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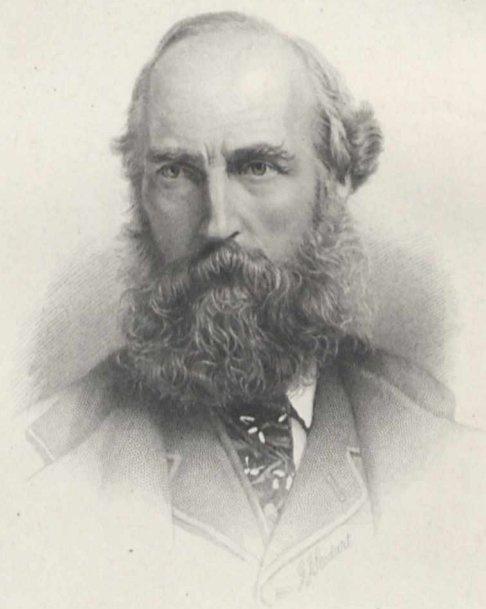
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Nature, April 26th 1883



William Spottiswoode.

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

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INDEX

- ABERCROMBY (Hon. R.), the Aurora and its Spectrum, 173
 Abney (Capt. W. de W., F.R.S.), Work in the Infra-red of the Spectrum, 15
 Acetate of Soda, Heating by, 344
 Acta Mathematica, 180
 Adamson (Chas. Murray), "Another Book of Scraps principally relating to Natural History," 480
 Aërolite near Alfianello, 496
 Aëronautics, *see* Balloons
 Africa: Joseph Thomson's Expedition, 21; H. M. Stanley's Report of his Expedition, 21; Milan Society's Expedition, 230; Lieut. Wissmann's Expedition, 348; Wissmann and Pogge's Expedition, 401; Central and West, H. Capello and R. Ivens, 391; Ethnology of North, Prof. A. H. Keane, 408; "Afrika's Ströme und Flüsse," Dr. J. Chavanne, 447; "Africana," Rev. D. Macdonald, 526; Dr. Emil Holub's New Expedition, 542
 Agram, Earthquakes at, 348
 Agricultural Students' Gazette, 230
 Agriculture in Madras, 607
 Aino Ethnology, Dr. J. J. Rein, 365; Prof. A. H. Keane, 389
 Ainos, Bear Festival among the, 19
 Air, Elasticity of, M. Kraevitch, 183
 Air, on the Movements of, in Fissures and the Barometer, A. Strahan, 375, 461
 Air-pump, Double-action Mercury, Serrawalle, 324
 Airy (Sir G. B., F.R.S.), on the proposed Forth Bridge, 131; C. S. Smith, 99; B. Baker, 222
 Airy (Dr. Hubert), the Magnetic Storm and Aurora, 86; Hovering of Birds, 294, 336, 388, 412; Soaring of Birds, 590
 Albuminous Bodies, on the Isomerism of, Shigetaké Sagiura, 103
 Aldis (Prof. W. Steadman), "Introductory Treatise on Rigid Dynamics," 265; the Sea Serpent, 338
Aletia xyliua in United States, Hibernation of, Dr. C. V. Riley, 214
 Aleutian Islands, Fauna and Flora of, 520
 Alfianello, Aërolite near, 496
 Alge in Palæozoic Strata, Invertebrate Casts *versus*, 46
 Algæ, Fossil, Marquis de Saporta, 514
 Algeria, *Projet de Mer Intérieure dans l'*, 133; an Algerian Winter Resort for Gout and Rheumatic Patients, 113; Heavy Rains in, 229
 Allen (Grant), Prof. Owen on Primitive Man, 31; the Shapes of Leaves, 439, 464, 492, 511, 552; F. O. Bower, 552; Leaves and their Environment, 604
 Almanac, the Churchman's, for Eight Centuries, 386
 Altaï Exploration Expedition, 161
 Altitude and Weather, Dr. Woeikoff, 223
 Aluminium, New Method of Producing, Morris, 183
 Amateurs and Astronomical Observation, W. F. Denning, 434
 America: American Naturalist, 24, 189, 450; American Journal of Science, 71, 450, 521; American Journal of Forestry, 89; Mathematics in, J. W. L. Glaisher, F.R.S., 193; American Researches on Water Analysis, 211; American Antiquities, Prof. F. W. Putnam, 277; Ensilage in, Prof. James E. T. Rogers, Prof. J. Wrightson, 479; Proceedings of the American Association for the Advancement of Science, Prof. T. G. Bonney, F.R.S., 501; Evolution of the American Trotting Horse, W. H. Brewer, 609; *see also* United States, &c.
