the small Variations in the Intensity of Gravitation, 431
Guillemin (Amédée), Autres Mondes, 485
Gum Arabic, Deterioration of, W. F. Howlett, 183
Gunther (Dr. Carl), Einführung in das Studium der Bakteriologie, 446
Gymnotus, Mormyrus, and Malapterurus, on the Origin of the Electric Nerves in the Torpedo, Gustav Fritsch, 271

Habenicht (H.), Gulf Stream Icebergs and Climatic Variations, 206
Haddon (Prof. Alfred C.), British New Guinea, 414
Hæmatite as an Illustration of the Tendency of Inorganic Matter to Emulate Organic Forms, 374
Haentzsch (Dr.), on the Potential Equation, 480
Hail Storms, H. C. Russell, F.R.S., 573
Hair, Mammalian, the Phylogenetic Position of, Herr Maurer, 87
Hale (Prof. G. E.), Ultra-Violet Spectrum in Solar Prominences, 186; Coincidence of Solar and Terrestrial Phenomena, 425; Prof. Hale's Solar Photographs, 498
Haliburton (R. G.), Racial Dwarfs in the Pyrenees, 294
Hall (H. S.), Algebra for Beginners, 28
Hall (James P.), a Short Cycle in Weather, 499
Hamilton (E. M.), Flexible Sandstone, 167
Hampson (G. F.), Remarkable Weapons of Defence, 199; the Fauna of British India, including Ceylon and Burma, 387
Hande (A.), Simple Instrument for Measuring Densities of Liquids, 471
Hanitsch (R.), Foraminifer or Sponge? 365, 439
Hannay (J. B.), Formation of Lunar Volcanoes, 7
Hansemann (Dr.), Photography of Microscopic Objects which when placed in a Stereoscope Presented an Appearance of Solidity, 287
Hansen (Prof. Carl), Pinetum Danicum, 619
Harcourt (Sir William), the Decimal System, 323
Hare-lip in Earthworms, Rev. Hilderic Friend, 316
Harmer (S. F.), Lion-Tiger Hybrids, 413
Harmer (Sidney F.), on the Occurrence of Embryonic Fission in Cyclostomatous Polyzoa, 524
Harrington (Prof. M. W.), Exploration of the Free Air, 574
Harris (R. A.), on the Use of Supplementary Curves in Isogonal Transformation, 380
Harrison (E. F.), on Isaconitine (Napelline), 430
Hart (W. E.), Parasitism of *Volucella*, 78
Harting (J. E.), Coleopterous Larvæ Voided by a Child, 190; the Field-Vole Plague in Thessaly, 545
Hartley (W. N.), the Origin of Colour and Fluorescence, 238; Methods of Observing and Separating Spectra of Easily Volatile Metals, 239; Manganese Borate, 239
Hartog (J.), Thionyl Bromide, 405
Harvard College Observatory, the, Prof. E. C. Pickering, 304, 403
Haslar Experimental Works, the Apparatus at the, R. E. Froude, 353
Hatching of a Peripatus Egg, the, Arthur Dendy, 508
Hatton-Richards (T. H.), the Native Papuans, 590
Hawaiian Islands, a Botanist's Vacation in the, Prof. D. H. Campbell, 236, 355
Hawkshaw (J. C.), Origin of Lake Basins, 558
Haycraft (Prof.), the Cross-Striping of Muscle, 92
Haycraft (John Berry), a New Hypothesis Concerning Vision, 478
Hayward (R. Baldwin, F.R.S.), the Algebra of Co-planar Vectors and Trigonometry, 266
Health Officer's Pocket-Book, the, Dr. E. F. Willoughby, 412
Health, Public, a Treatise on, Dr. Albert Palmberg, Dr. H. Brock, 507
Heat in August, 1892, Prof. W. J. van Bebber, 88
Heat, the Value of the Mechanical Equivalent of, E. H. Griffiths, 476, 537
Heat; Comparison of Formulæ for Total Radiation, W. de C. Stevens, 188
Heaviside (Oliver, F.R.S.), Electrical Papers, 505; Vectors *versus* Quaternions, 533
Hedin's (Sven) Ascent of Mount Demavend, 19
Heen (P. de), on a State of Matter Characterised by the Mutual Independence of the Pressure and the Specific Volume, 309
Height and Spectrum of Auroras, the, T. W. Backhouse, 151

Hellward (Frederick Heller von), Death of, 133
Helmholtz on Hering's Theory of Colour, Prof. J. D. Everett, HF.R.S., 365
Helvellyn's Shade, Beneath, Samuel Barber, 364
emiptera Heteroptera of the British Islands, the, Edward Saunders, 292
Hemsley (W. Botting, F.R.S.), Climbing Plants, Dr. H. Scherck, 514
Henderson (James B.), the Effects of Mechanical Stress on the Electrical Resistance of Metals, 478
Henrici (Prof.), Williams on the Relation of Dimensions of Physical Quantities to Directions in Space, 69
Henry (Charles), a Photoptometric Photometer, 24; on the Minimum Perceptible Amount of Light, 312
Henslow (Rev. Geo.), Egyptian Figs, 102, 152
Hepworth (Capt. M. W. C.), the Tracks of Ocean Wind Systems in Transit over Australasia, 286
Hepworth (T. C.), Oxygen for Limelight, 176
Herbertson (A. J.), on the Hygrometry of the Atmosphere at Ben Nevis, 431
Herdman (Prof. W. A., F.R.S.), Proposed Handbook to the British Marine Fauna, 231, 293
Heredity, Prof. August Weismann, 265
Hering's Theory of Colour, Helmholtz on, Prof. J. D. Everett, F.R.S., 365
Hermite (Gustave), Explorations of Higher Atmospheres by Means of Free Balloons with Automatic Recorders, 119; Exploration of the Higher Atmosphere, 600
Herschel (Prof.), Mr. Sutherland's Paper on the Laws of Molecular Force, 117
Hertwig (Prof. Dr. Oscar), Die Zelle und Die Gewebe, Grundzüge der Allgemeinen Anatomie und Physiologie, 314
Heurck (Dr. Henri van), the Microscope, its Construction and Management, Rev. Dr. Dallinger, F.R.S., 409
Heusler (Dr.), the Volatility of Manganese, 375
Heycock and Neville, Isolation of Gold and Cadmium Compound, 90
Heydweiler (Dr.), New Mirror Electrometer for High Potentials, 112
Hicks (Dr.), Prof. Wadsworth on the Geology of the Iron, Gold, and Copper Districts of Michigan, 117
Hickson (Dr.), Revision of Genera of *Alcyonaria stolonifera*, 215
Highlands, Geology of the North-west, Sir Archibald Geikie, F.R.S., 292
Highlands, Lunar Rainbow in the, 342
Hilgard (Prof.), on the Custom of Civilised Races of Antiquity to establish themselves in Dry Districts, 287
Hill (Prof. M. G. M.)- Cauchy's Condensation Test for Convergency of Series, 214
Himalayas, Dr. Karl Diener's Geological Expedition in, 133
Himmel und Erde, 88
Hinde (G. J.) Note on a Radiolarian Rock from Fanny Bay, Port Darwin, Australia, 407
Hinrichs (M. G.), on Stas's Determination of the Atomic Weight of Lead, 456
Hirsch (Emil), on the Influence of Temperature upon Circular Ferro-Magnetic Polarisation, 525
Hispar Pass, Mr. Conway's Crossing of the, 327
Histology: die Zelle und die Gewebe, Grundzüge der Allgemeinen Anatomie und Physiologie, Prof. Dr. Oscar Hertwig, 314; Ueber das Verhalten des Pollens und die Befruchtungsvorgänge bei den Gymnospermen, Prof. Eduard Strasburger, 484
History, British, some Geographical Aspects of, H. J. Mackinder, 519
Hobson (Bernard), the Earth's Age, 175, 226
Hodgkins Fund Prizes, the, Prof. S. P. Langley, 611
Hodgkinson (Alex.), Iridescent Colours, 92
Hodgkinson (W. R.), Methoxyamido-1: 3-dimethylbenzene, 165; Note on the Action of Phenylhydrazine on Mono- and Di-carboxylic Acids at Elevated Temperatures, 311; Some Relations between Constitution and Physical Constants in the Case of Benzenoid Amines, 479
Hoff (J. H. Van't), Stéréochimie, 436
Hoff's (Van't), Stereochemistry, Prof. Percy F. Frankland, F.R.S., Prof. F.R. Japp, F.R.S., 510
Hoffert (Dr.), Diffusion of Light, 191
Hofmann (A. W. von), Memorial Celebration for, 14
Hoho (M.), the Use of the Electric Current in Producing High Temperatures, 497; Intense and Rapid Heating Process by Means of the Electric Current, 503; a New Electrical Process permitting the Production of Temperatures Superior to those actually realisable, 525; Dynamo-electric Machinery with Compound Excitation, 599
Holland (W.), Hints on Sugaring for Moths, 131
Holmes, Comet (November 6, 1892), 114, 132, 159, 186, 209, 235, 281, 303, 351, 376, 425, 473; Lewis Boss, Rev. E. M. Searle, Mr. Roberts, 257; M. Schulhof, 257, 451, 498; Dr. F. Cohn, 326; Dr. R. Schorr, 326; W. F. Denning, 365; Prof. E. Barnard, 399; Prof. Keeler, Prof. C. A. Young, 518; Spectrum of, 235
Holt (Ernest W. L.), the Destruction of Immature Fish, 160
Honduras Expedition, Prof. Putnam, 476
Hongkong, Severe Frost at, W. T. Thiselton-Dyer, F.R.S., Charles Forel, 535; W. Doberck, 536
Hoogewerff (M. S.), on some Isoimides of Camphoric Acid, 288
Hooker (Sir J. D., F.R.S.), Locusts at Great Elevations, 582
Hooker (S. C.), the Conversion of Para- into Ortho-quinone Derivatives, 405; Dibromo-β-lapachone, 405
Hopkins (B. J.), Astronomy for Every-day Readers, 389
Hopkinson (Dr. Edward), Electrical Railways, 570
Hopkinson (Dr. John, F.R.S.), the Cost of Electric Supply, 38
Hoppe-Seyler (Herr), Fishes and Water-Oxygenation, 280
Horizontal Pendulum, the, Dr. E. von Rebeur-Paschwitz, 519
Horn Measurements and Weights of the Great Game of the World, being a Record for the Use of Sportsmen and Naturalists, Rowland Ward, 6
Hornby (Mr.), Flexible Sandstone, 167
Hornell (James), a Strange Commensalism; Sponge and Annelid, 78
Hornet's Nest, Remarkable, presented to Madras Museum by Lord Wenlock, 16
Hornsey Local Board, Museum of Sanitary Appliances, 587
Horticulture: Value of Electric Light for Lettuce and other Winter Crops, Prof. L. H. Bailey, 130; Ornithology in Relation to Agriculture and Horticulture, 533; Primer of Horticulture, J. Wright, Walter Thorp, 533; Tasmania the Paradise of Horticulturists, Sir Edward Braddon, 587; Conifers, 619
Hose (Charles), Travels in Borneo, 282
Höttinger Breccie, die Fossile Flora der, R. von Wettstein, 436
Howard (Dr. James L.), Gemeinverständliche Vorträge aus dem Gebeite der Physic, Prof. Dr. Leonhard Sohncke, 362
Howe, the Stranding of H.M.S., 257
Howes (Prof.), Some Abnormal Vertebræ of certain Ranidæ, *Rana catesbiana*, *R. esculenta*, and *R. macrodon*, 502
Howlett (W. F.) the Deterioration of Gum Arabic, 183
Howorth (Sir H. H.), the Glacial Nightmare and the Flood, 61; Difficulties of Pliocene Geology, 150, 270
Hudleston (W. H., F.R.S.), A Catalogue of British Jurassic Gasteropoda, H. Woods, 363
Hudson (W. H.) Idle Days in Patagonia, 483
Huggins (Mr.), Nova Aurigæ, 425
Hull (Prof. Edw., F.R.S.), Geology of Arabia Petræa and Palestine, 166
Human Eye, Seven Images of the, M. Tcherning, 354
Human Physiology, Elements of, E. H. Starling, 146
Humming-birds, The Food of, Dr. Morris Gibbs, 63
Hungary, Earthquake in, 562
Hunter (G. M.), the Origin of Caliche (Nitrate of Soda), 251
Hunter (John), (the Hunterian Lecture), Thomas Bryant, 372
Hurricane at Marseilles, Oct. 1, 1892, Severe, 61
Hurst (Dr. C. Herbert), On a Supposed New Species of Earthworms and on the Nomenclature of Earthworms, 31
Hutchinson (Rev. H. W.), Extinct Monsters, 250
Huxley (Rt. Hon. T. H., F.R.S.), Two Statements, 316
Hybrids, Lion-Tiger, S. M. Harmer, 413
Hybrids, Lion-Tiger and Tiger-Lion, Dr. V. Ball, F.R.S., 390, 607
Hydrazine, Further Studies on, A. E. Tutton, 522
Hydrodynamics: Stability and Instability of Viscous Liquids, A. B. Basset, F.R.S., 94; on a Hydrodynamical Proof of the Equations of Motion of a Perforated Solid with Applications to the Motion of a Fine Frame-work in Circulating Liquids, G. H. Bryan, 500
Hydrogen Line H B in the Spectrum of Nova Aurigæ, Herr Victor Schumann, 425

Hydrostatics: Mechanics and Hydrostatics for Beginners, S. L. Long, 437
Hygiene, an Elementary Text-Book of, H. Rowland Wakefield, 245
Hygrometers, Constructive Errors in some, W. W. Midgley, 623

Iberian Peninsula, The Geography and Social Conditions of the, Prof. Theobald Fischer, 547
Ice Consumed in Paris, the Question of the Purity of, 614
Ice, the Resistance of, M. Forel, 564
Ice Age, a Palæozoic, Dr. W. T. Blanford, F.R.S., 101, 152; Henry F. Blanford, F.R.S., 101
Ice Age, the Great, N. L. W. A. Gravelaar, 200
Ice Ages, Ancient, T. Mellard Reade, 174; J. Lomas, 227
Icebergs and Climatic Variations, Gulf Stream, H. Habenicht, 206
Ice-breaking Steamers, 350
Ice Crystals, C. M. Irvine, 31; B. Woodd Smith, 79
Ice Crystallites, Rev. Dr. A. Irving, 126
Ice-formation in Animal Body, Herr Kochs, 16
Ichthyology: the Fishes of Southern California, C. H. Eigenmann, 61; Fishes and Water Oxygenation, Duncan and Hoppe-Seyler, 280; Swimming Movements of the Ray-fish, M. Marey, 311; the Brain in Mudfishes, Dr. Rudolf Burckhardt, 339; Filtered Sewage Water Favourable to Fish Life, Herr Oesten, 350
Idaho, Botanical Explorations in, D. T. Macdougal, 206
Identity of Energy, the, Prof. Oliver Lodge, F.R.S., 293
Idle Days in Patagonia, W. H. Hudson, Dr. Alf. Russel Wallace, 483
Ilkewitsch (M.), Methods of Examining Milk for Tubercle Bacillus, 254
Illinois Wesleyan University, Bequest of Mr. Lichtenthaler's Natural History Collection to, 613
Illusions, Optical, R. T. Lewis, 31; W. B. Croft, 78
Image, the Photography of an, by Reflection, Frederick J. Smith, 10
Imagination, Measure of the, Francis Galton, F.R.S., 319
Imitation, Tracery, Prof. J. Mark Baldwin, 149
Immature Fish, the Destruction of, Ernest W. L. Holt, 160
Imperial Institute of the United Kingdom, the Colonies, and India, the Year-Book of the, 363
India: Industrial School Opened at Lucknow, 111; Mortality from Wild Beasts in, 157; Relics of Primitive Fashions in, Kedarnath Basu, 301; Government Meteorology in, 324; Troubles of the Quetta Railway Constructors, 325; the Fauna of British India, including Ceylon and Burma, G. F. Hampson, 387; Forestry in India, the Dehra Dun Forest School, Sir E. C. Buck, 614; Indo-China, Hon. G. N. Curzon's Journey in, 617
Indians, North American, Dr. Ten Kate on the Type-Characteristics of the, 374
Indies, West, Observations in the, Prof. A. Agassiz, 608
Induction, Magnetic, in Iron and other Metals, J. A. Ewing, F.R.S., E. Wilson, 460
Induction and Deduction: Edward T. Dixon, 10, 127; E. E. Constance Jones, 78
Industry, the Growth of Electrical, W. H. Preece, F.R.S., 327
Influenza, Epidemic, F. A. Dixey, 244
Inglis (John), Experiments with Engines of s.s. *Iveagh*, 521
Isolation and Temperature, Relationship between, Dr. Berson, 24
Institution of Mechanical Engineers, 19, 300, 353, 617
Institute of Naval Architects, 519; Annual General Meeting, 494
Instruments, Astronomical, Up to Date, Dr. L. Ambronn and Herr Julius Springer, 114
Instruments for the Earthquake Laboratory at the Chicago Exhibition, Prof. John Milne, F.R.S., F. Omori, 356
Instruments, Science, 100
International Committee of Weights and Measures, 21
International Congress of Prehistoric Archæology and Anthropology, 523
International Sanitary Convention Signed, 585
International Zoological Congress at Moscow, 236
International Zoological Record, An, Dr. Herbert H. Field, 606
Invisible, Astronomy of the, Dr. J. Scheiner, 88
Ireland, Arthur Young's Tour in (1776–79), 341; Glacial Drift of the Irish Channel, Prof. Grenville A. J. Cole, 464
Iridescent Colours: Alex. Hodgkinson, 92; Baron C. R. Osten Sacken, 102
Iron: on Iron Alloys, 58; Diamond in Meteoric Iron, C. Friedel, 192; on the American Iron Trade, and its Progress during Sixteen Years, Sir Lowthian Bell, F.R.S., John Parry, 195; on the Carburization of Iron, John Parry, 560; Magnetic Induction in Iron and other Metals, J. A. Ewing, F.R.S., E. Wilson, 460
Irrigation in New South Wales, Artesian Boring and, J. W. Boultbee, 183
Irvine (C. M.), Ice Crystals, 31
Irving (Rev. Dr. A.), Ice Crystallites, 126
Irving (Rev. Dr., F.R.S.), the Sandgate Landslip, 581
Isomerism, a Remarkable Case of Geometrical, A. E. Tutton, 65
Italiana, Bolletino della Società Botanicà, 23
Izarn (M.), Permanent Soap Bubbles formed with a Resinous Soap, 119; Photographic Reproduction of Gratings and Micrometers Engraved on Glass, 479; Photography of Certain Phenomena Furnished by Combinations of Gratings, 503; Photography of Gratings Engraved upon Metal, 623

Jackson (F. G.), Proposed Arctic Expedition by way of Franz Josef Land, 377
Jackson (Dr. M. J.), Printing Mathematics, 227
Jacoby (Prof. Harold), Rutherfurd Measures of Stars about β-Cygni, 77; Parallax of β-Cygni, 399; Parallaxes of μ and θ-Cassiopeiæ, 565
Jaeger (W.), on the Temperature Co-efficient of the Electrical Resistance of Mercury, and on the Mercury Resistances of the Imperial Institution, 286
Jaeger (Herr), Typhoid Fever attributed to Bathing in Polluted Water, 398
Jahrbuch der Astronomie und Geophysik, 566
Jamaica Botanical Department, the, W. Fawcett, 348
Jamieson (Prof.), Elementary Manual on Applied Mechanics, 147
Janet (Pierre), Electric Oscillations, 119; Hysteresis and Dielectric Viscosity of Mica for Rapid Oscillations, 432; Experiments on Electric Oscillations of Medium Frequency, 615
Jannetaz (Paul) on the Electric Figures produced at the Surface of Crystallised Bodies, 408; a New Sclerometer, 564
Japan: the Volcanoes of Japan, John Milne, F.R.S., 178; Japan and the Korean Fishery, 324; Japanese Camphor, 142; Japanese Magic Mirrors, Prof. S. P. Thompson, F.R.S., 381
Japp (Prof. F. R., F.R.S.), Van't Hoff's Stereochemistry, 510; Synthesis of Oxazoles from Benzoin and Nitriles, 430
Johns Hopkins University Palæontological Collections, 471
Johnson (Amy), Sunshine, 9
Johnson (Prof. T.), a New Irish Alga, 167
Johnston-Lavis (Dr. H. J.), a New Seismograph, 257; Stromboli, A. Ricco and G. Mercalli, 453
Joly (Dr. J., F.R.S.), on the Cause of the Bright Colours of Alpine Flowers, 431
Joly (M.), Ruthenium, 451; Metallic Osmium, 497
Jones (Chapman), Qualitative Analysis Tables and the Reactions of certain Organic Substances, E. A. Letts, 361
Jones (E. E. Constance), Induction and Deduction, 78
Jones (Rev. Edward), Relics found in Yorkshire Caves, 112
Jones (Francis) a German Science Reader, 125
Jones (Prof. Geo. William), Logarithmic Tables, 508
Jones (H. L.), a Graft-Hybrid between Red and White Geraniums, 563
Jones (L. M.), the determination of the Thermal Expansion and Specific Volume of certain Paraffins and Paraffin Derivatives, 405
Jones (T.), Descriptive Geometry Models for the Use of Students in Schools and Colleges, 413
Joubin (P.), the Passage of a Wave through a Focus, 143: Relation between Velocity of Light and Size of Molecules of Refracting Liquids, 192; Measurement of Large Differences of Phase in White Light, 528
Joule's (Dr.) Thermometers, Prof. Sydney Young, 317; Prof. Arthur Schuster, F.R.S., 364
Journal of Botany, 23, 261, 596

Journal of the Royal Agricultural Society of England, 285
Juba River, the, Commander F. G. Dundas, 186
Judd (Prof. J. W., F.R.S.), Macculloch's Geological Map of Scotland, 173
Junior Engineering Society, 38
Jupiter, Occultation of Mars and, by the Moon, Prof. Barnard, 41; the Fifth Satellite of, E. Roger, 71; A. A. Common, 208; E. E. Barnard, 377; the Sizes of Jupiter's Satellites, J. J. Landerer, 473; Jupiter and his Satellites, Prof. Pickering, 518
Jurassic Gasteropoda, a Catalogue of British, W. H. Hudleston, F.R.S., and Edward Wilson H. Woods, 363

Kapple (A. W.), Beetles, Butterflies, Moths, and other Insects, 148
Kapteyn (Prof.), Stellar Magnitudes in Relation to the Milky Way, 64; Distribution of Stars in Space, 432
Kangaroo at Westminster Aquarium, the Boxing, 111
Kansas, the Great Spirit Spring Mound, E. H. S. Bailey, 87
Karakoram Range Expedition, Conway's, 19
Karop (E. C.), Messrs. Swift's Aluminium Microscope, 47
Kaufmann (M.) on the Pathogeny of Diabetes, 384; the Pancreas and the Nervous Centres Controlling the Glycemic Function, 479; the Pancreas and the Nerve Centres Regulating the Glycemic Function; Experimental Demonstrations Derived from a Comparison of the Effects of a Removal of the Pancreas with those of Bulbary Section, 528
Kayser (Dr. E.), Text-Book of Comparative Geology, 578
Kedarnath Basu, Relics of Primitive Fashions in India, 301
Keeler (Prof.), Comet Holmes (1892, III), 518; Spectrum of β Lyræ, 616
Keltie (J. Scott), the Partition of Africa, 580
Kelvin (Lord, P.R.S.), Anniversary Address to Royal Society, 106; Prof. Rudolf Virchow, 110; Dr. Nils C. Dunér, 110; Prof. Charles Pritchard, F.R.S., 110; John Newport Langley, F.R.S., 110; the Velocity of Crooke's Cathode Stream, 164; Address at the Prescot Watch Factory, 279
Kempe (A. B., F.R.S.) on the Application of Clifford's Graphs to Ordinary Binary Quantities, 382
Keng (Lim Boon) on the Histology of the Blood of Rabbits which have been Rendered Immune to Anthrax, 502
Kennedy (Prof.), the Screw Propeller, 21
Kettler's Afrikanische Nachrichten, 115
Kew Bulletin, 38
Kimmins (C. W.), the Chemistry of Life and Health, 198
King (Clarence), the Age of the Earth, 285
King (F. H.), the Movements of Ground-water, 206
Kingsmill (Thos. W.), the Geology of the Asiatic Loess, 30; the Channels of Mars, 133
Kipping (F. S.), the Reduction Products of Dimethyldiacetylpentane, 238; Products of Interaction of Zinc Chloride or Sulphuric Acid and Camphor, 239; a New Synthesis of Hydrindone, 311; Sulphonic Derivatives of Camphor, 405; the Action of Phosphoric Anhydride on Fatty Acids, 479; Regularities in the Melting Points of Certain Paraffinoid Compounds of Similar Constitution, 479; Formation of the Ketone 2 : 6 dimethyl-1-ketohexaphane, 551
Kirby (W. Egmont), Beetles, Butterflies, Moths, and other Insects, 148
Kirman (Walter), Isolation of Fluorsulphonic Acid, 87
Kitasato, the Bacteriology of Tetanus, 158
Kitchener (F. E.), Naked-Eye Botany, 198
Klein (Dr. E., F.R.S.), Aminol, a True Disinfectant, 149, 246
Knight (S. R.), Algebra for Beginners, 28
Knott (Prof. C. G.), on Recent Innovations in Vector Theory, 287, 590
Kochs (Herr), Ice Formation in Animal Body, 16
Koksharoff (Nikolai Ivanovitch), Death of, 278
Korean Fishery, Japan and the, 324
Kossel (Prof.), Further Researches on Nucleinic Acid, 72
Kosutany, Investigations on Wine-yeast, 208
Kowalevsky, the Mantle-cells of Ascidians, 62
Kreichgauer (D.) on the Temperature Coefficient of the Electrical Resistance of Mercury and on the Mercury Resistances of the Imperial Institution, 286
Kreutz (Prof.), Comet Books (November 20, 1892), 159
Krotov (P.) on Layers of Stone Implements in the District of Taransk, 524
Krümmel (Prof.), Map of Salinity of Surface Water of North Pacific, 590
Kundt (Prof.), Experiments on the Influence of Temperature on Electro-magnetic Rotation of Light in Iron, Cobalt and Nickel, 503; Researches into the Study of Hall's Phenomenon, 624

Laboratories, Marine, in the United States, Prof. J. P. Campbell, 66
Laboratories, the South Kensington, and Railway, 494
Lacaze-Duthiers (M. de), on the Attempt at Oyster Culture in the Roscoff Laboratory, 456
Lagrange (M.), The use of the Electric Current in Producing High Temperatures, 497; Intense and Rapid Heating Process by Means of the Electric Current, 503; a New Electrical Process Permitting the Production of Temperatures Superior to those Actually Realisable, 525
Lake Bangweolo, Joseph Thomson's Journey to, 115
Lake of Geneva, Prof. F. A. Forel, Prof. T. G. Bonney, F.R.S., 5
Lake, Great Salt, Utah, Salinity of, 302
Lake Basins in France, some, Prof. T. G. Bonney, F.R.S., 341, 414
Lake Basins, Origin of, the Duke of Argyll, F.R.S., 485; J. C. Hawkshaw, 558
Lakeland, a Vertebrate Fauna of, Rev. A. Macpherson, 457
Lamp, Safety, a New Portable Miner's, Prof. Frank Clowes, 596
Landslip at Sandgate, 449; Prof. J. F. Blake, 467; Rev. Dr. Irving, F.R.S., 581
Landerer (J. J.), the Sizes of Jupiter's Satellites, 473
Landor (A. H. Savage), Yezo and the Ainu, 330
Langley (John Newport, F.R.S.), Lord Kelvin, 110
Langley (Prof. S. P.), Energy and Vision, 252; the Hodgkins Fund Prizes, 611
Langtoft, Thunderstorm, Cloudburst and Flood at, July 3, 1892, J. Lovel, 118
Lankester (Prof. E. Ray, F.R.S.), Blind Animals in Caves, 389, 486
Lapparent (Prof. A. de), Sir Archibald Geikie, 217
Larmor (Dr. J., F.R.S.), the Dioptrics of Gratings, 526
Latin Literature, the Were-Wolf in, Kirby W. Smith, 423
Lauder (A.), a New base from Corydalis Cava, 479
Laurie (A. P.), the Food of Plants, 556
Laurie (Malcolm), on the Anatomy of the *Eurypteridæ*, 527
Lausedat (M. A.), on the Progress of the Art of Surveying with the Aid of Photography in Europe and America, 384
Lava Lakes, Lunar Volcanoes and, S. E. Peal, 486
Lavigerie (Cardinal), the Death of, 210
Lawes (Sir John), Rothamsted Agricultural Experiments, Proposed Commemoration of the Jubilee of, 448
Laws and Properties of Matter, R. T. Glazebrook, F.R.S., 580
Laws and Wonders of Nature, a Description of the, Richard A. Gregory, 74
Le Chatelier (A.), the Mixed Character of the Population of Morocco, 61
Lea (A. Sheridan, F.R.S.), the Chemical Basis of the Animal Body, 340
Lea (M. C.), Notes on Silver Chlorides, 189
Leaper (Clement J.), Outlines of Organic Chemistry, 124
Lebour (G. A.), Arborescent Frost Patterns, 213
Leeds Naturalist Club, 112
Lehmann (Heinrich), Magnetisation of a Radially Slit Iron Ring, 525
Léman, Le: Monographie Limnologique, Prof. F. A. Forel, Prof. T. G. Bonney, F.R.S., 5
Lenard (Herr), Penetration of Thin Metallic Plates by Cathode Rays Causing Phosphorescence, 518
Lenard (Dr. P.), Experiments on Phosphorescence-Producing Kathode Rays of a Geissler Tube, 564
Lendenfeld (R. von), Australian Travels, 274
Lens, the New Telephotographic, T. R. Dallmeyer, 161
Lepidoptera: Catalogue of Eastern and Australian Lepidoptera Heterocera in the Collection of the Oxford University Museum, Col. C. Swinhoe, 53; Dr. F. B. White's Collection Presented to Museum of Perthshire Society of Natural Science, 206; on the Mimetic Forms of Certain Butterflies of the genus Hypolimnas, Col. C. Swinhoe, 429
Lépine (M. R.), on the Pepto-Saccharifiant Action of the Blood and the Organs, 335

Leroy (M. C. J. A.), on Spherical Aberration of the Human Eye; Measurement of Senilism of the Crystalline, 528
Lescœur (M. H.), on the Purification of Arsenical Zinc, 288
Letourneau (Charles), Property: Its Origin and Development, 123
Letts (E. A.), Qualitative Analysis Tables and the Reactions of Certain Organic Substances, Chapman Jones, 361
Leverson (Major), the Frontier Delimitation between British South Africa Company's Territories and Portuguese Possessions, 327
Lewis (R. T.), Optical Illusions, 31
Lewy-Dorn (Dr.), the Formation of Sweat, 600
Libraries, Public, Suggestions for Memorial Presentations of Books to, 61
Lichtenthaler (G. W.), Bequest of Natural History Collection to Illinois Wesleyan University by, 613
Lick Observatory, Miss Milicent W. Shinn, 209
Life in France, Statistics of Average, M. Turquan, 255
Life and Health, the Chemistry of, C. W. Kimmins, 198
Light of Planets, the, 64; John Garstang, 77
Light, Diffusion of, Dr. W. E. Sumpner, 190; A. P. Trotter, 191; Dr. Hoffert, 191; Mr. Blakesley, 191; Mr. Addenbrooke, 191; Dr. C. V. Burton, 191
Light, Method of Producing Intense Monochromatic, Dr. Du Bois, 255
Light, the Action upon Certain Micro-organisms of, Herr Buchner, 303
Light, Experiments on the Action of, on *Bacillus anthracis*, Prof. Marshall Ward, F.R.S., 331, 597
Light and Colour, Sensitiveness of the Eye to, Captain W. de W. Abney, F.R.S., 538
Light, Researches at the Berlin Imperial Physico-Technical Institute on the Siemens Platinum Foil Units as a Standard for the Intensity of a Source of, 615
Lightning Protection, W. H. Preece, F.R.S., on, Prof. Oliver Lodge, F.R.S., 536
Lightning, Notes on Two Photographs of, taken at Sydney Observatory, December 7, 1892, H. C. Russell, F.R.S., 623
Limelight, Oxygen for, T. C. Hepworth, 176
Limit, Roche's, 509
Limpach (L.), Methoxyamido-1 : 3-dimethylbenzene, 165 : Some Relations between Constitution and Physical Constants in the Case of Benzenoid Amines, 479
Linden (H.), Waves as a Motive Power, 438
Lindenschmidt (Ludwig), Death and Obituary Notice of, 449
Line of Sight, Motion in the, M. H. Deslandres, 88
Lines of Structure in the Winnebago County Meteorites and in other Meteorites, Prof. H. A. Newton, 370
Linnean Society, 71, 118, 190, 215, 334, 384, 430, 455, 527, 599
Linnean Society, Address of Congratulation to Rev. Leonard Blomefield, 85
Lion-Tiger and Tiger-Lion Hybrids, Dr. V. Ball, F.R.S., 390, 607
Lion-Tiger Hybrids, L. F. Harmer, 413
Lippmann (G.), Coloured Photographs of the Spectrum, 23
Lippmann's Coloured Photographs, Conditions of Production of, G. Meslin, 157
Liquid, Contrivance for Determining Refractive Index of a, H. Ruoss, 544
Liquids, the Laws of Compressibility of, E. H. Amagat, 48
Liquids, the Thermal Conductivities of, R. Wachsmuth, 350
Liquids, Simple Instrument for Measuring Densities of, R. Hanal, 471
Liquids, on the Common Cause of Surface Tension and Evaporation of, G. Van der Mensbrugghe, 621
Liverpool Derby Museum, Death of Mr. T. J. Moore, late Curator of, 37
Liverpool Geographical Society, 159, 426
Liverpool, University College, Opening of New Victoria Buildings, 155
Living Plants, Gases in, J. G. Arthur, 427
Lizard-Superstition of Shuswap Indians, British Columbia, Dr. Geo. Dawson, F.R.S., 184
Lobatcheffsky, Proposed Celebration of Centenary of Birth of, 469
Lockyer (J. Norman, F.R.S.): the Origin of the Year, 32, 228; on the Photographic Spectra of some of the Brighter Stars, 261; the Sacred Nile, 464
Lockyer (W. J.), the New Star in the Constellation of Auriga, 137
Lockyer (William J. S.), La Planète Mars et ses Conditions d'Habitabilité, Camille Flammarion, 553
Locusts at Great Elevations, Sir J. D. Hooker, F.R.S., 581
Lodge (Prof. A.), Williams on the Dimensions of Physical Quantities, 116
Lodge (Prof. Oliver, F.R.S.), Pioneers of Science, 268; the Identity of Energy, 293; Observations of Atmospheric Electricity in America, 392; W. H. Preece, F.R.S., on Lightning Protection, 536; on the Differential Equation of Electric Flow, 574; Soot Figures on Ceilings, 608
Loess, the Geology of the Asiatic, Thos. W. Kingsmill, Prof. G. H. Darwin, F.R.S., 30
Loewy (Dr. Ad.), Influence on Respiration of Upper Tracts leading from Cerebrum to Respiratory Centre, 144
Lœwy (M.), Photographic Chart of the Heavens, 589
Logarithmic Tables, Prof. Geo. William Jones, 508
Lomas (J.), Ancient Ice Ages, 227
Lommel (E.), a Simple Explanation of the Hall Effect, 254; Equipotential Lines due to Current flowing through Conducting Sheet fixed photographically, 544
London, Stanford's Map of County of, 40; Appointment of W. Flinders Petrie to Chair of Egyptology at University College, 111; the proposed University for London, 200; Technical Education in, Report of the London County Council Committee, 300; London County Council and Technical Education, 348; City and Guilds of London Institute, improvements in Technological Examinations, 612
Loney (S. L.), Mechanics and Hydrostatics for Beginners, 437
Longevity of the Perigal Family, Dr. C. T. Williams, 585
Lorentz (M.), Influence of the Motion of the Earth on the Propagation of Light in doubly refracting Media, 504
Lorenz (Prof.), the Volatility of Manganese, 375
Lossen (Prof. K. A.), Death of, 421
Loubat Prizes, the, Columbia College, New York, 496
Love (A. E. H.), on the Vibrations of an Elastic Circular Ring, 383; a Treatise on the Mathematical Theory of Elasticity, Prof. A. G. Greenhill, F.R.S., 529
Love (Mr.), on the Stability of a thin Rod loaded vertically, 526
Lovel (J.), Thunderstorm, Cloudburst, and Flood at Langtoft, July 3, 1892, 118
Lovibond (J. W.), on the Measurement of Direct Light by Means of the Tintometer, 501
Low (Dr.), Higher Education in the United States, 325
Lowe (E. J., F.R.S.), Earthquake Shocks, 247, 270
Löwenburg (Dr.), Death of, 14
Lubbock (Hon. Sir John, F.R.S.), the Beauties of Nature and the Wonders of the World we live in, 28; a Contribution to our Knowledge of Seedlings, Dr. Maxwell T. Masters, F.R.S., 243
Lucas (A. H. S.), an Introduction to the Study of Botany, with a Special Chapter on some Australian Natural Orders, 125
Lucknow, Industrial School opened at, 111
Lugard (Capt. F. D.), Uganda, 45
Lumière, Poincaré's Théorie Mathématique de la, A. B. Basset, F.R.S., 386
Lumière (MM. Auguste and Louis), Photographic Properties of Cerium Salts, 503
Luminous Earthworms, Rev. Hilderic Friend, 462
Lummer (Dr.), Use of Half-shade Polarimeters, 312
Lunar Craters, Mr. H. Maw, 31
Lunar Enlargements, Weenek's, 473
Lunar Rainbow in the Highlands, 342
Lunar Surface, the, 352
Lunar Volcanoes, Formation of, T. B. Hannay, 7
Lunar "Volcanoes" and Lava Lakes, S. E. Peal, 486
Lungo (Dr. Carlo del), a Highly Sensitive Mercury Barometer, 586
Lupton (Sydney), Dendritic Forms, 13
Lupton (T. N.), the Florida Phosphate Beds, 325
Lydekker (R.), on a Sauropodous Dinosaurian Vertebra from the Wealden of Hastings, 286; on the Presence of a Distinct Coracoidal Element in Adult Sloths, 431
Lynn (Mr.), Remarkable Comets, 376
Lyons (Capt. H. G.), the Stars and the Nile, 101
Lyræ, Spectrum of β, Prof. Keeler, 616