 Amsterdam, Earthquake at, 517
 "Ancient Scottish Lake Dwellings," by Dr. Munro, Sir John Lubbock, M.P., F.R.S., 145
 Ancient Monuments, Worthington G. Smith, 182
 Andernach, Discovery of Remains of Prehistoric Animals at, 445
 Anderson (Dr. J., F.R.S.), Catalogue of Mammalia in Calcutta Museum, 172
 Anemometric Observations on the *Djight*, Domojiroff, 445
 Animal Intelligence, Dr. Fritz Müller, 240; J. G. Grenfell, 292; J. Birmingham, 337; Dr. J. Rae, F.R.S., 366; D. Pidgeon, 366
 Animals, Protection of, International Congress for, 444; Benevolence in, Oswald Fitch, 580; Geo. J. Romanes, F.R.S., 607; on the Sense of Colour amongst some of the Lower, Sir John Lubbock, Bart., 618
 Annalen der Physik und Chemie, 143, 281, 330, 450
 Annonay Montgolfier Celebration, 517
 "Another Book of Scraps," C. M. Adamson, 480
 Antarctic Expedition, New Italian, 230
 Anthropology, an Urgent Need in, W. L. Distant, 101; of the Jews, B. Blechmann, 113; Anthropological Institute, 119, 191, 215, 307, 332, 426, 475; Anthro-Geographie, Dr. F. Ratzel, 125; Notes in the Solomon Islands, H. B. Guppy, 607
 Antinori (Marchese O.), Death of, 132
 Apatite, on a Fine Specimen of, from Tyrol, lately in the possession of Mr. Samuel Henson, 608
 Arabians, Nautical Matters among Mediæval, 42
 Aradas (Prof. Andrea), Death of, 160
 Arc Lamp, Lever's, 274
 Archegosaurus, Indian, R. Lydekker, 411
 Archene, Earthquake at, 277
 Archibald (E. Douglas): Shadows after Sunset, 77; Increase in Velocity of Wind with Altitude, 243; Diurnal Variations in the Velocity of the Wind, 461; Stevenson's Observations on Increase of Velocity of the Wind with the Altitude, 506
 Archives des Sciences Physiques et Naturelles, 93, 143, 306, 473
 Arctic Expedition, New Swedish, 400
 Arctic Voyaging, Season of 1882 exceptionally adverse to, 372
 Argentine, Description Physique de la République, d'après des Observations Personnelles et Etrangères, Dr. H. Burmeister, 28
 Argyll (Duke of): Mimicry in Moths, 125; Transit of Venus, 156; Hovering of Birds, 312, 387; Metamorphic Origin of Granite—Prehistoric "Giants," 578
 Arithmetic, Elementary Chemical, Sidney Lupton, 76
 Armour-plate Experiments, Recent, 405
 Asia (Central), Dr. Regel's Explorations in, 446
 Asphyxiation in Rapid Falling, 62
 Association for Improvement of Geometrical Teaching, 246, 299, 580
 Astronomy: Astronomical Column, 20, 43, 161, 210, 248, 300, 324, 348, 374, 400, 424, 445, 497, 517, 540, 567, 589, 617; Methods employed in Astronomical Physics, M. Wolf, 372; Amateurs and Astronomical Observation, W. F. Denning, 434; Astronomical Photography, Edward C. Pickering, 556
 Athens, Dr. Schliemann's proposed Excavations at, 276
 "Atlantis," a Search for, with the Microscope, Dr. Arch. Geikie, F.R.S., 25
 Atmosphere, Pollution of the, J. J. Murphy, 241; H. A. Phillips, 127, 266
 Atti della R. Accademia dei Lincei, 354
 Auden (A. W.), "The Vampire Bat," 411
 Auerbach, Sound-Vibrations of Solid Bodies (glass cylinders) in Contact with Liquid, 325
 Aurora Borealis: Prof. Lemström's Experiments with, 322; Sophos Trouholt, 394; in Sweden, 496
 Auroræ: C. L. Wragge, 54; Prof. Herbert McLeod, Rev. C. J. Taylor, Rev. S. H. Saxby, 99; A. Batson, 99; J. Rand Capron, 149, 413; J. J. Murphy, 434; at Gastemünde, 374; Swan Lamp Spectrum and the, J. Rand Capron, 149; Swan Lamp Spectrum and, J. Munro, 173; Aurora of November 17, Admiral Ommanney, F.R.S., Prof. Tacchini, J. R. Capron, W. M. F. Petrie, H. D. Taylor, E. Pollock, J. F. Cole, A. Batson, F. R. Clapham, T. W. Backhouse, Prof. L. G. Carpenter, 138 to 142; T. W. Backhouse, W. M. F. Petrie, Dr. H. Muirhead, 315; Auroræ in Sweden, 113; in Belgium, 160; Aurora and its Spectrum, Hon. R. Abercromby, 173; J. R. Capron, 198; the Heights of Auroras, T. W. Backhouse, 198; Auroral Meteoric Phenomena of November 17, 1882, Dr. Groneman, 388; T. W. Backhouse, 412; A. Batson, 412; Auroral Experiments in Finland, S. Lemström, 389; Rowell's Experiments on Auroræ, 443

- Avalanche in Western Switzerland, 181
 Ayrton (Prof. W. E.), Electric Railways, 255
- Bacillus of Tuberculosis (Koch), Preliminary Note on the, J. W. Clark, 492; Watson Cheyne's Report on, 563
- Backhouse (Thos. W.), Comet 1882 *b*, 52, 338; Aurora of November 17, 1882, 141, 315, 418; the Heights of Auroras, 198
- Baillie-Grohman (W. A.), "Camps in the Rockies," 551
- Baird's Hare and its Habits, 241; T. Martyr, 266
- Baker (B.), Sir Geo. Airy on the Forth Bridge, 222
- Balkan Peninsula, Earthquake in the Northern Part of, 19
- Ball (Dr. R. S.), Transit of Venus, 157; Great Tides, 201
- Balloons: Centenary of the Discovery of, 277; Tissandier's Electromagnetic Engine for directing, 323; Extraordinary Run of a small Hydrogen, 400; Electricity as a Motive Power for, Napoli, 517
- Baltic, Seals in the, 133
- Baluchistan, Wanderings in, Major-General Sir C. M. MacGregor, Prof. A. H. Keane, 359
- Barfoot (W.), the Sea-Serpent, 338
- Barkas (T. P.), the Magnetic Storm and Auroræ, 87; an Extraordinary Lunar Halo, 103
- Baro-manometrical Experiments, Kraevitch, 324
- Barometer, on the Movements of Air in Fissures and the, A. Strahan, 375, 461