McAdie (A.), the Electrification of the Lower Air during Auroral Displays, 454
Macalister (Prof.), on Egyptian Mummies, 623
McAulay (Alex.), Quaternions, 151
MacBride (E. W.), on the Development of the Genital Organs, Ovoid Gland, Axial and Aboral Sinuses in *Amphiura Squamata*, together with some Remarks on Ludwig's Hæmal System in this Ophiurid, 261
Macchiati (Signor), the Cultivation of Diatoms, 23
McClintock (Dr. Emory), on the Non-Euclidian Geometry, 286
Macculloch's Geological Map of Scotland, Prof. J. W. Judd, F.R.S., 173
Macdonald (A. C.), the Dairy Industry in Cape Colony, 471
Macdougal (D. T.), Botanical Explorations in Idaho, 206
Macfarlane (A.), Principles of the Algebra of Vectors, 3
Macfarlane (Dr.), Dionæa, 423
Mackenzie (D. F.), the Timber of Exotic Conifers, 619
Mackinder (H. J.), the Relation of Geography to History, 304; some Geographical Aspects of British History, 519; the Steppe Belt traversing Asia from East to West, 353: the Chief Lines of Communication between Asia and Europe, 400
McLeod (Prof. Herbert, F.R.S.), the Author of the word "Eudiometer," 536
MacMahon (Major P. A., F.R.S.), Memoir on the Theory of the Compositions of Numbers, 310; the Group of Thirty Cubes composed by six differently Coloured Squares, 406
McMillan (Conway), the Probable Physiognomy of the Cretaceous Plant Population, 587
Macpherson (Rev. A.), a Vertebrate Fauna of Lakeland, including Cumberland and Westmoreland, with Lancashire North of the Sands, 457
McPherson (Wm.), Racial Dwarfs in the Pyrenees, 294
Madder-staining of Dentine, on the, Dr. W. G. Aitchison Robertson, 287
Madras Meridian Circle Observations, 186
Madras Museum, remarkable Hornets' Nest presented by Lord Wenlock to, 16
Madsen (Herr Victor), Scandinavian Boulders at Cromer, 287
Magic Mirrors, the late Prof. Tennant on, Prof. Silvanus P. Thompson, F.R.S., 79
Magnetism: Displacements of Magnet on Mercury under Action of Electric Current, C. Decharme, 48; Dilatation of Iron on Magnetisation, M. Berget, 71; Magnetic Properties of Bodies at Different Temperatures, P. Curie, 96; Magnetic Observations, Washington, 209; Absolute Value of the Magnetic Elements on January 1, 1893, 288; Magnetism and Electricity, R. W. Stewart, 315; Magnetic Permeabilities of a Series of Diamagnetic Bodies, 336; Sun-Spots and Magnetic Perturbations in 1892, M. Ricco, 352; Magnetical and Meteorological Observations made at the Government Observatory, Bombay, 1890, with an Appendix, 379; a Magnetic Screen, Frederick J. Smith, 439; Magnetic Induction in Iron and other Metals, J. A. Ewing, F.R.S., E. Wilson, 460; Magnetic Observatory, Potsdam, Improvement in Registrations of Needle's Variations, Herr Eschenhager, 544; Magnetische Beobachtungen auf der Nordsee angestellt in den Jahren 1884 bis 1886, 1890 und 1891, A. Schück, 555; William Gilbert of Colchester, Physician of London, on the Loadstone and Magnetic Bodies, and on the Great Magnet the Earth. A New Physiology, demonstrated with many Arguments and Experiments, P. Fleury Mottelay, 556
Mair-Rumley (J. G.), Experiments on the Value of the Steam Jacket, 19
Majert (W.), Piperazine, 430
Malapterurus, on the Origin of the Electric Nerves in the Torpedo, Gymnotus, Mormyrus, and, Gustav Fritsch, 271
Mallet (Maurice), the Longest Balloon Ascent on Record, 182
Malta Pleistocene, Discovery of Ursus Arctos in, J. H. Cooke, 62
Malta, Depredations among the recently discovered Phœnician Tombs at, 396
Maltézos (C.), Lenticular Liquid Microglobules and their Conditions of Equilibrium, 71; Conditions of Equilibrium and Formation of Microglobules, 96
Mammalia, the Sense Organs of the Skin, Feathers, and Hairs in, Herr Maurer, 87
Man (E. H.), on Nicobar Pottery, 455
Man and the Glacial Period, Dr. G. Frederick Wright, 148
Man's Place in Nature, Evolution and, Henry Calderwood, 385
Manchester Geographical Society, 159
Manchester Municipal Technical School, Sir Henry E. Roscoe, F.R.S., 201
Manganese, the Volatility of, Prof. Lorenz and Dr. Hensler 375
Mance (Sir Henry), the Teredo and Electric Cables, 450
Manners and Monuments of Prehistoric Peoples, Marquis de Nadaillac, 316
Manurial Trials, Report on, Dr. William Somerville, 556
Maoris, some Reminiscences of, Rev. W. Colenso, F.R.S., 41
Map of County of London, Stanford's, 40
Map of Scotland, Macculloch's Geological, Prof. J. W. Judd, F.R.S., 173
Map of the World on a Uniform Scale, Prof. Penck's Scheme for a, 426
Map-colouring, the Objects of, 566
March (Dr. H. C.). Mythographic Origin of Polynesian Ornament, 239
Marchal (Émile), on a Process of Sterilisation of Albumin Solutions at 100° C., 310
Marckwald (Dr.), new Method of Preparing Glycol Aldehyde, 17
Marey (M.), Swimming Movements of the Ray-fish, 311
Marilaun (Anton Kerner Von), Pflanzenleben, 605
Marine Biology: the Destruction of Immature Fish, Ernest W. L. Holt, 160; Dredging Work at Plymouth, 375; the Rising and Sinking Process in the Radiolaria, Herr Verworn, 397; the Week's Work of the Plymouth Station, 398, 424, 451, 472, 497, 518, 546, 565, 589, 616; Port Erin (Isle of Man) Station, 515
Marine Fauna, Proposed Handbook to the British, Prof. W. A. Herdman, F.R.S., 231, 293; Prof. D'Arcy W. Thompson, 269; W. Garstang, 293
Marine Laboratories in the United States, Prof. J. P. Campbell, 66
Marine Shells of South Africa, G. B. Sowerby, 27
Marine Zoological Station at Trieste, the, 450
Mars, the Planet, Camille Flammarion, William J. S. Lockyer, 553; the Canals of Mars, 64; the Channels of, T. W. Kingsmill, 133; the markings on, Mr. Schaeberle, 209; the Recent Opposition of, Prof. W. H. Pickering, 235; Occultation of Mars and Jupiter by the Moon, Prof. Barnard, 41
Marseilles, Oct. 1, 1892, Severe Hurricane at, 61
Marsh (Prof. O. C.), Restoration of *Anchisaurus colurus*, 349
Marshall (Rev. T. A.), a New Species of Belytidæ from New Zealand, 17
Marshall (W.), the Resolution of Methoxysuccinic Acid into its Optically Active Components, 311
Martin (Ern.), Physiological Study of Opium Smoke, 168
Martin (Horace), Castorologia; or, the History and Traditions of the Canadian Beaver, 224
Marvin (Prof. C. F.), Sunshine Recorders, 261
Maryland from the Meteorological Point of View, the Surface Configuration of, Prof. W. B. Clark, 585
Mascart (M.) on the Diurnal Variations of Gravitation, 360
Mason (James), Field Experiments on the Fixation of Free Nitrogen, 285
Massee (George), British Fungus-Flora, 26
Masters (Dr. Maxwell T., F.R.S.), a Contribution to our Knowledge of Seedlings, Sir John Lubbock, F.R.S., 243; List of Conifers and Texads, 619
Mathematics: Principles of the Algebra of Vectors, A. Macfarlane, 3; Vector Analysis, Prof. P. G. Tait, 225; the Algebra of Co-planar Vectors and Trigonometry, R. Baldwin Hayward, F.R.S., 266; Prof. C. G. Knott on Recent Innovations in Vector Theory, 287, 590; Quaternions and the Algebra of Vectors, Prof. J. Willard Gibbs, 463; Vectors *versus* Quaternions, Oliver Heaviside, F.R.S., 533; Printing Mathematics, W. Cassie, 8; Dr. M. J. Jackson, 227; Bulletin of the New York Mathematical Society, 23, 428; Mathematical Society, 71, 214, 382, 406, 526; Certain General Limitations affecting Hyper-Magic Squares, S. Roberts, F.R.S., 71; Note on Secondary Tucker Circles, 71; Quaternions, Alex. McAulay, 151; Cauchy's Condensation Test for Convergency of Series, Prof. M. J. M. Hill, 214; on the Non-Euclidian Geometry, Dr. Emory McClintock, 286; Theory of Numbers, G. B. Mathews, 289;

Memoir on the Theory of the Compositions of Numbers, Major P. A. MacMahon, F.R.S., 310; on the Use of Supplementary Curves in Isogonal Transformation, R. A. Harris, 380; on the Application of Clifford's Graphs to Ordinary Binary Quantics, A. B. Kempe, F.R.S., 382; on the Vibrations of an Elastic Circular Ring, A. E. H. Love, 383; Poincaré's Théorie Mathématique de la Lumière, A. B. Basset, F.R.S., 386; the Group of Thirty Cubes composed by Six Differently-coloured Squares, Major MacMahon, F.R.S., 406; the Harmonics of a Ring, W. D. Niven, F.R.S., 406; on the Potential Equation, Dr. Haentzsch, 480; on the Applicability of Lagrange's Equations of Motion to a General Class of Problems, with Special Reference to the Motion of a Perforated Solid in a Liquid, Dr. C. V. Burton, 500; on a Hydrodynamical Proof of the Equations of Motion of a perforated Solid with Applications to the Motion of a Fine Framework in Circulating Liquids, G. H. Bryan, 500; the Discovery of the Potential, Ottavio Zanotti Bianco, Dr. E. J. Routh, F.R.S., 510; Motion of a Solid Body in a Viscous Liquid, A. B. Bassett, F.R.S., 512; on the Stability of a Thin Rod Loaded Vertically, Mr. Love, 526; on Complex Primes formed with the Fifth Roots of Unity, Prof. Lloyd Tanner, 526; a New Algebra, T. B. Sprague, 526; a Treatise on the Mathematical Theory of Elasticity, A. E. H. Love, Prof. A. G. Greenhill, F.R.S., 529; Graphical Solutions of Problems in Navigation, 547; American Journal of Mathematics, 620
Mathews (G. B.), Theory of Numbers, 289
Mathey (Edward, F.S.A.), Further Researches in Connection with the Metallurgy of Bismuth, 358
Matriculation Chemistry, Temple Orme, 99
Matthews (William), the Southampton Water-softening Plant, 353
Maurer (Herr), the sense Organs of the Skin, Feathers, and Hairs in Mammalia, 87
Maw (M. H.), Lunar Crater, 31
Mawley (E.), Report on the Phenological Observations for 1892, 430
Maxwell (C. F.), Remarkable Meteor in Texas, 279
Maycock (W. Perren), Electric Lighting and Power Distribution, 269
Mayer (Alfred Goldsborough), the Radiation and Absorption of Heat by Leaves, 596
Measure of the Imagination, Francis Galton, F.R.S., 319
Measurement of Distances of Binary Stars, C. E. Stromeyer, 199
Measures of Stars about β Cygni, Rutherfurd, Prof. Harold Jacoby, 77
Mechanics: Elementary Manual on Applied Mechanics, Prof. Jamieson, 147; Modern Mechanism, 241; a Correction, 281; Elementary Mechanics of Solids and Fluids, A. L. Selby, 315; Institution of Mechanical Engineers, 19, 300, 353, 617; Mechanics and Hydrostatics for Beginners, S. L. Loney, 437; the Value of the Mechanical Equivalent of Heat, E. H. Griffiths, 537
Mediæval Lore; an Epitome of the Science, Geography, Animal and Plant Folk-Lore and Myth of the Middle Ages, 388
Medical Education at Oxford, Lord Salisbury, 449
Medical Microscopy, Frank J. Wethered, Dr. A. H. Tubby, 51
Medical Science, Bibliographia Medica Italiana, Prof. P. Giacosa, 606
Medical Society, Oxford, Inaugural Address by Sir James Paget, 60
Medical Student, Biology and the, H. J. Campbell, 530
Medicine, Experimental, 593
Mee (Arthur), Observational Astronomy, 437
Melbourne Observatory, 498
Meldola (Prof. R., F.R.S.), Arborescent Frost Patterns, 125
Mellish (H.), Rainfall of Nottinghamshire, 1861–90, 286
Mem. Soc. degli Spettroscopisti Ital., 429
Mendenhall (T. C.): Uses of Planes and Knife Edges in Pendulums for Gravity Measurements, 380; Observations of Atmospheric Electricity in America, Prof. Oliver J. Lodge, F.R.S., 392
Mensbrugghe (G. Van der), on the Common Cause of Surface Tension and Evaporation of Liquids, 428, 621
Mentone, Prehistoric Interments of the Bahi Rossi Caves near, A. G. Evans, 239
Mercelli (G.), Stromboli, 453
Mercurial Air-Pumps, Automatic, Dr. August Raps, 369
Mercury, Berlin Method of Cleaning, 16
Meridian Circle Observations, Madras, 186
Meslin (G.), Conditions of Production of Lippmann's Coloured Photographs, 157; on Semicircular Interference Fringes, 384
Mesnard (E.), Mode of Production of Perfume in Flowers, 120; Researches on the Localisation of the Fatty Oils in the Germination of Seeds, 312
Metallurgy: Note on the Colours of the Alkali Metals, G. S. Newth, 55; Wm. L. Dudley, 175; the Copper Resources of the United States, James Douglas, 132; the Use of Tungsten in Improving Hardness of Steel, 351; Further Researches in Connection with the Metallurgy of Bismuth, Edward Mathey, F.S.A., 358; on a New Soldering Process for Aluminium and various other Metals, M. J. Novel, 384; the Value of Annealing Steel, E. G. Carey, 397; Volumetric Method for Determining Amount of Chromium in Steel, G. Georges, 397; Ready Preparation of Large Quantities of the more Refractory Metals by Means of the Electric Furnace, M. Moissan, 424; Magnetic Induction in Iron and other Metals, J. A. Ewing, F.R.S., E. Wilson, 460; the Alloys Research Committee, Second Report, Prof. W. C. Roberts-Austen, F.R.S., 617; the Action of Bismuth on Copper, Prof. W. C. Roberts-Austen, F.R.S., 618
Metazoan Development, on a Supposed Law of, J. Beard, 79; R. Assheton, 176
Meteorology: The Weather Week by Week, 15, 38, 60, 85, 111, 130, 155, 183, 206, 233, 253, 278, 300, 323, 348, 373, 395, 422, 449, 470, 496, 516, 543, 563, 585, 613; Meteorological Council, Summary of Rainfall and Mean Temperature of British Islands for September Quarter 1866–92, 15; Berlin Meteorological Society, 24, 287, 336, 552; Relationship Between Insolation and Temperature, Dr. Berson, 24; a Remarkable Rainfall, Alfred O. Walker, 31; Indications of a Rainy Period in Southern Peru, A. E. Douglass, 38; American Meteorological Journal, 46, 261, 574; Meteorological Balloon Ascent at Berlin, October 24, 1891, A. L. Rotch, 46; Severe Hurricane at Marseilles, October 1, 1892, 61; the Inspection of Canadian Meteorological Stations, Charles Carpmael, 61; Pilot Chart of North Atlantic for November, 86; Pilot Chart of the North Atlantic for February, 1893, 395; the Afterglow, Sereno E. Bishop, 102; Weather Forecasting for British Islands, Captain H. Toynbee, 111; Royal Meteorological Society, 118, 333, 430, 502; Thunderstorm, Cloudburst, and Flood at Langtoft, July 3, 1892, J. Lovel, 118; Measurement of Maximum Wind Pressure, W. H. Dines, 118; Curious Drift of a "Current Bottle," H. C. Russell, 131; Wind Measurement, H. W. Dines, 143; the Height and Spectrum of Auroras, T. W. Backhouse, 151; the Climate of the Canary Islands, 156; Cloud Observations at Blue Hill (Mass.) Observatory, H. H. Clayton, 183; Brilliant Afterglow, December 15 and 17, 1892, 183; the Movements of Ground Water, F. H. King, 206; Gulf Stream Icebergs and Climatic Variations, H. Habenicht, 206; Proposed International Conference of Meteorologists, 233; Eiffel Tower Experiments on Decrease of Air-temperature with Elevation, Alfred Angot, 240; the Weather of Summer, 245, 270; Super-abundant Rain, Sir H. Collett, 247; Atmospheric Electricity, Earth Currents, and Terrestrial Magnetism, Prof. C. Abbe, 261; Notes on the Use of Automatic Rain Gauges, J. E. Codman, 261; Sunshine Recorders, Prof. C. F. Marvin, 261; Shower of Pond Mussels at Paderborn, 278; Experiments on the Use of Oil in Calming Waves, Rear Admiral Cavalier de Cuverville, 278; Moving Anti-Cyclones in the Southern Hemisphere, H. C. Russell, F.R.S., 286; the Tracks of Ocean Wind Systems in Transit over Australasia, Capt. M. W. C. Hepworth, 286; Rainfall of Nottinghamshire, 1861-90, H. Mellish, 286; Prof. Assmann's Detailed Description of the Meteorographs set up in the "Urania-pillars," 287; Absolute Value of the Magnetic Elements on January 1, 1893, 288; the Evaporation from a Snow Surface, P. A. Müller, 301; Map showing Lines of Equal Magnetic Declination for January 1, 1893, in England and Wales, W. Ellis, 323; Government Meteorology in India, 323; Dr. C. T. Williams on the High Altitudes of Colorado and their Climates, 333; on the Diurnal Variations of Gravitation, M. Mascart, 360; Death and Obituary Notice of George Mathews Whipple,

372 ; D. A. van Bastelaer's Observations on Ozone, 373 ; Magnetical and Meteorological Observations made at the Government Observatory, Bombay, 1890, with an Appendix, 379 ; Colonial Meteorology, C. J. Symons, F.R.S., 390 ; Observations of Atmospheric Electricity in America, T. C. Mendenhall, Prof. Oliver J. Lodge, F.R.S., 392 ; Dew, Herr Wollny, 398 ; Report of the Meteorological Council for Year ending March 31, 1892, 422 ; Summary of Weekly Weather Report, 1892, 422 ; on the Particles in Fogs and Clouds, John Aitken, 431 ; on the Hygrometry of the Atmosphere at Ben Nevis, A. J. Herbertson, 431 ; Report on the Phenological Observations for 1892, E. Mawley, 430 ; Relation between the Duration of Sunshine, the Amount of Cloud, and the Height of the Barometer, W. Ellis, 431 ; Winter Temperatures on Mountain Summits, W. Piffe Brown, 431 ; High Atmospheric Pressures observed at Irkutsk from January 12 to 16, 1893, Alexis de Tillo, 432 ; Stonyhurst College Observatory, 450 ; Hot Winds in Texas, May 29 and 30, 1892, J. M. Cline, 454 ; the Electrification of the Lower Air during Auroral Displays, A. McAdie, 454 ; Scottish Meteorological Society, 469 ; the High Barometer Readings for January, 470 ; Observation made at Fotsdam Meteorological Institute on the Recent Coldest Day in January, Prof. Sprung, 480 ; a Short Cycle in Weather, James P. Hall, 499 ; on some Meteorological Problems, Shelford Bidwell, F.R.S., 502 ; on the True Theory of Waterspouts and Tornadoes, with special reference to that of Lawrence, Massachusetts, M. H. Faye, 503 ; Remarkable Cold Wave over China in January, 1893, S. B. J. Skertchly, 516 ; Severe Frost at Hongkong, W. T. Thiselton-Dyer, F.R.S., 535 ; Charles Ford, 535 ; W. Doberck, 536 ; Practical Meteorology in Spain, 543 ; Synoptic Daily Weather Charts of North Atlantic Ocean, 543 ; the Thermal Exchanges of the Atmosphere, Prof. von Bezold, 552 ; Hail Storms, H. C. Russell, 573 ; Exploration of the Free Air, Prof. M. W. Harrington, 574 ; the General Winds of the Atlantic Ocean, Prof. W. M. Davis, 574 ; Fossil Floras and Climate, Sir William Dawson, F.R.S., 556 ; J. Starkie Gardner, 582 ; the Afterglows and Bishop's Ring, T. W. Backhouse, 582 ; Complimentary Dinner to Mr. Henry Perigal, 585 ; the Surface Configuration of Maryland, Prof. W. B. Clark, 585 ; a Highly Sensitive Mercury Barometer, Dr. Carlo del Lungo, 586 ; Exploration of the Higher Atmosphere, Gustave Hermite, 600; New Methods of Disseminating Weather Forecasts in New England, 613 ; Harmonic Analysis of Hourly Observations of Air Temperature and Pressure at British Observatories, Lieut.-General R. Strachey, F.R.S., 621 ; the Direction of the Wind over the British Isles 1876–80, F. C. Bayard, 623 ; Notes on Two Photographs of Lightning taken at Sydney Observatory, December 7, 1892, H. C. Russell, F.R.S., 623 ; Constructive Errors in some Hygrometers, W. W. Midgley, 623

Meteors : Prof. C. A. Young, 150 ; Great Meteor in Alabama, 86 ; December Meteors (Geminids), W. F. Denning, 226 ; a Brilliant Meteor, W. Pollard, 247 ; Meteor Shower of November 23, 1892, 257 ; Remarkable Meteor in Texas, C. F. Maxwell, 279 ; a Meteor, W. L. Distant, 316 ; a Brilliant Meteor, Dr. Jas. Rorie, 495 ; Meteor of March 18, 1893, G. P. Bailey, 516 ; Meteor Showers, 590

Meteoric Iron, Diamond in, C. Friedel, 192

Meteoric Iron of Cañon Diablo, on the, C. Friedel, 408

Meteoric Stone Found at Makariwa, near Invercargill, New Zealand, on a, G.F.H. Ulrich, 381

Meteorites : a Large Meteorite from Western Australia, James R. Gregory, 90 ; Lines of Structure in the Winnebago County Meteorites and in other Meteorites, Prof. H. A. Newton, 370 ; Study of the Cañon Diablo Meteorite, Henri Moissan, 408 ; Observation on the Conditions which appear to have obtained during the Formation of Meteorites, M. Daubrée, 432 ; Mineralogical and Lithological Examination of the Meteorite of Kiowa county, Kansas, M. Stanislas Meunier, 456 ; Great Meteorite from Western Australia, 469 ; Fall of a Meteorite, 565

Meunier (M. Stanislas), Mineralogical and Lithological Examination of the Meteorite of Kiowa county, Kansas, 456

Meyer (Prof. A. B.), the Cause of Sexual Differences of Colour in Eclectus, 486

Mice, Field, in Thessaly and Scotland, the Plague of, 396

Michael (A. D.), a New Species (and genus) of *Acarus* found in Cornwall, 502

Michel (L.), Artificial Production of Rutile, 168

Michigan, Geology of the Iron, Gold, and Copper Districts of, Prof. M. E. Wadsworth, Sir Archibald Geikie, Dr. Hicks, H. Bauerman, 117

Micro-organisms at Various Temperatures, Investigations on the Behaviour of, 234

Micro-organisms and their Investigation, Mrs. Percy Frankland, 446

Microbes, Researches on the Fixation of Atmospheric Nitrogen, by M. Berthelot, 23

Microscopy : American Microscopical Society, Prizes offered for Encouragement of Research, 15 ; Messrs. Swift's Aluminium Microscope, G. C. Karop, 47 ; Medical Microscopy, Frank J. Wethered, Dr. A. H. Tubby, 51 ; the Reflector with the Projection Microscope, G. B. Buckton, F.R.S., 54 ; Fungus Internally Parasitic in Diatoms, C. H. Gill, Mr. Bennett, 118 ; on the Anatomy of *Pentastomum teretiusculum*, Prof. W. Baldwin Spencer, 260 ; Quarterly Journal of Microscopical Science, 260 ; on the Development of the Optic Nerve of Vertebrates and the Choroidal Fissure of Embryonic Life, Richard Assheton, 261 ; on the Development of the Genital Organs, Ovoid Gland, Axial and Aboral Sinuses in *Amphiura Squamata*, together with some Remarks on Ludwig's Hæmal System in this Ophiurid, E. W. MacBride, 261 ; on a New Species of Aquatic Oligochæta belonging to the family Rhinodrilidæ found in England by W. B. Benham, 261 ; on the Minute Structure of the Gills of *Palaemonetes Varians*, Edgar J. Allen, 261 ; Royal Microscopical Society, 359 ; the Microscope : its Construction and Management, Dr. Henri Van Hewick, Rev. Dr. Dallinger, F.R.S., 409

Midgley (W. W.), Constructive Errors in some Hygrometers, 623

Miers (H. A.), the Rare Silver Minerals Xanthoconite and Rittingerite, 70

Migration of Birds, the, an Attempt to Reduce Avian Season-Flight to Law, Charles Dixon, 169

Military Telephones, 182

Milk, Methods of Examining, for Tubercle Bacillus, Ilkewitsch and Thörner, 254

Milky Way, the, Dr. Otto Boeddicker, 337

Milky Way, Stellar Magnitudes in Relation to the, Prof. Kapteyn, 64

Millar (J. H.), Formation and Nitration of Phenyldiazoimide, 311

Milne (Prof. John, F.R.S.), the Volcanoes of Japan, Part 1, Fujisan, 178 ; Yezo and the Ainu, 330 ; Instruments for the Earthquake Laboratory at the Chicago Exhibition, 356

Milton (J. T.), Notes on Boiler-testing, 521

Mimicry, Aggressive, the *Volucellæ* as Examples of, Edward B. Poulton, F.R.S., 28 ; W. Bateson, 77

Mineralogy : Establishment of the Tetrahedral Hemisymmetry of Binnite, Dr. Trechmann, 70 ; the Rare Silver Minerals Xantheconite and Rittingerite, H. A. Miers and G. T. Prior, 70 ; Baddeleyite, Fletcher, 70 ; Mineralogical Society, 70 ; Artificial Production of Rutile, L. Michel, 168 ; Death of Nikolai Ivanovitch Koksharoff, 278 ; a New Coaly Mineral, 280 ; the Occurrence of Native Zirconia (Baddeleyite), L. Fletcher, F.R.S., 283 ; Hæmatite as an Illustration of the Tendency of Inorganic Matter to Simulate Inorganic Forms, 374 ; on a Meteoric Stone found at Makariwa, near Invercargill, New Zealand, G. H. F. Ulrich, 381 ; Study of the Cañon Diablo Meteorite, Henri Moissan, 408 ; on the Meteoric Iron of Cañon Diablo, C. Friedel, 408 ; on the Presence of Graphite Carbonado and Microscopic Diamonds in the Blue Earth of the Cape, Henri Moissan, 408 ; Mineralogical and Lithological Examination of the Meteorite of Kiowa county, Kansas, M. Stanislas Meunier, 456 ; Analysis of the Ashes of the Diamond, Henri Moissan, 479 ; Remarks on the Native Iron of Ovifak and the Bitumen of the Crystallised Rocks of Sweden, M. Nordenskiöld, 552 ; Valuable Ruby discovered at Burma Mines, 586

Miner's Safety-lamp, a New Portable, Prof. Frank Clowes, 596

Minervini (Signor), the Blood-vessels of the Skin in Different Parts, 254

Minor Planets, 352, 547

Mirrors, Japanese, Magic, Prof. S. P. Thompson, F.R.S., 381

Mohn (Prof.), the Climate of Greenland, 474

Moissan (Henri), Chemical Study of Opium Smoke, 168 ; a New Electric Furnace, 192 ; Action of High Temperature on Metallic Oxides, 192 ; Study of the Cañon Diablo

Meteorite, 408; on the Presence of Graphite Carbonada and Microscopic Diamonds in the Blue Earth of the Cape, 408; Ready Preparation of Large Quantities of the More Refractory Metals by Means of the Electric Furnace, 424; the Chemical Properties of the Diamond, 472; Analysis of the Ashes of the Diamond, 479; the Use of the Electric Current in Producing High Temperatures, 497; on the Preparation of a Variety of Swelling Graphite, 527
Molecular Force, Mr. Sutherland's Paper on the Laws of, Prof. Fitzgerald, Dr. Gladstone, S. H. Burbury, Prof. Ramsay, Macfarlane Gray, Prof. Herschel, 117
Mollusca, the Mantle-Cells of Ascidians, Kowalevsky, 62; on the Reproduction of Orbitolites, H. B. Brady, 119; Catalogue of the New Zealand Mollusca, H. Suter, 397
Mollusks, Hints for Collectors of, William H. Dall, 140
Monckton (Horace W.), on the Occurrence of Boulders and Pebbles from the Glacial Drift in Gravels south of the Thames, 501
Mondes, Autres, Amédée Guillemin, 485
Mongolia and Central Tibet, C. Woodville Rockhill, 426
Monsters, Extinct, Rev. H. N. Hutchinson, 250
Mont Blanc Observatory, 204
Montagu (S., M.P.), the Decimal System, 323
Monteil's (Capt.) Mission, Completion of, 89
Month and Year, a Simple Rule for finding the Day of the Week corresponding to any given day of the, 509
Monuments of Prehistoric Peoples, Manners and, Marquis de Nadaillac, 316
Moody (G. T.), Studies on Isomeric Change, ii. and iii., 311
Moon, Occultation of Mars and Jupiter by the, Prof. Barnard, 41; Lunar "Volcanoes" and Lava Lakes, S. E. Peal, 486
Moore (T. J.), Death of, late Curator of Liverpool Derby Museum, 37
Morbology: the Alleged Increase of Nervous Diseases with Growth of Civilisation, Dr. Brinton, 280, 374; on the Pathogeny of Diabetes, MM. A. Chaveau and Kaufmann, 384; Typhoid Fever attributed to bathing in polluted water, Herr Jaeger, 398; the Clasmatocytes, the Fixed Cellules of the Connective Tissue and the Pus Globules, M. L. Ranvier, 408; on the Urea contained in the Blood in cases of Eclampsia, M. L. Butte, 456; Experimental Medicine, 593; Prof. Behring's Experiments with Preventive Serum, 600
More About Wild Nature, Mrs. Brightwen, 125
Mormyrus and Malapterurus, on the Origin of the Electric Nerves in the Torpedo, Gymnotus, Gustav Fritsch, 271
Morocco, Lieut.-Col. Sir R. Lambert Playfair and Dr. Robert Brown, 298
Morocco, the Mixed Character of the Population of, A. Le Châtelier, 61
Morphology: The Sense Organs of the Skin, Feathers, and Hairs in Mammalia, Herr Maurer, 87; Contribution à l'Étude de la Morphologie et du Développement des Bactériacées, Dr. A. Billet, Dr. Rubert Boyce, 532
Morris (Rev. F. O.), Death and Obituary Notice of, 372
Morrison (Mr.), Experiments on the Value of the Steam-jacket, 20
Moscow, International Zoological Congress at, 236
Moth, the Death's Head, and Bees, J. R. S. Clifford, 234
Moths of India, 387
Motion in the Line of Sight, M. H. Deslandres, 88
Motion of Nova Aurigæ, Prof. W. W. Campbell, 256
Motion of β Persei, 115
Motion of the Solar System, Prof. J. G. Porter, 41
Motion of a Solid Body in a Viscous Liquid, A. B. Basset, F.R.S., 512
Motions, Proper, M. Deslandres, 115
Motive Power, Waves as a, H. Linden, 438
Mottelay (P. Fleury), William Gilbert, of Colchester, Physician, of London, on the Loadstone and Magnetic Bodies, and on the Great Magnet, the Earth. A New Physiology, Demonstrated with Many Arguments and Experiments, 556
Mouillefort (M.), the Vineyards of Cyprus, 517
Mound, Kansas, the Great Spirit Spring, E. H. S. Bailey, 87
Mound-Excavations in the Ohio Valley, the Recent, M. de Nadaillac, 16
Mountain Ranges, Experiments on Folding and on the Genesis of, Prof. E. Reyer, 81
Mountain Group in Podolia, a Curious, 617
Mudfishes, the Brain in, Dr. Rudolf Burckhardt, 339
Muir (Prof. James), Manual of Dairy Work, 555
Mulcaster (Richard), Foster Watson, 279
Müller (Dr. Felix), Tabular History of Astronomy to the year 1500 A.D., 18
Müller (P. A.), the Evaporation from a Snow Surface, 301
Mummies, Egyptian, Prof. Macalister, 623
Mundella (Mr.), the Board of Trade and the Electrical Engineers, 85
Munk (Dr. J.), Experiments on the Nutrition of Fasting Men, 552
Munro (Dr.), Yew Poisoning, 285
Murphy (Joseph John), an Optical Phenomenon, 365
Murray (Helen J.), a Wild Rabbit Tamed, 86
Murray (T. S.), Synthesis of Oxazoles from Benzoin and Nitriles, 430
Muscle, the Cross Striping of, Prof. Richard Ewald, and Prof. Haycraft, 92
Museum of Sanitary Appliances, Hornsey Local Board, 587
Music, Sound and, Rev. J. A. Zahm, 222
Musical Instruments, Women and, Henry Balfour, 55
Musk Ox, 559; Suggested introduction into Scotland of the, Col. H. W. Fielden, 349
Mycology, British Fungus-Flora, George Massee, 26; a new Luminous Fungus from Tahiti, 157
Myers (W. S.), Production of Orcinol, &c., from Dehydracetic acid, 237

Nadaillac (M. de), the Recent Mound-Excavations in the Ohio Valley, 16
Nadaillac (Marquis de), Manners and Monuments of Prehistoric Peoples, 316
Nagel (Herr), Food-recognising Sense of Sea-Anemones, 185
Naked-Eye Botany, F. E. Kitchener, 198
Names, Geographical, Colonel H. H. Godwin-Austen, F.R.S., 245
Nansen's (Dr.), Arctic Expedition, 65
Napoleonic Wars, Statistics of Survivors of the, M. Turquan, 233
Natal Observatory, 498
Nathan on the Improvement of Cider by Wine-Yeast, 208
Native Birds of New Zealand, the Preservation of the, 394
Natural History: Edgar B. Waite appointed Assistant Curator in Australian Museum, Sydney, 111; American Society of Naturalists, 205; the Naturalist on the River Amazons, Henry Walter Bates, F.R.S., 269; Lion-Tiger and Tiger-Lion Hybrids, Dr. V. Ball, F.R.S., 390; Lion-Tiger Hybrids, S. F. Harmer, 413; Idle Days in Patagonia, W. H. Hudson, Dr. Alf. Russel Wallace, 483; Applied Natural History, W. L. Calderwood, 492; the Musk-Ox, 559; Norfolk and Norwich Naturalists' Society, Annual Address of, H. B. Woodward, 562; Wild Spain, Abel Chapman and Walter J. Buck, 583; Natural History of Plants, Anton Kerner Von Marilaun, 605; Blind Animals in Caves, Prof. E. Ray Lankester, F.R.S., 389, 486; J. T. Cunningham, 439, 537; A. Anderson, 439; G. A. Boulenger, 608; Bequest to Illinois Wesleyan University of Mr. Lichtenthaler's Collection, 613
Nature, the Beauties of, and the Wonders of the World we Live in, Right Hon. Sir John Lubbock, F.R.S., 28
Nature, a Description of the Laws and Wonders of, Richard A. Gregory, 74
Nature, Evolution and Man's Place in, Henry Calderwood, 385
Naue (J.), Discovery near Schaffhausen of Prehistoric Drawings on Limestone, 279
Nautical Almanac for 1896, the, 326
Naval Architecture: the Strength of Bulkheads, Dr. Elgar, 529; Experiments on the Transmission of Heat through Tube-plates, A. J. Durston, 521; Notes on Boiler-testing, J. T. Milton, 521; an Apparatus for Measuring and Registering Vibrations of Steamers, E. Otto Schlick, 521; Experiments with Engines of ss. *Iveagh*, John Inglis, 521
Naval Architects, Institution of, 519; Annual General Meeting, 494
Navigation; the Measurement of Wake Currents, G. A. Calvert, 519
Navigation, Graphical Solutions of Problems in, 547
Nayudu (P. Lakshmi Narasu), Notes on Qualitative Chemical Analysis, 100
Neave (Newman), a Fork-tailed Petrel, 31

Nebula near ξ Persei (N. G.C. 1499), the Large, Dr. Scheiner, 546
Neesen (Prof.), Experiments on Photographic Recording of Oscillation of Projectiles, 216
Nelson (R. H.), Death of, 353
Neolithic Village of the Roche-au-Diable, near Tesnières, Canton of Lorez-le-Bocage (Seine-et-Marne), Armand Viré, 576
Netto (Eugen), the Theory of Substitutions and its Applications to Algebra, 338
Neumann (Herr), Power of Hydrogen-Absorption of Various Metals, 63
Neville and Heycock, Isolation of Gold and Cadmium Compounds, 40
New England, New Methods of disseminating Weather Forecasts in, 613
New England Grammar Schools, Change recommended by Association of College Officers in Curriculum of, 279
New Guinea, British, J. P. Thomson, Henry O. Forbes, 345, 414 ; Prof. Alfred C. Haddon, 414
New Hebrides, on some Islands of the, Lieut. Boyle T. Somerville, 455
New South Wales : Artesian Boring and Irrigation in, J. W. Boultbee, 183 ; Physical Geography and Climate of, H. C. Russell, F.R.S., 258 ; Plants most visited by Bees in, 614
New York Mathematical Society, Bulletin of, 23
New York State Pecuniary Contributions to Agriculture, the, 349
New York, Columbia College ; the Loubat Prizes, 496
New Zealand : Earthquake in, 372 ; the Preservation of the Native Birds of, 394 ; Catalogue of the New Zealand Mollusca, H. Suter, 397
Newall (H. F.), Nova Aurigæ, 7
Newberry (John Strong), Obituary Notice of, 276
Newcastle College of Science, Laying Foundation Stone of, 129
Newcomb-Engelmann's Populäre Astronomie, 291
Newth (G. S.), Note on the Colours of the Alkali Metals, 55 ; Chemical Lecture Experiments, Sir Henry E. Roscoe, F.R.S., 97
Newton (Prof. Alfred, F.R.S.), Palæontological Discovery in Australia, 606
Newton (E. T.), some New Reptiles from the Elgin Sandstone, 189
Newton (Prof. H. A.), Lines of Structure in the Winnebago County Meteorites and in other Meteorites, 370
Niagara Spray Clouds, the, Chas. A. Carus-Wilson, 414
Nicaragua, the Boundaries of Costa Rica and, Dr. H. Polakowsky, 257
Nicholls (H. A. Alford), a Text-book of Tropical Agriculture, 313
Nicobar Pottery, E. H. Man, 455
Nicolsky (Dr.), Study of the Form of Eggs, 253
Nikitine (S.), Constitution of the Quaternary Deposits in Russia and their Relations to the Finds resulting from the Activity of Prehistoric Man, 523
Nile, the Stars and the, Capt. H. G. Lyons, 101
Nile, the Sacred, J. Norman Lockyer, F.R.S., 464
Nitrate of Soda : the Origin of Caliche, G. M. Hunter, 254
Nitrogen, Atmospheric, Researches on the Fixation by Microbes of, M. Berthelot, 23
Niven (W. D., F.R.S.), the Harmonics of a Ring, 406
Nomenclature, Biological ; the rule "Once a Synonym, always a Synonym," Elliott Coues, 39
Nomenclature, Botanical, W. T. Thiselton Dyer, F.R.S., 53 ; Sereno Watson, 53
Nordenskiöld (M.), Remarks on the Native Iron of Ovifak and the Bitumen of the Crystallised Rocks of Sweden, 552
Noorden (Dr. von), Four Experiments on Nutrition, 504
Norfolk Coast, Sowerby's Whale on the, T. Southwell, 349
Norfolk and Norwich Naturalists' Society ; Annual Address by H. B. Woodward, 562
North Sea, Destruction of Immature Fish in the, Ernest W. L. Holt, 160
North Sea, Magnetic Observations in the, A. Shück, 555
North American Indians, Educational Work among the, 350
Notes from the Leyden Museum, 357
Nottingham Meeting of the British Association, the Coming, 612
Nova Aurigæ, 159, 399 ; H. F. Newall, 7 ; Prof. Barnard, 282 ; Mr. Huggins, 425 ; Motion of Nova Aurigæ, Prof. W. W. Campbell, 256 ; Hydrogen Line Hβ in the Spectrum of, Herr Victor Schumann, 425 ; Spectra of Planetary Nebulæ and Nova Aurigæ, M. Eugen Gothard, 352
Novel (M. J.), on a new Soldering Process for Aluminium and various other Metals, 384
Nubibus, Dynamics in, "Waterdale," 601
Nuttall (Zelia), the Calendar System of the Ancient Aztecs, 156

Obrutcheff (M.), Further Researches in Siberia, 255
Observatories : a New Observatory at Abastouman, 133 ; Companion to the Observatory for 1893, 159 ; Mont Blanc Observatory, 204 ; Lick Observatory, Miss Milicent W. Shinn, 209 ; the Harvard College Observatory, Prof. E. C. Pickering, 304, 403 ; Stonyhurst College Observatory, 450 ; United States Naval Observatory, 452 ; Yale Astronomical Observatory, 452 ; Bermerside Observatory, 473 ; the Melbourne Observatory, 498 ; Natal Observatory, 498 ; Wolsingham Observatory, 518 ; Circular No. 35, 590 ; T. E. Espin, 452 ; Paris Observatory in 1892, M. Tisserand, 546 ; Magnetical and Meteorological Observations made at the Government Observatory, Bombay, 1890, 379 ; "The Observatory," 566
Observational Astronomy, Arthur Mee, 437
Occultation of Mars and Jupiter by the Moon, Prof. Barnard, 41
Odorographia : a Natural History of Raw Materials and Drugs used in the Perfume Industry, J. Ch. Sawer, 52
Odours, Analysis of Complex, Jacques Passy, 48
Oesten (Herr), Filtered Sewage Water Favourable to Fish Life, 350
Ohio Valley, the Recent Mound-Excavations in the, M. de Nadaillac, 16
Oil in Calming Waves, Experiments on the use of, Rear-Admiral Cavelier de Cuverville, 279
Olphe-Galliard (Victor Aimé Leon), Death and Obituary Notice of, 395
Olszewski (Herr), Use of Total Reflection to determine Light-Refraction of Liquid Oxygen, 614
Omori (F.), Instruments for the Earthquake Laboratory at the Chicago Exhibition, 356
Ophthalmology : the Association of Shipping Disasters with Defective Vision in Sailors, Dr. T. H. Bickerton, 16
Opium Smoke, Chemical Study of, Henri Moissan, 168 ; Physiological Study of, G. Gréhant and Em. Martin, 168
Opposition of Mars, the Recent, Prof. W. H. Pickering, 235
Optics : Optical Illusions, R. T. Lewis, 31 ; W. B. Croft, 78 ; Refraction and Dispersion of Light in Metal Prisms, D. Shea, 68 ; a New "Shortened Telescope," Dr. R. Steinheil, 113 ; the Passage of a Wave through a Focus, P. Joubin, 143 ; Existence of Distinct Nervous Centres for Perception of Fundamental Colours of Spectrum, A. Chauveau, 143 ; the New Telephotographic Lens, T. R. Dallmeyer, 161 ; Remarkable Optical Phenomenon near Zermatt, F. Folie, 303 ; on the Minimum Perceptible Amount of Light, M. Charles Henry, 312 ; the Polarising Action of the Moon on the Atmosphere, Clémence Royer, 325 ; the Alleged Sexual Difference in the Eye, Herr Greef, 325 ; Optical Continuity, Francis Galton, F.R.S., 342 ; an Optical Phenomenon, Joseph John Murphy, 365 ; Helmholtz Physiological Optics, Prof. J. D. Everett, F.R.S., 365 ; Preliminary Note on the Colours of Cloudy Condensation, C. Barus, 380 ; the Perception of Colour, W. F. Stanley, 381 ; on Semicircular Interference Fringes, M. G. Meslin, 384 ; Modern Optics and the Microscope, Dr. Henri van Heurck, Rev. Dr. Dallinger, F.R.S., 409 ; Electrical Actinometers used by Messrs. Elster and Geitel in Measurement of Sun's Ultra-Violet Radiation, 422 ; Two Experimental Verifications Relative to Refraction in Crystals, J. Verschaffelt, 428 ; the Fundamental Law of Complementary Colours, Paul Glan, 455 ; a New Hypothesis Concerning Vision, John Berry Haycraft, 478 ; a New and Handy Focometer, Prof. J. D. Everett, F.R.S., 500 ; on the Measurement of Direct Light by Means of the Tintometer, J. W. Lovibond, 501 ; on the Chromatic Curves of Microscope Objectives, Dr. W. H. Dallinger, 501 ; Influence of the Motion of the Earth on the Propagation of Light in Doubly Refracting Media, Mr. Lorentz, 504 ; Penetration of Thin Metallic Plates by Cathode Rays causing Phosphorescence, 518 ; the Dioptrics of Gratings, Dr. J. Larmor, F.R.S., 526 ; on Spherical Aberration of the Human