- Batson (A.), Auroral "Meteoric Phenomena" of November 17, 1882, 100, 141, 412
- Bavaria, Lower, Discoveries of Gold and Silver in, 399
- Baxter College in Durdee, 247
- Bear Festival among the Ainos, 19
- Bees, Senses of, 46
- Beetles, "Die Kafer Westfalens," F. Westhoff, 239
- Belgrade, Skeleton of Mammoth found at, 63
- Bellesme (Prof. J. de), on Claude Bernard, 317
- Benevolence in Animals, Oswald Fitch, 580; Geo. J. Romanes, F.R.S., 607
- Ben Nevis, Meteorological Observatory on, 18, 39, 175, 399, 411, 487
- Bennett (A. W.), Nesting Habits of the Emu, 530
- Berlin: Physical Society of, 95, 143, 216, 236, 284, 380, 403, 476, 571, 620; Physiological Society of, 120, 191, 216, 355, 379, 403, 452, 524, 595; Berlin Agricultural Museum, 180
- Bernard (Claude), Prof. J. de Bellesme on, 317
- Berson (M.), Magnetisation of Metals, 183
- Bertillon (Dr.), Death of, 444
- Bhamo, Colquhoun's Journey to, 63
- Bidwell (Shelford), "Electrical Resistance of Carbon Contacts," 376
- Binary Star, ζ Cancri, 424
- Binary Star, ρ Eridani, 589
- Binary Stars, 518
- Biology: in Italy, 46; Biological Notes, 91, 230; New Lantern Slides for Illustrating Biology, 276; Deductive Biology, W. T. Thiselton Dyer, F.R.S., 554
- Bird of Paradise, New Species of, 276
- Bird-track by Sea-shore, apparent, 91
- Birds, a History of British, Part xv., Wm. Yarrell, 98
- Birmingham (John), Transit of Venus, 1882, British Expeditions, 180; Intelligence in Animals, 337
- Birmingham Natural History and Microscopical Society, 587
- Bischoff (Dr. T. L. W. von), Death of, 180
- Blackheath, "Denehole" at, 324
- Blanford's Sheep, 415
- Blasius (Dr.), Fossil Soudlik in Northern Germany, 247
- Blastopore, Prof. F. M. Balfour on the Existence of a, 215
- Blackman (B.), Anthropology of the Jews, 113
- Bles (E. J.), a "Natural" Experiment in Complementary Colours, 241
- Blomfield (Rev. L.): Ticks, 552; *Helix pomatia*, L., 553
- "Blowing Wells," 375
- Bobenhausen Pile Dwellings, 160
- Bohdalek (Dr.), Death of, 399
- Bohemia, Earthquake in, 400
- Bologna, Earthquake at, 445
- Bombay, Deaths from Snake-bite in, Sir Joseph Fayrer, F.R.S., 556
- Bonney (Prof. T. G., F.R.S.): Hornblende and other Schists of the Lizard District, 71; Proceedings of the American Association for the Advancement of Science, 501
- Botanic Gardens, Calcutta, Report of the Royal, 114
- Botanical Garden, Saharanpur, 588
- Botanical Year Book, Pringsheim's, 502
- Botany, Text Book of Morphological and Physiological, Julius Sachs, Prof. E. P. Wright, 263
- Botany of the *Challenger* Expedition, W. Botting Hemsley, 462
- Bottomley (J. T.), Thomson's Mouse-Mill Dynamo, 78
- Boulger (Prof. G. S.), Epping Forest, 455
- Bowditch (Dr. H. P.), Optical Illusions of Motion, 183
- Bower (F. O.), Mr. Grant Allen's Articles on "The Shapes of Leaves," 552
- Braces or Waistband? 531, 553, 580
- Branfill (B. R.), an Extraordinary Meteor, 149
- Brassey (Sir Thomas), "The British Navy; its Strength, Resources, and Administration," W. H. White, 549
- Brault on *Isanemones*, 20
- Brewer, Distiller, and Wine Manufacturer, 311
- Brewer (W. H.), Evolution of the American Trotting Horse, 609
- British Association, Proposed Visit to Canada, 41, 88, 299, 398, 538, 546, 587; Protest against Meeting in Canada, 209; and Local Societies, 132
- British Birds, a History of, Part xv., Wm. Yarrell, 98
- British Circumpolar Expedition, Capt. Dawson, R.A., 484
- "British Navy; its Strength, Resources, and Administration," Sir Thomas Brassey, W. H. White, 549
- Britten (F. J.), Watch and Clockmaker's Handbook, H. Dent Gardner, 76
- Brocklehurst (T. U.), "Mexico To-day," 503
- Bronze Hatchets Discovered at Salez, 540
- Brown (E.), the Magnetic Storm and Aurora, 86; Meteor, 508; the Zodiacal Light (?), 605
- Brown (J. Campbell), Practical Chemistry, 75
- Brunton (Dr. T. Lauder, F.R.S.): on Inhibition (of Structural Functions) and Action of Drugs thereon, 419, 436, 467, 485; Action of Calcium, &c., on Muscle, 450
- Brussels Observatory, 445
- Buchan (A.), Diurnal Variation of Wind on Open Sea and near and on Land, 413
- Buckley (Arabella), Winners in Life's Race, 51
- Buenos Ayres, the Comet 1882 *b* at, 62
- Bulletin de l'Académie Royale des Sciences de Belgique, 71, 281, 473
- Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, 94
- Bulletin de la Société d'Anthropologie de Paris, 214
- Bulletin de la Soc. Imp. des Naturalistes de Moscou, 47, 189
- Burmah, Colquhoun's Journey to, 92
- "Burman, The," Shway Yoe, 5; Dr. E. B. Tylor, F.R.S., 6
- Burmeister (Dr. H.), "Description Physique de la République Argentine d'après des Observations Personnelles et Etrangères," 28
- Burton (Richard F.) and Verney Cameron, "To the Gold Coast for Gold," 335
- Burton (F. M.), Sap-flow, 530
- Butterflies, Natural Enemies of, Henry Higgins, 338
- Butterflies of India, Burmah, and Ceylon, Marshall and Nicville's, H. J. Elwes, 50
- Cacao: How to Grow and How to Cure it, D. Morris, 566
- Cacciatore (Prof.), Transit of Venus, 180
- Calcium, Lockyer's Dissociation Theory of, Hermann W. Vogel, 233
- Calcium, Barium, and Potassium, Action of, on Muscle, Doctors Brunton and Cash, 450
- Calcutta: Report of the Royal Botanic Gardens, 114; Catalogue of Mammalia in the Museum, Dr. Anderson, F.R.S., 172; Proposed Exhibition in, 209
- Callard (T. K.), Palæolithic River Gravels, 54
- Cambridge: New Professorships at, 469; Philosophical Society, 62, 119, 143, 403
- Cameron (Verney), Richard Burton and, "To the Gold Coast for Gold," 335
- Campbell (Lewis, LL.D.), the Life of James Clerk Maxwell, 26
- "Camps in the Rockies," W. A. Baillie-Grohman, 551
- Canada, Proposed Visit of the British Association to, 41, 88, 299, 398, 538, 546, 587; Protest against, 209
- Candolle (A. de), "Origine des Plantes Cultivées," 429
- Cape Horn, French Mission to, 344
- Cape Sea Lion, 415
- Capello (H.) and Ivens (R.), Central and West Africa, 391
- Caprifig and Fig, Relations of the, W. B. Hemsley, 584

- Capron (J. Rand) : a Lunar Halo, 78 ; the Magnetic Storm and Aurora, 83 ; Shadows after Sunset, 102 ; Aurora of November 17, 139, 149 ; Swan Lamp Spectrum and the Aurora, 149, 198 ; the Weather, 198 ; Foam-balls, 531
- Carbo-hydrates in the Animal System, Physiology of, Dr. F. W. Pavy, F.R.S., 618
- Carbon Contacts, Electrical Resistance of, Shelford Bidwell, 376
- Carboniferous Vertebrate Palæontology, Notice of some Discoveries recently made in, T. Stock, 22
- Cariamias, Flamingoes and, James Currie, 389
- Carlisle (Bishop of), Weather Forecasts, 4, 51
- Carlsruhe, Meteor at, 540
- Carnivora, Siwalik, Richard Lydekker, 293
- Carpathian Club, 162
- Carpenter (Prof. L. G.), Aurora of November 17, 141
- Cascia, Earthquake at, 63
- Cash (Dr.), Action of Calcium, &c., on Muscle, 450
- Caspian, Changes of Level of, 541
- Cassell's Encyclopædic Dictionary, 423
- Cassell's Natural History, 367
- Cassine, Earthquake at, 299
- Cassini Division of Saturn's Ring, 374
- Cassowary, the One-wattled, 153
- Cathodes, Chemical Corrosion of, Dr. Gore, F.R.S., 374
- Caucasus : Volcanic Eruption in the, 63 ; Geography of the, 470 ; Administration of Public Instruction of, 497 ; Geographical Society of, 424 ; Bulletin of Society for History and Archaeology, 497
- Cecil (Henry) : Comet 1882 *b*, 52 ; the Transit of Venus, 159 ; Hovering of Birds, 388 ; Meteors, 483
- Central Asia, "Travels and Adventures East of the Caspian during the Years 1879-81, including Five Months' Residence among the Tekkés of Merv," Edmond O'Donovan, Prof. A. H. Keane, 359
- Ceraski's Variable Star, U Cephei, 424
- Cesati (Baron V.), his Botanical Collection, 20
- "Ceylon, Lepidoptera of," L. Reeve and Co., 150
- Challenger Expedition, Botany of the, W. Botting Hemsley, 462
- Challenger Reports (Zoology, ii., iii., and iv.), 73
- Challis (Prof. James, F.R.S.), Death of, 132
- Chambery, Phylloxera in, 133
- Chandler (Prof.), the Comet, 81
- Changy on Incandescent Lamps, 209
- Channel, Mountainous Seas in Calm Weather, 540
- Channel Tunnel, Prof. W. Boyd Dawkins, F.R.S., 338
- Chavanne (Dr. J.), "Afrika's Ströme und Flüsse," 447
- Chemistry : the Chemical Society, 47, 118, 191, 234, 306, 402, 426, 522, 570, 595 ; Recent Chemical Synthesis, 49 ; Practical Chemistry, J. Campbell Brown, 75 ; Elementary Chemical Arithmetic, Sidney Lupton, 76 ; Principles of Chemistry, Prof. Mendeléeff, 113 ; Chemical Notes, 301 ; Electrolytic Balance of Chemical Corrosion, Dr. Gore, F.R.S., 326 ; Properties (Physical and Chemical) of Simple and Compound Bodies, De Heen, 422 ; Chemistry of the Planté and Faure Accumulators, Dr. J. H. Gladstone, F.R.S., and Dr. A. Tribe, F.R.S., 583
- Chevreul (M.) : nominated President of the French Société Nationale d'Agriculture, 277 ; his long Scientific Career, 422
- Cheyne (Watson), the Bacillus of Tubercle, 563
- China : Foreign Education in, 90 ; Chinese Definition of Death, 160 ; the Electric Light in, 209 ; R. K. Douglas on, 221 ; Comets as Portents in, 229 ; "Chinarinden in Pharmakognostischer Hinsicht dargestellt, Die," F. A. Flückiger, 287 ; Scientific Heresies in, 342 ; Chinese Educational Mission, 566 ; Miss Dr. Howard's Success in, 567 ; Lead Ore discovered in, 588 ; Telegraph Extension in, 588 ; Waste Paper in, 588 ; Coroners' Science in, Robert K. Douglas, 612
- Chlorophyll Corpuscles of Hydra, Prof. E. Ray Lankester, F.R.S., 87
- Christie (W. H. M., F.R.S.), the Magnetic Storm and Aurora, 82
- Cinchona Culture, Handbook of, Karel Wessel van Gorkom, 287
- Circle, Medioscribed, 607
- Circumpolar Expedition : Notes from Capt. Dawson's Letters, 103, 242, 484 ; Dr. Rae, F.R.S., on, 508
- Civil Service Examinations, Natural Science in, 321
- Clapham (F. R.), Aurora of November 17, 141
- Clark (J. E.), the Magnetic Storm and Aurora, 84
- Clarke (C. B.), Equal Temperament of the Scale, 240
- Clark (J. W.) : Condensation of Liquid Films on Wetted Solids, 370 ; Preliminary Note on the Bacillus of Tuberculosis (Koch), 492
- Clerk-Maxwell on Stress, 314
- Clouds, Magnetic Arrangement of : C. H. Romanes, 31 ; Rev. W. Clement Ley, 53
- Coal Fire, Flames in, 183
- Coal Gas, Lime as a Purifier of Products of Combustion of, Dr. Joule, F.R.S., 496
- Coan (Rev. Titus), Death of, 398
- Cobbold (Dr. T. Spencer, F.R.S.) : Ticks, 552 ; *Simondsia paradoxa*, 547