Eye; Measurement of Senilism of the Crystalline, M. C. J. A. Leroy, 528; Measurement of Large Differences of Phase in White Light, M. P. Joubin, 528; Sensitiveness of the Eye to Light and Colour, Capt. W. de W. Abney, F.R.S., 538; Contrivance for Determining Refractive Index of Liquid, 544; Experiments on Phosphorescence-Producing Kathode Rays of a Geissler Tube, Dr. P. Lenard, 564; Use of Total Reflection to Determine Light-Refraction of Liquid Oxygen, Herren Olszewski and Witkowski, 614; Researches at the Berlin Imperial Physico-Technical Institute on the Siemens Platinum Foil Unit as a Standard for the Intensity of a Source of Light, 615
Orang-Utan, Remarkable Specimen of, 423
Orbitolites, on the Reproduction of, H. B. Brady, 119
Ordnance Survey, 447
Ordnance Survey and Geological Faults, the, James Durham, 510
Organic Chemistry, Outlines of, Clement J. Leaper, 124
Organic Substances, Qualitative Analysis Tables and the Reactions of Certain, E. A. Letts, Chapman Jones, 361
Origin, Unusual, of Arteries in the Rabbit, Philip J. White, 365
Origin of Colour, VII, VIII and IX, H. E. Armstrong, 551
Origin of Lake Basins, the Duke of Argyll, F.R.S., 485; J. C. Hawkshaw, 558
Origin of the Year, the, J. Norman Lockyer, F.R.S., 32, 228
Orme (Temple), Matriculation Chemistry, 99
Ornithology: a Fork-tailed Petrel, Newman Neave, 31; the Food of Humming-birds, Dr. Morris Gibbs, 63; Assumption of the Male Plumage by a Pea-hen, G. N. Douglas, 71; Corncrake Caught in Wales, December 8, 1892, 157; the Migration of Birds, an Attempt to Reduce Avian Season-Flight to Law, Charles Dixon, 169; Study of the Form of Eggs, Dr. Nicolsky, 253: Death and Obituary Notice of Rev. F. O. Morris, 372; the Flight-Speed of Wild Ducks, H. L. Clark, 374; the Preservation of the Native Birds of New Zealand, 394; Death and Obituary Notice of Victor Aimé Leon Olphe-Galliard, 395; the Flight of Birds, Herbert Withington, 414; Formation of a Cage-Bird Club, 495; Ornithology in Relation to Agriculture and Horticulture, John Watson, 533; the Protection of the Osprey in Scotland, 42
Osborn (Prof. Henry F.), Protocerus, the New Artiodactyle, 321; Artionyx, a Clawed Artiodactyle, 610
Osmium, Chemistry of, A. E. Tutton, 400
Osmium, Metallic, M. Joly and Vèges, 497
Osmotic Pressure, J. W. Rodger, 103; Prof. Spencer Pickering, F.R.S., 175
Osprey in Scotland, the Protection of the, 612
Ostwald's Klassiker der Exacten Wissenschaften, Nos. 31–37, 149, 38–40, 245
Oudemans (Dr. A. C.), the Great Sea Serpent, 506
Owen (Sir Richard), Obituary Notice of, 181; the Proposed Memorial to, 232, 252, 307
Oxford, Medical Education at, Lord Salisbury, 449
Oxford Medical Society, Inaugural Address by Sir James Paget, 60
Oxford: University Junior Scientific Club, 95, 119, 167, 359, 431, 502
Oxford University Museum, Catalogue of Eastern and Australian *Lepidoptera heterocera* in the Collection of the, Col. C. Swinhoe, 53
Oxley (Rev. W. H.), Travelling of Roots, 414
Oxygen for Limelight, T. C. Hepworth, 176
Oxygen, Liquid, Use of Total Reflection to Determine Light-Refraction of, Herren Olszewski and Witkowski, 614
Oyster Culture, on the Attempt at, in the Roscoff Laboratory, M. de Lacaze-Duthiers, 456
Oysters, Observations on, Prof. R. C. Schiedt, 375
Ozone, D. A. Van Bastelaer's Observations on, 373
Ozone, W. G. Black, 390

Pacific Slope, Grasses of the, including Alaska and the adjacent Islands, Dr. Geo. Vasey, 173
Paddington Railway, Clapham Junction and, 515
Paderborn, Shower of Pond Mussels at, 278
Padua, Galileo Galilei and the Approaching Celebration at, Prof. Antonio Favaro, 82, 180
Page (M.), the Stanley Falls District of the Congo, 282
Paget (Sir George E., K.C.B., F.R.S.), Some Lectures by, 485
Paget (Sir James), Inaugural Address to Oxford Medical Society, 60
Palæolithic or Unground Stage of the Implement-makers' Art, On the Rude Stone Implements of the Tasmanians, showing them to belong to the, Dr. Tylor, 527
Palæmonetes Varians, Anatomy of Larva of, E. J. Allen, 237
Palæontology, Discovery of Ureus Arctos in the Malta Pleistocene, J. H. Cooke, 62; Relics found in Yorkshire Caves, Rev. Edward Jones, 112; Walrus in the Thames Valley, W. J. L. Abbott, 132; Some New Reptiles from the Elgin Sandstone, E. T. Newton, 189; Death of Dr. D. Stur, 206; Protocerus, the New Artiodactyle, Prof. Henry F. Osborne, 321; Restoration of Anchisaurus Colurus, Prof. O. C. Marsh, 349; a Catalogue of British Jurassic Gasteropoda, W. H. Hudlestone, F.R.S., and Edward Wilson H. Woods, 363; Johns Hopkins University Palæontological Collections, 471; Fossil Fauna of the Black Sea, T. J. van Beneden, 544; Artionyx—a Clawed Artiodactyle, Prof. Henry F. Osborn, 610; Palæontological Discovery in Australia, Prof. Alfred Newton, F.R.S., 606
Palæozoic Ice-Age, a, W. T. Blanford, F.R.S., 101, 152; Henry F. Blanford, F.R.S., 101
Palestine, Geology of, Prof. Edward Hall, F.R.S., 166
Palmberg (Dr. Albert), a Treatise on Public Health, Dr. H. Brock, 507
Palmer (Mr.), a Lilac Colour produced from Extract of Chestnut, 132
Papasogli, Colorimeter for Comparing Intensity of Colour in Solution, 131
Papuans, the Native, T. H. Hatton-Richards, 590
Parallax of β Cygni, Harold Jacoby, 399
Parallaxes of μ and θ Cassiopeiæ, Harold Jacoby, 5
Parasitism of *Volucella*, W. E. Hart, 78
Parker (J.), Carnot's Principle applied to Animal and Vegetable Life, 95
Parker (Prof. T. Jeffrey, F.R.S.), on the Cranial Osteology, Classification and Phylogeny of the *Dinornithidæ*,, 431
Parker (Prof. W. N.), on an Abnormality in the Veins of the Rabbit, 270
Paris Academy of Sciences, 23, 47, 71, 96, 119, 143, 167, 192, 215, 239, 263, 287, 311, 335, 360, 384, 408, 431, 456, 479, 503, 527, 551, 576, 599, 623; Prize List for 1892, 215
Paris Observatory in 1892, M. Tisserand, 546
Paris, the Question of the Purity of Ice Consumed in, 614
Parry (John), on the American Iron Trade and its Progress during sixteen years, Sir Lowthian Bell, F.R.S., 195
Pasquale (Dr. B.), the "Mal Nero" Vine Disease, 130
Pasquale (Cav. G. A.), Death of, 421
Passy (Jacques), Analysis of Complex Odours, 48
Pasteur (M.), Proposed Testimonial to, 37
Pasteur's (M.), Seventieth Birthday, 204
Patagonia, Idle Days in, W. H. Hudson, Dr. Alf. Russel Wallace, 483
Pathology: the Croonian Lecture, 487; on the Histology of the Blood of Rabbits which have been rendered Immune to Anthrax, Lim Boon Keng, 502
Peal (S. E.), Lunar "Volcanoes" and Lava Lakes, 486
Pearcey (F. G.), Foraminifer or Sponge? 390
Peary (Lieut.), Proposed Arctic Expedition of, 133, 452
Pekelharing (Mr.), the Peptone of Kühne, 624
Peloponnes Der, Versuch einer Landeskunde auf Geologischer Grundlage, Dr. Alfred Philippson, 6
Pencils, Slate, Aluminium, 131
Penck's (Prof.), Scheme for a Map of the World on a uniform scale, 426
Perfume Industry, Odorographia: a Natural History of Raw Materials and Drugs used in the, J. Ch. Sawer, 52
Peripatus Egg, the Hatching of a, Arthur Dendy, 508
Perkin (W. H., sen.), Magnetic Rotation of Sulphuric and Nitric Acids, 165
Perigal (Mr. Henry), Complimentary Dinner to, 585
Perigal Family, Longevity of the, Dr. C. T. Williams, 585
Perry (G. H.), Interaction of Iodine and Potassium Chlorate, 165
Perry (Prof. J., F.R.S.) on the differential equation of Electric Flow, 574; on the Viscosity of Liquids, 575
Persei, Motion of β, 115
Persei, Relative Position of Stars in Cluster χ, Sir Robert Ball and Arthur Rambaut, 376

Persei, the large Nebula near ξ (N.G.C. 1499), Dr. Scheiner, 546
Perseids, Observations of, 88
Perthshire Society of Natural Science, Dr. F. B. Waite's Collection of Lepidoptera presented to, 206
Peru (Southern) Indications of a Rainy Period in, A. E. Douglass, 38
Petrel, a Fork-tailed, Newman Neave, 31
Petrie (Prof. W. Flinders), appointed to Chair of Egyptology at University College, London, 111; First Lecture on Egyptology, 278; Ancient Egypt, 301
Pflanzeneeben, Anton Kerner von Marilaun, 605
Phenological Observations for 1892, Report on the, E. Mawley, 430
Phœnician Tombs at Malta, Depredations among the recently-discovered, 396
Philippson (Dr. Alfred), Der Peloponnes; Versuch einer Landeskunde auf Geologischer Grundlage, 6
Phosphate Beds, The Florida, T. N. Lupton, 325
Phosphorescence: a new Luminous Fungus from Tahiti, 157
Phosphorescence in Centipedes, R. I. Pocock, 545
Photography: Traité Encyclopédique de Photographie, Charles Fabre, 6; the Photography of an Image by Reflection, Frederick J. Smith, 10; Photographic Dry Plates, Arthur E. Brown, 11; Coloured Photographs of the Spectrum, G. Lippmann, 23; American Opinion of Photography in England, Xanthus Smith, 86; a Manual of Photography, A. Brothers, 98; Conditions of Production of Lippmann's Coloured Photographs, G. Meslin, 157; the New Telephotographic Lens, T. R. Dallmeyer, 161; on the Photographic Spectra of some of the Brighton Stars, J. Norman Lockyer, F.R.S., 261; Photographic Absorption of our Atmosphere, Prof. Schaeberle, 304; Eclipse Photography, M. De la Baume Pluvinel, 326; a new Method of Photographing the Corona, M. H. Deslandres, 327; Photography first discovered by a Dr. Schultz in Halle, 336; Dust Photographs, W. T. Thiselton-Dyer, F.R.S., F. J. Allen, 341; Dust Photographs and Breath Figures, W. B. Croft, 364; on the Progress of the Art of Surveying with the aid of Photography in Europe and America, M. A. Lausedat, 384; on Electric Spark Photographs, or Photography of Flying Bullets, &c., by the Light of the Electric Spark, C. V. Boys, F.R.S., 415, 440; British Journal Photographic Almanac for 1893, 462; Photographic Reproduction of Gratings and Micrometers engraved on glass, M. Izarn, 479; Photography of certain Phenomena furnished by Combinations of Gratings, M. Izarn, 503; Photography of Gratings engraved upon Metal, M. Izarn, 623; Prof. Hale's Solar Photograph, 498; Photographic Properties of Cerium Salts, MM. Auguste and Louis Lumière, 503; Equipotential Lines due to current flowing through Conducting Sheet fixed Photographically, E. Lommel, 544; Anthropological Uses of the Camera, 548; Photographic Chart of the Heavens, M. Loewy, 589; Notes on two Photographs of Lightning taken at Sydney Observatory, December 7, 1892, H. C. Russel, F.R.S., 623
Photometer, a Photoptometric, Charles Henry, 24
Photometry: on Phosphorescent Sulphide of Zinc considered as a Photometric Standard, Charles Henry, 312
Photomicrography, the Use of Monochromatic Yellow Light in, T. H. Gill, 47
Physics: Berlin Physical Society, 24, 312; the Laws of Compressibility of Liquids, E. H. Amagat, 48; the Temperature of Maximum Density of Mixtures of Alcohol and Water, L. de Coppet, 48; Analysis of Complex Odours, Jacques Passy, 48; Physical Society, 69, 116, 165, 190, 358, 381, 429, 500, 574; Mr. Williams on the Relation of the Dimensions of Physical Quantities to Directions in Space, Prof. Fitzgerald, Mr. Madan, Prof. Rücker, Prof. Henrici, Dr. Sumpner, 69; Williams on the Dimensions of Physical Quantities, Dr. Burton, Prof. A. Lodge, Mr. Boys, W. Baily, Mr. Swinburne, Mr. Williams, 116; the Determination of the Critical Volume, Dr. Young, 70; Mr. Sutherland's paper on the Laws of Molecular Force, Dr. Young, 70; Prof. Fitzgerald, Dr. Gladstone, S H. Burbury, Prof. Ramsay, Macfarlane Gray, Prof. Herschel, 117; Lenticular Liquid Microglobules and their Conditions of Equilibrium, C. Maltézos, 71; Dilatation of Iron in a Magnetic Fluid, M. Berget, 71; Laws of Dilatation of Gases under Constant Pressure, E. H. Amagat, 96; Conditions of Equilibrium and Formation of Microglobules, C. Maltézos, 96; the Form of Isothermals of Liquids and Gases, E. H. Amagat, 143; Interesting Results in Application of Cold, 184; Breath Figures, W. B. Croft, 187; Method for determining Density of saturated Vapours and Expansion of Liquids at Higher Temperatures, B. Galitzine, 189; Relation between Velocity of Light and Size of Molecules of Refracting Liquids, P. Joubin, 192; Sound and Music, Rev. J. A. Zahm, 222; Employment of Springs in Measurement of Explosive Pressures, 236; the Temperature of the Electric Arc, J. Violle, 240; Magnetic Properties of Oxygen, P. Curie, 240; High Temperatures and Carbon Vaporisation, M. Berthelot, 240; a Simple Explanation of the Hall Effect, E. Lommel, 254; on Thermo-Electric Phenomena between two Electrolytes, Henri Bagard, 263; Pure Gases incapable of producing Electrification by Friction, Mr. Wesendonck, 280; on the Temperature Coefficient of the Electrical Resistance of Mercury and on the Mercury Resistance of the Imperial Institution, D. Kreichgauer and W. Jaeger, 286; Diffusion of Light by Rough Surfaces, Christian Wiener, 286; on the Solubility-Curve for Systems of two Bodies, Bakhuis Roozeboom, 288; Physical Education, Frederick Treves, 292; the Rate of Explosion in Gases, Prof. Harold B. Dixon, 299; on a State of Matter characterised by the Mutual Independence of the Pressure and the Specific Volume, P. de Heen, 309; the Thermal Conductivities of Liquids, R. Wachsmith, 350; on a Modification of the Transpiration Method suitable for the Investigation of very Viscous Liquids, C. Brodmann, 357; the Viscosity of Liquids, Prof. J. Perry, F.R.S., 575; Isothermals, Isopiestics, and Isometrics relative to Viscosity, C. Barus, 380; Gemeinverständliche Vorträge aus dem Gebeite der Physic, Prof. Dr. Leonhard Sohncke, Dr. James L. Howard, 361; Uses of Planes and Knife Edges in Pendulums for Gravity Measurements, J. C. Mendenhall, 380; the Determination of the Thermal Expansion of Liquids, T. E. Thorpe, 405; the Determination of the Thermal Expansion and Specific Volume of certain Paraffins and Paraffin Derivatives, T. E. Thorpe and L. M. Jones, 405; on Electric Spark Photographs or Photography of Flying Bullets, &c., by the Light of the Electric Spark, C. V. Boys, F.R.S., 415, 440; on the Common Cause of Surface Tension and Evaporation of Liquids, G. Van der Mensbrugghe, 428, 621; Description of an Instrument to show the small Variations in the Intensity of Gravitation, M. Bouquet de la Grye, 431; Simple Instrument for measuring Densities of Liquids, A. Handl, 471; the Value of the Mechanical Equivalent of Heat, E. H. Griffiths, 476, 537; the Effects of Mechanical Stress on the Electrical Resistance of Metals, James H. Gray and James B. Henderson, 478; Photographic Reproduction of Gratings and Micrometers engraved on Glass, M. Izarn, 479; Photography of certain Phenomena furnished by Combinations of Gratings, M. Izarn, 503; Photography of Gratings engraved upon Metal, M. Izarn, 623; the Specific Heat of Liquid Ammonia, C. Ludeking and J. E. Starr, 499; on the Influence of Time upon the Mode of Formation of the Meniscus at the Temperature of Transformation, P. de Heen, 500; Experiments on the Influence of Temperature on Electromagnetic Rotation of Light in Iron, Cobalt, and Nickel, Prof. Kundt, 503; on the Influence of Temperature upon Circular Ferro-Magnetic Polarisation, Emil Hirsch, 525; Magnetisation of a radially slit Iron Ring, Heinrich Lehmann, 525; on the Stability of a Thin Rod loaded vertically, Mr. Love, 526; the Resistance of Ice, M. Force, 564; on the Differential Equation of Electric Flow, T. H. Blakesley, Prof. Perry, Prof. O. J. Lodge, Dr. Sumpner, Mr. Swinburne, 574; on Action of Temperature upon the Rotatory Power of Liquids, M. A. Aignan, 576; Laws and Properties of Matter, R. T. Glazebrook, F.R.S., 580; the Radiation and Absorption of Heat by Leaves, Alfred Goldsborough Mayer, 596; the Absolute Thermal Conductivities of Copper and Iron, R. Wallace Stewart, 599; Expansion of Water at Constant Pressure and at Constant Volume, E. H. Amagat, 623
Physiography, Elementary, Richard A. Gregory, 74
Physiology: Obstacles to Ice-Formation in Animal Body, Herr Kochs, 16; Introduction to Physiological Psychology, Dr. Theodor Zichen, 28; Influence of Bodily Exertion on Digestive Process, Herr Rosenberg, 62; Further Researches on Nucleinic Acid, Prof. Kossel, 72; a Manual of Veterinary Physiology, Veterinary-Captain F. Smith, 76; on the Physiology of Grafting, Dr. Hermann Vöchting, 128; the Respiratory Centre, Prof. Gad, 144; Influence on Respiration of Upper Tracts leading from Cerebrum to Respiratory

Centre, Dr. Ad. Loewy, 144; Sensation of Warmth on immersing Hand in Carbon Dioxide, Dr. R. du Bois Reymond, 144; Elements of Human Physiology, E. H. Starling, 146; the Thyroid Gland (Experiments on Cats), Dr. J. Lorrain Smith, 167; Human Physiology, the Blood-Vessels of the Skin in different Parts, Signor Minervine, 254; Animal Physiology, on the Anatomy of of *Pentastomum tereliusculum*, Prof. W. Baldwin Spencer, 260; on the Minute Structure of the Gills of *Palæmonetes varians*, Edgar J. Allen, 261; on the Development of the Optic Nerve of Vertebrates and the Choroidal Fissure of Embryonic Life, Richard Assheton, 261; on the Development of the Genital Organs and Ovoid Gland, Axial and Aboral Sinuses in *Amphiura squamata*, together with some Remarks on Ludwig's Hæmal System in this Ophiurid, E. W. MacBride, 261; on a New Genus and Species of Aquatic Oligochæta belonging to the Family Rhinodrilidæ found in England by W. B. Benham, 261; Prof. Exner on the Innervation of the Crico-Thyroid Muscle in Rabbits and Dogs, 287; Photography of Microscopic Objects which when placed in a Stereoscope presented an Appearance of Solidity, Dr. Hausemann, 287; Custom of Civilised Races of Antiquity to establish Themselves in Dry Districts, Prof. Hilgard, 287; Berlin Physiological Society, 287, 552; Microscopic Researches on the Contractility of the Blood-Vessels, M. L. Ranvier, 312; Vegetable Physiology, Researches on the Localisation of the Fatty Oils in the Germination of Seeds, M. Eugène Mesnard, 312; on the Peptosaccharifiant Action of the Blood and the Organs, M. R. Lépine, 335; the Pancreas and the Nervous Centres controlling the Glycemic Function, Prof. Kaufmann, 479; the Pancreas and the Nerve-Centres regulating the Glycemic Function; Experimental Demonstrations derived from a Comparison of the Effects of a Removal of the Pancreas with those of Bulbary Section, MM. A. Chaveau and M. Kaufmann, 528; Four Experiments on Nutrition, Dr. von Noorden, 504; on the Origin of the Mammalian Hair, M. Weber, 504; on Numerical Variation in Digits in Illustration of a Principle of Symmetry, W. Bateson, 503; Experiments on the Nutrition of Fasting Men, Dr. J. Munk, Prof. Zuntz, Dr. Vogelius, 552; the Formation of Sweat, Dr. Lewy-Dorn, 600; Survival after the Successive Section of both the Branches of the Vagi, M. C. Vanlair, 621; on the Digestion of the Cœlenterata, Marcelin Chapeaux, 621

Piccioli (Signor), Biological Relations between Plants and Snails, 23

Pickering (Prof. E. C.), the Harvard College Observatory, 304, 403; Jupiter and his Satellites, 518

Pickering (Prof. Spencer, F.R.S.), Osmotic Pressure, 175

Pickering (S. U.), Refractive Indices and Magnetic Rotations of Sulphuric Acid Solutions, 165; Some Alkylamine Hydrates, 165; Isolation of two predicted Hydrates of Nitric Acid, 238; the Hydrates of Hydrogen Chloride, 479

Pickering (Prof. W. H.), the Recent Opposition of Mars, 235

Pictet (Prof. Raoul), Sarasin and de la Rive's Experiments in Measurement of Rate of Herz Electric Waves, 336

Pioneers of Science, Oliver Lodge, F.R.S., 268

Pitmen, Coal Pits and, R. Nelson Boyd, 481

Place (M. de), the Schiseophone, 23

Place-Names, African, the Orthography of, 400

Place-Names of German Protectorates, Official Rules for Spelling of, 89

Placet (Em.), Preparation of Metallic Chromium by Electrolysis, 144

Planets, Atmospheres of, 18

Planets, the Light of, 64; John Garstang, 77

Planetary Nebulæ, Spectra of, and Nova Aurigæ, M. Eugen Gothard, 352

Planets, Minor, 352, 457

Planet Mars, the, Camille Flammarion, William J. S. Lockyer, 553

Plant (James), Death of, 60

Plantamour (P.), Observations of Earth Oscillations, 254

Plants, Climbing, Dr. H. Schenck, W. Botting Hemsley, F.R.S., 514

Plants, the Food of, A. P. Lauri', 556

Playfair (Lord), on Scientific Education, 301

Playfair (Lieut.-Colonel Sir R. Lambert), Morocco, 298

Pliocene Geology, Difficulties of, Sir Henry H. Howorth, 150, 270

Plön, Forschungsberichte aus der Biologischen Station zu, Dr. Otto Zaccharias, 461

Pluvinel (M. de la Baume), Eclipse of April 16, 1893, 281, 304; Eclipse Photography, 326

Plymouth Marine Biological Station, the Week's Work of the, 398, 424, 451, 472, 497, 518, 546, 565, 589, 616

Plymouth Marine Biological Association, Dredging Work, 375

Pocock (R. J.), Phosphorescence in Centipedes, 545

Podolia, a Curious Mountain Group in, 617

Poincaré (H.), Théorie Mathématique de la Lumière, A. B. Basset, F.R.S., 386

Polakowsky (Dr. H.), the Boundaries of Costa Rica and Nicaragua, 257

Polarisation of Light: Use of Half-Shade Polarimeters, Dr. Lummer, 312; Prof. Goldstein's Experiments, 312

Pole (Dr. W.), Colour Blindness, 335

Polecat not Extinct in Cardiganshire, J. W. Salter, 450

Pollard (W.), a Brilliant Meteor, 247

Polynesia; the Tokelaus, 423

Polynesian Ornament-Forms, Mythographic Origin of, Dr. H. C. March, 239

Ponza, Earthquake in, 86

Pope (W. J.), Sulphonic Derivatives of Camphor, 405

Popular Lectures on Physical Subjects, Dr. James L. Howard, 361

Port Erin (Isle of Man) Marine Biological Station, 515

Porter (Prof. J. G.), Motion of the Solar System, 41

Potato Disease, Observations on the, Dr. J. Böhm, 254

Potato as a Diagnostic Agent in Bacteriology, Herr Krannhals, 545

Potential, Discovery of the, Ottavio Zanotti Bianco, Dr. E. J. Routh, F.R.S., 510

Potsdam Magnetic Observatory, Improvements in Registration of Needle's Variations, Herr Eschenhagen, 544

Pottery Glazes, W. P. Rix, 396

Pottery, Nicobar, E. H. Man, 455

Poulton (Edward B., F.R.S.), the *Volucellæ* as Examples of Aggressive Mimicry, 28; the *Volucellæ* as alleged Examples of Variation "almost unique among Animals," 126; Soot-Figures on Ceilings, 608

Pound, Imperial Standard, Decrease in Weight of, 86

Power Distribution, Electric Lighting and, W. Perren Maycock, 269

Praeger (R. L.), What is the True Shamrock? 302

Prantl (Dr. Karl), Death and Obituary Notice of, 495

Preece (W. H., F.R.S.), the Growth of Electrical Industry, 327; on Lightning Protection, Prof. Oliver Lodge, F.R.S., 536

Prehistoric Anthropology; the Quaternary Deposits in Russia and their Relations to the Finds resulting from the Activity of Prehistoric Man, S. Nikitine, 523

Prehistoric Archæology and Anthropology, the International Congress of, 523

Prehistoric Drawings on Limestone, Discovery near Schaffhausen of, J. Naue, 279

Prehistoric Ethnography of Central and North-East Russia, J. Smirnov, 524

Prehistoric Peoples, Manners and Monuments of, Marquis de Nadaillac, 316

Prescot Watch Factory, the; Address by Lord Kelvin, 279

Preservation of the Native Birds of New Zealand, 394

Pressure, Osmotic, J. W. Rodger, 103; Prof. Spencer Pickering, F.R.S., 175

Prince (Prof. Edward), appointed Commissioner of Fisheries for Canada, 37

Printing Mathematics, W. Cassie, 8; Dr. M. J. Jackson, 227

Prior (G. T.), the Rare Silver Minerals Xanthoconite and Rittingerite, 70

Pritchard (Prof. Chas., F.R.S.), Lord Kelvin, 110

Prizes, Astronomical Journal, 282

Problems in Navigation, Graphical Solutions of, 547

Projectiles, Oscillation of, Experiments on Photographic Recording of, Prof. Neesen, 216

Projection Microscope, the Reflector with the, G. B. Buckton, F.R.S., 54

Prominences, Ultra-Violet Spectrum in, Prof. G. E. Hale, 186

Proper Motions, M. Deslandres, 115

Property: Its Origin and Development, Charles Letourneau, 123

Protection, Lightning, W. H. Preece, F.R.S., on, Prof. Oliver Lodge, F.R.S., 536

Protocerus, the New Artiodactyle, Prof. Henry F. Osborn, 321
Protoplasms, Prof. Bütschli's Experiments on the so-called Artificial, Dr. W. H. Dallinger, 526
Prussian Government, the Centigrade Thermometer adopted by the, 60
Psychology, Introduction to Physiological, Dr. Theodor Ziehen, 28
Psychological Association, American, 348
Psychological Laboratory at Yale College, Establishment of, 253
Public Health, a Treatise on, and its Applications in different European Countries, Dr. Albert Palmberg, Dr. H. Brock, 507
Public Schools, Science in the, and the Scientific Branches of the Army, 513
Pupin (I.), a Method of obtaining Alternating Currents of Constant and Easily-Determined Frequency, 586
Purdie (T.), Optically Active Ethoxysuccinic Acid, 311; the Resolution of Methoxysuccinic Acid into its Optically Active Components, 311
Putnam (Prof.), Archæological Work in America, 474
Pyrenees, Racial Dwarfs in the, R. G. Haliburton, 294; Wm. McPherson, 294
Pyrometry: Experiments to Determine Temperature of Flame of Water-gas, E. Blass, 113

Qualitative Chemical Analysis, Notes on, P. Lakshmi Narasu Nayudu, 100
Qualities, Physical, Williams on the Dimensions of, Dr. Burton, Prof. A. Lodge, Mr. Boys, W. Baily, Mr. Swinburne, Mr. Williams, 116
Quarterly Journal of Microscopical Science, 260, 524
Quaternions, Alex. McAulay, 151
Quaternions and the Algebra of Vectors, Prof. J. Willard Gibbs, 463
Quaternions, Vectors *versus*, Oliver Heaviside, F.R.S., 533
Quetta Railway Constructors, Troubles of the, 325
Quetta, Earthquake at, 470

Rabbit, on an Abnormality in the Veins of the, Prof. W. N. Parker, 270
Rabbit Tamed, a Wild, Helen J. Murray, 86
Rabbit, Unusual Origin of Arteries in the, Philip J. White, 365
Race in Anthropology, M. Topinard on, 524
Racial Dwarfs in the Pyrenees, R. G. Haliburton, 294; Wm. McPherson, 294
Radiation (of Heat), Comparison of Formulæ for Total, W. de C. Stevens, 188
Radiolaria, the Rising and Sinking Process in the, Herr Verworn, 397
Railway Constructors, Troubles of the Quetta, 325
Railway, Paddington and Clapham Junction, 515
Railway, the South Kensington Laboratories and, 494
Railways: Mr. Robert Dundas on Improvements in Rolling Stock, 131
Railways in China, 400
Railways, Electrical, Dr. Edward Hopkinson, 570
Rain, Superabundant, Sir H. Collett, 247
Rainbow, Lunar, in the Highlands, 342
Rainfall, a Remarkable, Alfred O. Walker, 31
Raisin (Catharine A.), Variolite of the Lleyn and associated Volcanic Rocks, 334
Ramage (H.), Manganese Borate, 239
Rambaut (Prof. A.), Measurement of Distances of Binary Stars, 226; Relative Position in Cluster χ Persei, 376
Ramsay (Prof.), Mr. Sutherland's Paper on the Laws of Molecular Force, 117
Ramsay (W.), Atomic Weight of Boron, 165
Rance (Chas. E. De), Fossil Plants as Tests of Climate, 294, 342
Rankin's (Mr. D. J.) Zambesi Journey, 64
Ranvier (M. L.), Microscopic Researches on the Contractility of the Blood-Vessels, 312; the Clasmatocytes, the Fixed Cellules of the Connective Tissue, and the Pus Globules, 408
Raps (Dr. August), Automatic Mercurial Air-Pumps, 369; Photographic Registration Apparatus, 503
Rayleigh (Lord, F.R.S.), the Densities of the Principal Gases, 567
Reade (T. Mellard), Ancient Ice Ages, 174
Rebeur-Paschwitz (Dr. E. von), the Horizontal Pendulum, 519
Reduction of Tidal Observations, Prof. G. H. Darwin, F.R.S., 402
Reflection, the Photography of an Image by, Frederick J. Smith, 10
Reflector, the, with the Projection Microscope, G. B. Buckton, F.R.S., 54
Registering Instruments or Indicators, General Conditions to be fulfilled by, M. A. Blondel, 599
Reid (Clement), a Fossiliferous Pleistocene Deposit at Stone on the Hampshire Coast, 502
Reliquary, the Quarterly Archæological Journal and Review, 7
Research, Berlin Academy Grants in Aid of, 586
Research, Universities and, Prof. George Francis Fitzgerald, 100
Rive (M. de la), Improvement on the Herz Oscillator, 184
Reveries of a Naturalist, W. H. Hudson, Dr. Alfred Russel Wallace, 483

Reviews and Our Bookshelf:—

The Study of Animal Life, J. Arthur Thompson, 2
Principles of the Algebra of Vectors, A. Macfarlane, 3
Le Léman. Monographie Limnologique, F. A. Forel, Prof. T. G. Bonney, F.R.S., 5
Horn Measurements and Weights of the Great Game of the World, being a Record for the Use of Sportsmen and Naturalists, Rowland Ward, 6
Der Peloponnes. Versuch einer Landeskunde auf Geologischer Grundlage, Dr. Alfred Philippson, 6
Traité Encyclopédique de Photographie, Charles Fabre, 6
The Reliquary: Quarterly Archæological Journal and Review, 7
Experimental Evolution, Henry de Varigny, 25
British Fungus Flora, a Classified Text-book of Mycology, George Massee, 26
Marine Shells of South Africa, G. B. Sowerby, 27
The Framework of Chemistry, W. M. Williams, 28
The Beauties of Nature and the Wonders of the World We Live in, Right Hon. Sir John Lubbock, Bart, F.R.S., 28
Algebra for Beginners, by H. S. Hall and S. R. Knight, 28
Introduction to Physiological Psychology, Dr. Theodor Ziehin, 28
Geological Map of Scotland, Sir Archibald Geikie, F.R.S., Prof. A. H. Green, F.R.S., 49
Medical Microscopy, F. J. Wethered, Dr. A. H. Tubby, 51
Odorographia; a Natural History of Raw Materials and Drugs used in the Perfume Trade, J. C. Sawer, 52
Catalogue of Eastern and Australian Lepidoptera Heterocera in the Collection of the Oxford University Museum, Col. C. Swinhoe, 53
Charles Darwin: His Life Told in an Autobiographical Chapter and in a Selected Series of his Published Letters, 53
Strange Survivals: Some Chapters in the History of Man, S. Baring-Gould, 53
Animals' Rights, H. S. Salt, 73
A Description of the Laws and Wonders of Nature, Richard A. Gregory, 74
A Handybook for Brewers, H. E. Wright, 75
A Manual for Veterinary Physiology, Vet.-Captain F. Smith, 76
The Principal Starches Used as Food, Wm. Griffiths, 76
Les Alps Françaises, Albert Falsan, 76
Chemical Lecture Experiments, G. S. Newth, Sir H. E. Roscoe, F.R.S., 97
A Manual of Photography, A. Brothers, 98
Matriculation Chemistry, Temple Orme, 99
Vegetable Wasps and Plant Worms: a Popular History of Entomogenous Fungi, or Fungi Parasitic upon Insects, M. C. Cooke, 99
Notes on Qualitative Chemical Analysis, by P. Lakshmi Narasu Nayudu, 100
Science Instruments, 100
In Savage Isles and Settled Lands, B. F. S. Baden-Powell, 122
Property, its Origin and Development, Chas. Letourneau, 123
Outlines of Organic Chemistry, C. J. Leaper, 124
An Introduction to the Study of Botany, with a Special

Chapter on Some Australian Natural Orders, Arthur Dendy and A. H. S. Lucas, 125
A German Science Reader, Francis Jones, 125
More About Wild Nature, Mrs. Brightwen, 125
Elements of Human Physiology, E. H. Starling, 146
Elementary Manual on Applied Mechanics, Prof. Jamieson, 147
Man and the Glacial Period, G. F. Wright, 148
Beetles, Butterflies, Moths, and other Insects, A. W. Kapple and W. E. Kirby, 148
Ostwald's Klassiker der Exakten Wissenschaften, Nos. 31–37, 149
The Migration of Birds: an Attempt to Reduce Avian Season-Flight to Law, Charles Dixon, 169
Domestic Electric Lighting treated from the Consumer's Point of View, E. C. De Segundo, 172
Grasses of the Pacific Slope, including Alaska and the Adjacent Islands, Dr. George Vasey, 173
Aids to Experimental Science, Andrew Gray, 173
Science in Arcady, Grant Allen, 173
The Visible Universe, J. Ellard Gore, A. Taylor, 193
On the American Iron Trade and its Progress during Sixteen Years, Sir Lowthian Bell, F.R.S., John Parry, 195
The Fauna and Flora of Gloucestershire, Charles A. Witchell and W. Bishop Strugnell, 197
The Chemistry of Life and Health, C. W. Kimmins, 198
Naked-Eye Botany, with Illustrations and Floral Problems, F. E. Kitchener, 198
The Great World's Farm: Some Account of Nature's Crops and How they are Grown, Selina Gaye, 198
Sound and Music, Rev. J. A. Zahn, 222
Atlas der Völkerkunde, Dr. George Gerland, Dr. Edward B. Tyler, F.R.S., 223
Castorologia, or The History and Traditions of the Canadian Beaver, Horace Martin, 224
An Atlas of Astronomy, Sir R. S. Ball, F.R.S., 225
Modern Mechanism, 241
A Contribution to Our Knowledge of Seedlings, Rt. Hon. Sir John Lubbock, Bart., F.R.S., Dr. Maxwell T. Masters, F.R.S., 243
Epidemic Influenza: A Study in Comparative Statistics, F. A. Dixey, 244
An Elementary Text-Book of Hygiene, H. Rowland Wakefield, 245
Ostwald's Klassiker der Exakten Wissenschaften, Nos. 38—40, 245
Das Keimplasma. Eine Theorie der Vererbung, August Weismann, 265
The Algebra of Co-planar Vectors and Trigonometry, R. Baldwin Hayward, F.R.S., 266
Fossil Plants as Tests of Climate, being the Sedgwick Prize Essay for the Year 1892, A. C. Seward, J. Starkie Gardner, 267
Pioneers of Science, Oliver Lodge, F.R.S., 268
Electric Lighting and Power Distribution, W. Perren Maycock, 269
The Naturalist on the River Amazons, Henry Walter Bates, F.R.S., 269
Theory of Numbers, G. B. Mathews, 289
Darwin and after Darwin: an Examination of the Darwinian Theory, and a Discussion of Post-Darwinian Questions, George John Romanes, F.R.S., 290
The Ferns of South Africa, containing Descriptions and Figures of the Ferns and Fern-allies of South Africa, Thomas R. Sim, J. G. Baker, F.R.S., 291
Newcomb-Engelmann's Populäre Astronomie, Zweite vermehrte Auflage, Dr. H. C. Vogel, 291
The Hemiptera Heteroptera of the British Islands, Edward Saunders, 292
Physical Education, Frederick Treves, 292
A Text-book of Tropical Agriculture, H. A. Alford Nicholls, 313
Die Zelle und Die Gewebe, Grundzüge der allgemeinen Anatomie und Physiologie, Prof. Dr. Oscar Hertwig, 314
Elementary Mechanics of Solids and Fluids, A. L. Selby, 315
Magnetism and Electricity, R. W. Stewart, 315
Manners and Morals of Prehistoric Peoples, Marquis de Nardaillac, 316
The Milky Way from the North Pole to 10° of South Declination, drawn at the Earl of Rosse's Observatory at Birr Castle, Otto Boeddicker, 337
The Theory of Substitutions and its Applications to Algebra, Dr. Eugen Netto, 338
Das Centralnervensystem von Protopterus annectens; eine vergleichend Anatomische Studie, Dr. Rudolf Bu[illegible] 339
The Chemical Basis of the Animal Body, A. Sheridan Lee, F.R.S., 340
Chambers's Encyclopædia, vol. x., 340
Arthur Young's Tour in Ireland, 1776–79, 341
Qualitative Analysis Tables and the Reactions of certain Organic Substances, E. A. Letts; Chapman Jones, 361
Gemeinverständliche Vorträge aus dem Gebeite der Physic, Prof. Dr. Leonhard Sohncke; Dr. James L. Howard, 362
A Catalogue of British Gasteropoda, W. H. Hudleston, F.R.S., and Edward Wilson, H. Woods, 363
The Year Book of the Imperial Institute of the United Kingdom, the Colonies and India, 363
Beneath Helvellyn's Shade, Samuel Barber, 364
Evolution and Man's Place in Nature, Henry Calderwood, 385
Théorie Mathématique de la Lumière, H. Poincaré; A. B. Basset, F.R.S., 386
The Fauna of British India including Ceylon and Burma, 387
The Year-book of Science (1892), 388
Treatise on Thermodynamics, Peter Alexander, 388
Mediæval Lore: an Epitome of the Science, Geography, Animal and Plant Folklore and Myth of the Middle Ages, Robert Steele, 388
Astronomy for Every-day Readers, B. J. Hopkins, 389
The Microscope: its Construction and Management, Dr. Henri van Heurck, 409
The Earth's History: an Introduction to Modern Geology, R. D. Roberts, 412
The Health Officer's Pocket-book, E. F. Willoughby, 412
Engler's Botanische Jahrbücher fur Systematik, Pflanzengeschichte und Pflanzengeographie, 413
Descriptive Geometry Models for the Use of Students in Schools and Colleges, I. Jones, 413
Théorie du Soleil, A. Brester, 433
A Course of Practical Elementary Biology, John Bidgood, 434
Stéréochimie, J. H. van't Hoff, 436
Die Fossile Flora der Höttinger Breccie, R. Von Wettstein, 436
Observational Astronomy, Arthur Mee, 437
Mechanics and Hydrostatics for Beginners, S. L. Loney, 437
A Vertebrate Fauna of Lakeland, including Cumberland and Westmorland, with Lancashire North of the Sands, Rev. H. A. Macpherson, 457
Die Entwickelung der Doppelstern-Systeme, T. J. J. See, Prof. G. H. Darwin, F.R.S., 459
Magnetic Induction in Iron and other Metals, J. A. Ewing, F.R.S., E. Wilson, 460
Forschungsberichte aus der Biologischen Station zu Plön, Dr. Otto Zacharias, 461
The British Journal Photographic Almanac for 1893, J. Traill Taylor, 462
Studies in Corsica, John Warren Barry, 462
Coal Pits and Pitmen: a Short History of the Coal Trade and the Legislation affecting it, R. Nelson Boyd, 481
Idle Days in Patagonia, W. H. Hudson, Dr. Alfred Russel Wallace, 483
Ueber das Verhalten des Pollens und die Befruchtungsvorgänge bei den Gymnospermen, Eduard Strasburger, 484
Autres Mondes, Amédée Guillemin, 485
Some Lectures by the late Sir George E. Paget, K.C.B., F.R.S., 485
Electrical Papers, Oliver Heaviside, 505
The Great Sea-Serpent, A. C. Oudemans, 506
A Treatise on Public Health and its Applications in Different European Countries, Albert Palmberg, M.D., Dr. H. Brock, 507
The English Flower Garden, W. Robinson, 508
Logarithmic Tables, Prof. George William Jones, 508
Catalogue of the British Echinoderms in the British Museum, F. Jeffrey Bell, 508
Treatise on the Mathematical Theory of Elasticity, A. E. H. Lowe, Prof. A. G. Greenhill, F.R.S., 529

Text-book of Elementary Biology, H. J. Campbell, 530
Contribution à l'Étude de la Morphologie et du Développement des Bactériacées, A. Billet, Dr. Rubert Boyce, 532
Introductory Modern Geometry of Point, Ray, and Circle, W. B. Smith, 532
Primer of Horticulture, J. Wright, 533
Ornithology in Relation to Agriculture and Horticulture. John Watson, Walter Thorp, 533
La Planète Mars et ses Conditions d'Habitabilité, Camille Flammarion, William J. S. Lockyer, 553
Magnetische Beobachtungen auf der Nordsee angestellt in den Jahren 1884 bis 1886, 1890, und 1891, A. Schück, 555
Manual of Dairy Work, James Muir, Walter Thorp, 555
William Gilbert, of Colchester, Physician of London, on the Loadstone and Magnetic Bodies, and on the Great Magnet the Earth, P. Fleury Mottelay, 556
Report on Manurial Trials, Dr. William Somerville, 556
The Food of Plants, A. P. Laurie, 556
Text-book of Comparative Geology, E. Kayser, 578
Der Nord-Ostsee Kanal, C. Beseke, 579
Laws and Properties of Matter, R. T. Glazebrook, F.R.S., 580
The Partition of Africa, J. Scott Keltie, 580
Forest Tithes and other Studies from Nature, by a Son of the Marshes, 580
Waterdale Researches: Fresh Light on Dynamics, Waterdale, 601
Textbook of Biology, H. G. Wells, 605
Pflanzenleben, Anton Kerner von Marilaun, 605
Bibliografica Medica Italiana, P. Giacosa, 606
The Evolution of Decorative Art, Henry Balfour, 606
Reyer (Prof. E.), Experiments on Folding and on the Genesis of Mountain Ranges, 81
Reymond (Dr. R. du Bois), Sensation of Warmth on Immersing Hand in Carbon Dioxide, 144
Rhodes (Cecil), Proposed Exploration of Africa by Telegraph, 210
Ricci (Admiral Marquis), Munificent Bequest for Founding Scientific Institution in Genoa by, 613
Ricco (M.), Sun-spots and Magnetic Perturbations in 1892, 352; Fumo di Vulcano Veduto dall' Osservatorio di Palermo durante l'eruzione del 1889, 428; La Grandissima Macchia Solare del Febbrajo 1892, 429; Stromboli, 453
Richards (J. T.), Arborescent Frost Patterns, 162
Richer (Paul), the Relation of Anatomy to Art, 470
Ridley (Sir M. W.), on Technical Education, 130
Rifles, the Making of, John Rigby, 163
Rights, Animals', H. S. Salt, 73; the Reviewer, 151
Rimington (J. W.), Experiments in Electric and Magnetic Fields, Constant and Varying, 165
Rising and Setting of Stars, the, 519
Rix (W. P.), Pottery Glazes, 396
Roberts (J. W.). Comet Holmes, 256
Roberts (R. D.), the Earth's History: an Introduction to Modern Geology, 412
Roberts (S., F.R.S.), Certain General Limitations affecting Hyper-magic Squares, 71
Roberts (T.), Notes on the Geology of the District West of Caermarthen, 407
Roberts-Austen (Prof. W. C., F.R.S.), the Alloys Research Committee, Second Report, 617; the Action of Bismuth on Copper, 618
Robertson (Dr. W. G. Aitchison), on the Madder-staining of Dentine, 287
Robinson (W.), the English Flower Garden, 508
Roche's Limit, 509; Prof. G. H. Darwin, F.R.S., 581
Rockhill (E. Woodville), Mongolia and Central Tibet, 426
Rodger (J. W.), Osmotic Pressure, 103
Roger (E.), the Fifth Satellite of Jupiter, 71
Romanes (Dr. Geo. J., F.R.S.), a Criticism on Darwin, 127; Darwin and after Darwin, 290
Rome, Solar Observations at, Prof. Tacchini, 304, 399, 565
Roots, Travelling of, W. T. Thiselton-Dyer, F.R.S., 414
Roozeboom (Bakhuis), on the Solubility Curve for Systems of Two Bodies, 288; Cryohydrates in Systems of Two Salts, 624
Rorie (Dr. James), a Brilliant Meteor, 495
Roscoe (Sir Henry E., F.R.S.), Chemical Lecture Experiment, 97; the Manchester Municipal Technical School, 201; Technical Education in Birmingham, 301
Roscoff Laboratory, on the Attempt at Oyster Culture in the, M. de Lacaze-Duthiers, 456
Rosenberg (Herr), Influence of Bodily Exertion on Digestive Process, 62
Rothamsted Agricultural Experiments, Proposed Commemoration of the Jubilee of Sir John Lawes, 448
Rotch (A. L.), Meteorological Balloon Ascent at Berlin, October 24, 1891, 46
Routh (Dr. E. J., F.R.S.) Discovery of the Potential, 510
Rowland (Prof. H. A.), A New Table of Standard Wave-lengths, 590
Royal College of Science, the Proposed New Buildings for the, Mr. Shaw-Lefevre, 448
Royal Dublin Society, 287, 431
Royal Geographical Society, 65, 89, 115, 209; Women not to be admitted as Fellows, 617; Medal Awards, 617
Royal Meteorological Society, 118, 286, 333, 430, 502, 623
Royal Microscopical Society, 47, 118, 359, 501, 526
Royal Society, 37, 94, 164, 182, 189, 237, 261, 310, 331, 358, 381, 429, 476, 596, 621; Medal Awards, 60; Anniversary of the, 106: Anniversary Dinner of the, 134; Criticism of the, 145
Royal Society of New South Wales, 311, 335
Rubens (H.), a Modified Astatic Galvanometer, 455
Ruby, Valuable, discovered at Burma Mines, 586
Rücker (Prof.), Williams on the Relation of the Dimensions of Physical Quantities to Directions in Space, 69
Ruhemann (S.), the Formation of Benzyldihydroxypyridine from Benzylglutaconic Acid, 311
Rule for Finding the Day of the Week Corresponding to any given Day of the Month and Year, a Simple, 509
Ruoss (H.), Contrivance for Determining Refractive Index of Liquid, 544
Russell (H. C., F.R.S.), Curious Drift of a Current Bottle, 131; Physical Geography and Climate of New South Wales, 258; Moving Anti-Cyclones in the Southern Hemisphere, 286; Hail Storms, 573; Notes of two Photographs of Lightning taken at Sydney Observatory, December 7, 1892, 623
Russell (H. L.) on the Bacterial Investigation of the Sea and its Floor, 285
Russell (Hon. R.), Dew and Frost, 210
Russia: Attempted Silk Production in South Russia, 184; the Pinsk Marshes and non-Russian Atlases, M. Venukoff, 282; Constitution of the Quaternary Deposits in Russia and their Relations to the Finds Resulting from the Activity of Prehistoric Man, S. Nikitine, 523; Russian Steppes Past and Present, Prof. W. W. Dokoutchaïev, 523; Which is the most Ancient Race in Russia? Prof. A. Bogdanov, 524; Prehistoric Ethnography of Central and North-east Russia, J. Smirnov, 524
Ruthenium, M. Joly, 451
Rutherfurd Measures of Stars about β-Cygni, Prof. Harold Jacoby, 77
Rutley (Frank) on the Dwindling and Disappearance of Limestones, 623

Sabatier (Paul) on Nitrogenised Copper, 600
Sacken (Baron C. R. Osten), Iridescent Colours, 102
Sacred Nile, the, J. Norman Lockyer, F.R.S., 464
Sailors, Defective Vision in, Association of Shipping Disasters with, Dr. T. H. Bickerton, 17
Saint-Martin (M. L. de) on the Mode of Elimination of Carbonic Oxide, 384
Salamander from North America, a New Blind Cave, L. Stejneger, 62
Salinity of Great Salt Lake, Utah, 302
Salis (M. de), the Regulation of Swiss Torrents, 377
Salisbury (Lord), Medical Education at Oxford, 449
Salt (H. S.), Animals' Rights, 73, 127
Salter (J. W.), Polecat not Extinct in Cardiganshire, 450
Samothrace, Earthquake in, 372
Sanderson (Prof. Burdon), Electric Currents in Plants, 255
Sanderson (F. W.), Science Teaching, 358
Sandgate, Landslip at, 449; Prof. J. F. Blake, 467; Rev. Dr. Irving, F.R.S., 581
Sanitarian's Travels, a, Robert Boyle, 105
Sanitary Appliances, Hornsey Local Board Museum of, 587
Sanitary Convention Signed, International, 585

Sanitation: a Treatise on Public Health, Dr. Albert Palmberg, Dr. H. Brock, 507
Sarasin (M.), Improvement on the Herz Oscillator, 184
Satellite, Jupiter's Fifth, A. A. Common, 208; E. E. Barnard, 377
Satellites, Jupiter's, the Sizes of, J. J. Landerer, 473
Satellites, Jupiter and his, Prof. Pickering, 518
Saunders (Edward), the Hemiptera Heteroptera of the British Islands, 292
Savage Isles, and Settled Lands in: Malaysia, Australasia, and Polynesia, 1888–1891, B. F. S. Baden-Powell, 122
Saville (M. H.), the Destruction of Ancient Monuments in Central America, 302
Sawer (J. Ch.), Odorographia: a Natural History of Raw Materials and Drugs used in the Perfume Industry, 52
Sawyer (Edwin), Catalogue of Southern Star Magnitudes, 589
Schæberle (Prof.), the Markings on Mars, 209; Photographic Absorption of our Atmosphere, 304
Schaffhausen, Discovery of Prehistoric Drawings on Limestone near, J. Naue, 279
Scheele (Carl Wilhelm), Prof. T. E. Thorpe, F.R.S., 152; Proposed Testimonial to, 37
Scheiner (Dr. J.), Astronomy of the Invisible, 88; the Large Nebula near ξ Persei (N. G. C., 1499), 546
Schenck (Dr. H.), Climbing Plants, W. Botting Hemsley, F.R.S., 514
Schiedt (Prof. R. C.), Observations on Oysters, 375
Schiseophone, the, M. de Place, 23
Schleswig-Holstein, Distribution of Population of, Dr. A. Gloy, 499
Schlick (E. Otto), an Apparatus for Measuring and Registering Vibrations of Steamers, 521
Schmidt (A.), Piperazine, 430
Schonheyder (Mr.), Experiments on the Value of the Steam Jacket, 20
School Gradation Committee, Formation of, 613
Schorr (Prof. R.), Comet Barnard (October 12), 18; Comet Holmes, 326
Schubert (Dr.), Recent Researches on the Influence of Forests, 480
Schück (A.), Magnetische Beobachtungen auf der Nordsee angestellt in den Jahren 1884 bis 1886, 1890 und 1891, 555
Schulhof (M.), Comet Holmes, 256, 451, 498
Schulz (Dr.), Photography first Discovered by, 336
Schumann (Herr Victor), Hydrogen Line Hβ in the Spectrum of Nova Aurigæ, 425
Schunck (Edward, F.R.S.), Notes on some Ancient Dyes, 22
Schuster (Prof. Arthur, F.R.S.), Dr. Joule's Thermometers, 364
Schwatka (Lieut. Frederick), Death of, 65; the Cause of Death of, 89
Science: Scientific Titles; Abuse of Letters indicating Membership of Societies, Prof. Tilden, 15; Science and Brewing, H. E. Wright, 75; Science Instruments, 100; a German Science Reader, Francis Jones, 125; Laying Foundation Stone of Newcastle College of Science, 129; Science in Arcady, Grant Allen, 173; Aids to Experimental Science, Andrew Gray, 173; Scientific Worthies—Sir Archibald Geikie, Prof. A. de Lapparent, 217; Shaking the Foundations of Science, 220; Pioneers of Science, Oliver J. Lodge, F.R.S., 268; Science Teaching, F. W. Sanderson, Prof. A. M. Worthington, L. Cumming, Dr. Stoney, W. B. Croft, Prof. Ayrton, E. J. Smith, Dr. Gladstone, 358; the Year-Book of Science (for 1892), 388; the proposed New Buildings for the Royal College of Science, Mr. Shaw-Lefevre, 448; Forthcoming Scientific Books, 453; Science in the Public Schools and in the Scientific Branches of the Army, 513; Durham College of Science, Appeal for Relief from Financial Difficulties, 585
Sclerometer, a New, Paul Jannetaz, 564
Scotland: Geological Map of Scotland, Sir Archibald Geikie, F.R.S., Prof. A. H. Green, F.R.S., 49; the Fishery Board for, 85; Geology of, Prof. Grenville A. J. Cole, 101; the Plague of Field Voles in, 155; Scottish Geographical Society, 159; Macculloch's Geological Map of Scotland, Prof. J. W. Judd, F.R.S., 173; the Mice Pest in Scotland, 396; Scottish Meteorological Society, 469; Report of the Scottish Technical Education Committee, 543; the Colour-Variations of the Voles of Southern Scotland, Robert Service, 587; the Protection of the Osprey in Scotland, 612
Scott (Dr. D. H.), Teaching of Botany, 228
Screen, a Magnetic, Frederick J. Smith, 439
Sea-Anemones, Food-recognising Sense of, Herr Nagel, 185
Sea Fish, the Protection of, C. H. Cook, 396
Sea-Serpent, the Great, Dr. A. C. Oudemans, 506
Sealing Industry, the, 350
Searle (Arthur), Observations of Zodiacal Light, 473
Searle (Rev. E. M.), Comet Holmes, 256
Sedgwick Prize Essay for the Year 1892, Fossil Plants as Tests of Climate, A. C. Seward, J. Starkie Gardner, 267
Sedlaczek (G.), Silk Culture in the Caucasus, 397
See (T. J. J.), Die Entwickelung der Dopplestern-Systeme, Prof. G. H. Darwin, F.R.S., 459
Seedlings, a Contribution of Our Knowledge of, Sir John Lubbock, F.R.S., Dr. Maxwell T. Masters, F.R.S., 243
Seeds, Foods, Dangers of Adulteration with Corn-Cockle of, 185
Segundo (E. C. De), Domestic Electric Lighting, Treated from the Consumer's Point of View, 172
Seismograph, a New, Dr. H. J. Johnston-Lavis, 257
Sulby (A. L.), Elementary Mechanics of Solids and Fluids, 315
Selous (F. C.), Twenty Years in Zambesia, 377
Senderens (J. B.), on Nitrogenised Copper, 600
Sensitiveness of the Eye to Light and Colour, Capt. W. de W. Abney, F.R.S., 538
Sergent (C. S.), The Silva of North America, Prof. W. R. Fisher, 275
Servia, Earthquake in, 562
Service (Robert), the Colour-Variations of the Voles of Southern Scotland, 587
Seven Images of the Human Eye, M. Tcherning, 354
Sewage Water, Filtered, Favourable to Fish Life, 350
Seward (A. C.), Fossil Plants as Test of Climate, being the Sedgwick Prize Essay for the Year 1892, J. Starkie Gardner, 267; on a New Fern from the Coal Measures, 360
Sexual Differences of Colour in Eclectus, the Cause of the, Prof. A. B. Meyer, 486
Shaking the Foundations of Science, 220
Shamrock? What is the True, Nathaniel Colgan, R. L. Praeger, 302
Sharp (Dr.), on Stridulating Ants, 501
Shaw-Lefevre (Mr.), the proposed New Buildings for the Royal College of Science, 448
Shea (D.), Refraction and Dispersion of Light in Metal Prisms, 68
Shells, Marine, of South Africa, G. B. Sowerby, 27
Shenstone (W. A.), the Preparation of Phosphoric Oxide Free from the Lower Oxide, 430; Note on the Preparation of Platinous Chloride and on the Interaction of Chlorine and Mercury, 479
Shepheard (W. F.), the Identity of Caffeine and Theine and the Interactions of Caffeine and Auric Chloride, 311
Sherwood (William), Glaciers of Val d'Herens, 174
Shuswap Indians (British Columbia), Lizard Superstition of, Dr. George Dawson, F.R.S., 184
Shuswaps of British Columbia, Superstitions of the, Col. C. Bushe, 199
Shield (Mr.), the Screw Propeller, 21
Shinn (Miss Milicent W.), the Lick Observatory, 209
Shipping Disasters, Association with Defective Vision in Sailors of, Dr. T. H. Bickerton, 16
Siberia, Further Researches in, M. Obrutcheff, 255
Siemens Platinum Foil Unit as a Standard for the intensity of a Source of Light, Researches at the Berlin Imperial Physico-Technical Institute on the, 615
Siemens (A.), the Distribution of Power by Electricity from a Central Generating Station, 378
Siemens (Dr. Werner), Death of, 129; Obituary Notice of, 153
Sight, Motion in the Line of, M. H. Deslandres, 88
Silk Production in South Russia, Attempted, 184
Silk Culture in the Caucasus, G. Sedlaczek, 397
Silva of North America, the, C. S. Sergent, Prof. W. R. Fisher, 275
Sim (Thos. R.), the Ferns of South Africa, J. G. Baker, F.R.S., 291
Sims (W. E.), Thionyl Bromide, 405
Skertchly (S. B. J.), Remarkable Cold Wave over China, 516
Slate Pencils, Aluminium, 131
Slöjd Association of Great Britain, 324
Sloths, on the Presence of a distinct Coracoidal Element in Adult, R. Lydekker, 431

Smirnov (J.), Prehistoric Ethnography of Central and North-east Russia, 524
Smith (B. Woodd), Ice Crystals, 79
Smith (E. J.), Science Teaching, 359
Smith (Vety.-Capt. F.), A Manual of Veterinary Physiology, 76
Smith (Frederick J.), the Photography of an Image by Reflection, 10; a Magnetic Screen, 439
Smith (Dr. J. Lorrain), the Thyroid Gland (Experiments on Cats), 167
Smith (W. B.), Introductory Modern Geometry of Point, Ray, and Circle, 532
Smith (Wythe), Experiments in Electric and Magnetic Fields, Constant and Varying, 165
Smith (Xanthus), American Opinion of Photography in England, 86
Snake Laboratory, the proposed Calcutta, 253
Snakes in India, Mortality from, 157
Snakes in Thatch (Burmah), 113
Snow-Rollers, W. S. Ford, 422
Snow (B. W.), Infra-red Spectrum of Alkali Metals, 39
Soap Bubbles, Permanent, formed with a Resinous Soap, M. Izarn, 119
Society of Arts, Opening Meeting of, 63
Sohncke (Prof. Dr. Leonhard), Gemeinverständliche Vorträge aus dem Gebeite der Physic, Dr. James L. Howard, 361
Solid Body, Motion of a, in a Viscous Liquid, A. B. Basset, F.R.S., 512
Solids and Fluids, Elementary Mechanics of, A. L. Selby, 315
Solar Corona, a New Method of Photographing the, M. H. Deslandres, 327
Solar Eclipse of April, 15-16, 1893, 304, 376, 584, 611; M. de la Baume Pluvinel, 304; A. Taylor, 317
Solar Observations at Rome, Prof. Tacchini, 304, 399, 565
Solar Photographs, Prof. Hale's, 498
Solar and Terrestrial Phenomena, Coincidence of, Prof. G. E. Hale, 425
Solar System, Motion of the, Prof. J. G. Porter, 41
Sollas (Prof., F.R.S.), Growth of Crystals, 213; on the Variolite and associated Igneous Rocks of Roundwood, co. Wicklow, 287; Pitchstone and Andesite from Tertiary Dykes in Donegal, 287
Somerville (Lieut Boyle T.), on some Islands of the New Hebrides, 455
Somerville (Dr. William), Report on Manurial Trials, 556
Soot-figures on Ceilings, E. B. Poulton, F.R.S., 608; Prof. Oliver Lodge, F.R.S., 608
Soudan and Sahara Journey, Completion of Capt. Monteil's, 89
Soudanese, Use of Chloride of Potassium instead of Salt by, M. Dybowski, 499
Sound and Music, Rev. J. A. Zahm, 222
South Kensington Laboratories and Railway, the, 494
Southwell (T.), Sowerby's Whale on the Norfolk Coast, 349
Sowerby (G. B.), Marine Shells of South Africa, 27
Spain, Practical Meteorology in, 543
Spain, Wild, Abel Chapman and Walter J. Buck, 583
Spectrum Analysis: Coloured Photographs of the Spectrum, G. Lippmann, 23; Spectroscopy, Infra-Red Emission Spectrum of Alkali Metals, B. W. Snow, 39; Existence of Distinct Nervous Centres for Perception of Fundamental Colours of Spectrum, A. Chauveau, 143; the Height and Spectrum of Auroras, T. W. Backhouse, 151; Ultra-Violet Spectrum in Prominences, Prof. G. E. Hale, 186; Spectra of Various Orders of Colours in Newton's Scale, W. B. Croft, 190; Method of Observing and Separating Spectra of easily Volatile Metals and their Salts, W. N. Hartley, 239; Method of Producing Intense Monochromatic Light, Dr. Dubois, 255; on the Photographic Spectra of some of the Brighter Stars, J. Norman Lockyer, F.R.S., 261: Hydrogen Line H β in the Spectrum of Nova Aurigæ, Herr Victor Schumann, 425; Spectra of Planetary Nebulæ and Nova Aurigæ, M. Eugen Gothard, 352; Spectrum of β Lyrae, Prof. Keeler, 616
Spencer (Prof. W. Baldwin), on the Anatomy of *Pentastomum teretiusculum* 260
Spengler (Dr.), Record of Medical Experience at Davos Platz, 517
Sphenophyllum, the Genus, Prof. William Crawford Williamson, 11
Spider, Notes on a, H. H. J. Bell, 557
Spider, the Trap-door, D. Cleveland, 375
Spirit Spring Mound, the Great, Kansas, E. H. S. Bailey, 87
Sponge and Annelid, a Strange Commensalism, James Hornell, 78
Sponge? Foraminifer or, R. Hanitsch, 365, 439; F. G. Pearcey, 390
Sportsmen and Naturalists, Horn Measurements and Weights of the Great Game of the World, being a Record for the use of, Rowland Ward, 6
Sprague (T. B.), a New Algebra, 527
Spray Clouds, the Niagara, Charles A. Carus-Wilson, 414
Springer (Herr Julius), Astronomical Instruments up to Date, 114
Sprung (Prof.), Observations made at Potsdam Meteorological Institute on the Recent Coldest Day in January, 480
Squarey (E. P.), Yew Poisoning, 285
Stael (J. M.), Wood-ashes as a Medicine for Farm Animals, 397
Stainton (H. T.), Death of, 155
Standard Pound, the Imperial, Decrease in Weight of, 86
Standard Barometry, Dr. Frank Waldo, 511
Standards, Electrical, 128
Stanford's Map of County of London, 40
Stanley (W. F.), the Perception of Colour, 381
Starches used as Food, the Principal, W. Griffiths, 76
Starling (E. H.), Elements of Human Physiology, 146
Stars: Rutherfurd Measures of Stars about β Cygni, Prof. Harold Jacoby, 77; the Stars and the Nile, Captain H. G. Lyons, 101; the New Star in the Constellation of Auriga, W. J. Lockyer, 137; the Star of Bethlehem, J. H. Stockwell, 177; Measurement of Distances of Binary Stars, C. E. Stromeyer, 199; Prof. A. Rambaut, 226; on the Photographic Spectra of some of the Brighter Stars, J. Norman Lockyer, F.R.S., 261; Burnham's Double-Star Observations 281; the Milky Way, Dr. Otto Boeddicker, 337; Relative Position of Stars in Cluster χ Persei, Sir Robert Ball and Arthur Rambaut, 376; the Star Catalogue of the Astronomische Gesellschaft, 399; Distribution of Stars in Space, Prof. J. C. Kapteyn, 432; Die Entwickelung der Doppelstern-Systeme, T. J. J. See, Prof. G. H. Darwin, F.R.S., 459; the Rising and Setting of Stars, 519; Catalogue of Southern Star Magnitudes, Edwin Sawyer, 589; Distance of the Stars by Doppler's Principle, G. W. Colles, jun., 596
Stas (Jean Servais), Proposed Memorial to, 182
Statements, Two, Right Hon. T. H. Huxley, F.R.S., 316
Statistics of Average Life in France, M. Turquan, 255
Statistics of Survivors of the Napoleonic Wars, M. Turquan, 233
Steam Engine Trials, 594
Steam Jacket, Experiments on the value of the, J. G. Mair-Rumley, 19; Col. English, Prof. Unwin, Bryan Donkin, Bryan Donkin, jun., and Messrs. Day, Morrison, and Schonheyder, 20
Steamers, Ice-breaking, 350
Steel, the Use of Tungsten in Improving Hardness of, 351
Steel, the Value of Annealing, E. G. Carey, 397
Steel, Volumetric Method for Determining Amounts of Chromium in, G. Giorgis, 397
Steel (Thomas), Zoological Gardens in Great Britain and Australia, 496; Zoological Gardens in Europe and Australia, 587
Stejreger (L.), a New Blind Cave Salamander from North America, 62
Stellar Magnitudes in Relation to the Milky Way, Prof. Kapteyn, 64
Stereochemistry, J. H. Van't Hoff, 436; Prof. Percy F. Frankland, F.R.S., Prof. F. R. Japp, F.R.S., 510
Stevens (W. de C.), Comparison of Formulæ for Total Radiation (of Heat), 188
Stewart (R. W.), Magnetism and Electricity, 315; the Absolute Thermal Conductivities of Copper and Iron, 599
Stockwell (J. H.), the Star of Bethlehem, 177
Stokes (H. N.), Isolation of Amidophosphoric Acid, 615, 616
Stone Implements in the District of Iaransk, P. Krotov, on Layers of, 524
Stoney (Dr.), Science Teaching, 359
Stonyhurst College Observatory, 450
Storms, Hail, H. C. Russell, F.R.S., 573
Stracey (Lieut.-General, F.R.S.), Harmonic Analysis of Hourly

Observations of Air Temperature and Pressure at British Observatories, 621
Strange Survivals: Some Chapters in the History of Man, S. Baring-Gould, 53
Strasburger (Prof. Edward), Ueber das Verhalten des Pollens und die Befruchtungsvorgänge bei den Gymnospermen, 484
Strenitz (Herr), Power of Hydrogen-absorption of various Metals, 63
Stromboli, L. W. Fulcher, 89; A. Ricco and G. Mercalli, Dr. H. J. Johnston Lavis, 453
Stromeyer, (C. E.), Measurement of Distances of Binary Stars, 199
Strugnell (W. Bishop), the Fauna and Flora of Gloucestershire, 197
Struthers (Dr. John), the Rudimentary Hind-limb of Great Fin-Whale, Humpback and Greenland Right-Whale compared, 588
Struve (Wilhelmus), Centenary of Birth of, 585
Studies in Corsica, John Warren Barry, 462
Study of Animal Life, the, J. Arthur Thomson, 2
Stuhlmann (Dr.), Two Akka Girls brought to Germany by, 470
Stur (Dr. D.), Death of, 206
Substitutions, the Theory of, and its Applications to Algebra, Dr. Eugen Netto, 338
Suchsland on the Micro-organisms of Tobacco Fermentation, 208
Sudborough (J. J.), the Action of Nitrosyl Chloride and of Nitric Peroxide on some Members of the Olefine Series, 430
Sulphuric Acid, the Amide and Imide of, Dr. Traube, A. E. Tutton, 566
Summer, the Weather of, 245, 270
Sumpner (Dr.), Williams on the Relation of the Dimensions of Physical Quantities to Directions in Space, 69; Diffusion of Light, 190; on the Differential Equation of Electric Flow, 574
Sun, Theory of the, Dr. A. Brester, 433
Sunshine, Amy Johnson, 9
Sunshine, Comparative, Bishop Reginald Courtenay, 150
Sunspots, H. Faye, 167
Sunspots and Magnetic Perturbations in 1892, M. Ricco, 352
Sunspots: La Grandissima Macchia Solare del Febbrajo, 1892, A. Ricco, 429
Superabundant Rain, Sir H. Collett, 247
Superstitions of the Shuswaps of British Columbia, Colonel C. Bushe, 199
Surface, the Lunar, 352
Surgery, John Hunter (the Hunterian Lecture), Thomas Bryant, 372
Survivals, Strange, some Chapters in the History of Man, S. Baring-Gould, 53
Suter (H.), Catalogue of the New Zealand Mollusca, 397
Sutherland's Paper on the Laws of Molecular Force, Dr. Young, 70; Prof. Fitzgerald, Dr. Gladstone, S. H. Burbury, Prof. Ramsay, Macfarlane Gray, Prof. Herschel, 117
Swarts (Frédéric), on a New Fluorine-derivative of Carbon, 309
Swinburne (Mr.), Williams on the Dimensions of Physical Quantities, 116; on Messrs. Rimington and Smith's Experiments in Electric and Magnetic Fields, Constant and Varying, 166; on the Differential Equation of Electric Flow, 574
Swinhoe (Col. C.), Catalogue of Eastern and Australian Lepidoptera Heterocera in the Collection of the Oxford University Museum, 53; on the Mimetic Forms of Certain Butterflies of the genus *Hypolimnas*, 429
Swift's Comet, Prof. Barnard, 186
Swift, Comet (A. 1892), A. E. Douglas, 546
Swift's (Messrs.), Aluminium Microscope, G. C. Karop, 47
Swiss Torrents, the Regulation of, M. de Salis, 377
Sydney, Australian Museum, Edgar B. Waite appointed Assistant Curator in, 111
Sydney, Royal Society of New South Wales, 311, 335
Symbolism in Burmah, Developments in Buddhist Architecture and, Major Temple, 46
Symons (C. J., F.R.S.), Colonial Meteorology, 390; Arborescent Frost Patterns, 162
Synchronisation, Problem of Integral, M. A. Blondel, 599

Tabular History of Astronomy to the Year 1500 A.D., Dr. Felix Müller, 18
Tacchini (Prof.), Solar Observations at Rome, 304, 399, 565
Tahiti, a New Luminous Fungus, 157
Tait (Prof. P. G.), Vector Analysis, 225
Tanganyika (Lake), a New Medusa from, R. T. Günther, 563
Tanner (Prof. Lloyd), on Complex Primes formed with the Fifth Roots of Unity, 526
Tarantula, the Bite of the, C. W. Meaden, 184
Tashkend, Tobacco Cultivation at, 86
Tasmania the Paradise of Horticulturists, Sir Edward Braddon, 587
Tasmanians, on the Rude Stone Implements of the, showing them to belong to the Palæolithic or Unground Stage of the Implement-makers' Art, Dr. Tylor, 527
Taylor (A.), the Visible Universe, J. Ellard Gore, 193; the Approaching Total Solar Eclipse, April 15–16, 1893, 317
Tcherning (M.), Seven Images of the Human Eye, 354
Tea Industry, the Development of the Ceylon, D. Trimen, 317
Teaching of Botany, 151; Dr. D. H. Scott, 228
Teall (J. J. H., F.R.S.), Notes on some Coast-sections at the Lizard, 407; on a Radiolarian Chert from Mullion Island, 407
Technical Education, Dr. W. Anderson on, 155; Sir M. W. Ridley on, 130; Industrial School opened at Lucknow, 111; Report of the Scottish Technical Education Committee, 543; the Universities and the County Council, 586; the Cambridge University Extension Movement, 586; Improvements in City and Guild of London Institute Technological Examinations, 612
Technical School, the Manchester Municipal, Sir Henry E. Roscoe, F.R.S., 201
Technological Examinations, 35
Telephone, Demonstration of Existence of interference of Electric Waves in Closed Circuit by means of, R. Colson, 96
Telephones in Warfare, Use of, 182
Telephotographic Lens, the New, T. R. Dallmeyer, 161
Telescope, a Large, 18
Telescopes, Large, 616
Tell-el-Hesy Excavations, the, F. J. Bliss, 302
Temperature and Insolation, Relationship between, Dr. Berson, 24
Temple (Major), Developments in Buddhist Architecture and Symbolism in Burma, 46
Ten Kate (Dr.), on the Type-characteristics of the North American Indians, 374
Tennant (the late Prof.) on Magic Mirrors, Prof. Silvanus P. Thompson, F.R.S., 79
Teredo and Electric Cables, the, Sir Henry Mance, 450
Terra, Quæstio de Aqua et, Edmund G. Gardner, 295
Terrestrial Phenomena, Coincidence of Solar and, Prof. G. E. Hale, 425
Tests of Climate, Fossil Plants as, Charles E. De Rance, 294, 342; J. Starkie Gardner, 364
Tetanus, the Bacteriology of, Kitasato, 158
Texas, Remarkable Meteor in, C. F. Maxwell, 279
Texas, Hot Winds in, May 29 and 30, 1892, I. Cline, 454
Thatch in Burmah, Snakes in, 113
Thaxter (R.) Proposed Establishment of New Order (Myxobacteriaceæ) of Schizomycetes, 373
Theory of Numbers, G. B. Mathews, 289
Therapeutics: a New Method of Treatment for Cholera, 83; Epidemic Influenza, F. A. Dixey, 244; the Blood-serum Therapeutists, Dr. Behring, 336; Record of Medical Experience at Davos Platz, Dr. Spengler, 519
Thermal Conductivities of Copper and Iron, the Absolute, R. Wallace Stewart, 599
Thermal Conductivities of Liquids, the, R. Wachsmuth, 350
Thermodynamics: Determination of Low Temperatures of Platinum Thermometer, Griffiths and Clerk, 95; Carnot's Principle Applied to Animal and Vegetable Life, J. Parker, 95; Treatise on Thermodynamics, Peter Alexander, 388
Thermometer, the Centigrade, adopted by Prussian Government, 60
Thermometer, Mercury, an Improved, Prof. L. Weber, 497
Thermometer, Platinum, Determination of Low Temperature by, Griffiths and Clerk, 95
Thermometer, Depression of Zero in Boiled, L. C. Baudin, 143
Thermometers, Dr. Joule's, Prof. Sydney Young, 316, 389
Thermometers, Dr. Joule's, Prof. Arthur Schuster, F.R.S., 364

Thessaly, the Plague of Field-Mice in, 396; J. E. Harting, 545
Thiselton-Dyer (W. T., F.R.S.), Dust Photographs, 341; Travelling of Roots, 414; Severe Frost at Hong Kong, 535
Thompson (Prof. D'Arcy W.), a Proposed Handbook of the British Marine Fauna, 269
Thompson (Prof. Silvanus P., F.R.S.), the late Prof. Tennant on Magic Mirrors, 79; Japanese Magic Mirrors, 381; on Messrs. Rimington and Smith's Experiments in Electric and Magnetic Fields, Constant and Varying, 166
Thomson (Prof. Elihu), Apparent Attraction of Closed Circuits by Alternating Magnetic Poles, 517
Thomson (Joseph), Journey to Lake Bangweolo, 115
Thomson (J. Arthur), the Study of Animal Life, 2
Thomson (J. P.), British New Guinea, 345
Thörner (M.), Methods of Examining Milk for Tubercle Bacillus, 254
Thorp (Walter), Primer of Horticulture, J. Wright, 533; Ornithology in Relation to Agriculture and Horticulture, J. Watson, 533; Manual of Dairy Work, Prof. James Muir, 555
Thorpe (Prof. T. E., F.R.S.), Isolation of Fluorsulphonic Acid, 87; Carl Wilhelm Scheele, 152; Interaction of Iodine and Potassium Chlorate, 165; Experiment on Triethylamine Hydrate, 165; the Determination of the Thermal Expansion of Liquids, 405; the Determination of the Thermal Expansion and Specific Volume of Certain Paraffins and Paraffin Derivatives, 405
Thoulet (J.), on a Modification to be Applied to the Construction of Bottles Designed to Collect Specimens of Deep Waters, 408
Thümen (Dr. F. von), Death of, 130
Thunderstorms and Auroral Phenomena, J. Ewen Davidson, 582
Thurn (E. F. im), Anthropological Uses of the Camera, 548
Thys (Major), Progress of the Congo Railway, 159
Tibet, Captain Bower's Journey in, 400
Tibet, Central, Mongolia and C. Woodville Rockhill, 426
Tidal Observations, Reduction of, Prof. G. H. Darwin, F.R.S., 402
Tiger-Lion Hybrids, Lion-Tiger and, Dr. V. Ball, F.R.S., 390, 607
Tiger, Lion-, Hybrids, S. F. Harmer, 413
Tilden (Prof.), Abuse of Scientific Titles (Letters Indicating Membership of Societies), 15
Tilden (W. A.), Formation and Nitration of Phenyldiazoimide, 311; the Hydrocarbons Derived from Dipentenedihydrochloride, 405; the Action of Nitrosyl Chloride and of Nitric Peroxide on some Members of the Olefine Series, 430
Tillo (Alexis de), High Atmospheric Pressures observed at Irkutsk from January 12 to 16, 1893, 432
Time, Universal, 451
Tisserand (M.), Paris Observatory in 1892, 546
Titles, Scientific, Abuse of Letters indicating Membership of Societies, Prof. Tilden, 15
Tobacco Culture in Australia, 324
Tobacco Cultivation at Tashkend, 86
Tobacco and Vinous Fermentation, the Micro-organisms of, Suchsland, Nathan and Kosutany, 208
Tokelaus, the, 423
Topinard (M.), on Race in Anthropology, 524
Torpedo, on the Origin of the Electric Nerves in the Gymnotus, Mormyrus and Malapterurus, Gustav Fritsch, 271
Torrents, Swiss, the Regulation of, M. de Salis, 377
Total Solar Eclipse, April 15–16, 1893, 304, 376; M. De la Baume Pluvinel, 304; A. Taylor, 317
Tourney (J. W.), Cliff and Cave-dwellings in Central Arizona, 112
Tower Bridge, Foundations of Two River Piers of, G. E. W. Cruttwell, 545
Tracery Imitation, Prof. J. Mark Baldwin, 149
Traube (Dr.), the Amide and Imide of Sulphuric Acid, A. E. Tutton, 566
Travelling of Roots, W. T. Thiselton-Dyer, F.R.S., 414
Travels, Australian, R. von Lendenfeld, 274
Travels in Borneo, Charles Hose, 282
Travels, a Sanitarian's, Robert Boyle, 105
Travers (M. W.), a Method for the Preparation of Acetylene, 406
Trechmann (Dr.), Establishment of the Hemisymmetry of Binnite, 70
Tremayne (L. J.), Vanessa Polychloros in London, 563
Treves (Frederick), Physical Education, 292
Trieste, the Marine Zoological Station at, 450
Trigonometry, the Algebra of Co-planar Vectors and, R. Baldwin Hayward, F.R.S., 266
Trimen (Dr.), the Development of the Ceylon Tea Industry, 613
Trinidad Field Naturalist's Club, 131
Troll's (Dr. J.), Journey through Central Asia, 160
Tropical Agriculture, a Text-book of, H. A. Alford Nicholls, 313
Trotter (A. P.), Diffusion of Light, 191
True (F. W.), Dr. W. L. Abbott's Collection of African Mammals, 39
Tubby (Dr. A. H.), Medical Microscopy, Frank J. Wethered, 51
Tubercle Bacillus, Methods of Examining Milk for, Ilkewitsch and Thorner, 254
Turin Royal Academy of Sciences, the Bressa Prize, 233; the Ninth Bressa Prize, 543
Turquan (M.), Statistics of Survivors of the Napoleonic War, 233; Statistics of Average Life in France, 255
Tutton (A. E.), a Remarkable Case of Geometrical Isomerism, 65; Chemistry of Osmium, 400; the Connection between the Atomic Weight of the Contained Metals and the Magnitude of the Angles of Crystals of Isomorphous Series, 430; Further Studies on Hydrazine, 522; the Amide and Imide of Sulphuric Acid, Dr. Traube, 566
Tylor (Dr.), on the Rude Stone Implements of the Tasmanians, showing them to belong to the Palæolithic or Underground Stage of the Implement-Maker's Art, 527
Tylor (Dr. Edward B., F.R.S.), Atlas der Volkerkunde, Dr. Georg Gerland, 223
Typhoid Fever attributed to Bathing in Polluted Water, Herr Jaeger, 398
Typhoid and Coli Communis Bacilli, Dunbar on the Questions of the Separate Identification of the, 472

Uganda, Captain F. D. Lugard, 45
Uganda Commission, the, 210
Ulrich (G. H. F.), on a Meteoric Stone found at Makariwa, near Invercargill, New Zealand, 381
Ultra-Violet Spectrum in Prominences, Prof. G. E. Hale, 186
United States: Marine Laboratories in the, Prof. J. P. Campbell, 66; the Copper Resources of the, James Douglas, 132; Agriculture in the United States, Experiment Stations, R. Warrington, F.R.S., 157; Investigations on Soils, 157; Higher Education in the United States, Dr. Low, 325; Government Botanical Stations in the United States, 450; United States Naval Observatory, 452
Universal Time, 451
Universe, the Visible, J. Ellard Gore, A. Taylor, 193
Universities: University Commission, 1; University Intelligence, 68, 94, 116, 143, 163, 285, 331, 357, 380, 404, 428, 454, 476; Universities and Research, Prof. George Francis Fitzgerald, F.R.S., 100; Appointment of W. Flinders Petrie to Chair of Egyptology at University College, London, 111; Prof. Flinders Petrie's First Lecture on Egyptology, 278; the New University Question, 121; Opening of New Victoria Buildings, University College, Liverpool, 155; University Extension, the Cambridge Summer Meeting Programme, 183; University Extension Movement, the, Summer Meeting Programme, 586; the Proposed University for London, 200, 577; the University of Chicago, 278; a University Extension Manual, R. D. Roberts, 412
Unwin (Prof.), Experiments on the Value of the Steam-jacket, 20
Urobilin, on, A. Eichholz, 360
Utah, Salinity of Great Salt Lake, 302

Val d'Herens, Glaciers of, William Sherwood, 174
Value of the Mechanical Equivalent of Heat, the, E. H. Griffiths, 537
Vanessa Polychloros in London, L. G. Tremayne, 563