- Cold, Unprecedented, in the Riviera—Absence of Sunspots, C. J. B. Williams, 551
- Cole (J. F.), Aurora of November 17, 141
- Colour, on the Sense of, amongst some of the Lower Animals, Sir John Lubbock, Bart., 618
- Colours, Complementary, 78 ; J. J. Murphy, 8 ; C. R. Cross, 150 ; at Niagara Falls, H. G. Madan, 174 ; "Natural" Experiment in, E. J. Bles, 241 ; Chas. T. Whitmell, 266 ; Novel Experiment in, John Gorham, 294
- Colquhoun (A. R.) : Journey through Yunnan to Burmah, 92 ; Journey to Bhamo, 63
- Coltsfoot, Early, R. McLacblan, F.R.S., 266
- Comets : Schmidt's Cometary Object, 20 ; the Great Comet 1882 *b*, 4, 21, 43, 56, 80, 161, 210, 300, 349, 400, 446, 540, 589 ; Major J. Herschel, Geo. M. Seabroke, Arthur Watts, 4 ; Spectroscopic Observations of, 24, 42 ; C. J. B. Williams, 29 ; W. J. Millar, 29 ; W. Higginson and B. Manning, 29 ; seen at the Paris Observatory, 42 ; J. P. McEwen, 52 ; T. W. Backhouse, 52, 338 ; G. M. Seabroke, 52 ; H. Cecil, 52 ; B. J. Hopkins, 62, 78 ; at Buenos Ayres, 62 ; J. K. Rees, Prof. Chandler, 81 ; "Anomalous" Tail of, Rev. T. W. Webb, 89 ; Major J. Herschel, 101 ; Commander Sampson, U.S.N., Dr. C. J. B. Williams, F.R.S., 108 ; Division of the Nucleus of the, 113 ; W. C. Winlock, 128 ; Dr. Doberck, 128 ; A. Ainslie Common, 150 ; F. Stapleton, 150 ; Photograph of, 180 ; during last November and December, C. J. B. Williams, F.R.S., 198 ; Elements of, Vice-Admiral Rowan, Prof. E. Frisby, 226 ; J. P. McEwen on, 247 ; W. T. Sampson, 266 ; Dr. B. A. Gould, 267 ; E. Ristori, 314 ; Orbit of, E. Ristori, 388 ; Ephemeris of, Prof. Frisby, 415 ; Prof. Schiaparelli on the, Francis Porro, 533 ; Comet 1882 *c*, 161, 248 ; Comets as Portents in China, 229 ; Figure of the Nucleus of Comet of 1882 (Gould), Prof. Holden, 246 ; D'Arrest's Comet, 324, 567, 589, 618 ; Reported Discovery of a Comet, 324 ; Denning's Comet, 348 ; Comet of 1771, 374 ; Comet 1883 *a*, 445, 517 ; Comet of 1812, 498
- Common (A. Ainslie), the Comet, 150
- Complementary Colours, 78 ; J. J. Murphy, 8 ; C. R. Cross, 150 ; at Niagara Falls, H. G. Madan, 174 ; a "Natural" Experiment in, E. J. Bles, 241 ; Chas. T. Whitmell, 266 ; Novel Experiment in, John Gorham, 294
- Condensation of Liquid Films on Wetted Solids, J. W. Clark, 370
- Congo (Upper), Departure of Belgian Expedition for, 182
- Coniferae, Female Flowers in, 231
- Coné (Dr. Le), on Darwinism, 275
- Cooke (Conrad), &c., "Electric Illumination," 264
- "Cookery, the Rudiments of," A. C. M., 323
- Cooley (W. Desborough), Death of, 469
- Copernicus, Darwin and, Prof. E. du Bois Reymond, 557
- Coral-eating Habits of Holothurians, Surgeon-Major H. B. Guppy, 7 ; Saville Kent, 433 ; J. G. Grenfell, 508
- Cordiner (R.), Age of Dogs, 79
- Corona, Photographing the, Dr. W. Huggins, F.R.S., 199
- Coroners' Science in China, Robert K. Douglas, 612
- Corpuscles, Red Blood, Prof. Quincke on, 355
- Cosmical Dust collected by M. Marx, Prof. Lenz, 422
- Cothenius Medal, 540
- Cowan (Mr. D.), Explorations in Madagascar, 372
- Crombie (J. M.), Function of Membrana Flaccida of Tympanic Membrane, 129
- Cross (C. R.), Complementary Colours, 150
- Crystal Palace Electricity and Gas Exhibition, 180
- Cryptogamic Flora of Germany, Austria, and Switzerland, Mary P. Merrifield, 385
- Cryptophyceæ, a New Genus of, 230
- Cumming (Miss Gordon), "Fire Fountains," 525
- Cunningham (David), Hovering of Birds, 336

- Cunningham (Major Allan), Roorkee Hydraulic Experiments, 1 ; the Indian Survey, 97
 Cunningham (J. T.), Zoological Station in Naples, 453
 Currie (James), Flamingoes and Cariamias, 389
 Cutting Tools worked by Hand and Machine, Robert H. Smith, 577
 Cyprus, Earthquake in, 497
- Dachstein Glaciers, Decrease of, 42
 Dairy Industry in France, 90
 D'Arrest's Comet, 324, 567, 589, 618
 Darwin (Charles), on Tower-like Dejections made by Worms, 20 ; Enthusiasm in Sweden on the Memorial to, 275 ; Darwin and Copernicus, Prof. E. du Bois Reymond, 557 ; Hostile Criticism in Germany of the Address on, 565
 Darwin (Prof. G. H., F.R.S.), Numerical Estimate of the Rigidity of the Earth, 22 ; Elected Plumian Professor of Astronomy and Experimental Philosophy at Cambridge, 275 ; Formation of Mudballs, 507
 Darwinism, Dr. Le Conte on, 275
 Dary (Georges), on Electric Navigation, 41, 42
 Davis Lectures for 1883, 565
 Dawkins (Prof. W. Boyd), Channel Tunnel, 338
 Dawson (Capt.), Notes on Circumpolar Expedition from Letters of, 103, 242, 484
 De La Rue's Diaries, &c., 161
 De Morgan (Augustus), Memoir of, R. Tucker, 217
 Death, Chinese Definition of, 160
 Deaths from Snake Bite in Bombay, Sir Joseph Fayrer, F.R.S., 556
 Dechevrens (Father Marc), a Curious Halo, 30
 Deductive Biology, W. T. Thiselton Dyer, F.R.S., 554
 Deep-Sea Exploration, French, 587
 "Deneholes" at Blackheath, 324
 Denmark : Oyster Fisheries of, 346 ; Glacial Phenomena in, Prof. Johnstrup, 373
 Denning (W. F.) : Transit of Venus, 158 ; Markings on Jupiter, 365 ; Amateurs and Astronomical Observation, 434 ; the Large Meteor of March 2, 1883, 461
 Denning's Comet, 348
 Deprez's Electrical Theories, 399
 Derby Free Library, Reference Catalogue, 62
 Desborde (Col.), on Banks of Niger, 424
 Dewar (Prof.) and Prof. Liveing, Origin of Hydrocarbon Flame Spectrum, 257
 Diamond in its Matrix, a, 90
 Dickert (Herr Thomas), Death of, 399
 Dickson (Dr. Oscar) : his liberality in aiding Scientific Research, 444 ; Account of Swedish Expedition to Greenland, 541
 Dier (Prof.), Shadows after Sunset, 150
Djimplna, Search Expedition for the, 64
 Diluvial Mammals, Discovery of Remains of, on Wolga, 373
 Dip-Circle, Gooldeen's Simple, 127
 Direct-Vision Prisms, 182
 Distant (W. L.) : an Urgent Need in Anthropology, 101 ; "Rhopalocera Malayana," 444
 Diurnal Variation in the Velocity of the Wind, E. Douglas Archibald, 461
Djighit (the), Domojiroff's Anemometric Observations on, 445
 Doberck (Dr. W.) : the Comet 1882 *b*, 129 ; Transit of Venus, 158 ; appointed to the Hong Kong Observatory, 565
 Dobson (J. L.), the Magnetic Storm and Aurora, 87
 Doe with Horns, 209
 Dogs, Age of, R. Cordiner, 79
 Dolmen, Discovery of, at St. Pierre Quiberon, 540
 Domojiroff's Anemometric Observations on board the *Djighit*, 445
 Doradús, Supposed Variable μ , a Spurious Star, 498
 Double Stars, Measures of, 182
 Douglas (Robert K.) : China, 221 ; Coroners' Science in China, 612
 Draper (Prof. Henry) : Death of, 88 ; Obituary Notice of, 108
 Dredging Implement, a New : Prof. A. Milnes Marshall, 11 ; W. A. Herdman, 54
 Dreyer (J. L. E.), Transit of Venus, 158
 Dublin : Royal Society of, 235 ; Experimental Science Association, 571 ; Aurora at, 589
 Dumas (J. B. A.) : Presentation to, 132 ; his Address at the Commemoration of the Fiftieth Anniversary of his Membership of the Paris Academy, 174 ; the Dumas Medal, 227 ; Illness of, 346
 Duncan (Prof. P. Martin, F.R.S.), Heroes of Science, 76
 Dust Showers in Norway, 496
- "Dutton (Capt. C. E.), a Monograph by," Tertiary History of the Grand Cañon District, Dr. Arch. Geikie, F.R.S., 357
 Dyer (W. T. Thiselton, F.R.S.) : Influence of "Environment" on Plants, 82 ; the Sacred Tree of Kum-Bum, 224 ; Deductive Biology, 554
 Dynamics, Introductory Treatise on Rigid, W. Steadman Aldis, 265
 Dynamo, Thomson's Mouse-Mill, J. T. Bottomley, 78
 Dynamo Machine, Elphinstone-Vincent, 516
 Dynamo-electric Machine with Continuous Induction, Prof. Pfaunder the Inventor of, 517
 Dynamo-electric Machines, Recent, 58
- Earth, Numerical Estimate of the Rigidity of the, Prof. G. H. Darwin, F.R.S., 22
 Earth Currents during Magnetical Perturbations, 89
 Earth's Crust, Physics of the, Rev. O. Fisher, 76
 Earth's Surface, Causes of Elevation and Depression of, W. F. Stanley, 523
 Earthquakes : in the Northern part of the Balkan Peninsula, 19 ; at Cascia, 63 ; in Perugia, 90 ; in the Valais, 181 ; in Greece and Panama, 248 ; at Murcia, in Spain, at Archæna, &c., 277 ; at Halifax, Prof. Geo. Lawson, 293 ; Clifton, Geo. F. Burder, 293 ; Hastings, R. H. Tiddeman, 293 ; at Cassine, 299 ; at Agram, 348 ; at Freiburg-im-Breisgau, 374 ; in Silesia and Bohemia, 400 ; in Hungary, 423 ; at Bologna, 445 ; in Japan, Proposed Study of, 469 ; in Cyprus, 497 ; at Amsterdam, 517 ; in Hungary and Italy, 540 ; at Pedara in Sicily, 567
 Earthslip in Switzerland, 209
 Earthworms in New Zealand, 91
 Eastlake (F. W.), Geology of Hong Kong, 177
 Eclipse, Total Solar, of May 6, 1883, 111, 248 ; the English Observers, 346, 398, 556, 567
 Eclipse, Solar, Total, of May 17, 1901, 424
 Eclipses, Solar, the Recent Coming and Total, J. Norman Lockyer, F.R.S., 185
 Edinburgh: Royal Society of, 168, 283, 307, 403, 428, 452, 548, 571, 595 ; Mathematical Society of, 346, 500, 595
 Education : Foreign Education in China, 90 ; of our Industrial Classes, J. Norman Lockyer, F.R.S., 248 ; A. J. Mundella on, 276 ; Prof. Huxley on, 396 ; Minister of, 495 ; in Japan, 617
 Edwards (E. W. W.) and Higgins (C.), the Electric Lighting Act, 1882, 410
 Eggs, Effect of Railway Transport on, 444
 Egyptian Exploration Fund, 401
 Elastische Nachwirkung, M. Hesehus, 183
 Electricity : International Electrical Conference, 18 ; Tricycles propelled by, 19 ; Electric Illumination of a Railway Bridge over the Ticino, 20 ; Electric Lighting, Sir C. W. Siemens, F.R.S., 67 ; Electric Lighting at Society of Arts, 133 ; Electric Light in China, 209 ; "Electric Illumination," Conrad Cooke, &c., 264 ; Priority in Photographing with Electric Light, 276 ; Public Electric Lighting, 384 ; Electric Lighting Act, 1882, C. Higgins and E. W. W. Edwards, 410 ; Electric Light at Savoy Theatre, 418 ; Electric Lighting in Switzerland, 497 ; in Paris, 516 ; Some Points in Electric Lighting, Dr. John Hopkinson, F.R.S., 592 ; Electric Navigation, Georges Dary, 41, 42 ; M. Tresca's Papers on Electrical Measures, 62 ; Electrical Transmission of Force and Storage of Power, Sir C. W. Siemens, F.R.S., 67, 518 ; Electrical Disturbances in Sweden and Norway, 89 ; Electrical Exhibition at the Crystal Palace, 180 ; Electric Reaction, Prof. Melde, 183 ; Electrical Phenomena, 199 ; Electric Railways, Prof. W. E. Ayrton, 255 ; "Electricity," by Robert M. Ferguson, 264 ; Electric Exhibition at Vienna, 373, 444 ; "Electricity," a New Hungarian Journal, 299 ; Il Potentiale Elettrico nell' Insegnamento Elementare della Elettrostatica, Prof. A. Serpieri, 312 ; Electric Railways, 338 ; "Electrical Resistance of Carbon Contacts," Shelford Bidwell, 376 ; Electrical Theories, Deprez's, 399 ; Light-Phenomena of Electric Discharges, Dr. Hertz, 403 ; Influence of Vacuum on Electricity, A. M. Worthington, 434 ; Electro-technical Society formed at Vienna, 469 ; Successful Trial of an Electrically-moved Tram-car, 470 ; Electricity as a Motive Power for Balloons, Napoli, 517 ; Proposed Electric Railway from Charing Cross to Waterloo, 566