Vanlair (M. C.), Survival after the Successive Section of both the Branches of the Vagi, 621
Variation, the Volucellæ as Alleged Examples of, Almost Unique Among Animals, Edward B. Poulton, F.R.S., 126
Varigny (Henry de), Experimental Evolution, 25
Variolite of the Lleyn and Associated Volcanic Rocks, Catherine A. Raisin, 334
Vasey (Dr. Geo.), Grasses of the Pacific Slope, including Alaska and the Adjacent Islands, 173
Vasey (Dr. G.), Death and Obituary Notice of, 495
Vector Analysis, Prof. P. G. Tait, 225
Vector Theory, on Recent Innovations in, Prof. C. G. Knott, 287, 590
Vectors, Principles of the Algebra of, A. Macfarlane, 3
Vectors, Co-planar, the Algebra of, and Trigonometry, R. Baldwin Hayward, F.R.S., 266
Vectors, Quaternions and the Algebra of, Prof. J. Willard Gibbs, 463
Vectors *versus* Quaternions, Oliver Heaviside, F.R.S., 533
Vegetable Wasps and Plant Worms, M. C. Cooke, 99
Vegetation, Influence of Moisture on, E. Gain, 119
Veins of the Rabbit, on an Abnormality in the, Prof. W. M. Parker, 270
Veitch (H. G.), Coniferæ of Japan, 619
Veley (V. H.), Necessity of Water in Chemical Reactions, 167
Venukoff (M.), the Pinsk Marshes and non-Russian Atlases, 282 ; the Form of the Geoid, 566 ; Measurement of the Parallel of 47° 30′ in Russia, 576
Verschaffelt (J.), Two Experimental Verifications Relative to Refraction in Crystals, 428
Vertebrate Biology, H. G. Wells, 605
Vertebrate Fauna of Lakeland, a, Rev. A. Macpherson, 457
Verworn (Herr), the Rising and Sinking Process in the Radiolaria, 397
Veterinary Physiology, a Manual of, Vety. Capt. F. Smith, 76
Vezes (M.), Metallic Osmium, 497
Victoria Field Naturalists' Club, 62
Vine-disease, American, Appearance of the Black-rot in Europe, 16
Vine-disease, the Mal Nero, Dr. B. Pasquale, 130
Vineyards of Cyprus, the, M. Mouillefort, 517
Violle (J.), the Temperature of the Electric Arc, 240 ; the use of the Electric Current in Producing High Temperatures, 497
Virchow (Prof.), on the Immediate Task for Anthropologists, 38 ; Lord Kelvin, 110 ; the Croonian Lecture, 487
Viré (Armand), Neolithic Village of the Roche-au-Diable, near Tesnières, canton of Lorez-le-Bocage (Seine-et-Marne), 576
Viscous Liquid, Motion of a Solid Body in a, A. B. Basset, F.R.S., 512
Visible Universe, the, J. Ellard Gore, A. Taylor, 193
Vision, Defective, in Sailors, Association of Shipping Disasters with, Dr. T. H. Bickerton, 16
Vision, Energy and, Prof. S. P. Langley, 252
Vision, a New Hypothesis concerning, John Berry Haycraft, 478
Viticulture ; the Mal Nero Vine Disease, Dr. B. Pasquale, 130
Vladimiroff (M.), Rule for Estimating Quality of Vulcanised Caoutchouc, 563
Vöchting (Dr. Hermann), Ueber Transplantation am Pflanzenkörper, 128
Vogelius (Dr.), the Construction of Carbohydrates in the Fasting Body, 552
Volcanoes : Formation of Lunar, J. B. Hannay, 7 ; Stromboli in 1891, L. W. Fulcher, 89 ; A. Ricco and G. Mercalli, Dr. H. J. Johnstone Lavis, 453 ; Volcanoes of Japan, John Milne, F.R.S., 178 ; on the Age of the most Ancient Eruptions of Etna, M. Wallerant, 264 ; Fumo di Vulcano Veduto dall'Osservatorio di Palmero durante l'éruzione del 1889, A. Ricco, 428 : Kilauea in August 1892, Frank S. Dodge, 499
Vole Plague in Thessaly, the Field, J. E. Harting, 545
Voles in Scotland, the Plague of Field, 155
Voles in Southern Scotland, the Colour Variations of the, Robert Service, 587
Volucella, Parasitism of, W. E. Hart, 78
Volucellæ as Examples of Aggressive Mimicry, Edward B. Poulton, F.R.S., 28
Volucellæ, the alleged Aggressive Mimicry of, William Bateson, 77
Volucellæ as Alleged Examples of Variation, almost Unique among Animals, Edward B. Poulton, F.R.S., 126
Wachsmuth (R.), the Thermal Conductivities of Liquids, 350
Wadsworth (Prof. M. E.), Geology of the Iron, Gold, and Copper Districts of Michigan, Sir Archibald Geikie, Dr. Hicks, H. Bauerman, 117
Waite (Edgar B.), appointed Assistant Curator in Australian Museum, Sydney, 111
Wakefield (H. Rowland), an Elementary Text-book of Hygiene, 245
Waldo (Dr. Frank), Standard Barometry, 511
Wales, December 8, 1892, Corncrake caught in, 157
Wales, the Origin and Progress of the Educational Movement in, O. M. Edwards, 421
Walker (Mr.), the Screw Propeller, 21
Walker (Alfred O.), a Remarkable Rainfall, 31
Walker (Sir Andrew Barclay), Death of, 421
Walker (J.), Electrolysis of Sodic Ethylic Camphorate, 479
Walker (J. W.), Optically Active Ethoxysuccinic Acid, 311
Walking of Arthropoda, on the, Henry H. Dixon, 56
Wallace (Dr. Alfred R) : an Ancient Glacial Epoch in Australia, 55 ; the Earth's Age, 175, 226 ; the Glacier Theory of Alpine Lakes, 437 ; Idle Days in Patagonia, W. H. Hudson, 483
Wallerant (M.), on the Age of the most Ancient Eruptions of Etna, 264
Ward (Prof. Marshall) : Experiments on the Action of Light on *Bacillus anthracis*, 331 ; Further Experiments on the action of Light on *Bacillus anthracis*, 597
Ward (Rowland), Horn Measurements and Weights of the Great Game of the World, being a Record for the use of Sportsmen and Naturalists, 6
Ward (R. de C.), the First Aërial Voyage across English Channel, 143
Warrington (R., F.R.S.), Experiment Stations in United States, 157
Washington, Geological Society of, Founded, 613
Washington Magnetic Observation, 209
Wasps, Vegetable, and Plant Worms, M. C. Cooke, 99
Watch Factory, the Prescot, Address by Lord Kelvin, 279
Water and Water Supply, Major L. Flower, 183
Water, Dilatation and Compressibility of, E. H. Amagat, 288
Water, Expansion of, at Constant Pressure and at Constant Volume, E. H. Amagat, 623
Water-boring in Cape Colony, 349
Water-Gas, Experiments to determine Temperatures of Flame of, E. Blass, 113
Water-Pollution, Improved Ball Hydrant for Preventing, J. R. Wigham, 167
Water-Purification by Bacteriological Methods, Messrs. V. and A. Babes and Percy Frankland, 588
Waterdale Researches : Fresh Light on Dynamics, 601
Watson (John), Ornithology in Relation to Agriculture and Horticulture, 533
Watson (Sereno), Botanical Nomenclature, 53
Wave-Lengths, a New Table of Standard, Prof. H. A. Rowland, 590
Waves as a Motive Power, H. Linden, 438
Waves, Electromagnetic, Oliver Heaviside, 505
Weapons of Defence, Remarkable, G. F. Hampson, E. Ernest Green, 199
Weather of Summer, the, 245, 270
Weber (Mr.) on the Origin of the Mammalian Hair, 504
Weber (Prof. L.), an Improved Mercury Thermometer, 497
Weber (William), Proposed Monument to, 106
Webster (A. D.), Conifers for Economic Planting, 619
Weights and Measures, Changes in the Imperial Standard, 86
Weights and Measures, International Committee of, 21
Weinek's Lunar Enlargements, 473
Weismann (Prof. August), Das Keimplasma, 265
Wells and Wheeler, Isolation of Penta- Iodide and -Bromide of Cæsium, 113 ; Preparation of Chloraurates and Bromaurates of Cæsium and Rubidium, 158
Wells (H. G.), Text-book of Biology, 605
Wenlock (Lord), Remarkable Hornet's Nest presented to Madras Museum by, 16
Were-Wolf in Latin Literature, the, Kirby W. Smith, 423
Wesendonck (Mr.). Pure Gases incapable of producing Electrification by Friction, 280
West Indies, Observations in the, Prof. A. Agassiz, 608
Westwood (Prof.), Death of, 232
Wethered (Frank J.), Medical Microscopy, Dr. A. H. Tubby, 51

Wetterhan (Dr.), Arborescent Frost Patterns, 162
Wettstein (R. von), Die Fossile Flora der Höttinger Breccie, 436
Whale, Sowerby's, on the Norfolk Coast, T. Southwell, 349
Whale, Comparison of Rudimentary Hind-limb of Great Finwhale Hump-back, and Greenland Right-, Dr. John Struthers, 588
Whaling Fleet, Return of the Dundee, 473
Wheat Conference, the South Australian Rust in, 86
Wheeler and Wells, Isolation of Penta- Iodide and -Bromide of Cæsium, 113; Preparation of Chloraurates and Bromaurates of Cæsium and Rubidium, 158
Whetham (W. C. D.), Ionic Velocities, 164
Whipple (George Mathews), Death and Obituary Notice of, 372
White (Dr. F. B.), Collection of Lepidoptera presented to Museum of Perthshire Society of Natural Science, 206
White (Philip J.), Unusual Origin of Arteries in the Rabbit, 365
Whitehead (Charles), Yew Poisoning, 285
Wiedemann's Annalen der Physik und Chemie, 68, 189, 286, 357, 455, 525
Wiener (Christian), Diffusion of Light by Rough Surfaces, 286
Wigham (J. R.), Improved Ball Hydrant for Preventing Water-Pollution, 167
Wild Nature, More about, Mrs. Brightwen, 125
Wilkinson's (E.) Journey in the Kalabari Desert, 134
Williams on the Relation of the Dimensions of Physical Quantities to Directions in Space, Prof. Fitzgerald, Prof. Reicker, Prof. Henrici, and Dr. Sumpner, 69
Williams on the Dimensions of Physical Quantities, Dr. Burton, Prof. A. Lodge, Mr. Boys, W. Baily, Mr. Swinburne, Mr. Williams, 116
Williams (Dr. C. T.), on the High Altitudes of Colorado and their Climates, 333; Longevity of the Perigal Family, 585
Williams (W. M.), the Framework of Chemistry, 28
Williams (W. Matthieu), Death of, 130
Williamson (S.), the Hydrocarbons derived from Dipentene Dihydrochloride, 405
Williamson (Prof. Wm. Crawford), the Genus Sphenophyllum, 11
Willis (J. C.), Gynodiœcism in the Labiatæ, II., Observations on Origanum (continued), 119
Willoughby (Dr. E. F.), the Health Officer's Pocket-book, 412
Wilson (Edward), a Catalogue of British Jurassic Gasteropoda, H. Woods, 363
Wilson (E.), Magnetic Induction in Iron and other Metals, J. A. Ewing, F.R.S., 460
Wine-growing in Alsace-Lorraine, Statistics of, 614
Wine-Yeast, the Improvement of Cider by, Nathan; Investigations on, Kosutany, 208
Winnebago County Meteorites, Lines of Structure in the, and in other Meteorites, Prof. H. A. Newton, 370
Wislicenus (Dr. Wilhelm), New Mode of Preparing Hyponitrous Acid, 588
Witchell (Charles A.), the Fauna and Flora of Gloucestershire, 197
Withington (Herbert), the Flight of Birds, 414
Witkowski (Herr), Use of Total Reflection to determine Light-Refraction of Liquid Oxygen, 614
Wolle (Rev. T.), Death of, 561
Wollheim (Hugo), Aminol, 246
Wollny (Herr), Dew, 398
Wolsingham Observatory, 518; J. E. Espin, 452; Circular No. 35, 590; No. 34, 616
Women and Musical Instruments, Henry Balfour, 55
Wood Ashes as a Medicine for Farm Animals, J. M. Stahl, 397
Woods (H.), a Catalogue of British Jurassic Gasteropoda, W. H. Hudleston, F.R.S., and Edward Wilson, 363
Woodward (H. B.), Norfolk and Norwich Naturalists' Society, Annual Address by, 562
Woolls (Rev. W.), Death and Obituary Notice of, 495
Worthington (Prof. A. M.), on the Need of a New Geometrical Term—"Conjugate Angles," 8; Science Teaching, 359
Wright (Dr. G. Frederick), Man and the Glacial Period, 148
Wright (Herbert Edwards), a Handy Book for Brewers, 75
Wright (J.), Primer of Horticulture, Walter Thorp, 533
Wurtz (Dr. R.), Technique Bactériologique, 446
Wynne (W. P.), Griess, Sandmeyer Interactions-and Gattermann's Modification thereof, 239

Yale Astronomical Observatory, 452
Yale College, Establishment of Psychological Laboratory at, 253
Year, the Origin of the, J. Norman Lockyer, F.R.S., 32, 228
Year-Book of the Imperial Institute of the United Kingdom, the Colonies, and India, the, 363
Year-Book of Science (for 1892), the, 388
Yew Poisoning, E. P. Squarey, Charles Whitehead, W. Carruthers, F.R.S., and Dr. Munro, 285
Yezo and the Ainu, Prof. J. Milne, F.R.S., 330; A. H. Savage Landor, 330
Yorkshire Caves, Relics found in, Rev. Edward Jones, 112
Young's (Arthur) Tour in Ireland, 341
Young (Prof. C. A.), Meteors, 150; Comet Holmes (1892, III.) 518
Young (Dr.), the Determination of the Critical Volume, 70; on Sutherland's On the Laws of Molecular Force, 70
Young (Prof. Sydney), Dr. Joule's Thermometers, 389; the Zero Point of Dr. Joule's Thermometer, 316
Yoxall (J. H.), the Decimal System, 323

Zacharias (Dr. Otto), Forschungsberichte aus der Biologischen Station zu Plön, 461
Zahm (Rev. J. A.), Sound and Music, 222
Zambesia Journey, Mr. D. G. Rankin's, 64
Zambesia, Twenty Years in, F. C. Selons, 377
Zante, Earthquake at, 378, 394, 585, 620
Zermatt, Remarkable Optical Phenomenon near, F. Folie, 303
Zero Point of Dr. Joule's Thermometer, the, Prof. Sydney Young, 316
Zichen (Dr. Theodor), Introduction to Physiological Psychology, 28
Zirconia, Native, the Occurrence of (Baddeleyite), L. Fletcher, 282
Zodiacal Light, Observations of the, Arthur Searle and Prof. Bailey, 473
Zoology: Zoological Gardens, Additions to, 17, 40, 63, 87, 113, 133, 158, 186, 208, 235, 256, 281, 303, 325, 351, 375, 399, 425, 451, 472, 497, 518, 546, 565, 589, 616; Zoological Gardens in Great Britain and Australia, Thomas Steele, 496; Zoological Gardens in Europe and Australia, Thomas Steele, 587; Zoological Society, 70, 118, 215, 335, 431, 455, 502, 575; Dr. W. L. Abbott's Collection of African Mammals, F. W. True, 39; the Mantle-Cells of Ascidians, Kowaleosky, 62; a New Genus and Species of Blind Cave Salamander from North America, L. Stejneger, 62; Large Male Gorilla Acquired by Berlin Aquarium, 86; International Zoological Congress at Moscow, 236; the Proposed Snake Laboratory at Calcutta, 253; a Proposed Handbook of the British Marine Fauna, Prof. d'Arcy W. Thompson, 269; Prof. W. A. Herdman, F.R.S., 293; W. Garstang, 293; the Brain in Mudfishes, Dr. Rudolf Burckhardt, 339; Suggested Introduction of the Musk-ox into Scotland, Col. H. W. Fielden, 349; Lion-Tiger and Tiger-Lion Hybrids, Dr. V. Ball, F.R.S., 390, 607; Lion-Tiger Hybrids, S. F. Harmer, 413; Remarkable Specimen of Orang-utan, 423; on the Cranial Osteology, Classification, and Phylogeny of the Dinornithidæ, Prof. T. Jeffrey Parker, F.R.S., 431; on the Presence of a Distinct Coracoidal Element in Adult Sloths, R. Lydekker, 431; Observations on the Development of the Rostrum in the Cetacean Genus Mesoplodon, Henry O. Forbes, 455; the Marine Zoological Station at Trieste, 450; Polecat not Extinct in Cardiganshire, J. W. Salter, 450; a Vertebrate Fauna of Lakeland, Rev. A. Macpherson, 457; Blind Animals in Caves, Prof. E. Ray Lankester, F.R.S., 486; J. T. Cunningham, 537; G. A. Boulenger, 608; Some Abnormal Vertebræ of Certain Ranidæ (*Rana Catesbiana*, *R. esculenta* and *R. macrodon*), Prof. Howes, 502; the Musk-ox, 559; a New Medusa from Lake Tanganyika, R. T. Günther, 563; the Colour Variations of the Voles of Southern Scotland, Robert Service, 587; the Alligator's Nest, S. Devenish, 587; an International Zoological Record, Dr. Herbert H. Field, 606
Zuntz (Prof.), Respiratory Interchange in the Fasting Body, 552

NATURE

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

THURSDAY, NOVEMBER 3, 1892.

THE UNIVERSITY COMMISSION.

THE University Commission is sitting frequently and has heard witnesses representing nearly every interest and every shade of opinion which have a right to be represented before it. We have no knowledge of the effect which the evidence laid before them has produced upon the minds of the Commissioners; but we are sure that it must largely depend on the view which they have adopted as to the nature of their duties. They may regard themselves as entrusted with the task of finding the terms on which a heterogeneous crowd of colleges and mechanics' institutes may be huddled together, called a university, and allowed to confer degrees on such conditions as the rivalries of competing institutions may permit when tempered by the moderating influence of Crown nominees, county councillors, representatives of the School Board and of the learned societies, and any other assessors whom fancy may suggest. Such a solution might no doubt secure peace in the sense that, wearied out by long debate and hopeless of a satisfactory solution, those who are most nearly interested in the question might at last be compelled to make the best of a bad job.

This, however, must be urged against it: That almost every teacher of eminence in London, together with a large number of those best qualified to represent the educational views of the provinces, have declared *a priori* that it would be unsatisfactory.

The other view which the Commissioners may take is that they are charged with the responsible task of defining the ideal system which would best provide for the supply of the higher education in London. That having defined this ideal they are then to proceed to show by what means the closest approximation to it which present circumstances will allow can be made, and so to fashion the constitution of the University as to ensure in the future a closer approximation still. That this is the wider and more statesmanlike view is beyond question, and we sincerely hope that the Commission will adopt it. We may further hope that they will remember that although the new University should be able and willing to undertake all the multifarious duties which modern Universities have accepted as their own, the provision of the highest education and the doing all that in it lies for the advancement of learning must after all be the first and the highest duty of a University worthy of the name. As to the means which would best realize these ideals there cannot be a doubt. The present educational chaos must be reduced to order, the unwholesome rivalry between the London Colleges must be checked.

On this point Prof. Rücker, in an address recently delivered at the Yorkshire College in Leeds, made some remarks which we cannot do better than quote in full:—

"The great provincial colleges are grouping themselves into greater Universities. In the north Manchester, Liverpool, and Leeds have concluded a formal alliance. Negotiations are already in progress for the establishment of a similar confederation in Wales. The Midlands will no doubt follow suit. But if these afford, if in particular the north of England affords in the Victoria University, one of the happiest illustrations of the advantage of allowing free play to the tendencies which make for union no less than to those which encourage separation, we have, unfortunately, in London a striking instance of the harm which follows if the action of either the one or other is artificially restrained.

"The northern colleges were indeed happy in that the tendency to union was called into play while they were still in a sufficiently early and plastic stage of their history to yield easily to its influence. In London difficulties, which seemed far more serious half a century ago than they do to most of us now, have unfortunately retarded all centralizing action, till the sentiment and traditions which accumulate round institutions that have long moved independently, have enormously increased the inertia which tends to keep them in their separate paths.

"This is the more unfortunate, as if a new University of London is to be a really great teaching university, the relations between the London colleges must ultimately be closer than those which obtain between Manchester, Liverpool, and Leeds. The principle of recognizing as colleges of the University institutions for the teaching and management of which the University is not responsible, has worked and is working admirably in the north of England. It does not follow that it would succeed in London. There

is indeed a fundamental difference between the two cases. The colleges of the Victoria University are widely separated, and appeal to the strong local feeling of powerful and independent districts. A generous rivalry may therefore exist between them without ill result. Each should be left, as they have been left, to work out their own success with as little external interference as possible.

"It is sometimes argued that because the population of London largely exceeds that even of such districts as Lancashire or the West Riding, there ought to be room within it for the separate and independent institutions in which teaching of the highest type could be provided. This view ignores the importance of geographical separation, and unduly exalts that of the numerical magnitude of the population whose wants are to be met. If Manchester and Leeds were on opposite sides of the Irwell or the Aire, if they were connected by an elaborate system of overground and underground railways, then it would be more economical to concentrate, in one or the other, the higher teaching which must now perforce be given in both. The loss of time to the students in reaching the scene of their daily labours would be but imperceptibly increased, while the prestige of the colleges, great as it already is, their claims on the State, strong as they already are, would be enhanced in a proportion greater than that calculated by merely adding their separate reputations and resources. In a city of the size of London it is desirable to multiply institutions in which preparatory work of all sorts is undertaken, but I think it may be assumed as almost axiomatic that it is impossible, at present at all events, to create in one town more than one institution in which laboratories and lecture rooms and the other machinery of scientific instruction shall be provided on the large scale which the elaboration of the highest modern scientific teaching demands. In London, then, the teachers in almost all existing institutions feel the necessity for a combination of forces. They have expressed themselves as willing to be formed into battalions and regiments rather than to be left to carry on their work as isolated companies. I will not dwell on the fact that this desire could only be declared by men who were willing to risk their personal position for the public good, but I want you to observe how in this case also the work of decentralization, which began with the foundation of University College, London, has been followed and would have been far more effective had it been accompanied by a corresponding manifestation of centralizing force."

With these views we heartily agree.

If ever we are to have in London laboratories such as those which are to be found in Germany, it can only be if the higher teaching in each subject is concentrated in some one great central institution, and if rival colleges are allowed to combine their forces for the public good, instead of being compelled as at present to fritter them in suicidal competition.

Taking it for granted that all will admit that such an ideal would be the best if it could be realized, we believe that the possibility of its realization is chiefly doubted on two grounds, to neither of which any real importance is to be attached.

It has been supposed, in the first place, that those who advocate a policy of union among the London colleges think that this union must be carried out in all particulars immediately; and secondly that in order to secure this end it must be carried out by compulsion, even if the practical confiscation of the property of the existing colleges were necessary.

It need hardly be said that such a statement is a parody of the views of men who have had at least as much experience as their critics of the tone of mind of the governing bodies of great educational institutions, and who therefore would be the first to anticipate the difficulties which such demands would inevitably cause.

No responsible body has, as far as we are aware, advocated more than the establishment of a University on a basis which would permit the union of the various colleges, in whose buildings the University teaching might at first be carried on, if the colleges were themselves willing that such a union should be effected. The advocates of union have all along been striving, not to attain an immediate and complete realization of their ideal University of London, but to prevent the Charter being drawn so as to make that realization impossible. It cannot be beyond the limits of human skill to frame a scheme which shall offer every inducement to the London colleges to effect an immediate fusion, and shall further provide that any approximation which may at first take place shall easily become closer in the future.

The Victoria University does not consist of competing colleges. A federal University of London would consist of colleges which from their mere local proximity would, whether they willed it or no, be necessarily antagonistic. Unless the Commissioners fairly grasp this fact and realize that they have it in their power to lay down the lines on which a great institution shall be founded in close connection with the State, which shall concentrate under one central directing power all the educational efforts which are at present partly wasted through want of joint action, they will have failed to make the most of a great opportunity, and will have frittered the forces which, if allowed free play, are competent to do for the higher education of London all that the best friends of London can desire.

THE STUDY OF ANIMAL LIFE.

The Study of Animal Life. By J. Arthur Thomson, M.A., F.R.S.E. "University Extension Manuals." (London: Murray, 1892.)

THE chief aim of an "Extension Manual" as of "Extension lectures" is to stimulate interest and to spread information. In natural science, at any rate, it is impracticable through the medium of either Extension lectures or Extension manuals, to give that training which the student, be he specialist or generalist, can obtain only by practical work, aided by practical instruction. But there are a great number of people, some already busily engaged, others on the threshold of their life's work, who possess some interest in, and some information about, those matters, with the study of which scientific men are occupied. For them Extension lectures and manuals are a great boon; and to them Mr. Thomson's work on "The Study of Animal Life" may be cordially recommended. We trust it will stimulate them, as he would desire, to become themselves observers.

The work is divided into four parts, of which the first, entitled "The Everyday Life of Animals," deals with the wealth of life, the web of life, the struggle of life, the shifts for a living, the social life of animals, the domestic life of animals, and the industries of animals. The second part, on "The Powers of Life," contributed by Mr. Norman Wyld, treats of vitality, the divided labours of the body, and instinct. The third par describes "The Forms of Animal Life" and includes chapters on the life-history of animals, and their past history as read in the geological record. The fourth and last part treats of "The Evolution of Animal Life" and, besides a discussion of the influence of habits and surroundings, and of heredity, gives a sketch of the evolution of evolution theories. Appendices on the relation of animal life to human life, and on some of the best books on animal life bring the work to a conclusion.

The general arrangement of the subject-matter is, as will be seen by the above summary, well and carefully thought out, and the facts given in elucidation of the varied tendencies of organic development are skilfully marshalled and are derived from the most trustworthy sources. The information given is therefore accurate and up to date. The only suggestion we have to offer in this connection is that a little more selective elimination might have been exercised. Some facts are given in so terse and condensed a form that no one but a zoologist could appreciate their value. If a considerable number of these had been struck out and the space thus gained had been utilized in expanding those that remained, the Extensionee would have been the gainer. "The Zoological Summary of the Animal Kingdom" (pp. 210-272) might by some such process have been replaced by a sketch with more life and go in it. As it stands it will, by many readers, be gracefully skipped.

In such a work style is an important element. Here Mr. Thomson is often exceedingly happy. He has imagination and a feeling for the poetic aspect of nature. But his imagination and poetry need at times just a little chastening. When he tells us that in birds "the breathing powers are perfected and economized by a *set of balloons around the lungs*," and that their brains "are not *wrinkled with thought* like that of mammals"; when he speaks of the sponge as "*a Venice-like city of cells*"; when he describes the ciliated cells of the windpipe as "*lashed* cells," or the embryonic membranes as "*birth-robes*," and when he says that in ponds subject to drought the organism often "*sweats off a protective sheath which is not a shroud*, and waits until the rain refreshes the pools"; in these and sundry other cases of which these are samples, one may question whether the expressions which we have underlined are justified either by special elegancy or by real helpfulness to a beginner. And this we say in no spirit of hypercriticism, but as desirous of aiding the author in what is by no means an easy task.

Somewhat deeper would be our criticism of sundry expressions which are of essentially human implication and which in our opinion should not lightly be applied to animal activities. Much is said of the "love" of animals for their mates when some such phrase as "sexual appetence" would be more appropriate. For example, concerning ants we read:—"After this midsummer day's delight of love death awaits many, and sometimes most." And in the analysis of the forms of struggle for existence, we have the "struggle between rivals in love." Again, of the cuckoo it is said that, "in spite of the poets, the note of this 'blessed bird' must be regarded as suggestive of sin"! And again, "It is not quite correct to say that the cuckoo-mother is immoral because she shirks the duties of maternity; it is rather that she puts her young out to nurse because she is immoral." It is true that Mr. Thomson adds this footnote:—"The student will notice that I have occasionally used words which are not strictly accurate. I may therefore say definitely that I do not believe that we are warranted in crediting animals with moral, æsthetic, or, indeed, any conceptions." We are glad to be thus assured. But why implant notions in the text which have to be eradicated in a footnote? Does not Mr. Thomson know how easy it is to sow tares and how difficult to root them out?

Mr. Norman Wyld's chapter on "Instinct" is short, but quite to the point. We hope that he may further observe and experiment in the field of comparative psychology, for he is fully alive to the peculiar difficulties of the subject, and there is a wide field before him in which the scientific workers are none too many. In criticizing Mr. Lloyd Morgan's definition of instincts as "oft-recurring or essential to the continuance of the species," Mr. Wyld says:—"This is not quite satisfactory, for many actions that are instinctive are not oft-recurring, and many are not necessary to the preservation of the species." He does not show that there are any such actions which are neither the one nor the other. We have reason for supposing that he understood Mr. Lloyd Morgan to say that instinctive actions were "oft-recurring *and* essential to the continuance of the species." But this he did not say.

In conclusion we may repeat that "The Study of Animal Life," though by no means faultless, may be recommended to Extension students and the general reader as, in the main, accurate, readable, and suggestive.

C. LL. M.

VECTOR ALGEBRA.

Principles of the Algebra of Vectors. By A. Macfarlane, M.A., D.Sc., LL.D., F.R.S.Edin., Professor of Physics in the University of Texas. Reprint from the Proceedings of the American Association for the Advancement of Science, Vol. XL., 1891, pp. 65-117. (Salem Press, Salem, Mass., 1891).

THIS is a very suggestive contribution to the foundations of the Algebra of Vectors as recently so strongly advocated in America by Prof. Willard Gibbs, and in this country by Mr. Oliver Heaviside.

The extensive use of quaternions among physicists has been prevented by the fact that the meaning of a product of vectors has been made to depend on the use of a vector as a quadrantal versor, and by the fact that this method leads to the square of a vector being negative. The advocates of the new algebra define a product of vectors independently and in such way that the square of a vector is positive. Rotations are expressed by means of dyadics, or ratios between vectors

and the quaternion notion of a vector being also a quadrantal versor is not entertained at all.

The author of this pamphlet devotes a portion of it to the consideration of quaternions, which he holds should form a distinct algebra by themselves, and he suggests a special notation for them. He restricts a quaternion proper to a *pure number* (a stretching factor) combined with a certain amount of turning. A vector, on the contrary, may be a quantity of any dimensions, possessing direction, with no suggestion of turning attached to it.

He clearly shows that the objectionable *minus* which occurs in scalar products in quaternions arises from the attempt to use the same symbol both for a quadrantal versor and for a vector, so that the laws established for dealing with one set of quantities may hold also for the other set, or for a combination of the two.

It may be worth while to notice that this minus sign of the quaternionists would disappear as an explicit symbol if they considered the second vector as being drawn from the end of the first, as AB, BC, and then took the angle ABC as being the angle between the vectors—that is to say, if, in a polygon of vectors, they were to define the angles between the successive vectors to be the *internal* angles of the polygon. Indeed, by many the internal angles of a polygon (or triangle) are considered as being *the* angles between the sides, though there is loss of real naturalness and of symmetry caused by so considering them: for instance, the connection between A, B, C and a, b, c in a spherical triangle would be greatly simplified if A, B, C were to denote the *external* angles. However, if we consider these internal angles to be the angles considered by the quaternionists, the reason for the square of a vector being negative appears at once; for if a be the quantitative part (freed from the notion of direction) of a vector **A**, we have $\mathbf{A}\,\mathbf{A} = a^2 \cos 180°$, **A** and **A** being consecutive sides of the polygon which have straightened out till the internal angle between them is 180°.

It may therefore be contended that the quaternionists' *minus* is not quite irrational in vector algebra (though it cannot be said not to be inconvenient there), and that the advantage of being able to treat a vector as a quadrantal versor without having to establish a new set of formulæ far more than compensates for the loss of symmetry. On the other hand, the advocates of vector algebra without the *minus* would probably reply that they have to deal with vectors which are not in any sense the same as quadrantal or any other kind of versors, and that the imaginary completeness gained does not in any degree whatever compensate for the loss of naturalness and loss of symmetry involved in the *minus*.

The author differs from Prof. Gibbs and Mr. Heaviside in the mode in which he defines the product of two vectors, as he considers the *complete* product formed on the understanding that the multiplication shall obey the distributive law. He then finds that this complete product consists of a non-directed part, and of a directed or vector part, the former consisting of the product of the two quantities into the cosine of the angle between them, and the latter of the product of the two quantities into the sine of the same angle, having as axis the normal to the plane containing the two vectors. The angle is the angle through which the first vector (occurring on the left-hand side of the product) would have to turn to make its direction coincide with that of the second.

Prof. Gibbs and Mr. Heaviside, on the contrary, define the scalar product and the vector product as if they were entirely distinct and independent quantities. Finally the same result is attained, but Prof. Macfarlane's mode of introducing these partial products as arising naturally from applying the distributive law of multiplication would seem to have an advantage from the point of view of a student.

Prof. Macfarlane dwells emphatically on the importance of considering *dimensions* of vectors, as well as their direction, and to emphasize this he separates his vector, not into *tensor* and *unit-vector*, but into *quantity* and *direction*. Thus in the equation $\mathbf{X} = xi$, x is the quantity, and i denotes the axis. Hence the equation $jk = i$ is not a violation of dimensions, but is merely a convention as to the interpretation of a composite direction, a convention, moreover, which could only be adopted in space of three dimensions, and is the statement that the plane in which j and k lie has its orientation sufficiently indicated by the normal direction i, with the further convention that the angle from j to k shall be considered positive.

The author's notation is novel, and forms a very important feature in his treatment of the subject. The scalar product of **AB**, which is $ab \cos(ab)$, he calls cos (**AB**) and the vector product he calls *Sin* **AB**, its magnitude, irrespective of direction, being denoted by *sin* **AB**. Possibly an improvement in this latter would be to denote it by *sin ab*, and then the capital letter in the complete vector would become unnecessary.

The particular symbol used to denote a scalar or a vector product is a matter of secondary importance, but is a matter which must sooner or later be settled if vector-algebra is to come into general use. Lord Kelvin is of opinion that a function-symbol should be written with not less than three letters, and Prof. Macfarlane's notation obeys that law, and is moreover easy to work with, but is incomplete, being applicable to products of two vectors only. Mr. Heaviside uses no prefix at all to a scalar product, but considers that **AB** means the scalar product. He uses the quaternionic expression $V\,\mathbf{AB}$ for the vector product. Prof. Gibbs uses no prefix for either, but denotes the scalar product by $\mathbf{A} \cdot \mathbf{B}$, and the vector product by $\mathbf{A} \times \mathbf{B}$. The three-lettered prefix seems the clearest in both cases to denote the special product intended, and the symbols *cos* and *sin* are more or less suggestive.

In forming a product of three vectors, Prof. Macfarlane makes the convention that **ABC** shall mean (**AB**)**C**, the combination commencing on the left. In his notation this product expands into

$$(\cos \mathbf{AB} + \operatorname{Sin} \mathbf{AB})\mathbf{C}$$
$$= \cos(\cos \mathbf{AB} \cdot \mathbf{C} + \operatorname{Sin} \mathbf{AB} \cdot \mathbf{C}) + \operatorname{Sin}(\cos \mathbf{AB} \cdot \mathbf{C} + \operatorname{Sin} \mathbf{AB} \cdot \mathbf{C})$$
$$= \cos(\operatorname{Sin} \mathbf{AB} \cdot \mathbf{C}) + \operatorname{Sin}(\cos \mathbf{AB} \cdot \mathbf{C}) + \operatorname{Sin}(\operatorname{Sin} \mathbf{AB} \cdot \mathbf{C})$$
$$= \operatorname{vol} \mathbf{ABC} + \mathbf{C} \cdot \cos \mathbf{AB} + \operatorname{Sin}(\operatorname{Sin} \mathbf{AB} \cdot \mathbf{C})$$

which finally becomes

$$= \operatorname{vol} \mathbf{ABC} + \mathbf{C} \cos \mathbf{AB} + \mathbf{B} \cos \mathbf{AC} - \mathbf{A} \cos \mathbf{BC};$$

where vol (**ABC**) denotes the volume of the parallelopiped of which **ABC** are three adjacent edges. The only objection to this name lies in its suggesting that **A**, **B**, **C** are *linear* vectors.

Here appears the defect in the author's *cos* and *sin* notation, in that it cannot be applied to the products of three vectors, or at least that the special reason for its use has disappeared, and the author does not suggest so applying it.

But there is a certain perspicuity attained by this very limitation of the *cos* and *sin* notation to the products of only two vectors, inasmuch as there can be no ambiguity in the meaning of an expression in which they occur, even if brackets are omitted or placed differently. Indeed, instead of cos (Sin **AB**. **C**) the author writes cos (Sin **AB**)**C**, which seems a curious use of the bracket. But cos Sin **AB**. **C**, or preferably cos **C** Sin **AB**, is just as explicit, and even cos Sin **ABC**, though wrong to write as being puzzling, can only have the same meaning.

The author concludes with short sections on dyads and matrices, on scalar- and vector-differentiation, including scalar-differentiation of a quaternion. On the last page are a series of propositions relating to the addition of scalar and vector quantities situate at, or passing through, specified points.

The pamphlet is confined solely to statements of principles and the section devoted to dyads and matrices is very condensed, so that it is not in any sense a text-book for students. It is rather a synopsis of the subject, with the introduction of a special notation which the author has found useful. A text-book of vector algebra, with examples showing its application to problems in geometry, mechanics, and general physics, and contrasting the method with the Cartesian method of treating the same problems, is much needed, as many physicists are becoming interested in the new algebra, owing in great measure to Mr. O. Heaviside's able exposition of its principles and applications in the *Electrician* and elsewhere.

THE LAKE OF GENEVA.

Le Léman: Monographie Limnologique. F. A. Forel. Tome Premier. (Lausanne: F. Rouge, 189..)

PROF. FOREL has been for some years occupied in studying the Lake of Geneva, and has now published the first instalment of the fruits of his labours. The work, when finished, is intended to be a complete monograph of the history of a single lake, and will be a most important contribution to an interesting branch of physical geography. In the present volume the geography, the hydrography, the geology, the climatology, and the hydrology of Lake Léman are discussed, after some introductory matter relating to the instruments employed in sounding with other preliminaries. But, though only a single volume, the work embraces so many questions that we must, for want of space, confine our notice mainly to one, which, of late years, has attracted the most attention, at any rate in this country, viz. What has been the origin of the lake basin? Was it formed by the old Rhone glacier or in some other way? The especial value of Prof. Forel's memoir is the number of new facts which it brings to bear on the problem thus propounded.

The Lake of Geneva, however it may have been caused, is more modern than the middle of the Miocene period: "Le lac n'existait pas encore, la vallée du Léman n'était pas même indiquée quand la mer helvétienne déposait les mollasses d'Epalinges et du Mont." Its slopes, and almost certainly its bed, are covered with glacial deposits, of later date than the formation of its basin. Terraces around its shore indicate that its waters once reached a higher level, the greatest elevation which can be identified with certainty, being about 30m. above the present surface. The next pause was at 10m.; after that the lake sank (the fall always being rapid) to its present level. Traces of still higher terraces are to be found on the north shore, but as these neither can be identified on the opposite side, nor correspond with any natural barrier in the course of the Rhone below the lake. Prof. Forel doubts whether they indicate old levels of its waters.

Lake Léman consists of two basins. The first and larger extends from the embouchure of the Rhone to the narrow of Promenthoux. At the east end the slope of the cone of alluvium deposited by the Rhone in no part exceeds 25°. First comes a zone of very shallow water off the actual shore line; to this succeeds a more rapid slope, which gradually eases off as it descends. The current of the Rhone has made and maintains a well-marked channel in this mass of detritus, and the contour lines are affected down to 250m. At the embouchure of the Dranse, on the south shore, another alluvial cone has been deposited. This, however, is rather steeper, but it is much smaller, and does not perceptibly affect the course of the subaqueous contour lines below about 200m. On the north side of the basin the slope varies. Under the walls of Chillon the descent is rapid, amounting to 137 in 100; it is nearly the same near St. Gingolph on the opposite shore, doubtless indicating submerged crags; but it is generally more moderate. West of Vevay it is about one in four, whence it changes gradually to one in ten opposite to Ouchy.

West of this port the descent is still more gentle, and so it continues round the western end of the basin, the lip of the latter being 75m. below the surface. The contours of the south side correspond generally with those of the north, and the form of the basin is evidently related to the geology of the district, being narrower and steeper among the harder rocks at the eastern end. The deepest part is a large rudely triangular area, the apex pointing towards the west, and the base lying roughly north and south, extending from almost opposite to the embouchure of the Dranse to near Lutry. All this area is an almost level plain, for it is wholly below the 300m. contour line, but the greatest depth obtained was only 309·7m.

The Petit Lac may be described as a comparatively narrow and shallow trough, rising very slowly from a depth of about 70 to 50 metres, and then gradually mounting to the embouchure of the Rhone, its bed being slightly interrupted by five small shallow basins, which roughly speaking, have a linear arrangement, but their floors only sink four or six yards at most below the general level.

The lake to some extent is still held up by the huge mass of gravel brought down by the Arve, through which the two rivers have now cut their channels on either side of the plateau of La Bâtie below Geneva. But it is

in the main a true rock basin, though its bed no doubt is concealed beneath glacial deposits and the finer mud brought down by rivers. This alluvium has been studied by Prof. Forel, but into the matter we are unable to enter.

Both the origin of lake basins in general and of that of Léman in particular are carefully discussed by Prof. Forel. He examines, only to reject as attended by insuperable difficulties, the hypothesis that it was excavated by the old glacier of the Rhone. He shows that the subaqueous portion corresponds in its general features with a river valley, and is only a prolongation of that of the Rhone. This valley was first defined at a very early period in the uprising of the Alps ; its excavation progressed with their growth ; it was practically completed at a time when they were higher, perhaps by some 1000 m., than at present. Then the lake was formed by a general subsidence of the mountain region, the lowland remaining comparatively unaffected. The movements of the parts depressed may have been to some extent differential ; but this, in Prof. Forel's opinion, is not a necessary assumption. To us, however, it appears that it would be very difficult to explain the rock barrier at St. Maurice between the upper and lower plains without some amount of differential movement. Prof. Forel's view, of course, is not novel ; for it has been long maintained in England as a general explanation of the greater Alpine lakes by a few geologists, who never bowed the knee to the glacial Baal With their writings, however, Prof. Forel does not appear to be acquainted, though they appeared in publications generally accessible.

The remainder of the present volume is occupied by a discussion of the temperature, rainfall, and general hydrology of the Lake Léman region. It is full of interesting facts and discussions, which we would gladly notice did space permit. The book is well printed, and contains many illustrations, together with a large map of the lake on which the subaqueous contours are depicted. If the book were less diffuse its scientific value would have been greater, but Prof. Forel pleads in excuse that he aimed at writing a volume which would be also acceptable to the general public, or in other words, would combine meat for men with milk for babes. As a comprehensive history of a lake is a great desideratum, it would be ungracious to find fault with Prof. Forel's very natural desire to secure a large number of readers and of purchasers. T. G. BONNEY.

OUR BOOK SHELF.

Horn Measurements and Weights of the Great Game of the World, being a Record for the use of Sportsmen and Naturalists. By Rowland Ward, F.Z.S. (London : Published by the Author, 1892.)

IN these days, when every one is striving to "beat the record," it is only right that sportsmen should have clearly put before them the results already arrived at as regards the size of the trophies and the weight of game-animals already obtained by their brother Nimrods. No one is in so good a position to do this as Mr. Rowland Ward, to whose well-known "jungle" in Piccadilly all the leading shooters of the present day send their "heads" to be mounted and their "skins" to be stuffed. It is, however, much to be regretted that Mr. Ward did not take into his councils some brother "F.Z.S." more versed in scientific knowledge than himself when he prepared this volume, or at any rate did not have the proof-sheets revised by some zoologist with a good knowledge of the Mammalia. The consequence of this want of foresight is that the nomenclature and localities upon which the importance of the records entirely depends are in a very confused state, and in many cases quite erroneous.

Take the Deer (Cervidæ), for instance. Of this family a very correct and accessible list, drawn up by the late Sir Victor Brooke, has been published in the "Proceedings" of the Zoological Society for 1878, which Mr. Ward would have done well to follow. But we find under the Sambur (*Cervus aristotelis*) a head from "Java," where this species certainly does not occur, recorded in the list. Next to this (p. 10) comes the "Central and South Indian Sambur, *Rusa hippelaphus*" (whatever this may be), but three out of the four specimens assigned to it are from Nepal! On the other hand, several heads from Java are attributed (p. 22) to *Cervus rusa*, which is merely a synonym of *Cervus hippelaphus*.

The heads of the large Deer of the Caucasus obtained by Mr. St. George Littledale are assigned (p. 28) to the Red Deer (*Cervus elaphus*). But we have good reason to know that they really belong to the Persian Deer (*C. maral*), quite a different species.

Looking over the list of Antelopes, we find similar errors prevalent, though perhaps not quite to so great an extent. The specimens of the Chiru (*Panthalops hodgsoni*) are assigned to "India," whereas this Antelope is only met with in the snow-fields of Ladakh and Tibet. Nor can the "Takin" (*Budorcas taxicolor*) be properly stated to be from "India." It occurs only in the Mishmi Hills on the frontiers of Assam.

These and many like mistakes are the more serious as Mr. Ward's volume is well got up, nicely illustrated, and likely to be frequently used by the sporting naturalist. But the statements contained in it cannot be relied upon for scientific accuracy.

Der Peloponnes. Versuch einer Landeskunde auf geologischer Grundlage. Von Dr. Alfred Philippson. (Berlin : R. Friedländer and Son, 1891-1892.)

GREECE has hitherto been interesting mainly to scholars, archæologists, and lovers of art ; and no doubt it is from their various points of view that the country will always be most eagerly studied. The subject, however, has also elements of attraction for students of natural science, and it is to these elements, so far as the Peloponnese is concerned, that Dr. Philippson has sought to do justice in the present work. His results have been obtained by direct personal observation, and are set forth with admirable clearness. The book is divided into two parts, the first of which is called "special," the second "general." In the "special" part the author deals with particular regions of the Peloponnese ; in the "general" part he presents an account of the peninsula as a whole. Dr. Philippson is a careful and accomplished geologist, and has been remarkably successful not only in throwing fresh light on the geological phenomena of the country, but in showing their relation to the various orders of facts which come more especially within the province of the geographer. He has also excellent chapters on the forms and phenomena of the surface, on climate, on vegetation, on the animal world, and on the population. In dealing with the last of these subjects he has much that is valuable to say about productive industry, means of communication, density of population, and towns, villages, and other settlements. The interest of the work is greatly increased by maps and profile-sketches.

Traité Encyclopédique de Photographie. By Charles Fabre. (Paris : Gautier-Villars and Sons, 1892.)

IN a previous number of NATURE (vol. xlvi. p. 464) we noticed the first part of the supplement which M. Fabre

proposes to bring out triennially. The present two volumes form a continuation, and extend as far as § 5 of the second chapter in the second book. The author proceeds on the same lines as formerly, and places before the reader in a concise way all the new methods of development, measuring lenses, apparatus, &c., from the particulars of constitution which characterize developers down to the latest form of kodak or tie camera. Not only is each subject treated with the greatest care, but illustrations are numerously distributed. That which will add great value to the work as a whole is the insertion of references, for what, after all, is more annoying than having to wade through a great quantity of literature when the presence of one or two words would have eliminated all trouble? W.

The Reliquary: Quarterly Archæological Journal and Review. Vol. VI. (New Series). (London: Bemrose and Sons, 1892.)

THIS volume consists of the four numbers of *The Reliquary* which have appeared during the present year. The contents include many things which do not quite come within the scope of NATURE; but it is satisfactory to be able to note that the writers, speaking generally, have done their work in a thoroughly scientific spirit. Mr. J. Lewis André contributes an interesting and well-illustrated paper on leather in the useful and ornamental arts, and a clear account is given by the editor of a part of an early dial, bearing runes, which he was lucky enough to find some months ago in the churchyard of Skelton, Cleveland. An illustration gives a good impression of the general character of the stone, the runes on which, according to Canon Browne, are "Danish." Among the other papers are two articles, by Mr. D. A. Walter, on ancient woodwork, and a discussion, by the Rev. A. Donovan, of some of the problems connected with the career of Columbus.

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.*]

Nova Aurigæ.

ON October 5 the Nova Aurigæ was again observed under favourable circumstances, and the observation as to precautions in focussing necessary on account of chromatic aberration of the refractor was amply verified. [NATURE, September 22, p. 489, in which note two corrections should be made: eighth line, for "varying" read "ranging," and fourteenth line, for "(?F)" read "(?G)"] The line near C was distinctly seen at times; but the blue and violet lines observed on September 14 were not seen; the three green lines were very distinct.

On October 14 the red line was much fainter, but there was an obvious bright line in the yellow, which may be the line which Dr. Copeland estimated as 580·1 on August 28 (NATURE, September 15), or may be that which has been measured several times at the Lick Observatory (*Astrophysics*, October, p. 717), and appears to have a wave length of about 575. It had escaped my notice before, but I was induced to look most carefully in the yellow by considerations arising out of an attempt to reconcile Mr. Barnard's observations of apparent nebulosity surrounding the Nova, as seen in the 36-inch refractor at Mount Hamilton, with my own observations of September 14. Mr. Barnard's "stellar *nucleus*" was the difficulty. There appears to be no doubt that the Nova is emitting a spectrum similar to that of a planetary nebula, but it seems to me necessary to have further spectroscopic evidence before it is established that nebulous extension can be seen; if it is to be seen with a simple eyepiece, it must be looked for in a reflecting telescope, as the following considerations will show.

Prof. Keeler's study of the chromatic correction of the Lick Refractor shows ("Pub. Ast. Soc. Pacific," Vol. II. p. 164) that the circle of aberration of F light on the focal plane for the D line has a diameter which is in terms of the focal length ·0000349. We may take this diameter as very nearly that of the circle of aberration of D light on the focal plane for the F line. Thus if a star emits only D and F light, and the F light is focussed, then the D light will fill a circle nearly 7″ in diameter, and the star will look like a planetary nebula with a stellar nucleus. If the star emits light of wave lengths 500 and 575, then interpolation based on Keeler's measurements shows that round a stellar nucleus in the focus for wave length 500 there must be a circle of aberration of nearly 4″ diameter.

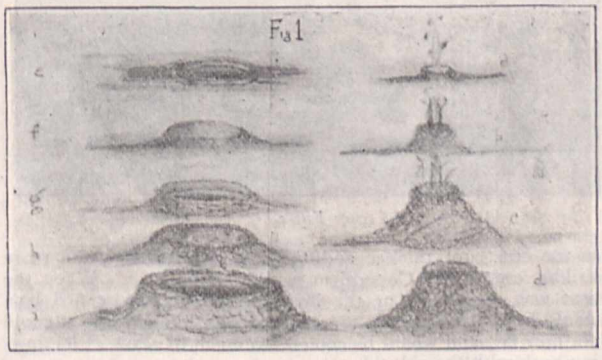
Mr. Campbell found lines of wave lengths 500 and 575 in the spectrum of Nova Aurigæ with respective intensities 10 and 1. Mr. Barnard describes the appearance of nebulosity as "pretty bright and dense," and as measuring 3″ diameter. My own inability to see either the circle of aberration for the yellow line when the green was focussed, or the alleged nebulosity, may be explained in several ways (*e.g.* smaller aperture of object glass, climatic conditions, &c.). The spectroscope could probably decide the question at Mount Hamilton by showing whether the *minimum* length of any of the lines is that corresponding with 3″ diameter on the slit. I have not been able to do more than observe that the yellow line is not visible when the 500 line is focussed on the slit of a spectroscope having an effective dispersion of two 60° prisms. H. F. NEWALL.

Observatory, Cambridge, October 24.

Formation of Lunar Volcanoes.

WHILE we have, on the lunar surface, a series of markings so evidently volcanic that no one thinks of applying any other term to them, we have on the other hand no explanation of their mode of formation which will stand examination. The explanation given by Messrs. Nasmyth and Carpenter in their splendid work on the moon, founded upon explosive expulsion of lava, fails to satisfy the mind when applied to wide craters with a low wall such as Shickard or Grimaldi, of which there are so many on the moon, and which look more like some disturbance in a semi-liquid surface than an accumulation of volcanic débris.

The umbrella-like eruption figured in Messrs. Nasmyth and Carpenter's book does not represent any phenomenon within our experience, as the erupted material (unless light enough to be driven by wind) invariably falls back into the neighbourhood of



the vent, and we could not conceive of its being shot neatly out twenty-five miles on every side to form the familiar ring.

An explanation of the mode of formation founded upon lunar tidal motion occurred to me about seventeen years ago, from observations on a cooling slag; but until the recent publication of Mr. Darwin's work on the history of the tides I was doubtful if that force were sufficient to account for observed results.

I had noticed that the rise and fall of a fused slag through holes in its solidifying crust, formed craters exactly like those in the moon; and I enclose a photograph of a piece of that slag in which is reproduced all the salient features of the lunar surface.

The mode of formation was as follows:—

The fused liquid (which was potash "black ash" containing a mixture of substances of very varied melting point) was still giving off some gas, which escaped as at *a* in Fig. 1, building up

a miniature crater as at *b*, *c*, *d*. But the crater vent becoming intermittently choked, the accumulation of gas beneath the crust caused the liquid "lava" to rise through any neighbouring holes as at *e*, *f*, giving rise to a ring crater. The pressure of the accumulated gas now drove out the obstruction in *a*, when the liquid lava receded in *e* as at *g*. This intermittent action went on till the crater *i* was built up—entirely by "rise and fall" (as of a tide), no gas escaping at this hole.

In the case of the moon the rise and fall would be caused by the tidal motion of the still liquid interior. The solid crust would resist the periodic rise of the liquid interior, and the liquid would well through the crust and recede again as the wave passed.

When the crust was thin, and the lava very liquid, the large ring structures would be formed, as the lava would flow far; but

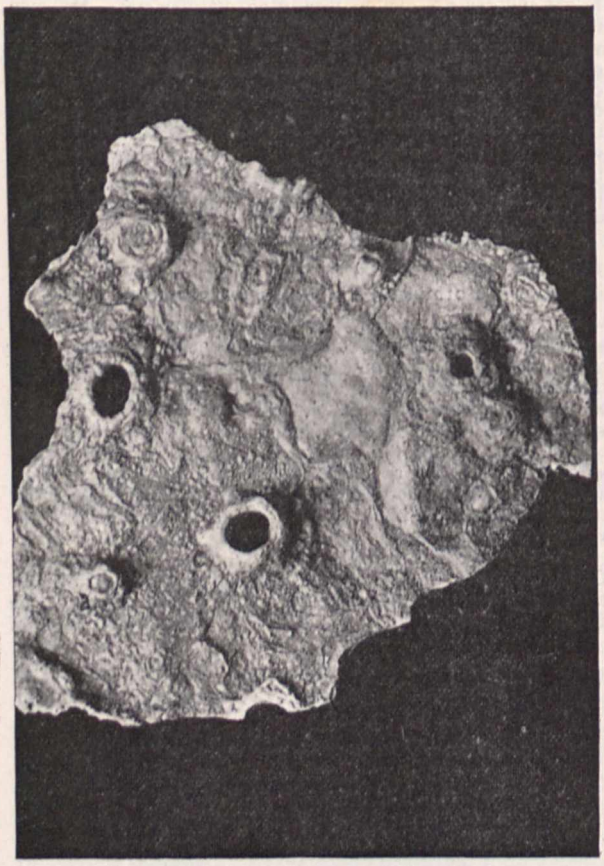


FIG. 2.

as the crust got thicker and the lava more viscid, the more striking craters like Copernicus would be built up. When the vent was very small, or the lava very viscid, the exuded lava would build up mountain ranges, or peaks like Pico, as it could not flow far, and would be cooled too much to allow of its flowing back with the ebb tide.

The existence of the cause proposed by Messrs. Nasmyth and Carpenter, viz., expansion on solidification, is very doubtful. The proof they adduced was that a piece of solid slag would float on liquid slag. But when slag solidifies it becomes filled with small cracks, which doubtless contain air, and so aid in the flotation. When I was working at this subject I had some slag poured into an iron mould kept cool by immersion in water. When the slag had cooled a distinct depression was seen on the upper free surface, showing that the slag had contracted during solidification. No doubt its contraction or expansion will depend upon its composition, and we do not know the composition of the moon's surface, but we need not depend upon a doubtful property for an explanation when a set of conditions have existed which must have yielded an ample force for the production of the observed results.

In the photograph marked Fig. 2, at *a* can be seen a crater with a raised floor and a central cone, at *b* a crater filled to the lip like "Wargentin," while on the plain near *b*, and round the open crater *c*, will be seen numerous minute craters, as on the moon's surface in the neighbourhood of "Aristotle" or "Copernicus," while in other photographs are seen walled plains like the "Mare Crisium," so that all the important features of lunar topography are reproduced in this slag, and there are many minor points of agreement which cannot be gone into in the limits of a letter.

Although I have always considered the tides the cause of the wonderful lunar configuration, I was not satisfied that that cause alone was of sufficient magnitude, till the work of Mr. Darwin placed the matter in such a clear light that I now venture to submit the idea to your readers as a feasible explanation of the familiar lunar features.

J. B. HANNAY.

On the Need of a New Geometrical Term—"Conjugate Angles."

IN geometrical discussions, such as arise out of a great variety of physical problems, it is frequently necessary to refer to an acute or obtuse angle A as being equal to another acute or obtuse angle B, because contained by two straight lines which are respectively perpendicular to those containing the angle B. Such a statement of the reason of the equality is, however, cumbrous. Sometimes, indeed, such angles when acute might be described as equal because they are the complements of equal (because vertically opposite) angles; but it will often happen that the figure does not show the vertically opposite angles that would be referred to.

I should be glad to know whether there is any term expressing the relation in question in use among either English or foreign writers, and, in default of such, would suggest that such angles be called *conjugate*, or if greater precision is required, *rectangularly conjugate*, the general term *conjugate* to be used when we wish to refer to an angle A as equal to an angle B because contained by sides whose directions are the directions of the sides of B, after each has experienced an equal and similar rotation in the plane of the diagram, whether the rotation is through a right angle or not.

The shorter inclusive term *conjugate* could always be used for the less general but longer term *rectangularly conjugate*, when brevity was aimed at.

A. M. WORTHINGTON.

R.N.E. College, Devonport, October 30.

Printing Mathematics.

THE main features of mathematical work that give trouble in printing are three: the expressing of (1) fractions, (2) powers, (3) roots.

(1) To simplify the expression of fractions we have the solidus suggested by Sir G. Stokes. But the solidus has been hitherto much less used than it might be, on account of the uncertainty as to how far its influence reaches in any expression more complicated than the simplest fractions. This uncertainty can easily be removed, and the usefulness of the solidus greatly extended by defining more definitely its exact meaning. This is done in the simple conventions proposed below.

(2) To express the process of involution, the sign $\backslash$, suggested by Mr. C. T. Mitchell in the *Electrician*, is more concise and clearer than that mentioned by Prof. S. P. Thompson in NATURE. And Mr. Mitchell's sign, if defined by conventions similar to those applied below to the solidus, is capable of a like extensive application.

(3) To express roots we have the sign $\surd$. But, when accompanied by a horizontal line above to show the extent of its influence, this sign also requires special spacing. But it can be brought into line with the rest by the use of the same conventions.

Taking then for

α. the sign of division	...	...	...	/
β. ,, ,, involution		...	...	$\backslash$
γ. ,, ,, evolution	...	...	...	$\surd$

we may use each of these signs in either of two ways:—

I. Simply as a sign of operation, in which case it can influence only the quantities immediately adjacent to it.

II. In a double capacity—

(1) As a sign of operation.

(2) As one end of a bracket, of which the other end is |. This bracketing influence may be directed either forwards or backwards, or both ways at once.

Examples.

α. Division.

$$\frac{a+b}{c}+d = |\,a+b/c+d,$$

$$a+\frac{b}{c+d} = a+b/c+d\,|\,,$$

$$\frac{a+b}{c+d} = |\,a+b/c+d\,|\,,$$

$$a\,.\,\frac{b}{c}\,.\,d = ab/cd, \quad a\,.\,\frac{b}{cd} = ab/cd\,|\,,$$

$$\frac{a}{b+c}(d+e) = a/(b+c)(d+e),$$

$$\frac{a}{(b+c)(d+e)} = a/(b+c)(d+e)\,|\,,$$

$$\sin\frac{\theta}{n} = \sin\theta/n, \frac{\sin\theta}{n} = |\sin\theta/n.$$

Continued fractions also can readily be brought into one line by this notation—

$$\frac{a}{b+}\,\frac{c}{d+e+}\,\frac{f}{g} = |\,a/b+c/(d+e)+f/g\,|\,,$$

β. Involution.

$$a^b = a\backslash b,\ a^{-b} = a\backslash -b,$$

$$(a+b)^c+d = |\,a+b\backslash c+d,$$

$$a+b^{c+d} = a+b\backslash c+d\,|\,,$$

$$(a+b)^{c+d} = |\,a+b\backslash c+d\,|\,,$$

$$(a+b)^{\frac{c+d}{e+f}} = |\,a+b\backslash|\,c+d/e+f\,|\,.$$

$a\backslash b/c$ and $a/b\backslash c$ are ambiguous, but $|\,a\backslash b/c = \frac{a^b}{c}$, because | being unnecessary for \ in this case, can apply only to /. $\frac{(a+b)^c}{d}$ is $\|\,a+b\backslash c/d$, two vertical lines being required.

Similarly $|\,a/b\backslash c = \left(\frac{a}{b}\right)^c$, $a/b\backslash c\,| = \frac{a}{b^c}$, $a\backslash b/c\,| = a^{\frac{b}{c}}$, $a\backslash b\backslash c\backslash d = a^{b^{c^a}}$.

γ. Evolution—

$$\sqrt[n]{a+b+c} = |\,n\backslash a+b+c\,|\,,$$

$$\sqrt[a+b]{c+d} = |\,a+b\backslash c+d\,|\,,$$

$$\sqrt[a+b]{c+d} = |\,a+b\backslash c+d,$$

$$a+b\,.\,\sqrt{c+d} = a+b\backslash c+d\,|\,,$$

$$a+\sqrt[b]{c+d} = a+|\,b\backslash c+d.$$

In some cases lines of differing thickness might be advisable; for instance—

$$\frac{b^{\frac{1}{2}}}{c(d+e)^3} = |\,b\backslash 1/2\,|\,/\,c\,|\,d+e\backslash 3\,|\,.$$

There are many other ways in which this notation might be used; but the above will suffice to illustrate the advantages of it. And these advantages are substantial. It enables the work to be printed in the same space as ordinary letterpress, and thus avoids the special spacing, from which nine-tenths of the troubles in mathematical printing arise. It requires no new types, except, perhaps, \, and each of the signs used is suggestive of the original mode of writing for which it is a substitute. It can be used without confusion in conjunction with all ordinary brackets. How far this notation would suit very complicated expressions, is a point that would have to be determined by experience; but for printing mathematics of ordinary complexity it would be useful in economizing space and diminishing the risk of printers' errors without any sacrifice of clearness.

Cambridge, October 27. W. CASSIE.

"Sunshine."

IN acknowledging the courteous criticism and the kind remarks which "C. V. B." has been pleased to make about my little book, may I be permitted to comment on one or two points, which I think he has imperfectly understood from the text. We all know that when "C. V. B." undertakes to review a book, he does his work in a thorough and searching manner, and from his critique it is evident that "Sunshine" has been well read. Notwithstanding this, in one or two of the instances selected for criticism the meaning, at once simple and obvious to a little child, who neither knows nor suspects any other, seems to have missed him, presumably because he knows all the bearings of the subject. Thus it is sometimes a disadvantage to be learned. Of this I propose presently to give an instance in the order which it occurs.

After poking fun at me, because, the "Sunshine" course being ended, Tommy meets King Sol face to face and "has it out with him," my critic proceeds to discuss the limits within which the imagination may be appealed to as a factor in scientific education, and while I agree with him in the main, I am tempted in passing to remind him of what Tyndall terms "the scientific use of the imagination," to which the clearness and (to me) the charm of his own lectures is largely due. Be that as it may, in one of "Nature's Story Books" I feel fully justified in employing, *within the limits of scientific accuracy*, any or all of the powers of the mind, which shall help children and others to *realize* the relation they bear to their surroundings, assured that in a course based upon some hundreds of experiments synthetically worked out and deductions made—a course whose main object is to lead children to go direct to Nature, *via* experiment, for their knowledge, there is little danger that the imagination be cultivated at the expense of the reasoning faculties. The experience of the writer is that the children attending the lectures became extremely critical—a state of mind which, although of inestimable value in acquiring knowledge, is not one of the happiest in other respects. Therefore it was thought desirable to provide them with some necessary ballast, and this is my defence of the hypnotic visit to the moon, and the other two chapters to which the critic alludes.

Natural science apart, it seems to me that the tendency of the school-teaching of to-day is calculated rather to make children hard and matter of fact. For this reason I have endeavoured in these Sunshine Stories to interest children in the poetry of their common lives, myself playing somewhat the *rôle* of an optical instrument, presenting images sometimes real, sometimes virtual of those physical beauties which touch them at every point. The fact that "C. V. B." recognizes in "Sunshine" the *realism* which the "picturesque language" was intended to convey, disposes of the case of my Cape Town reviewer, who mildly insinuates that I have been guilty of some *fraud* upon little children in calling "Sunshine" a story-book. Therefore I am the more glad that "C. V. B." agrees with me that the mathematical side of these questions should not be obtruded. There are so many excellent text-books which supply that information for older pupils. I need not say that I shall be most happy to add the exception in the case of the rainbow. I thank him also for pointing out a passage in the notes where an additional clause is necessary, owing to the transposition of a paragraph. But I take exception to the statement about the top, for it is evident that the experiment is not made under the same conditions as that which "C. V. B." has in mind, because my boys get green and he gets (he says) white, or nearly white. The home experiment reads: "I am giving each of you squares of coloured paper to take home . . . then you may have the papers to put on your tops—*e.g.*, cover half blue and half yellow, spin the top and you will see green." A note on page 341 refers to the kind of paper. Now it seems to me from the expression "painted disc," which "C. V. B." has made use of, that possibly he may have had Clerk Maxwell's top in mind when he wrote.

When I say to a boy, "Here are two squares of paper, one blue and one yellow; when you've done so and so, you can have the paper to keep—cover your top, half yellow, half blue, &c.," the lad understands me, and when I am not there he takes out his halfpenny whip-top, tears a piece of the blue paper, and rendering it slightly adhesive hammers it down on the top with his right fist; he tears a similar piece and treats it in the same way, and so on until he has covered half. Then he takes the yellow paper and covers the other half with irregular patches of yellow. He spins the top and sees green.

How different is this from Clerk Maxwell's top. Clerk Maxwell selected for his top the purest of paper and pigments. He endeavoured to match the spectral colours (considerably diluted). He selected a scarlet red with a tinge of orange like *orange-red vermilion*, lying in the spectrum one-third the way towards D, between the lines C and D. His green was one-fourth the distance from E, between E and F, and resembled emerald green. He also selected a blue violet midway between F and G, which was imitated by that purest of colours—ultramarine. Now let us try the given experiment under the favourable conditions guaranteed by Maxwell's discs, viz., the purest of colours painted on Whatman's paper. Taking up a disc of ultramarine and another of pale (not orange) chrome yellow, and concealing half of one disc behind the other, on rotating the compound disc so that the eye shall receive simultaneously blue and yellow light, the result is not white or even practically white, but a *grey, tinged with yellow*. By a careful adjustment, hiding more of the yellow and exposing more of the blue (thereby *altering* the proportions of the text), it is possible to get rid of this yellowness and to obtain an absolutely neutral grey which it might be possible to persuade some grown-up people represented white, but which on analysis yields 71½ per cent. black to 28½ per cent. white. This may be proved by revolving a disc of black and white sectors in the above proportions, the results in each case being identical. But even this result, unsatisfactory as it is, does not apply to the passage quoted in the text, in which no special conditions are observed. I maintain what is easily proved by experiment in less time than it takes to write it, that when ordinary colours, *e.g.*, gamboge and Prussian blue, are used, the residual light is green. [1]

I fear that already this letter is too long, and since I do not wish to monopolize the space kindly placed at the disposal of your correspondents, I must defer the consideration of the annotations on soap films. The other points are dealt with in the preface. AMY JOHNSON.

52 Lower Sloane Street, S.W., October 12

I DO not think that the observations on my review of "Sunshine" require more than a very short answer.

I considered that the authoress had not by any means cleared the confusion which usually exists as to the meaning of the expression "mixing of colours." It is applied both to the case where two or more colours are seen superposed, *e.g.* by spinning coloured paper where the resultant tint is due to the sum of the separate colours in the constituents, and to the case of mixed pigments where the resultant tint is that which is common to the constituents. Now as the common "paint box" rule says that blue and yellow make green, that is that blue and yellow pigments mixed produce a green pigment, it seems to me very misleading to say "Cover half (of your top) blue and half yellow and you will see green." Of course it may happen that the slight departure from white which will be observed may be in a greenish direction, but it may also be inclined towards pink, or, for anything I know, towards any other colour. The one thing it will not do, however, is to make a green such as is obtained by mixing the pigments, and such as I fancy from the context any one would expect. C. V. B.

The Photography of an Image by Reflection.

THE great utility of spark photography for obtaining time records of quickly-moving objects must be apparent to all who know the experiments of Mr. C. Bell, Prof. Boys, and Lord Rayleigh. By means of spark photography the shadow of any object such as a jet of water, a flying bullet, or a broken soap film can be produced with perfect definition. The shadow of the moving object illuminated by an electric spark is thrown on to a sensitive plate in a dark room, and the plate is developed in the usual manner. The process of spark shadow photography will be found, I believe, of great service in physiological research. With a view to try this I attached a long sensitive plate to the traversing carriage of a chronograph; the moving carriage closed and opened the primary circuit of an induction coil at prearranged equal intervals of time. In front of the moving plate a frog's heart was placed in a slit on a screen; at each break a shadow of the heart was thrown on to the plate by means of the induced spark. By this means thirty positions of the heart were registered; the pictures were all sharp and clear. I have also used the same method for photographing the movements of insects.

Since these experiments which I showed during the University Extension Meeting in Oxford this year, I have made several attempts to get spark photographs of the front view of objects (not their shadows). In my first experiments the objects were illuminated by an electric spark, the image being received on a plate in an ordinary camera. I found that so much useful light was shut off by the lenses that only a dim picture could be produced. A quartz lens was next tried and the results were rather better. I then determined to use no lens, but in its place a silvered mirror. A concave reflector made by silvering a concave lens of about 10 c.m. diameter was so placed that it reflected the image of a white paper star 7 c.m. diameter, revolving about 60 times in a second, on to an ordinary photographic plate, the total length traversed by the light being 80 c.m. The star was illuminated with a spark exactly similar to that used in the previous experiment; on development a good picture of the star came out. The reflector was neither well made nor well silvered. The idea was suggested by observing some spark photographs I obtained of waves on the surface of mercury reflecting light. When a steady light is used a photograph of any object is readily obtained by reflection from a suitable mirror. Probably a steel surface would be best. The mirror and plate were placed in a long box provided with a hole at one end through which the light reflected from the object passed. A few experiments made on living objects to test the time of exposure in Reflection Photography showed that in order to avoid over-exposure, a very rapid shutter must be used.

FREDERICK J. SMITH.

Trinity College, Oxford, October 25.

Induction and Deduction.

AS your correspondent invites discussion on this subject I hope you will allow me to repeat in a new form the views I expressed upon it in your columns some months ago. I quite agree with Mr. Russel in maintaining that "true induction is utterly unable to yield us any conclusion that is more than probable and approximate," understanding by induction inference from one or more special cases to a more general rule. But on the other hand it appears to me that Miss Jones's criticism is quite destructive of Mr. Russel's interpretation of geometrical reasoning. The point which both have missed I believe to be this, that a proposition stated in given words, such as the enunciation of Euclid's *pons asinorum* does not always and to every one convey the same information; and if it is meant in one sense its degree of reliability, and the method by which it must be proved, will be quite different from what they would be if it were meant in another. There are at least three different kinds of interpretation which may thus be put upon the proposition. It may mean (1) the triangle used to illustrate this proposition has equal sides; therefore it has equal angles; or (2) I have conceived a triangle which has equal sides, therefore I have conceived one which has equal angles; or (3) the connotation ascribed by the adjective "isosceles" implies the connotation "having equal sides."

It is not necessary for me here to dwell upon the distinction between the first two interpretations; but the difference between either of them and the third is that this latter gives us no information about any real thing or concept, but only about what is implied by using certain terms. And this latter kind of information clearly does not require to be based upon any real knowledge of things, but may be based solely on definitions of words. Arguments with propositions interpreted only in this sense are what I call symbolic arguments; and symbolic conclusions therefore give no real information unless they can be interpreted by the aid of real assertions, such as "I can conceive," or "There actually exist, things possessing the connotations ascribed to these terms by their definitions."

If this distinction has not before been recognized, it is because in most logical discussions we can in this way give a real meaning to our arguments. In elementary geometry, for example, we can—with more or less effort—conceive things, or even actually draw them, which answer to our definitions with sufficient accuracy. And, indeed, the reason why "Euclid"

[1] The purport of the experiment will be best understood if I state that it rollows a series of chapters on colour, viz.: the rainbow, the spectrum, its ecomposition by refraction and by reflection; while the last chapter discusses and explains, with experiments, the question of spectral lights *versus* pigments. The common surface papers, which the children are daily in the habit of using, are then analysed by the prism, and found to be anything but monochromatic.

and "Newton" are generally considered to yield a more valuable mental training than such subjects as analytical geometry is that the older authors, perhaps because they were a bit afraid of purely symbolic argument, tried constantly to keep real pictures and ideas before the minds of their readers. But even so our conviction of the truth of any but the simplest theorems of geometry depends chiefly on the symbolic argument, not on the realization in succession of the actuality of the relations and operations discussed in the course of the proof. This is perhaps sufficiently obvious in the higher branches of even Euclidian geometry, but it becomes absolutely indisputable when we reach such theorems as "Any two conics in one plane intersect in four points." Not only may some of these points be at an infinite distance, but some, or all, may be what is called, on the *lucus a non lucendo* principle, "imaginary"; that is, they may be such that they cannot be imagined by anybody, much less actually drawn.

Accordingly I cannot admit that the theorems of geometry are established by induction at all. If they are interpreted in either of the first two ways I have described, they are only particular propositions, and the inference from them to a general proposition would no more yield a "mathematical certainty" in this case than in any other. And though the third way of looking at the proposition may be paraphrased into a form which appears general (*e.g.*, anything which may fairly be called "an isosceles triangle" may also be said "to have two equal angles"), it is really only a particular proposition about the words "isosceles triangle," and so on. Its wide applicability and usefulness depends on the fact that we can, and do, often find things which can fairly be called isosceles triangles; but it must be admitted that the assertion that, on any given occasion, we have found such a thing,—is not a mathematical certainty. If the triangle in question is an objective one, we can only say that it is probably, or approximately, isosceles; and though perhaps we may subjectively conceive perfectly isosceles triangles, and so regard the *pons asinorum* as a subjective necessary truth, it must be doubtful whether we could do so in the case of a more complex proposition such as Pascal's Theorem, and it is quite certain that we could not do so in the case of such theorems as that about the intersections of two conics.

It is to be hoped, therefore, that logicians will come to recognize the importance of symbolic reasoning, as mathematicians have already done. And when they do so we may hope for this further advantage, that they in turn will teach mathematicians and others not to confuse a purely symbolic with a real conclusion—not to assume that, because they have correctly proved a conclusion symbolically, that it therefore necessarily gives any information about real things, or even real concepts.

EDWARD T. DIXON.

Trin. Coll., Cambs., October 22.

Bell's Idea of a new Anatomy of the Brain.

IN NATURE of October 27 the writer of the review of Mr. Horsley's "Structure and Functions of the Brain," speaking of the rarity of the above book, states that he only knows of one copy in London, viz., that in the British Museum. It may be useful to some of your readers to know that there is a very interesting copy in the library of the Royal College of Surgeons. It is the presentation copy to Dr. Roget "from Mr. C. Bell, 34, Soho Square": by Dr. Roget it was given to Lady Bell, who presented it to the Royal College of Surgeons through Mr. Alexander Shaw.

Mr. Shaw has added in MS. a copy of the letter received from the printers fixing the original date of publication, and also the list of persons to whom presentation copies were sent. The letter and the list are both published in Mr. Shaw's reprint of the Tract in the *Journal of Anatomy*, vol. iii., 1869.

JAS. B. BAILEY,

October 27. Librarian Roy. Coll. Surgeons.

Photographic Dry Plates.

IN reference to "Prevention's" note on Photographic Dry Plates, one cannot but agree with him that packets should be dated when issued from the factory.

I would venture, however, to suggest that good makers' plates do not deteriorate within a reasonable length of time.

As an illustration of my experience I may mention that in April this year I opened a box of plates (½ plate Extra Rapid) which I bought in July 1886. I had carried them on a three months' tour in the Mediterranean in 1888, and had taken no special care of them since.

They proved in every way as good as new, both in sensitiveness, and perfection and evenness of film.

ARTHUR E. BROWN.

THE GENUS SPHENOPHYLLUM.

NOTWITHSTANDING the small size and comparative scarcity of the plants belonging to this Palæozoic genus, they have long attracted a rather unusual amount of attention. This has been partly due to their peculiar external forms, which suggested even to the earliest observers the idea of resemblances to the Marsiliæ; but the interest they have excited has been further increased of late years by discoveries respecting the peculiar organizations of their stems. In 1822 Adolph Brongniart assigned to them the name of "Spenophyllites," and in 1823 Sternberg figured some of them under the generic title of "Rotularia."[1] Sternberg's figures appeared in his "Versuch einer Geognostisch-Botanischen Darstellung der Flora der Vorwelt," which work is now best known through the French translation of it by Comte de Bray. To the first of his specimens figured (*loc. cit.*, tab. xxvi., figs. 4*a* and *b*), Sternberg gave the name of *Rotularia pusilla*, and the example so designated is very characteristic of the simpler type of the group, in which we have a somewhat branched stem, with verticils of wedge-shaped leaves at each node. A second form was figured on a later plate of the same work. It is interesting to note that Sternberg associated with these figures the observation, "Plantæ organisatione foliorum Marsileis, forma caulis Hippuri Maritimæ." The generic name thus given by this author represents the rotate arrangements of the leaves in each verticil, as the wedge-shaped contour of each separate leaf is further indicated by Brongniart's generic term, "Sphenophyllites." In 1820 Von Schlotheim had also included similar examples in his too comprehensive genus, "Palmacites."[2]

In 1828 Brongniart published his classic "Prodrome d'une Histoire des Végétaux Fossiles," in which work we find the generic name of these plants changed to Sphenophyllum, which name they have retained to the present time. In this work Brongniart examines in some detail the probable affinities of these plants, which even in 1822 he inclined to regard as having some affinities with the Marsileæ. He defines them as having six, eight, ten, or twelve leaves in each nodal verticil, each leaf being wedge-shaped; sometimes entire, truncated at its apex, which is denticulate. In some others these leaves are bilobed, and in other species they are not only profoundly bifid, but each of these lobes is either divided into two, or their ends are laciniated. Lastly, in some cases the lobes become narrow and linear. Brongniart here compares these leaves with those of Ceratophyllum and Marsilea, concluding with the statement, "We cannot for the moment decide between these two relationships." At this date the fructification was wholly unknown.

In his introduction to the "Natural System of Botany," p. 37, Brongniart again reverts to the idea that Sphenophyllum had Marsileaceous affinities.

In 1831 the authors of the "Fossil Flora of Great Britain" commenced their publication of that work, and in one of its early numbers they figured and described under the name of *Spenophyllum erosum* what appears to be identical with the first figure published by Sternberg. When discussing the relationships of this plant, Lindley and Hutton

[1] These figures were preceded in 1709 by a still earlier one by Scheuchzer in his "Herbarium Diluvianum." (Coemans and Kickx, "Monographie des Sphenophyllum d'Europe").

[2] "Die Petrefactenkunde auf ihrem jetzigen Standpuncte,"

reject Brongniart's idea of its possible affinity to the Marsileæ, inclining to the belief that it approached nearer to the Coniferæ, and especially to Salisburia. This impression they retained when, at a later date, they described a second species of the same genus.

In his "Tableau des Genres de Végétaux Fossiles," published in 1849, Brongniart returns to the subject. He here calls attention to the readiness with which Sphenophyllum may be confounded with the genus Asterophyllites, which some forms of the former genus closely resemble; but he again repeats that the two can be distinguished by the fact that in the former genus the leaves never exceed ten in number, whilst their form is triangular with a truncated summit. He again dwells upon the fact that in some Sphenophylla the leaves become so deeply lobed, narrow, and linear, as to be easily mistaken for those of Asterophyllites. He now affirms that the fructification is closely related to that of Asterophyllites.

As to the affinities of Sphenophyllum, Brongniart now asks, "Does the plant combine the leaves of a Marsilea with the verticillate of an Equisetum, or is it a Gymnospermous Phanerogam, the leaves of which approach those of the Gingko?" He does not answer the question, but concludes that this cannot be done until the fructification of the plant is better understood.

In 1864 a monograph on the species of the genus was published by M. Eugene Coemans and M. J. Kickz; but the authors make no serious effort to solve the vexed question of the affinities of the genus.

We now enter upon a new stage in the history of the genus. In 1870, M. Renault presented an important memoir to the French Academy of Science, which, for the first time, threw light upon the internal organization, especially of the stems, of Sphenophyllum. He described two examples, one from Autun and the other from St. Etienne, both of which exhibited a structure wholly different from that of any plant previously known, recent or fossil. In the centre of each stem was a primary vascular bundle, the transverse section of which was a triangle with three concave sides and three prolonged, narrow, intermediate arms. This axial organ underwent no subsequent growth after its first formation. But it was invested by a secondary zone, which was deposited upon the primary triangle layer after layer like a secondary xylem, producing a circular axis, which enlarged as the plant advanced in age. But this secondary growth did not consist of layers of vessels, but of vertical columns of thick-walled cubical cells. The cortex also exhibited specially distinctive features. These discoveries made it clear that Sphenophyllum constituted, not only a very distinct genus, but a type of plant far removed from everything previously described.

It fell to my lot to make the next advances in our knowledge of this genus. In 1871 I described in the memoirs of the Literary and Philosophical Society of Manchester a new fructification, to which further reference will be made later on. In 1872 I obtained from the Oldham deposits some new stems which obviously belonged to the same type as those discovered by M. Renault, but from which they differed in important points of detail. These were described in my Memoir, Part V., published in the Philosophical Transactions for 1874. Transverse sections of these closely resembled in their dominant features M. Renault's corresponding ones, but with two differences. When my plants attained to a certain stage of their exogenous growth, a well-defined circular boundary marked a temporary arrest of that growth, but which started afresh from a zone of much smaller vessels (*loc. cit.* Pl. II., Figs. 11 and 12), that increased in size as the diameter of the axis increased, as they had previously done in the more internal series. Still greater and more important differences presented themselves in the longitudinal sections. The zones of secondary or exogenously developed xylem, which in M. Renault's examples consisted solely of vertical columns of thick-walled, cubical cells, were composed, in mine, of true tracheidal vessels with reticulated (not with bordered pits) walls; presumably a higher stage of development. Another new and more advanced feature than characterise Renault's cells, seen best in tangential sections of this zone (*loc. cit.* Fig. 13), was the existence, between contiguous tracheids, of vertical, but interrupted, series of small cells, which I can only regard as rudimentary medullary rays. In the same memoir (*loc. cit.* Pl. IV.) a still more distinct form from the Burntisland deposits in Fifeshire was figured and described. M. Renault and Count Solms Laubach refuse to recognize a Sphenophyllum in this type, but they have not yet convinced me that I am in error on the point. The fact is that, though widely aberrant from the form described above, it scarcely differs more from that form than the latter does from M. Renault's examples.

But my Oldham specimens raised another debated question. When the Memoir V. was published, all authorities agreed that the maximum number of true leaves in each verticil was ten or twelve; that, however deeply subdivided, their outline was a sphenoid one, and not linear, and that they were multinerved. But I am still convinced that in my specimens there were more than twenty such leaves; that they were linear in outline, and had a single median nerve. It followed that, continuing to accept the existing definitions of the genus Sphenophyllum, my plant was Asterophylloid rather than Sphenophylloid. I am now prepared to admit that it is a Sphenophyllum; but only on the condition that we alter our definitions of the latter genus, and admit the possibility that some of the forms may possess twenty or more undivided and linear leaves. The accumulating evidence that the foliage of at least some of the Sphenophylla was dimorphic makes the acceptance of my proposition a matter of necessity.