 Electrolytic Balance of Chemical Corrosion, Dr. Gore, F.R.S., 326
 Electromotive Force and Resistance of Batteries, Effects of Temperature on, W. H. Reece, F.R.S., 426

- Electromotive Force of Galvanic Combinations, Kittler's Normal Element of, 325
- Electronomie Industrie, Manuel d', R. V. Picou, 146
- Electroscope, a Modification of the Gold Leaf, F. J. Smith, 102
- Elephant in Italy, Discovery of Fossil, 181; Threatened Extinction of the, E. E. Prince, 509
- Elger (P. G.), the Magnetic Storm and Aurora, 85
- Elphinstone-Vincent Dynamo Machine, 516
- Elwes (H. J.), Marshall and Nicéville's Butterflies of India, Burmah, and Ceylon, 50
- Emu, Nesting Habits of, A. W. Bennett, 530
- Encyclopædic Dictionary, 423
- Enemies, Natural, of Butterflies, Henry Higgins, 338
- Energy in the Spectrum, Distribution of, Lord Rayleigh, F.R.S., 559
- Energy, Transmission of, on Marcel-Deprez System, 372
- Engineering Laboratory, University College, 160
- "Ensilage in America," Prof. James E. T. Rogers, Prof. Wrightson, 479
- Entomological Society, 451, 500
- "Environment" on Plants, Influence on, W. T. Thiselton Dyer, F.R.S., 82; Howard Fox, 315
- Eocene Section, the Lower, J. Starkie Gardner, 331
- "Episodes in the Life of an Indian Chaplain," 99
- Epping Forest and the Railway Companies, 399; Prof. G. S. Boulger, 455; Conservation of, from the Naturalist's Stand-Point, 447
- Equal Temperament of the Scale, C. B. Clarke, 240
- Ergot, Poisonous Principle of, 517
- Eridani, Binary Star ρ , 589
- Ernst (Dr. A.), Abnormal Fruit of *Opuntia Ficus-Indica*, 77
- Essex County, Mass., Flora of, 173
- Essex Field Club, 298, 347
- Ether, the, and its Functions: Prof. Oliver Lodge, 304, 328; S. Tolver Preston, 579
- Ethnographical Exhibition at Stockholm, Dr. A. B. Meyer, 370
- Ethnology, Aino: Dr. J. J. Rein, 365; Prof. A. H. Keane, 389
- Ethnology of North Africa, Prof. A. H. Keane, 408
- Etna (Mount), Eruption of, 422, 496, 515, 539
- Evans (C.), Palæolithic River Gravels, 8
- Evolution of the American Trotting Horse, W. H. Brewer, 609
- Evolution, Hypothesis of Accelerated Development, by Primogeniture and its Place in the Theory of, Prof. A. A. W. Hubrecht, 279, 301
- Explosive Waves, Berthelot and Vielle on, 301
- Eye, Internal Reflections in the, H. Frank Newall, 376
- "Fall Machine," Poggendorff's, 300
- Falling, Asphyxiation in Rapid, 62
- Farr (Dr. W., C.B.), Death of, 587
- Fatio (Dr. Victor), Fishes of Switzerland, 220
- Fauna, Pelagic, of Freshwater Lakes, 92
- "Faune des Vertébrés de la Suisse," Dr. Victor Fatio, 220
- Fayrer (Sir Joseph, F.R.S.), Destruction of Life in India by Poisonous Snakes, 205, 556; Destruction of Life in India by Wild Animals, 268; Speke and Grant's Zebra, 604
- Feed-Water Heater and Purifier, Geo. Strong, 90
- Ferghana Naphtha Well, 445
- Ferguson (Robert M.), "Electricity," 264
- Fermat, the Statue of, 180
- Ferns of Kentucky, John Williamson, 336
- Fertilisation of the Speedwell, A. Ransom, 149
- "Feuille des Jeunes Naturalistes," 19
- Fibre-balls, 580
- Fig and the Caprifig, on the Relations of the, W. Botting Hemsley, 584
- Fingal's Cave, Is, Artificial? F. Cope Whitehouse, 285
- Finland, New Lyceums in, 42; Auroral Experiments in, S. Lemström, 389
- Finsbury Technical College, 318; Opening of, 425
- Finsch (Dr. Otto), his Exploration of New Guinea, 43; his Report of his Travels, 181
- "Fire-Fountains," Miss Gordon Cumming, 525
- Fires in Russia, Forest, 113
- Fish, New Deep-Sea, from Mediterranean, Dr. Giglioli, 198
- Fish Culture Association, National, 323
- Fisher (Rev. Osmond), Physics of the Earth's Crust, 76
- Fisheries Exhibition, the International, 322, 389, 468, 496, 516, 536
- Fisheries, Herring and Salmon, 442
- Fishes of Switzerland, Victor Fatio, 220
- Fishes, the Skeleton of Marsipobranch, W. K. Parker, F.R.S., 330
- Fissures, on the Movements of Air in, and the Barometer, A. Strahan, 375, 461
- Fitch (Oswald), Benevolence in Animals, 580
- Flames in Coal Fire, 103
- Flamingoes and Cariamas, James Currie, 389
- Flegel (Robert), News of, 230
- Fleische (E. von), Wartmann's Rheolyzer, 127
- Flora of Essex County, Massachusetts, John Robinson, 173
- Flora, Siberian, 445
- Flora, Vernal, Origin of our, J. E. Taylor, 7
- Flower (Prof. W. H.), Lectures on Anatomy of the Horse, 372
- Flower, Two Kinds of Stamens with Different Functions in the same, Dr. Hermann Müller, 30; Dr. Fritz Müller, 364
- Flowers, Double, Dr. M. T. Masters, 126
- Flowers, Insects Visiting, 498
- Flückiger (F. A.), "Die Chinارين in Pharmakognostischer Hinsicht dargestellt," 287