Yet more recent researches have revealed new and important facts connected with the history of these plants. I have already alluded to the new fructification which I described in 1871, and to which I gave the name of *Volkmannia Dawsoni*. M. Renault's memoir already noticed was laid before the French Academy in May 1870, and noticed in the *Comptes Rendus* of that date; but owing to accidents growing out of the Siege of Paris, it was not published until three years later. Meanwhile my memoir on *Volkmannia Dawsoni* was published, and a copy of it forwarded to M. Brongniart. After giving details of the structure of the strobilus I arrived at the conclusion that "it is the fruit either of Asterophyllites or of Sphenophyllum."

Two years later M. Renault's memoir of 1870 was combined with a second one on the same subject, and published. It contained a note by M. Brongniart, referring to my memoir of 1871, in which note he says, "This work agrees in many important points with the results obtained a year previously by M. Renault, though Mr. Williamson was unacquainted with the article in the *Comptes Rendus* of May 30, 1870. The fossil plant studied by Mr. Williamson, and named by him *Volkmannia Dawsoni*, doubtless differs, at least specifically, from that described by M. Renault, by the form of the central vascular bundle, and by the absence of the zones of quadrangular cells which surround it in the French specimens; cells which in consequence of the thickness of their walls would not be readily destroyed."[1]

[1] M. Brongniart has here failed to comprehend an important point. The cells, the absence of which he notices, really belonged to the secondary xylem of the older stem, which did not become developed in the youngest twigs. But it was only upon these twigs that the fructifications were formed, and of which they were but extensions. Hence their absence was merely a consequence of difference of age, and not a feature of specific value.

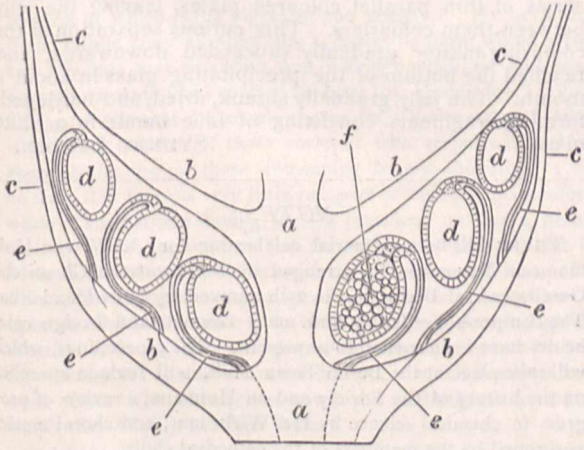
In 1890 I figured in my Memoir XVIII. (Phil. Trans. 1890) a transverse section of what was obviously a stem of *Bowmanites Dawsoni*, in which the primary triangular axis of the strobilus was invested by a thick zone of the secondary xylem. So far as the arrangement of its tissues is concerned this stem is constructed on exactly the same plan as appears in M. Renault's and my own Sphenophylla. In describing it I further said, "We must unite Sphenophyllum with some forms of Asterophyllites in the same genus. It is equally clear that Bowmanites, though its peculiar fructification demonstrates that it constitutes a perfectly distinct genus, has strongly marked features of affinity in the structure of its stem to the Sphenophylloid type."

The above reference to differences between the fructification of Bowmanites and of Sphenophyllum were based upon the minute description of the fruits of the latter plant, published by M. Renault ("Etudes sur le Terrain Houiller de Commentry," pp. 481-2). Those descriptions differ widely from what exists in my Bowmanites, but M. Renault distinctly identifies them with the fructification of Sphenophyllum. I obtained additional and important specimens of Bowmanites in 1890, which threw much new light upon its organization, and which were recorded in my Memoir XVIII. (Phil. Trans. 1891).

In July last an important communication was laid before the Academy of Sciences by my friend M. Zeiller, the distinguished director of the Superior National School of Mines at Paris. In it he records his identification of a fructification of a Sphenophyllum of the type of *S. pusillum* of Sternberg and *S. erosum* of Lindley and Hutton, with my *Bowmanites Dawsoni*. If this determination is correct, and I see no reason for doubting that it is so, we now have some more definite facts than we have hitherto possessed, guiding us alike in identifying the true fructification of Sphenophyllum and in determining its position in the vegetable kingdom.

Before explaining M. Zeiller's observations more in detail, a few words explanatory of the structure of Bowmanites will make M. Zeiller's views more intelligible to the reader.

The accompanying diagram represents two nodes and



one internode from a vertical section of this fruit, with the sporangia and three sporangiophores *in situ*.

So far as external contours are concerned, it is undistinguishable from many of the true Calamarian forms of fructification. It is only when cut into sections that its characteristics can be discovered. Its central axis (*a*) has nodes (*b*) at short and regular intervals, and at each node is a verticil of from 16 to 20 sporophylles or fertile bracts (*c*). At their basal portions these bracts are coalesced into a lenticular disk (*d*), from the margin of which the thinner and narrowing bracts extend upwards, overlapping from two to three internodes. From the upper surface of the disk numerous slender sporangiophores (*e*) spring, each one proceeding upwards and outwards, to become attached to the *upper or distal* extremity of a large oval sporangium (*f*). Each of these sporangiophores has running through it a small bundle of barred tracheids, which terminate at the point of attachment to the sporangium. Each tracheal bundle is a prolongation of one of a circle of similar ones that ascend from the central axis into the disks. These fructifications, besides being manifestly eusporangiate, are extremely characteristic of the plant, nothing identical with them having been observed by any of the authors who have investigated the Carboniferous strobili. After these illustrations I will allow M. Zeiller to explain his views in his own words. After referring to the details given in my Memoir XVIII., M. Zeiller says :—

"L'aspect de ces sporanges, ainsi attachés au bout de ces pédicelles recourbés, est exactement, à part les dimensions moindres, celui de sporocarpes de *Marsilea*. L'analogie parait du reste n'être pas purement superficielle; M. Williamson a reconnu en effet, dans le pédicelle de chaque sporange, un cordon vasculaire bien caractérisé, qui prouve qu'on n'a pas affaire là à une simple formation épidermique, comme pour les sporanges de Fougères ou de Lycopodinées. Il faut, à ce qu'il semble, regarder ces pédicelles comme représentant des lobes ventraux des bractées, analogues au lobe fertile des frondes d'Ophioglossées, ou à ceux des Marsiliacées ; seulement ils portent à leur extrémité non pas une série de sporanges comme chez les premières, ou plusieurs sores comme chez ces dernières, mais un sporange unique à paroi formée d'une seule assise de cellules."

"De cette constitution des épis du *Sphen. cuneifolium*[1] il report que, si les Sphenophyllum rappellent les Lycopodinées par la structure de leur axe, ils s'en éloignent notablement par la disposition toute spéciale de leur appareil fructificateur, qui tend à les rapprocher plutôt des Rhizocarpées, et qu'ils doivent donc bien décidément être considéré comme formant une classe distincte parmi les Cryptogames vasculaires."[2] Agreeing thoroughly with these conclusions further comments are needless. WM. CRAWFORD WILLIAMSON.

DENDRITIC FORMS.

THE curious appearances presented by certain native specimens of silica have been observed for so long, that it is somewhat surprising that so little is known about their real constitution and mode of formation.

Rock-crystal is frequently found to contain bubbles of liquid, usually either water, carbon dioxide, or petroleum, or crystals, such as scales of mica, forming aventurine, and fibres, such as asbestos, forming cat's-eye. More rarely, however, forms of apparently vegetable origin are seen ; one of the most remarkable specimens is a prolate spheroid, about five inches long and four inches across, cut from a clear colourless rock-crystal, in which are embedded numerous fragments about the size of a large pea, presenting the exact appearance of club-moss.

Agate is frequently found with distinct coloured layers, either flat or distorted, and usually milk-white, red, brown, or black. It is then known as onyx.

More rarely, agates are found with markings like moss or foliage distributed through them ; they are then known as moss-agates, or Mocha stones.

In 1814, Dr. J. MacCulloch described some cryptogamic forms in the agates of Dunglas (Geological Trans., ii.,

[1] The species of Sphenophyllum to which M. Zeiller's strobili were attached.

[2] *Comptes Rendus des Séances de l'Académie des Sciences*, Paris, July 11, 1892.

iv., 398). It is stated that the Earl of Powys possesses an onyx containing the chrysalis of a moth.

It seems to be generally assumed, without any strong evidence, that rock-crystal and agate have been formed from solution in water, possibly superheated, and that in such cases as those mentioned above, various crystalline or fibrous minerals and low forms of plant life have been inclosed during the process of solidification.

Though this explanation is very possibly true in many cases, it does not account for all the appearances seen in moss-agates; and another possible mode of formation may be suggested by a brief account of some experiments made more than twenty years ago.

Ordinary crystals of ferrous sulphate dissolve readily in cold water; but if they are placed in a dilute solution of an alkaline silicate, an entirely new series of phenomena are produced, which were first described by J. D. Heaton, M.D., in a paper "On certain Simulations of Vegetable Growths by Mineral Substances" (Brit. Assoc. Report, 1867, p. 83). On immersing crystals of ferrous sulphate in a solution of sodium silicate of the density 1·065, very beautiful arborizations will soon begin to shoot perpendicularly upwards, attaining the height of three or four inches in a few hours. In a weaker solution roots can be caused to shoot downwards from a suspended crystal. The fibres contain silica and iron (less the weaker the solution); they are brittle, and more dense than the liquid in which they are formed. Examined by the microscope, the ultimate ramifications are cylindrical, tapering tubes, the walls of which are granular, showing no sign of crystallization. The roots are more abrupt and occasionally club-shaped in their terminations. The growth is interstitial like that of organized living tissue. "Supposing such purely mineral substances to have been formed in by-gone geological eras, and to have been accidentally fossilized in some primary or other ancient rock, they would very probably, when discovered by recent investigation, be pronounced to be an evidence of organized beings having existed contemporaneously with the formation of such rock."

In the following year a similar observation was made by Prof. W. C. Roberts-Austen (J. C. S., 1868, xxi., 274). A solution containing 4·9 per cent. of silica, when allowed to gelatinize, and dried for two days over sulphuric acid, left a solid residue similar to opal from Zimapan, but containing 21·4 per cent. of water. All the specimens of jelly dried in air contained dendritic forms, varying in size from 0·2 to 0·5 mm. When magnified 90 times they appeared as radiating fibres; when the power was increased to 700 times linear, each fibre resolved itself into a series of elongated beaded cells with clusters of circular cells at intervals. Mr. Slack indicated their remarkable analogy to common blue mould or mildew. The cells appeared to be hollow, and did not blacken with sulphuric acid.

A few years later I repeated Dr. Heaton's experiments, and made some additional ones, a brief account of which may induce some one with better means at his disposal to investigate an interesting and somewhat neglected subject.

If a crystal of copper sulphate be suspended in a solution of potassium silicate, which has been carefully neutralized and has a density of 1·065, in the course of a few minutes a hollow green column will be seen to run down from the crystal to the bottom of the beaker. Sodium silicate may be used instead of potassium silicate, but the appearance and rapidity of the growth is somewhat changed. The solution may be neutralized with hydrogen sulphate, chloride, or acetate, but hydrogen fluoride appears to prevent all growth. If the solution has a density less than 1·06, no growth occurs, and the crystals generally dissolve; the weaker the solution down to this limit the more rapid the growth. If the solution be stronger, the time required for the growth to commence may be lengthened from minutes to many days. If the density be above 1·25, no growth takes place.

Copper sulphate gives the best results, but it may be replaced by ferrous, manganous, or nickel sulphate; with changes in the shape, and of course in the colour, of the growths. The growths take place most readily from a clean sharp crystal, and always from an angle or edge; an edge obtained by cleavage requires more time. Other salts besides the sulphates may be used, but do not act so rapidly, probably owing to less perfect crystallization of the specimens used.

In a neutral or very feebly alkaline solution the growths are comparatively rapid, and consist of long, branching, tapering fibres, not unlike the roots of a tree. They grow rather more rapidly downwards than upwards. If the solutions be decidedly alkaline, the growths are much slower, and consist of fine stalks with comparatively large lumps at the extremities.

The tubes seem to be composed of silica with a small proportion of the metal used; they differ much in colour, are more dense than the liquid in which they grow, and are insoluble in water or dilute acids. When magnified 100 times, the substance of the tube shows no appearance of crystalline form, but seems to consist of concretions of ovoid granules. In this particular it differs from the substance of lead or silver trees, and from the curious fibres of potassium, iodide, and chloride described by Mr. Warington (J. C. S., v., 136, viii., 31).

It is generally assumed that the formation of onyx is due to the successive deposition of layers of silica coloured by different substances, but the following experiment suggests another possible method of formation, especially when the extreme permeability of gelatinous silica by liquids is remembered. So readily are even the hardest agates permeated by hot aqueous solutions of salts, that "staining" is a common commercial process.

A little too much sulphuric acid was accidentally added to a moderately strong solution of potassium silicate in which some crystals of copper sulphate were lying. The copper sulphate dissolved, and the solution set to a uniform blue jelly. After standing for about a week, the blue colour at the top of the jelly had separated into a series of thin parallel coloured plates, leaving the jelly between them colourless. This curious separation of the colouring-matter gradually proceeded downwards, and reached the bottom of the precipitating glass in about a month. The jelly gradually shrank, dried, and hardened, forming fragments consisting of blue bands in a white mass.

SYDNEY LUPTON.

NOTES.

THERE will be a memorial celebration for A. W. von Hofmann on November 12, arranged by the Deutsche Chemische Gesellschaft, at Berlin on the 25th anniversary of its foundation. The Empress Frederick and many German and foreign celebrities have been invited to be present. The proceedings, which will take place at the Berlin Town Hall, will include speeches on the history of the Society and on Hofmann, a review of progress in chemical science by Hr. Wislicenus, and choral music, performed by the members of the cathedral choir.

WE regret to have to record the death of Mr. Robert Grant, F.R.S., Professor of Practical Astronomy at the University of Glasgow. He died at Grantown-on-Spey, his native place, at the age of seventy-eight.

THE death of Dr. Löwenherz, director of the Imperial Physical Institute, Berlin, has been announced. He died at Berlin on Sunday last.

PROF. VIRCHOW has been appointed an honorary member of the Imperial Russian Natural Philosophy Society.

AN international ethnographical exhibition is to be held next year in St. Petersburg. It will be organized by the Russian Geographical Society.

THE American Microscopical Society offers prizes for the encouragement of microscopical research, two of the value of 50 dollars each, and two of the value of 25 dollars each, for the best papers which shall give the results of an original investigation made with the microscope, and relating to animal and plant life respectively; also two of the value of 30 and 15 dollars respectively for the best six photomicrographs in some subject of animal or vegetable histology; and two of the same value for the best collections of six mounted slides illustrating some one biological subject.

IN a letter to the *Times* on scientific titles and their abuse Prof. Tilden has opened a subject of considerable interest to men of science. It is well known that the letters indicating membership of a society are sometimes used by persons who have no right to use them, and Prof. Tilden notes that an effort is to be made to deal with this evil by getting a Bill before Parliament "for the purpose of securing to the respective societies the copyright of these letters." This, however, is a comparatively unimportant aspect of the question. The real difficulty is that membership of scientific societies is frequently "represented in courts of law or by candidates for public appointments as evidence of professional trustworthiness," whereas in very many cases it does not at all necessarily imply any extensive or accurate knowledge of the subjects in which the societies are especially interested. "Fellowship of the Royal Society, indicated by the letters F.R.S.," says Prof. Tilden, "is a real distinction which is justly prized. But what is the public to understand regarding such alliterations as F.B.S., F.C.S., F.E.S., F.G.S., F.L.S., F.S.S., F.Z.S., and of F.S.A., M.R.I., F.R.A.S., F.R.M.S., F.R.G.S., F.R.S.E., &c.? With the exception of one or two of the societies represented here, admission is to be gained by almost any one who is willing to pay the customary contribution to the funds of the society, and who can get two or more members of the society to testify to his fitness for admission, which generally means respectability and a profession of interest in the subject, the cultivation of which is the object of the society." He adds that if the public knew all about the societies no harm would arise; but "judges and barristers, and county councillors and town councillors cannot be expected to have this knowledge." Prof. Tilden thinks that "the only chance for a better state of things is for every member of these societies who respects himself to abandon the use of these unmeaning letters altogether"; but he fears that there is very little prospect of such a general reform while "an Institute having for its president no less a person than the Heir Apparent to the throne condescends to bait its advertisements for subscribers with the offer of more letters. The *Times*, discussing the subject in a leading article, expresses the opinion that in the main "we must trust, imperfect though the security is, to the ability of grown-up men and women to protect themselves against a form of deception which has most hold over those who themselves covet the meaningless letters to which they blindly pin their faith."

THE weather during the past week has been characterized by a marked increase of temperature and excessive rainfall, accompanied by strong southerly winds and gales. Between Wednesday the 26th and Friday the 28th October, the temperature in parts of England increased upwards of 30°, while the air became very humid and unpleasant. The continuance of comparatively high temperature, during which the thermometer reached 60° in the central and southern parts of the kingdom, was due to the track of the depressions, causing a continual indraught of warm air from off the Atlantic. On Thursday the 27th ult., about 1¾ inch of rain was measured in the West of Ireland, and heavy falls occurred on the following days in the Midland counties. A further downpour, amounting to 1½ inch in the Channel Islands, and to 1·3 inch in London, occurred on Sunday night, and the amount which has fallen on the east coast of Norfolk during the month of October is about equal to three times the average. During the first part of the present week, the disturbance which caused the heavy rainfall passed away, and a small area of high pressure temporarily advanced over the United Kingdom from the Atlantic, while the temperature fell several degrees, with mist or fog in places; but conditions were very unsettled, and a change of wind to the south-eastward in Ireland gave indications of probable further disturbances. During the week ended the 29th ultimo, the amount of bright sunshine exceeded the mean in nearly all districts.

THE Meteorological Council have recently issued a summary of the *Weekly Weather Report* for the quarter ending September 1892, which shows the rainfall and mean temperature in each district for each similar quarter for the twenty-seven years 1866–92, grouped in five yearly averages, and also the means for individual years from 1881. The average rainfall of the quarter for the whole of the British Islands was 10·2 inches, or only 0·7 inch in excess of the mean for the whole period. This result is almost entirely due to an excess in the grazing or western districts, amounting to 1·5 inch, while in the wheat-producing or eastern districts the fall for the quarter is slightly below he mean. The temperature for the quarter has been below the mean generally; for the whole of the country the deficiency amounted to 1°·8, and was 1°·7 in the grazing districts and 1°·9 in the wheat-producing districts. Similar returns show that the excess of rainfall amounted to 1·5 inch in the same quarter of 1891, prior to which there had been a series of seven dry quarters, while the temperature has been uniformly below the mean for six corresponding quarters. The coldest quarter was in 1888, when the deficiency amounted to 2°·5, this being, in fact, the coldest corresponding quarter during the last twenty-seven years.

THE late Mr. George Grote, the historian of Greece, expressed in writing, eight years before his death, a desire that after his decease his cranium should be opened and his brain weighed and examined. The task was undertaken by the late Prof. John Marshall, and the results of his observations are set forth in a full report printed in the current number of the *Journal of Anatomy and Physiology*. The entire encephalon was somewhat above the average in size, if compared with the adult male brain at all ages. If allowance be made for the effects of senile wasting, it must be regarded as a rather large brain, but not as an actually or especially large one. There can be no doubt, however, that it was, at death, further diminished in size and weight through the effects of disease, as shown by its marked deviation from the ordinary ratio as compared with the body-weight. As tested by the standard of macrocephaly adopted by Welcker, its utmost allowable weight was below that standard; and as contrasted with the encephala of certain other eminent men, it would find its place about one-third up from the lower end of the list. The general form of the cranium was rather or nearly brachycephalic, but it was decidedly higher than usual. The cerebrum itself was, in accordance with the shape of the cranium, short, broad, and deep. The cerebral convolutions were very massive, being not only broad and deep, but well folded, and marked with secondary sulci. This condition was observable all over the cerebrum, but chiefly remarkable in the frontal and parietal regions. Studied in reference to Dr. Ferrier's researches into the localization of function in the brain, the relative size of certain convolutions or groups of convolutions suggested some reflections as to individual peculiarities, but these reflections did not seem to Prof. Marshall

to be quite trustworthy. From the size and richness of the convolutions, the sufficiency of the grey matter both on the surface and in the interior of the hemispheres, and from the remarkable number of the white fibres, especially of the transverse commissural ones, the brain of Mr. Grote is pronounced to have been of very perfect and high organization.

THE method of cleaning mercury adopted at the Physikalisch-technische Reichsanstalt at Berlin is described in the *Zeitschrift für Instrumentenkunde*. The raw material is brought in iron bottles from Idria. It is filtered and dried, and twice distilled in a vacuum to get rid of the heavy metals. Great care is taken to eliminate fatty vapours derived from greased valves and cocks, which is accomplished by means of a mercury pump working without a stopcock. Finally, the electro-positive metals, such as zinc and the alkalies, are separated by electrolysis. The mercury is precipitated from a solution of mercurous nitrate obtained by the action of nitric acid on excess of mercury. The solution, together with the impure mercury acting as an anode, is contained in an outside glass vessel, into which a current from a Gülcher thermopile is conducted by an insulated platinum rod. The cathode rod dips into an interior shallow glass vessel, in which the pure mercury is collected. On careful analysis it was found that no perceptible non-volatile residue was left by 200 grammes of the purified metal. Thus the mercury is well fit for use in standard barometers and resistances.

WITH regard to the revival of animals after exposure to great cold, Herr Kochs (in the *Biologisches Centralblatt*) points out two things which retard formation of ice in the animal body. First, the body does not contain pure water, but salt and albumen solutions, which only freeze under zero C. Then capillarity and adhesion hinder freezing. Herr Kochs states that water in a glass tube of 0·3 to 0·4 mm. diameter may be cooled to $-7°$ and even $-10°$ C. without freezing. With a diameter of only 0·1 to 0·2 mm. the water is not frozen, even though the end of the tube be put in freezing liquid. Thin liquid sheets between two glass plates behave in the same way. If a salt solution freezes, the salts are excluded; and pure water, in freezing, gets rid of its absorbed gas. Fresh blood, according to the author's experiments, freezes only after being strongly cooled to $-15°$C., and after complete elimination of gases and salts. The blood corpuscles are dissolved and the blood loses colour. The same elimination doubtless occurs in freezing of protoplasm. Experiments cited to show the possibility of "anabiosis" may probably be explained by the decomposition process not having gone so far as to bring life completely to a standstill. Similar results were obtained in experiments on drying of seeds and various animals. It was shown with what tenacity many animals, under most unfavourable circumstances, retain the moisture necessary to life.

THE very destructive American disease of the vine known as the "Black-rot" has, for some years past, made its appearance in Europe, and its life-history has now been thoroughly investigated by Viala, Ráthay, and others. The ravages of the disease have been traced to a parasitic fungus, *Læstadia Bidwellii*, the mycele of which develops in the interior of the organ attacked, chiefly the young branches and berries, and produces spermogones and pycnids in the course of the summer. It is especially by the pycnospores that the fungus is disseminated. Towards the end of the period of vegetation sclerotes are formed, usually within the pycnids, and the conidiophores spring from these. Peritheces are also formed in May and June on the fallen and infected berries of the previous year. Until recently the ravages of this pest in Europe were confined to the French vineyards, but it has recently been detected in Austria and in Italy. The most effectual remedy for it is salts of copper.

THE results obtained from the botanical work done at the various experiment stations in the United States will in future be published in the form of an "Experiment Station Record," issued by the Department of Agriculture, under the editorship of Mr. Walter H. Evans.

ANGLO-INDIAN papers record the presentation of an interesting "piece of architecture" to the Madras Central Museum by Lord Wenlock. It is a hornets' nest, belonging probably to the species *Vespa cincta*. It is conical in shape, and is constructed of a material resembling rough paper or cardboard composed of woody portions of plants gummed up by the insects, and brought into the condition of paste by means of a viscid salivary secretion. The combs are placed in tiers and attached to each other by small columns of the same paper-like material of which the nest is composed. It is two feet in height, and about the same in circumference at the base. It was obtained in the course of one of His Excellency's tours.

M. DE NADAILLAC, in the current number of *La Nature*, discusses the significance of some of the facts which have been brought to light by the recent excavations of mounds in the Ohio Valley. The mound builders knew how to construct earth fortifications, which were of considerable extent and always remarkably adapted to the sites chosen. They buried their dead under tumuli of astonishing dimensions. Copper was the only metal they could work, and they undertook long journeys in search of it. Their weapons and implements were of stone. They made vases of pottery, and were able to produce representations of the human figure and of animals, both by sculpturing them in stone and by modelling them in clay. At least in some districts they were sedentary, and, like all sedentary populations, they had to obtain the means of subsistence in part by cultivation of the soil. They were often engaged in fighting, and numerous burials in which the bodies are crowded together bear witness to the fury of their struggles. Whence did they come and who are their descendants? M. de Nadaillac thinks that these questions can never be definitely answered unless investigators discover some traces of the language of the mound-builders.

AN interesting and valuable paper on the association of shipping disasters with colour-blind and defective far-sighted sailors, read by Dr. T. H. Bickerton before the section of Ophthalmology at the last annual meeting of the British Medical Association, has been reprinted for the author from the *British Medical Journal*. Dr. Bickerton takes anything but a hopeful view of the prospects of legislation on this important question. He greatly fears that "many a shipping disaster will occur before the Royal Society's suggestions become part of the law of the land." Accordingly he urges all who interest themselves in the subject to abate not a tittle of their endeavours. "There are none," he says, "so difficult to convince as those who will not believe, and the men who have had the framing of the rules of the road at sea are the very men who hitherto have turned from all suggestions on the eyesight question with contempt. True it is that their language, judged from examples to be found in the *Nautical Magazine*, is becoming moderate, and even polite, but they lack knowledge of this subject, and they will still require our best attention." Meanwhile, Dr. Bickerton presses on the attention of the public the following facts:—that 4 per cent. of the whole male population are colour blind; that about 8 per cent. more have marked impairment of sight from refractive errors; that there is no official test whatever as to a sailor's eyesight; that a man may be the subject of any of the forms of eye disease, may have any degree of blindness, or may be so short-sighted as to be unable to see distinctly more than a few inches in front of his nose, and yet be at perfect liberty to be a sailor and to become an officer; and that, although there is a

compulsory colour examination (in many cases a most inefficient one) to be passed before a sailor can become an officer, there is no check to a colour-blind man being a sailor, or to his remaining one to his life's end.

THE Rev. T. A. Marshall describes in the November number of the *Entomologist's Monthly Magazine* a new genus and species of Belytidæ from New Zealand. The paper is accompanied by representations of two insects in fine condition. Mr. Marshall abstains from giving tedious details, as the figures will, he believes, convey a better idea of these creatures than many words, and he thinks they will now be unmistakable, at least until other species of the same genus shall be discovered. He has not taken any characters from the under-side, the specimens being carded; hence the oral organs could not be described, but they may be pretty safely assumed to resemble those of *Belyta*, *Anectata*, &c., and their details would have been of little value.

A CORRESPONDENT of the New York journal *Electricity*, writing from Paris, describes some electrical peculiarities which he has seen in a cat. This cat, called Michon, is a half wild animal, and dislikes handling. It belongs to the household of Dame Gais, whose residence on the Carnier Mount, near Monte Carlo, looks directly down on the noted gambling casino and its botanical reservation. On some of the cold and very dry nights common to Monte Carlo in the winter, Michon, while in the dark, is quite a spectacle. Every movement of its body sends off hundreds of minute bluish sparks, something like those thrown off by ill-adjusted brushes, though not so pronounced in colour. They make a noise on a small scale, like the crackling of burning furze. Stroking the cat increases the sparking, and ruffling its fur the reverse way produces a miniature pyrotechnic display quite remarkable. The cat itself does not seem to mind the sparking, but, like all cats, dislikes to have its fur rubbed in a wrong direction. The writer has never seen the electric element so abundant in a cat, and many who have seen the coruscations that have given notoriety to Michon, confirm him in the opinion that the cat is an electrical curiosity.

A USEFUL account of "Biological Teaching in the Colleges of the United States," by Prof. John A. Campbell, of the University of Georgia, has been issued by the United States Bureau of Education. The writer's object is to present the actual extent and scope of the biological courses offered by the colleges of the United States, together with the methods of teaching employed. He also aims at presenting as fully as possible an account of the equipment and facilities for teaching which the various colleges possess. The statements he makes are therefore based largely upon the printed accounts found in the college catalogues, supplemented in many cases by letters containing additional information. These have usually been re-written, but where they are in suitable form they are quoted directly. Prof. Campbell notes that many of the colleges announce more in their catalogues than they can possibly do thoroughly with the teaching force employed. This is often perfectly apparent, but in more than one letter received the statement has been made that certain courses have no existence save on paper. Prof. Campbell, however, thinks that it is worth while to record the views of the professors in charge in regard to the nature and aims of such work, and the ideals towards which they are striving.

THE "Treatise on Hygiene and Public Health," edited by Dr. T. Stevenson and Mr. Shirley Murphy, and reviewed in NATURE last week, is published by Messrs. J. and A. Churchill.

MESSRS. J. AND A. CHURCHILL are publishing a second edition, revised and enlarged, of "Commercial Organic Analysis," by Alfred H. Allen. The second part of the third volume has just appeared. The third part of the same volume will be issued as soon as possible, and will complete the work. In the second part he has sought to describe fully and accurately such of the organic bases as have any practical interest, and to give trustworthy information as to their sources.

THE new number of *Natural Science* includes articles on the evolution of consciousness, by C. Lloyd Morgan; primæval man: a palæolithic floor near Dunstable, by W. G. Smith; the evolution of sharks' teeth, by A. S. Woodward; the walk of arthropods, by G. H. Carpenter; the falling of leaves, by A. B. Rendle; and Norwich Castle as a museum, by H. Woodward.

A REVISED edition of "London Birds and London Insects," by Mr. T. Digby Pigott, has been issued by Mr. H. Porter. Along with the essays on these subjects have been printed several other bright and attractive sketches.

AN elaborate index to the genera and species described in the "Palæontologia Indica," up to the year 1891, by W. Theobald, has just been issued. It is included among the Memoirs of the Geological Survey of India. Mr. Theobald has also prepared "Contents and Index of the Memoirs of the Geological Survey of India, 1859 to 1883."

A SECOND edition of Dr. F. H. Hatch's "Text-book of Petrology" has been issued by Messrs. Swan Sonnenschein and Co. The author explains that he has taken advantage of this opportunity to revise the book thoroughly, while largely increasing its scope.

THE Society for Promoting Christian Knowledge has published a second edition of Klein's "Star Atlas." Mr. E. McClure, the translator of Dr. Klein's explanatory text, has sought to bring up to date the German writer's descriptions of the more interesting fixed stars, star clusters, and nebulæ.

MESSRS. ROBERT GRANT AND SON, Edinburgh, and Messrs. Williams and Norgate, London, have issued Parts II. and III. of Vol. XXXVI. of the Transactions of the Royal Society of Edinburgh. The following are the subjects of some of the papers:—the foundations of the kinetic theory of gases (IV.), by Prof. Tait; the solid and liquid particles in clouds, by J. Aitken; the development of the carapace of the chelonia, by J. B. Haycraft; the composition of oceanic and littoral manganese nodules, by J. Y. Buchanan; the winds of Ben Nevis, by R. T. Omond and A. Rankin; and the Clyde sea area, by H. R. Mill.

THE University College of Wales, Aberystwith, has issued its calendar for its twenty-first session, 1892–3.

THE City and Guilds of London Institute has issued its programme of technological examinations for the session 1892-93.

MESSRS. GEORGE PHILIP AND SON announce that a work on "British New Guinea," by Mr. J. P. Thomson, Hon. Sec. to the Brisbane Branch of the Royal Geographical Society of Australasia, is almost ready for publication. An appendix will contain contributions to the geology, fauna, flora, &c., by Sir William Macgregor, K.C.M.G., Baron Ferdinand von Mueller, Professor Liversidge, F.R.S., and others. The proof-sheets have been revised by Dr. H. Robert Mill and Dr. Bowdler Sharpe.

ANOTHER and apparently much more convenient mode of preparing glycol aldehyde, $CH_2OH.CHO$, the first member of the series of aldehyde-alcohols, is described in the current number of the *Berichte*, by Drs. Marckwald and Ellinger, of

Berlin. It may be remembered that in our note of a fortnight ago (vol. 46, p. 596), it was announced that Prof. Emil Fischer and Dr. Landsteiner had succeeded for the first time in preparing this interesting substance in a state of tolerable purity by a reaction analogous to that of barium hydrate upon acrolein dibromide, the reaction which yielded the first synthetical glucose. They first prepared the mono-bromine, derivative of common aldehyde, $CH_2Br.CHO$, and subsequently reacted upon this new substance, a liquid possessing an intolerably sharp odour, with baryta water. After removal of the baryta by sulphuric acid, and the hydrobromic and sulphuric acids by means of carbonate of lead, a liquid was obtained which possessed the properties of a dilute solution of glycol aldehyde. Some time ago Pinner obtained a derivative of this aldehyde which bore the same relation to glycol aldehyde, that the compound known as acetal, $CH_3.CH(OC_2H_5)(OC_2H_5)$, bears to common aldehyde. This substance, glycol acetal, $CH_2OH.CH(OC_2H_5)(OC_2H_5)$, Pinner attempted to decompose, by the action of mineral acids, into ethyl alcohol and glycol aldehyde. The attempt, however, did not succeed, inasmuch as the decomposition went further, any glycol aldehyde that may have been formed during the first stage of the reaction being subsequently broken up. Drs. Marckwald and Ellinger now find that the reaction succeeds admirably, provided the acid employed is extremely dilute, and as glycol acetal is a substance very easily prepared, they show that the reaction affords a very convenient and advantageous method of preparing large quantities of glycol aldehyde. The glycol acetal is added to an equal volume of water acidified with only a few drops of sulphuric acid. The liquid is then heated to boiling. After a short time the two liquids mix, and the reaction is completed when upon the addition of water to a few drops of it no separation of oil occurs. Upon distilling the liquid product, alcohol first passes over, then there distils a mixture of water and glycol aldehyde until decomposition of the residue commences. Glycol aldehyde, as thus obtained in a tolerably concentrated form, appears to be much more volatile in steam than was observed by Prof. Fischer and Dr. Landsteiner, in case of their more dilute solutions. From a few cubic centimetres of the distillate Drs. Marckwald and Ellinger obtained a very considerable quantity of Prof. Fischer's phenylhydrazine compound, and confirm in every detail the other properties of glycol aldehyde described in our previous note above referred to. The chemistry of this first member of the series which includes the sugars is now, therefore, fairly complete, and the difficulties in the way of its preparation surmounted.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♂) from India, presented by Mr. Pascoe Grenfell, F.Z.S.; a Philantomba Antelope (*Cephalophus maxwelli*) from West Africa; three Gambian Pouched Rats (*Cricetomys gambianus*) from West Africa; a Ground Rat (*Aulacodus swindernianus*) from West Africa; and a White-faced Tree Duck (*Dendrocygna viduata*) from West Africa, presented by Mr. C. B. Mitford; a Martial Hawk-Eagle (*Spizaetus bellicosus*) from South Africa, presented by Mr. T. White; two Weaver Birds (*Hyphantornis sp. inc.*) from South Africa, presented by Mr. A. W. Arrowsmith; two Silver Pheasants (*Euplocamus nycthemerus* ♂ ♂) from China, presented by Mr. E. Mitchener; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Miss Kate Higgins; a Thick-tailed Opossum (*Didelphys crassicaudata*) from South America; a Garden's Night-Heron (*Nycticorax gardeni*); and two Saracura Rails (*Aramides saracura*) from South America, purchased; and a Squirrel Monkey (*Chrysothrix sciurea*) from Guiana, deposited.

OUR ASTRONOMICAL COLUMN.

COMET BROOKS (AUGUST 28).—The following ephemeris, which we take from *Astronomische Nachrichten*, No. 3125, gives the apparent Right Ascensions and Declinations of Comet Brooks, which is brightening very rapidly:—

12h. Berlin M.T.

1892.	R.A. app. h. m. s.	Decl. app. ° ′	Log. *r*.	Log. Δ.	Br.
Nov. 3 ...	9 23 51 ...	+9 55·8			
4 ...	27 50 ...	9 9·8 ...	0·1265 ...	0·0225 ...	11·60
5 ...	31 51 ...	8 22·7			
6 ...	35 55 ...	7 34·6			
7 ...	40 1 ...	6 45·4			
8 ...	44 10 ...	5 55·2 ...	0·1125 ...	0·0034 ..	13·51
9 ...	48 21 ...	5 4·0			
10 ...	52 34 ...	4 11·8			

Lying in the extreme northern corner of the constellation of Sextans, and nearly midway between ρ Leonis and ε Hydræ, it will not be an easy object for observation owing to its very late rising.

COMET BARNARD (OCTOBER 12).—Prof. R. Schorr, of Hamburg, communicates to *Astronomische Nachrichten*, No. 3125, the elements and ephemeris of Comet Barnard, deduced from observations made on October 16, 18, and 20, at Vienna, Hamburg, and Pulkowa respectively. As this ephemeris differs rather considerably from the one we gave last week, the following places may prove of service to observers:—

12h. Berlin M.T.

1892.	R.A. h. m. s.	Decl. ° ′	Log *r*.	Log Δ.	Br.
Nov. 3 ...	20 24 38 ...	+5 3·7			
4 ...	27 20 ...	4 44·0			
5 ...	30 4 ...	4 24·6 ...	0·2298 ...	0·1539 ...	1·00
6 ...	32 48 ...	4 5·4			
7 ...	35 34 ...	3 46·4			
8 ...	38 21 ...	3 27·8			
9 ...	41 9 ...	3 9·4 ..	0·2278 ...	0·1590 ...	0·99
10 ...	43 58 ...	2 51·2			

This comet will still be found to form approximately an equilateral triangle with α Aquilæ and β Delphini on November 5.

TABULAR HISTORY OF ASTRONOMY TO THE YEAR 1500 A.D.—Dr. Felix Müller, of Berlin, has just completed a small volume entitled "Zeittafeln zur Geschichte der Mathemathik, Phisik und Astronomie bis zum Jahre 1500," which will be welcomed by all interested in the very early history of the exact sciences. The book is arranged chronologically and gives a short account of the chief workers in these branches of science up to the year 1500. At the end of each reference a list of the literature likely to be needed is added. The work is published by Messrs. B. G. Teubner, Leipzig.

A LARGE TELESCOPE.—The Americans seem to have made up their minds to be the possessors of the largest telescopes in existence, for in spite of their owning the great Lick Refractor (36-inch) we hear now that the University of Chicago are about to have "the largest and most powerful telescope in the world." This instrument will be the gift of Mr. Charles Jerkes, and will cost half a million dollars. The object-glass will have a diameter of 45 inches and will be made by Messrs. Alvan Clark, of Cambridge, Mass.

THE ATMOSPHERES OF PLANETS.—Of all the planets that revolve round our sun, Jupiter affords the most suitable of them for the study of atmospheric circulation. That his circulation will not be exactly like ours will be at once evident, for not only does the sun pour his rays on his vast surface, but he possesses himself heat, as is suggested by the rapid changes which these cloud masses undergo. A recent hypothesis, explaining the various movements in this planet's atmosphere, has been put forward by Mr. Marsden Manson, in the fifth number (vol. ix.) of the "Transactions of the Technical Society of the Pacific Coast," San Francisco. The chief element which produces these movements is the action of the sun, and it is on this reasoning that he attempts to unravel the laws underlying the circulation in Jupiter's atmosphere. In this pamphlet he first brings together some of the facts relating to our own wind system, which are generally conceded, together with the important results that were gathered from the path taken by the Krakatoa

dust-cloud. The spots observed on Jupiter's surface are next dealt with, a table of their rotation periods and latitudes being included. From the latter he deduces that the mean periods of rotation of matter in the following latitudes are:—

Lat.			h.	m.	s.
12° N.	from 17 N.	Temp. spots	9	55	36·49
4° N.	,, 5 N.	Equat. ,,	9	50	40·06
8° S.	,, 21 S.	Equat. ,,	9	50	22·4
30° S.	,, 3 spots		9	55	17·1

In treating of the spots themselves, he suggests that those which are of a white appearance are gyrating uprushes of warm air from the lower regions, while the dark ones are simply descending columns of cool air, "the two forming parts of the system of vertical circulation." The red spot, he suggests, is caused by a local escape of internal heat, the repellent force it appears to possess being due to the "spreading of the heated currents as they rise." He explains the retardation and acceleration of its period of revolution by the increasing force of the west winds, brought about by the exposure of the southern hemisphere during Jupiter's half-year (5·93 of our years); in this way the spot is sometimes situated over and sometimes to one side of the source of heat underneath. The author also deals with other spots in a similar manner.

GEOGRAPHICAL NOTES.

MR. SVEN HEDIN'S account of his ascent of Mount Demavend is published in the last number of the *Verhandlungen* of the Berlin Geographical Society. Demavend is a volcanic peak rising abruptly from the sedimentary rocks of the parallel Elburz chains. Starting from the village of Ranah on the south-eastern slope with two guides on July 10, 1890, Hedin reached the summit on the afternoon of the next day. On the summit a large elliptical crater was found; the edges of which were strewn with blocks of porphyritic lava and sulphur. After discussing the aneroid and boiling point observations, Mr. Hedin arrived at 5465 metres (17,930 feet) as the height of the summit. This is lower than any of twelve earlier estimates which are cited, the highest of them being 6559 metres.

THE Italian possession of Eritrea on the coast of the Red Sea gives some promise of becoming useful agriculturally. Several small settlements of Italians on the plateau have succeeded in growing large crops of wheat and barley, and only the unsettled state of the surrounding natives threatens the prosperity of the farmers. The districts of Oculé-Cusai and Guro are already fully cultivated, and Saraé, as yet almost unoccupied, has fertile land and plenty of room for colonists. The Italians are able to work in climatic conditions which would rapidly exhaust the natives of northern Europe.

THE general summary of Mr. Conway's expedition in the Karakoram range telegraphed from India (p. 525) has now been supplemented by a full narrative, written to the secretaries of the Royal Geographical Society from a camp on the Baltoro Glacier on August 29, with a postscript added at Skardo, on the way to Leh, on September 12. The difficulties of the preliminary journey were very great, not the least being the fording of several swollen glacier streams by a party numbering four Europeans, four sepoys, seventy coolies, an indefinite number of followers, and flocks of goats and sheep. The moraines on the Baltoro glacier were of almost incredible extent; for two-thirds of its entire length the ice is entirely concealed by stones, except where crevasses or lakes occur, and the irregularity of the surface made travelling extremely slow. Mr. Conway limits the name of Godwin-Austen to the highest peak of "K^2," giving to the whole mountain the somewhat cumbrous title of the Watch Tower of India. One branch of the Baltoro Glacier results from the union of seven glaciers from this mass; the larger branch descends from the snow-swathed, throne-shaped mountain, hitherto unmapped, for which the auriferous quartz found in its rocks suggested the name of The Golden Throne. This was fixed upon as the goal to be attained. The first attempt landed the Europeans and Ghoorkas, who made excellent climbers, on Crystal Peak, 20,000 feet in elevation, a peak as hard to climb as the Matterhorn, and isolated from the surrounding higher summits. No inconvenience was felt from the rarity of the air, and the party remained on the summit for an hour and a quarter. In the grand attempt on the Golden Throne serious difficulty was encountered from the terrible extremes of heat and cold. The last few thousand feet proved very exhausting; one of the Ghoorkas had to be left behind, suffering from mountain-sickness. Every step had to be cut in hard ice. Finally the summit was reached at an elevation of 23,000 feet; but the Golden Throne stood revealed much higher, and separated by a deep depression. From the summit of Pioneer Peak, probably the highest yet reached by man, a series of photographic views was obtained and prismatic compass bearings taken to the surrounding features. As long as the party were at rest they felt no discomfort, but the sphygmograph showed that the heart's action was very laboured. A stay of an hour and a quarter was made on the summit, the view from which baffled description. The descent was safely made, but fatigue and bad weather stopped further exploration.

THE INSTITUTION OF MECHANICAL ENGINEERS.

ON the evenings of Wednesday and Thursday of last week, the 26th and 27th ult., an ordinary general meeting of the Institution of Mechanical Engineers was held in the theatre of the Institution of Civil Engineers, by permission of the council of the latter Society. The President, Dr. William Anderson, occupied the chair during the proceedings.

There were two papers on the agenda. The first was the report of the Institution's committee appointed to enquire into the value of the steam jacket. Mr. Henry Davey is the chairman of this committee, and he had prepared the report; which is a bare record of facts without comment, and in this respect is, we think, defective. Numberless experiments have been made in time past as to the value of the steam jacket, and those now added by the labours of the committee do not largely differ from many that have gone before. We take it that the general opinion of competent engineers is that an advantage in efficiency is to be obtained by jacketing engine cylinders in an efficient manner, and cases in which the jacket has not been proved efficient are those in which it has not been properly applied. What was wanted, therefore, was guidance as to the proper method of application, and it is significant that the most help in this direction came, during the discussion, from those who were not members of the committee. Timidity in expressing opinion will be excusably construed as indicating something of incompetence, and if the members are not capable of expressing opinion they are not suitable persons to form a research committee of an important institution. We frame our remarks hypothetically, because, with such names as Unwin, Bryan Donkin, and Mair-Rumley on the title-page, there can be no doubt that the power to afford guidance was present, and for this reason the decision to give only bare fact is the more to be regretted. The general conclusion to be drawn from the experiments, as quoted, is that "the expenditure of a quantity of steam in an efficient jacket produces a saving of a greater quantity in the cylinder." It does not follow from this that the jacket is always desirable, as the saving may be so small as not to justify the additional complication and increased outlay at first cost. That, however, is a matter upon which steam users must themselves decide upon a commercial basis; and is, of course, outside the province of the committee, but what would have been valued would have been some critical remarks giving guidance as to what goes to constitute the "efficient jacket," what fresh engineering practice is opened up by the use of the efficient-jacket, and under what conditions it may be most effectually applied.

The first series of experiments quoted were carried out by Mr. J. G. Mair-Rumley, of the firm of James Simpson and Co., of Pimlico, upon a compound jet-condensing beam pumping-engine. The diameters of the cylinders are 29 inches and 47·5 inches, with strokes of 65·1 and 96 inches respectively. Only the body of each cylinder is jacketed, the steam being supplied direct from the boiler at a pressure of 49lbs. per square inch above atmosphere. Experiments were made both with and without steam in the jackets. The total feed water per indicated horse power per hour when the jackets were not in use was 18·20 lbs., with the jackets in use the corresponding figures were 16·64 lbs., thus showing a percentage of less steam used due to the jackets of 8·6. The quantity of jacket water condensed was 1·20 lbs. per I.H.P. per hour. The boiler pressure here was not high, 49·7 lbs. without and 49 lbs. with jackets.

The number of revolutions were also low, 14·8 without jackets and 15·78 with jackets. This was evidently an engine which should pay for jacketing. We next come to an experiment of a different nature, carried out by Mr. Davey and Mr. W. B. Bryan. The engine is triple expansion surface-condensing engine of the inverted direct-acting marine type, and is placed in the Waltham Abbey pumping station of the East London Water Works. The cylinders were 18″, 30·5″, and 51″ in diameter, by 36 inches stroke. There is a Meyer expansion valve to the high pressure cylinder, by means of which the speed of the engine was regulated during the experiment. The bodies and both ends of all three cylinders are steam-jacketed. The jacket steam of the high pressure cylinder is at full boiler pressure, but the other two cylinders have the pressure reduced to a little above that of their steam-chests by means of reducing valves. Each cylinder is therefore jacketed with steam a little above its own initial pressure. Without the jackets in use the amount of feed water per I.H.P. per hour was 17·22 lbs, and with the jackets in use 15·45 lbs, showing a percentage of less steam used owing to the jacket of 10·3. The total jacket water was 1·72 lbs. per I.H.P. per hour. The coal consumption is given in these experiments, being 2·09 lbs. per I.H.P. per hour without the jackets, and 1·79 lbs. with. The amount of coal burnt is not, of course, necessarily a measure of economy of the engine, but possibly the steam-generating plant —which included an economizer—was practically constant in its duty during both trials, and if so the commercial gain by the use of the jacket is quite an appreciable quantity. The boiler pressure here was 130 lbs. above atmosphere, the number of expansions without the jacket 22, and with the jacket 30. The revolutions were 23 per minute, so that the jacket had again a favourable chance.

The next series of experiments were carried out by Colonel English, Mr. Davey, and Mr. Bryan Donkin, and in these we reach a much higher piston speed, so that the results stand on a somewhat different footing in this respect to those before quoted. We have no positive knowledge of this engine beyond that given in the report, but it would be desirable to know something more of its working before accepting the very high percentage of gain in steam used—19·0 per cent.—as that due to a steam jacket used on a good engine. The feed used per I.H.P. per hour was 24·68 lbs. without the jacket, and with the jacket in use the quantity was 20 lbs. The following are the particulars of this trial :—Horizontal surface condensing compound engine, with intermediate receiver cylinders, 18 and 32 ins. by 48 ins. stroke. The ends of the cylinders are not jacketed, and the receiver jacket was not in use during the experiment. The boiler pressure was 50 lbs., the revolutions 57·06 without jackets, and 63·62 with, the feed water supply as stated, and the jacket water condensed per I.H.P. per hour 1·13 lbs. The coal used without jackets was 3·26 lbs. per I.H.P. per hour, and with jackets 2·66 lbs.

The last set of experiments we shall quote were made by Prof. Unwin, upon the experimental engine[1] at the City and Guilds of London Central Institution, South Kensington. It is a two-cylinder horizontal-surface condensing engine, and can be worked either simple or compound. The cylinders are 8·73 inch and 15·76 in diameter, by 22″ stroke. The high pressure cylinder is fitted with Hartnell expansion gear, and the low pressure with Meyer expansion gear. Only the bodies and the back ends of the cylinders are covered. We will first give results of trials working the engine with the low pressure cylinder only. The pressure was 60 lbs. above atmosphere, the jacket pressure being taken direct from the boiler. The revolutions without the jacket were 112·40, and with the jacket 101·73. The feed water per I.H.P. per hour without the jacket was 32·14 lbs., and with the jacket 26·69 lbs. This gives a saving of 17 per cent. working simple. It will be seen presently that when the engine was working compound, the saving was 7·3 per cent. The jacket-water per I.H.P. per hour was 1·88 lbs. We will now take the records of the compound trial. The boiler pressure was 66·73 lbs. without the cylinders, and 67·80 lbs. with the jackets. The revolutions were 93·66 without the jacket, and 96·11 with. The feed water used per I.H.P. per hour was 21·06 lbs. without the jackets in use, and 19·52 lbs. with. The saving, as stated, made by the use of the jacket was therefore 7·3 per cent. The jacket water used per I.H.P. per hour was 2·40 lbs. We regret we are not able to give all the interesting details which Prof. Unwin includes in his instructive report, but for these we must refer our readers to the original paper.

Probably Prof. Unwin's 7·3 per cent. saving in steam used is a far better measure of the value of the jacket than the inflated promise of 19 per cent. in Major English's trial. It should be remembered that the jacket is more effective in small than in large engines, the area of cylinder will be in a higher ratio to the contained steam in the former than in the latter case. The number of expansions in the South Kensington engine working without jackets was 7·23, and with jackets 9·29. The corresponding figures in the case of the Woolwich engine were 9·4 and 12·6. The boiler pressure with the Woolwich engine was, however, 16 to 17 lbs. higher than in the other case. The revolutions were 57·06 and 63·62 respectively in the two trials at Woolwich, whilst at South Kensington they were 93·66 and 96·11. It would have been instructive if the committee had had the courage to attempt some balance of these figures, and then have endeavoured to account for the large difference which we believe would have remained still to be accounted for.

The next experiments quoted comprise a series made by Mr. Bryan Donkin, junr., at the works of his firm at Bermondsey. Mr. Donkin's labours in this field are well known, and engineering science is largely indebted to him for the contributions he has made to its lore. One most valuable feature in connection with these investigations is the means he has used to ascertain the temperature of the walls of the cylinder at various distances from the surface. In this lies the essence of the problem. If the Jackets Committee would give us minute and trustworthy information on this point we could evolve the rest from existing data. If we do not quote Donkin's figures in full it is partly because his experiments are not yet complete and partly because they have been dealt with more fully in "another place," namely, the Proceedings of a Society other than tha with which we are now dealing.[1] We may state, however, that in one case when the steam in the jacket space was 298° Fahr. the cylinder walls averaged 290° Fahr., whilst at 0·06 in. from the piston the temperature of the cylinder wall was 284° Fahr. These temperatures were ascertained by thermometers placed in holes drilled in the cylinder. Other instances are given, but the matter is far too interesting to deal with in a cursory manner, such as a report of this nature alone warrants. The difficulty that suggests itself is the fact that a thermometer itself has a very appreciable thickness, and the record will be but a mean of the temperature due to that thickness. It is possible that Mr. Donkin gets over this difficulty in some way. Perhaps the thermo-couple as used by Le Chatelier might afford a solution, although this apparatus is not so useful for recording small differences at low temperatures, being rather adapted for such work as hot blast stoves and other metallurgical purposes. Mr. Bryan Donkin's experiments are the most suggestive in the report, as might be anticipated. Trials were made with steam at various rates of expansion to determine the effects of the steam-jacket on the speed of engine and temperature of the cylinder walls, and on superheating. The engine used was a small one (6″ × 8″), but it was specially constructed and arranged for the work. We again repeat Mr. Donkin's investigations are well worthy of the study of all interested in these matters.

The report concludes with a valuable appendix in the shape of suggestions for the use of those desirous of experimenting in this field.

The discussion on this paper was of a protracted nature, but was not of a kind altogether worthy of the leading mechanical institution of the country. Mr. Morrison, of Hartlepool, made the most weighty contribution amongst the speakers. He pointed out the difficulty of maintaining a good circulation of steam in the jacket—one of the most important points to which the designer of jacketed engines should turn his attention—and illustrated a simple method by which he had secured this end. His arrangement consisted of a series of diaphragms, by means of which the steam was made to take a devious course through the jacket. Mr. Schonheyder pointed out a mistake the committee had made in placing an air-cock on the top of the jacket, when it was required to draw off air from the steam. Of course, this is one of those little slips which the wisest are apt to make, for it would be absurd to suppose such authorities as those en-

[1] This engine is stated in the report to have been fully illustrated and described in *Engineering* of November 16, 1888. The triple expansion engine at Waltham Abbey is also said to have been illustrated and described in the issue of August 8, 1890, of the same publication.

[1] See Proceedings Inst. Civil Engineers.

gaged did not know that air is heavier than steam. One might as well say one's grocer did not know sand from sugar.

The Jackets Committee has not yet concluded its labours, and another report will be forthcoming in due course. Mr. Aspinall has offered a locomotive for trial, and we heard that Mr. Yarrow will put a torpedo boat at the disposal of the committee, and has even promised to cast special cylinders for experimental purposes. The locomotive will afford an interesting field of research, running as it does so largely linked up. The torpedo boat experiments will be no less interesting, especially in view of the great number of revolutions the engines of these craft make in a given time.

On the second day of the meeting the paper by Mr. Walker of Bristol on the screw propeller. This paper gives the details of some experiments made by the author on a form of screw propeller invented by the late Mr. B. Dickinson. It would be in vain for us to attempt to condense this paper within the limits at our disposal. With regard to the merits of the Dickinson propeller we have nothing to say. It consists essentially of two narrow blades in place of one, and reminds us strongly of a Mangin propeller with one blade set somewhat back on the shaft. Mr. Walker contends that his researches prove the advantages of long narrow blades, but he did not appear to have converted the high authorities present, including Mr. Froude, Mr. Thorneycroft, and Mr. Barnaby—the three best-known names in connection with the subject—to his views. It is difficult to see wherein the value of the paper exists. Prof. Kennedy in the discussion stated that the generally received opinion as to the increase of the friction of the load was erroneous, and that the power absorbed in this way does not increase in the manner stated, a fact which he illustrated by means of a diagram. Mr. Thorneycroft pointed out that "life was not long enough" for the larger trials proposed by the author, but that he might decide one point if he would confine himself to models. Mr. Barnaby stated that a broad-bladed propeller should not be a uniform pitch. Mr. Froude's speech was a lucid criticism of the author's paper, the speaker pointing out in a kindly but convincing manner that the conclusions arrived at by the author might be subject to revision. Mr. Dunell, whose previous experiments the author had quoted, added to the information given by putting forward some other experiments he had made upon screw propellers fitted to a torpedo boat, in this case the results being opposed to those claimed by the author, inasmuch as the shorter and broader blade had proved the more advantageous. Mr. Shield, of Liverpool, described a form of propeller which has been in use on the Mersey, and appears to offer some advantages. The blades are attached to the boss in two parts, and are joined in a loop at the top. According to Mr. Shield's statement, the arrangement gives great advantages in towing, and also increased steadiness in running. The latter we can accept as a fact, but the great increase in towing capacity seems almost too good to be accepted literally. Twenty-five per cent. additional efficiency is a very large gain without further expenditure than an exchange of screws; but this is what the propeller in question is said to realize.

The meeting concluded with the usual votes of thanks.

INTERNATIONAL COMMITTEE OF WEIGHTS AND MEASURES.

THE International Committee of Weights and Measures, which was established in consequence of the Metric Convention of 1873, has recently issued its fifteenth annual report to the Governments represented at that Convention.[1] The committee have also lately published the minutes of their proceedings (Procès-Verbaux des Séances. Paris, 1892. 1 vol. 8vo) at the annual meeting held at Paris in September, 1891. It appears to be hardly possible that the proceedings of the committee at their meeting which was held last month may be issued before next year, but from the above publications, as well as from a recent volume of their "Travaux et Mémoires," we gather that they continue to carry on their investigations with all despatch.

In their last report the committee deplore the death of their colleague, Jean-Servais Stas, whose analyses of the platinum alloys have, together with those of St. Claire Deville and George Matthey, so largely helped forward the principal work of the committee; the metallurgical studies of Stas are indeed recognized as veritable models of classical research in this particular field.

The new instruments added to the Bureau at Sèvres during the last year include a normal barometer (le Baromètre Fuess) and manometer, originally verified for reference as an international standard in accordance with the decisions of the Meteorological Conferences, particularly that at Munich last year. The committee have also obtained a new apparatus for determining the normal thermometric "boiling point," or the temperature of 100° Centigrade, as it has been found that the form of apparatus used by Regnault was unreliable for this purpose. In the reading of the standard manometer it would appear that higher accuracy has been obtained by raising the surface of the mercury up to a fixed point, the image of the point in the mercury being observed at the same time by means of a microscope. The Wild-Pernet barometer has been remounted, and the Bureau are now prepared to undertake the verification of any standard barometer.

The readings of all mercurial thermometers are given at the Bureau in terms of the hydrogen thermometer; and a 30-litre holder for methyl chloride, or liquid carbonic acid, has been made by Brigonnet and Navile. The low temperature experiments have been continued by M. Chappuis down to —75° Cent.; and toluol and alcohol thermometers have been compared with the hydrogen thermometer. It has been found that "toluol" is more sensitive and reliable for low temperatures than alcohol.

We note that the meteorological work of the committee has largely developed itself; and that, as in geodetic research, the Bureau at Sèvres is now recognized as a central and international station of reference. Standard thermometers have been verified, for instance during 1892, for the Governments of Russia, France, and Roumania; for the Universities of Rome, St. Petersburg, and Odessa; for Owens College, Manchester; and for several recognized meteorological observatories. Great Britain has also been supplied by the committee with standard thermometers similar to those supplied to other contracting States.

Besides the standard metre and kilogramme already delivered to this country, the Bureau is undertaking the construction of a further standard metre for the Board of Trade, at a cost of 12,588 francs. The new standard appears to have been nearly two years in construction, but its verification is now promised this year.

There are twenty-one different governments who have joined the Convention and who contribute annually towards the expenses of the Bureau (the annual budget of which is 75,000 francs), sums varying from 134 francs (Denmark) to 9482 francs (Germany) the annual contribution of Great Britain and Ireland for 1892 being stated at 4699 francs, or nearly £188; and that of the United States at 8471 francs.

At the instance of Dr. B. A. Gould the committee are now also undertaking an inquiry affecting measurement by light waves. By the use of the "Refractometer" Dr. Michelson found (*Philosophical Magazine*, April, 1891, and September, 1892) that accuracy of measurement by light-waves may be increased to a high degree of accuracy. By the best spectroscopic instruments now in use it has been stated to be difficult to "resolve" lines as close together as the components of the yellow sodium lines, but that if the width of the lines themselves be less than their distances apart, then there is no limit to their accuracy of measurement by the "Refractometer." We shall look forward with interest to the publication of Dr. Michelson's further results, in the next volume of the "Travaux et Mémoires" of this committee.

The new instrument designed by M. Gustave Tresca, of the Conservatoire des Arts et Metiers at Paris, for the adjustment and polishing of the terminal surfaces of end-measures of length appears also to be better than anything yet adopted in England.

The committee not only undertakes the verification of standards and instruments for the High Contracting Governments (who have the right to demand such verifications), but they also verify for any scientific authorities or persons. To those of our readers, therefore, who may desire to have standards or instruments verified by the committee, the following information may be useful:—

Applications for the verification of instruments should first be made to M. le Directeur du Bureau International (Dr. René-Benoît), au Pavillon de Breteuil, Sèvres, près de Paris.

[1] Rapport du Comité International. Gauthier-Villars. Paris. 1 Vol. 50 pp., 1892.

Standards may be sent to Sèvres by post or railway (at the cost and risk of the owner); or still better, they may be delivered and removed from the Bureau by the owner or his agent. A certificate of verification will be given when the standards are ready for removal. In any application to the director the denomination of the standard, or the description of the instrument, should be stated, and the nature and extent of the verification demanded.

The committee will verify metric standards of length of one, two, three, and four metres, or subdivisions of the standard metre, if made in metal or some durable stone. Line-measures should have their graduations so fine as to be well observed with a microscopic power of sixty diameters; and end-measures should have their terminal surfaces sufficiently adjusted and polished so as accurately to define the length of the bar. Measures of mass may be made also of metal or some durable stone, but each must be in one piece without handles, grooves, or adjusting holes. For thermometers and barometers special regulations are issued, which may be obtained at a small charge from MM. Gauthier-Villars, 55, Quai des Grand Augustins, Paris.

The fees on verification of measures of length vary from 60 to 400 francs, according, of course, to the extent of the verification demanded; for metric weights from 20 to 120 francs; and for thermometers and barometers from 10 to 80 francs.

What should be the true equivalent length of the yard measure in terms of the metre, may appear to some to be almost a trifling matter—because the measurement in dispute, or probable error of the equivalent at present adopted in this country, amounts only to 0·0008 inch. It is, however, a fact that so small a difference as 0·0008 in this equivalent would not only be felt in scientific researches but also in practical work. Messrs. Comstock and Tittman, of the United Coast Survey, as well as Dr. Peters, of Germany, and the Director of the International Committee, have found that the equivalent length of the metre (39·3708 inches) as ascertained by Kater and Arago, in 1818, is inaccurate, to the extent of 0·0008 inch, and that the true equivalent ought to be nearly 39·3700 inches. This latter value will, we have no doubt, be ultimately recognized in scientific work.

In the field of electrical measurements, we find that Dr. Guillaume is continuing his investigations as to the measurement of temperature by electrical methods; and as to the variations of mercurial standards of resistance, a work originally begun at the Bureau, by Dr. Benoît, in connection with the standard ohm. It would not appear that mercurial thermometers can be superseded for ordinary measurements of temperature, but that measurement by resistances may afford useful results in determining the temperature of a given mass or space, as the whole length of a column of mercury. Dr. Guillaume gives an account of his work on mercurial standards in the Procès-Verbaux recently issued (page 183).

During the past year Commandant Defforges, of the Geographical service of the French army, has been undertaking at the Bureau an inquiry into the effect of the force of gravity at the latitude of Breteuil, by means of a seconds pendulum and apparatus constructed by Brunner. M. Defforges found that at Breteuil (longitude east of Paris 0°·131, latitude north 54·260, and altitude 70·4 metres) G = 9·80991 m.

We cannot conclude this glance at the recent work of the International Committee without expressing an opinion that the scientific success of their work and the accuracy of its record, owe much to the energy and watchful care of the new president, Dr. Foerster, and the secretary of the committee, Dr. A. Hirsch.

NOTES ON SOME ANCIENT DYES.[1]

THE fragments of ancient dyed fabrics which I have examined I owe to the kindness of Mr. R. D. Darbishire. They are specimens from a lot found by Mr. Flinders Petrie in a tomb at Garob, Lower Egypt, supposed to date from 400–500 A.D. They were used apparently for filling the mummy cases where required, not strictly speaking as grave clothes. My object in examining them was to ascertain, if possible, what were the materials employed in producing the various colours seen on them. The fabrics examined consisted almost entirely of wool. Here and there in the warp of some of the specimens were threads, conspicuous for difference in colour, consisting of linen. The following colours could be distinguished:—blue, yellow, green, red, maroon, purple or claret, black. I will take them in the order named.

Blue.—The colour of the fabric was a dull medium blue. On treatment with hot caustic lye a great part of the wool dissolved. The residue, which was dark blue, having been filtered off, washed and dried, was treated with boiling aniline, to which it communicated a bright blue colour. The blue solution having been filtered boiling, deposited on cooling a quantity of blue crystalline scales, which, after being filtered off, washed with alcohol and dried, were found to consist of indigo blue. On being treated in a tube they gave a sublimate of regular crystals, blue by transmitted, copper-coloured by reflected, light; they dissolved in concentrated sulphuric acid, giving a blue solution, and the solution in aniline showed the absorption spectrum of indigo blue. It is evident, therefore, that indigo in some form or other was the material used in dyeing this colour.

Yellow.—The colour of the patches dyed yellow was so evidently faded, and showed so little intensity, as to make it very uncertain whether analysis would lead to any precise result; the examination was therefore omitted.

Green.—Of the material dyed this colour, I had but a small quantity, but it was sufficient to allow of some conclusion regarding the means whereby the colour was produced. On being treated for some days with dilute hydrochloric acid it imparted to the latter a deep yellow colour. The portion left by the acid, after being washed and dried, yielded indigo blue on treatment with boiling aniline. It is probable, therefore, that the colour was produced by first dyeing the fabric with indigo, then treating with some mordant, such as alum, and, lastly, dyeing with some yellow colouring matter, most likely of vegetable origin. With the small quantity of material at my disposal, I found it impossible to ascertain the nature of the yellow colouring matter employed.

Red.—This was the most pronounced, and at the same time the most interesting, of the colours examined. The colour of the fabric was a full deep red. It might be called a Turkey red; the dye, in fact, proved on examination to be a kind of Turkey red as having the characteristic properties of that dye.

On being burnt, the fabric left a considerable quantity of ash, consisting of calcium sulphate, alumina, aluminium phosphate, ferric oxide, and silica. A large portion of this ash no doubt represents the mordant employed in producing the colour. On treatment with hot dilute hydrochloric acid, the fabric lost its red colour and became yellow. After removal of the acid by washing with water, and pressing between blotting paper, treatment with boiling alcohol deprived the wool of the greater part of the yellow colour, a faint tinge only being left. The deep yellow alcoholic liquid obtained left on evaporation a reddish-brown amorphous residue. This, on being treated with a boiling solution of alum, dissolved in part, yielding a pink fluorescent liquid, which had exactly the same colour, and showed precisely the same absorption bands as a solution of purpurin from madder in alum liquor. On adding hydrochloric acid to the pink solution and heating, the colouring matter was precipitated in orange-coloured flocks, the liquid becoming almost colourless. The flocks after being filtered off and washed with water dissolved easily in boiling alcohol, yielding a yellow solution, which, on spontaneous evaporation, left a quantity of dark yellow needles arranged in rosettes. These needles dissolved in caustic alkali, giving a cherry-red solution, which showed the absorption bands of purpurin. The solution, on exposure to air and light, became colourless.

Some of the precipitated colouring matter, on being employed in the usual way for dyeing a bit of calico to which various mordants had been applied, yielded colours exactly like those obtained with purpurin from madder, *i.e.*, the alumina mordant gave a bright red, the iron mordant dull purple to black tints. The matter left undissolved, after repeated treatment with boiling alum liquor, was still highly coloured. It dissolved easily in alcohol, the solution leaving on evaporation a brown amorphous residue, which remained soft even after long standing. This residue consisted for the most part of fatty matter, but it also contained some colouring matter insoluble in alum liquor. That this colouring matter was alizarin seemed probable, since the colour which the mixture imparted to alka-

[1] Reprinted from "Memoirs and Proceedings of the Manchester Literary and Philosophical Society," 1891-92 (Fourth Series, vol. 5, No. 2).

line lye resembled that of an alkaline solution of impure alizarin.

These experiments lead to the conclusion that the red colour of the fabric was produced by dyeing with some kind of madder, either wild or cultivated, the fabric having been previously treated with a mixed aluminous and ferric mordant, and then probably oiled—that it was, in fact really a kind of Turkey red.

Maroon.—The dull chestnut colour of this fabric presented a striking contrast to the bright red of the preceding. Its constitution was, however, similar. Having treated it in the same way as the other, I found that the colouring matter must have been derived from madder; fatty matter was also present, but the mordant contained a larger proportion of ferric oxide, a fact which sufficiently explains the brown tint of the dyed fabric.

Purple.—The fabric in which this colour was seen was made up of a pale yellow warp, and a weft of a dull purple or claret colour. The latter colour was found to be due to an intimate mixture of red and blue, for the threads, on examination under the microscope, were seen to consist partly of red, partly of blue fibres, the former predominating. The two sets of fibres had, of course, been mixed before spinning. The blue fibres were certainly dyed with indigo, the red probably with madder.

Black.—The colour of the black fabric, like that of the green, was a compound of two colours, one overlying the other. Under the microscope the individual threads appeared grey. On treatment with a mixture of alcohol and hydrochloric acid they changed colour, a yellow liquid being obtained, while the fabric itself now appeared blue, and after washing and drying yielded indigo by appropriate treatment. The yellow alcoholic liquid was found to contain purpurin. The colour may be supposed to have been produced in the following manner:—The woollen fabric having first been dyed blue was mordanted, to use a modern phrase, and then dyed with madder, the two colours together producing the effect of black.

EDWARD SCHUNCK.

SCIENTIFIC SERIALS.

IN the *Botanical Gazette* for July, August, and September, there are several papers of general interest. Mr. G. A. Rex presents a further contribution to our knowledge of the Myxomycetes in an account of the genus *Linbladia*.—Mr. D. T. McDougal gives a detailed account of the morphology and anatomy of the tendrils of *Passiflora cærulea*.—Mr. M. B. Thomas describes and figures an apparatus for determining the periodicity of root-pressure in plants.—Mr. C. L. Holtzman has a short paper on the Apical growth of the stem and the development of the sporange in *Botrychium virginianum*, his observations favouring the view that the Ophioglossaceæ are a more primitive form than the typical Filices.—Mr. A. F. Foerste continues his observations on the Relation of autumn to spring-blossoming plants.—Mr. Charles Robertson gives a further instalment of his series of papers on Flowers and insects.—A brief report is given of the botanical papers read at the recent meeting of the American Association for the Advancement of Science.

IN the *Journal of Botany* for September and October, no less than four new species are added to the British flora and to science—*Hieracium hibernicum*, *H. duriceps*, and *H. Bread-albanense*, by Mr. F. J. Hanbury; and *Ranunculus petiolaris* (sect. Flammula) by Rev. E. S. Marshall.—Rev. W. Moyle Rogers continues his Essay at a key to British Rubi; Mr. E. G. Baker his Synopsis of genera and species of Malveæ; and Mr. W. A. Clarke his First Records of British Flowering Plants.

Bulletin of the New York Mathematical Society. Vol. ii. No. 1, October, 1892. (New York.)—Prof. Cajori opens this number with an interesting note on the evolution of criteria of convergence (pp. 1–10), in which he discusses some special and general criteria furnished in the writings of Gauss, Cauchy, Abel, DeMorgan, Bertrand, Kummer, and others, and notices specially the remarkable advance made by Pringsheim (*Math. Ann.* vol. xxxv.).—Dr. A. Martin calls attention (pp. 10–11) to a slip in Ball's "Short History of Mathematics" (p. 102), the probable origin of which is accounted for by Mr. Ball.—There is a slight review of Chapman's "Elementary Course in the Theory of Equations" (pp. 11–12), and the rest of the issue is taken up with the usual list of new publications and notes. In these last Dr. Martin points out a curious error in the Royal Society "Catalogue of Scientific Papers," vol. ix. (1874–1883), where, of the papers accredited, on p. 790, to Ezekiel Brown Elliott, Nos. 5–11, 14–17 should be assigned to Mr. Edwin Bailey Elliott, of Oxford, and not to the late Mr. Ezekiel Brown Elliott, of America, to whom Nos. 4, 12, 13 are rightly attributed.

IN the *Bullettino* of the Botanical Society of Italy, we find in addition to papers of more local interest, a further communication from Sig. Macchiati on the Cultivation of diatoms, in which he states that the presence of infusoria and of diatoms in the water is mutually beneficial to one another, while the most destructive enemies of the latter are bacteria.—A paper by Sig. Piccioli on the Biological relations between plants and snails, is chiefly devoted to the protective contrivances found in the former against the attacks of the latter, the most important of which are of a chemical nature—tannin, latex, oleiferous glands, and poisonous salts such as calcium oxalate: mechanical means of protection, such as hairs and a comparatively thick cuticle, play a subordinate part.—In a further communication by Prof. Arcangeli on the Cultivation of *Cynomorium coccineum*, he states that he does not find such an intimate parasitism with its host as is the case with the Rafflesiaceæ and the Balanophoraceæ.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 24.—M. de Lacaze-Duthiers in the chair.—Researches on the fixation of atmospheric nitrogen by microbes, by M. Berthelot. The investigation was made in order to elucidate the mechanism of the fixation of atmospheric nitrogen. It appears that the presence of green vegetable material is not essential to the process. The colourless bacteria are able to absorb nitrogen when supplied with humic acid only as nutriment. The assimilation takes place more readily with natural than with artificial humic acid, probably because the former contains more nitrogen. In experiments with hermetically sealed cultivations it was found that the gain of nitrogen by the organic material under cultivation was 6 or 9 per cent. in excess of that supplied by the humic acid, the difference being derived from the enclosed air. With an occasional stream of dust-laden air this was brought up to 30 per cent.—Coloured photographs of the spectrum on albumen and bichromated gelatine, by M. G. Lippmann. Albumenized and gelatinized plates soaked in bichromate of potash may be employed for photographing in colours. They are used like silver-salt plates, being placed so that the mercury is in contact with the film. The colours will appear immediately after immersion in water, which develops and also fixes the image. It disappears on drying, but reappears as soon as the plate is soaked. The colours are very brilliant, and visible at all angles. Those of gelatine plates are brought out by simple breathing. The theory is analogous to that of silver plates, the maxima and minima of interference producing hygroscopic and non-hygroscopic layers with varying refractive indices.—The irrigation canals of the Rhone, by M. Chambrelent.—A new apparatus, the schiseophone, serving the purpose of exploring the internal structure of metallic masses by means of an electro-mechanical process, by M. de Place. The apparatus consists of a microphone and an induction sonometer. To the microphone is attached a rod of hard steel, kept oscillating once or twice per second, and striking each time against the casting or other mass of metal under investigation. The sonometer, consisting of two coils movable towards or away from each other along a divided scale, with a telephone connected with one of the coils, is placed in another room, and joined by wires to the microphone. The coils being so adjusted that the tapping is scarcely perceptible at the sonometer, the casting is moved so as to expose various portions to the impacts. If the thickness be uniform, any flaw or fissure will be at once indicated by a change in the sound.—Observations of the comet Barnard (D 1892), made at the Paris Observatory, by M. G. Bigourdan.

—Elements of the comet Barnard, of October 12, 1892, by M. L. Schulhof.—On the algebraic integrals of the differential equation of the first order, by M. L. Autonne.—On centres of geodesic curvature, by M. Th. Caronnet.—On Pfaff's problem, by M. A. J. Stodolkievitz.—Sunspots and magnetic disturbances in 1892, by M. Ricco.—On considerations of homogeneity in physics; reply to M. Clavenad, by M. Vaschy.—Verification of parallelism of optic axes in uniaxial crystalline plates, by M. Bernard Brunhes.—On a photoptometric photometer, for the measurement of feeble illuminations, by M. Charles Henry. This is based upon the constancy of the phosphorescent sulphide of zinc. Its law of loss of brilliance being determined, it may be used for measuring very feeble illuminations, such as distant artificial light or the general luminosity of the sky due to the stars. The decrease of light after the first 900 seconds being given by $i^{0.5}(t - 18.5) = \text{const.}$, it is easy to calculate the luminosity at any instant. In the instrument in question there are two screens of ground glass, one of which is illuminated by the phosphorescent sulphide, brought to its maximum glow at a certain time by burning magnesium ribbon, the other exposed to the source of light. It is then only necessary to wait till both the screens are equally illuminated, and to note the time.—On the dissociation of chrome alum, by MM. H. Baubigny and E. Pechard.—On the temperatures of maximum density of aqueous solutions, by M. L. de Coppet.—On some double salts of quinine, by M. E. Grimaux.—On the thermal value of the three functions of orthophosphoric acid, and on its constitution, by M. de Forcrand.—Preparation and properties of fibroine, by M. Leo Vignon.—Regeneration of the so-called sporangial form in the diatoms, by M. P. Miquel.—On the hematozoaria of cold-blooded vertebrates, by M. Alphonse Labbé.—Influence of coloured light on the development of animals, by M. E. Yung.—On the mode of fixation of the hexapod parasitic larvæ of the acarians, by M. S. Jourdain.—The cavern of Brassempouy, by M. Édouard Piette.—Discovery of a skeleton of *Elephas meridionalis* in the basaltic ashes of the volcano of Senèze, by M. Marcellin Boule.—Vegetable prints of the Dover boring, by M. R. Zeiller.

BERLIN.

Meteorological Society, October 11.—Prof. von Bezold, president, in the chair.—Dr. Berson reported on an interesting relationship which he had discovered between insolation and temperature. Since it has not yet been possible to determine accurately the absorption due to the atmosphere, the speaker had calculated the insolation at the external limit of the atmosphere, which admits of rigid mathematical treatment, both for the whole year and for the months of January and July. The mean of insolation for the whole year was found to lie at the thirtieth degrees of northerly and southerly latitude, so that the zone between these parallels, or about 60 per cent. of the whole external surface, receives more insolation than the mean, whereas the two polar caps, or the remaining 40 per cent., receive less. A similar calculation of the annual temperature gave the mean as at latitude 38° N. and 35° S., giving as before 60 per cent. of the surface with the temperature above the mean, and 40 per cent. below. In January 61·35 per cent. of the surface experienced an insolation above the mean and 60 per cent. a temperature above the mean, while in July the percentages were respectively 61·37 and 61·33.—Dr. Zenker gave a short account of a research on the relationship between temperature and insolation on the earth's surface. He had accurately calculated the relationship both for regions comprising land only and water only, and arrived at some interesting conclusions as to the theoretical temperatures at various latitudes of continents and oceans.

Physical Society, October 21.—Prof. Kundt, president, in the chair.—Dr. Jäger gave an account of the measurements he had made, in conjunction with Dr. Kreischgauer, of the temperature-coefficient of electric conductivity of mercury. Dr. Arons demonstrated an arc-light between mercurial electrodes in vacuo. It yielded a dazzling white light, which was steady at the anode but flickered and jumped at the cathode: its intensity approximated to that of an ordinary carbon arc-light. The heat given off by it was but slight so that the tube could be held in the hand; the temperature was highest at the cathode. Attempts were made to determine the resistance of the arc, but without result. It was found by the use of a telephone that the current is discontinuous. A spectroscopic investigation of the light revealed a lime-spectrum showing very brilliantly a yellow, a green, and a blue line. In addition to the ordinary lines due to mercury some twenty new lines were observed. No satisfactory results were obtained by using amalgams instead of mercury, with the one exception of sodium-amalgam. It is proposed to make further experiments with fluid amalgams of sodium and potassium.

BOOKS and SERIALS RECEIVED.

BOOKS.—The Great World's Farm: S. Gaye (Seeley).—The Zoological Record, 1891 (Gurney and Jackson).—Castorologia, or the History and Traditions of the Canadian Beaver: H. T. Martin (Stanford).—Transactions of the Royal Society of Edinburgh, vol. xxxvi. Parts 2 and 3 (Edinburgh).—Les Alpes Françaises: A. Falsan (Paris, Baillière).—Calendar of the University College of Wales, Aberystwith, 1892-93 (Manchester, Cornish).—London Birds and other Sketches, revised edition: T. D. Pigott (Porter).—Contents and Index of the First Twenty Volumes of the Memoirs of the Geological Survey of India, 1859-83: W. Theobald (Calcutta).—Memoirs of the Geological Survey of India; Index to the Genera and Species described in the Palæontologia Indica, up to the Year 1891: W. Theobald (Calcutta).—Star Atlas: Dr. H. J. Klein, translated, &c., by E. McClure, new edition (S.P.C.K.).—City and Guilds of London Institute Programme of Technological Examinations, 1892-93 (London).—Appareils d'Essai à froid et à chaud des Moteurs à Vapeur: M. Dudebout (Paris, Gauthier-Villars).—Canon Torpilles et Cuirasse: A. Croneau (Paris, Gauthier-Villars).—Ostwald's Klassiker der Exakten Wissenschaften, Nos. 31-37 (Leipzig, Engelmann).—Gesammelte Abhandlungen über Pflanzen-Physiologie. Erster Band: J. Sachs (Leipzig, Engelmann).—On the American Iron Trade and its Progress during Sixteen Years: Sir L. Bell (Ballantyne).—Universal Atlas, Part 20 (Cassell).

SERIALS.—The Physical Society of London, Proceedings, vol. xi. Part 4 (Taylor and Francis).—Botanical Gazette, October (Bloomington, Indiana).—Traité Encyclopédique de Photographie, Premier Supplément A. quat. fasc.: C. Fabre (Paris, Gauthier-Villars).—Zeitschrift für Wissenschaftliche Zoologie, liv. Band, 4 Heft (Williams and Norgate).—Morphologisches Jahrbuch, xix. Band, 1 Heft (Williams and Norgate).

CONTENTS.

	PAGE
The University Commission	1
The Study of Animal Life. By C. Ll. M.	2
Vector Algebra	3
The Lake of Geneva. By Prof. T. G. Bonney, F.R.S.	5
Our Book Shelf:—	
Ward: "Horn Measurements and Weights of the Great Game of the World, being a Record for the Use of Sportsmen and Naturalists"	6
Philippson: "Der Peloponnes. Versuch einer Landeskunde auf geologischer Grundlage"	6
Fabre: "Traité Encyclopédique de Photographie."—W.	6
"The Reliquary": Quarterly Archæological Journal and Review	7
Letters to the Editor:—	
Nova Aurigæ.—H. F. Newall	7
Formation of Lunar Volcanoes. (*Illustrated.*)—J. B. Hannay	7
On the Need of a New Geometrical Term—"Conjugate Angles."—Prof. A. M. Worthington	8
Printing Mathematics.—W. Cassie	8
"Sunshine."—Amy Johnson; C. V. B.	9
The Photography of an Image by Reflection.—Frederick J. Smith	10
Induction and Deduction.—Edward T. Dixon	10
Bell's Idea of a New Anatomy of the Brain.—Jas. B. Bailey	11
Photographic Dry Plates.—Arthur E. Brown	11
The Genus Sphenophyllum. (*With Diagram.*) By Prof. Wm. Crawford Williamson, F.R.S.	11
Dendritic Forms. By Sydney Lupton	13
Notes	14
Our Astronomical Column:—	
Comet Brooks (August 28)	18
Comet Barnard (October 12)	18
Tabular History of Astronomy to the Year 1500 A.D	18
A Large Telescope	18
The Atmospheres of Planets	18
Geographical Notes	19
The Institution of Mechanical Engineers	19
International Committee of Weights and Measures	21
Notes on some Ancient Dyes. By Edward Schunck, F.R.S.	22
Scientific Serials	23
Societies and Academies	23
Books and Serials Received	24