- Foamballs, J. Rand Capron, 531
- Föhn, the, A. Irving, 605
- Fonville (W. de), "Les Passages de Venus," 132
- Food Adulteration Act of 1881, 301
- Foraminifera in British Museum, Catalogue of Fossil, Prof. T. Rupert Jones, 173
- Forbes (H. O.), Forbes' Visit to Timor-Laut, 159
- Forbes (W. A.), Zoological Expedition up the Niger, 14; Letters from Shonga, 276; Death of, 614
- Force, Transmission of, by Electricity, Dr. C. W. Siemens, F.R.S., 67
- Forecasts, Weather: Bishop of Carlisle, 4, 51; Rev. W. Clement Ley, 29; C. W. Harding, 79
- Forel (Prof.), Recent Rhone Glacier Studies, 183
- Forest Fires in Russia, 113
- Formic and Acetic Acid in Plants, 91
- Forms of Leaves, Sir John Lubbock, Bart., 605
- Forth Bridge, New, 62, 89; Sir Geo. B. Airy on the, C. S. Smith, 99, 131; Herbert Tomlinson, 147; B. Baker, 222
- Fossil Elephant in Italy, Discovery of, 181
- Fossil Soudlik in Northern Germany, Dr. Blasius, 247
- Fountain (J. M.), the Weather, 32
- Fox (Howard), Influence of "Environment" on Plants, 315
- France: Annual Meeting of Institute of, 18; Dairy Industry in, 90; Heavy Rains in, 229; French Mission to Cape Horn, 344; Annual Meeting of French Learned Societies, 539
- Frankland (Lr. E., F.R.S.): Smoke Abatement, 407; Chemistry of Storage Batteries, 568
- Freiburg-im-Breisgau, Earthquake at, 374
- Freshwater Lakes, Pelagic Fauna of, 92
- Friele (Hermann), "Der Norske nord-hass-expedition, 1876-1878," Dr. J. Gwyn Jeffreys, F.R.S., 457
- Frisby (Prof. E.): Elements of the Great Comet 1882 δ , 226; Ephemeris of it, 415; Transit of Venus, 266
- Fritsch (Prof.), On the Torpedo, 403
- Fulgurite found near Warmbrunn, 540
- Galloway (William), Hovering of Birds, 336
- Galton (Francis, F.R.S.), Hydrogen Whistles, 491
- Galvanometers, on the Graduation of, for the Measurement of Currents and Potentials in Absolute Measure, Andrew Gray, 32, 105, 319, 339; New Torsion, 620
- Gambetta's Body, Autopsy of, 247
- Gardner (H. Dent), Britten's Watch and Clockmaker's Handbook, 76
- Gardner (J. Starkie), the Lower Eocene Section between Reculvers and Herne Bay, 331
- Garman (S.): Scream of Young Burrowing Owl like Warning of Rattlesnake, 174; a Possible Cause of the Extinction of the Horses of the Post-Tertiary, 313
- Garrod (Dr. A. B., F.R.S.), on Uric and Hippuric Acid, 451
- Gases, Action of Gravity on, Kraevitsch, 324
- Geestemünde, Aurora at, 374
- Geikie (Dr. Arch., F.R.S.): a Search for "Atlantis" with the Microscope, 25; Recent Researches in Metamorphism of Rocks, 121; "Geological Sketches," G. K. Gilbert, 237, 261; "Text-Book of Geology," G. K. Gilbert, 237, 261; Tertiary History of the Grand Cañon District, 357
- Geikie (Prof. James, F.R.S.), Aims and Method of Geological Inquiry, 44, 64
- Geodetical Operations, Norwegian, 224, 341
- Geographentag, Third German, 447

- Geographical Notes, 21, 43, 63, 92, 161, 400, 424, 541, 589
 Geography in Schools, 209
 Geography of the Caucasus, 470
 Geology : Railway Geology—a Hint, 8 ; Aims and Method of Geological Inquiry, Prof. James Geikie, F.R.S., 44, 64 ; Geological Society, 71, 118, 215, 282, 331, 355, 426, 474, 500, 547 ; Distribution of Medals, 427 ; Geology, Part I, A. H. Green, 98 ; Geology of Hong Kong, F. W. Eastlake, 177 ; "Geological Sketches at Home and Abroad," Arch. Geikie, LL.D., F.R.S., G. K. Gilbert, 237, 261 ; "Text-Book of Geology," Arch. Geikie, LL.D., F.R.S., G. K. Gilbert, 237, 261 ; New Appointments for the Geological Survey of Scotland, 246 ; Geologists' Association, 523
 Geometrical Teaching, the Association for the Improvement of, 246, 299, 580
 German Aeronautical Society, Meeting of, 348
 German African Expedition, Drs. Wissmann and Pogge, 92
 German African Society, Report of, 92
 German Fishery Society, 276
 German North Polar Expedition, 43
 German Ornithological Society, Annual Meeting at Berlin, 19
 German Society for Prevention of Pollution of Rivers, &c., 89
 Gibney (Robert Dwaris), the Zodiacal Light (?), 605
 Giglioli (Dr.), New Deep-Sea Fish from Mediterranean, 198
 Gilbert (G. K.), Arch. Geikie's "Geological Sketches at Home and Abroad," and "Text-Book of Geology," 237, 261
 Gill (H. C.), the Magnetic Storm and Aurora, 85
 Glacial Formations of Russia, 497
 Glacial Phenomena in Denmark, Prof. Johnstrup, 373
 Glacier Motion, Solar Radiation and, Rev. A. Irving, 553
 Glacier Studies, Prof. Forel on Recent Rhone, 183
 Glaciers, Decrease of the Dachstein, 42
 Gladstone (Dr. J. H., F.R.S.), Chemistry of the Planté and Fauré Accumulators, 583
 Glaisher (J. W. L., F.R.S.), Mathematics in America, 193
 Glazebrook (R. T., F.R.S.): a Common Defect of Lenses, 198 ; "Physical Optics," 361
 Goddard (A. F.), Intra-Mercurial Planets, 148
 Gold-leaf Electroscope, a Modification of the, F. J. Smith, 102
 "Gold Coast, to the, for Gold," Richard F. Burton and Verney L. Cameron, 335
 Good (S. A.), Extraordinary Lunar Halo, 150
 Gooden's Simple Dip Circle, 127
 Gore (Dr., F.R.S.): Electrolytic Balance of Chemical Corrosion, 326 ; Chemical Corrosion of Cathodes, 374
 Gorham (John), Novel Experiment in Complementary Colours, 294
 Gorkom (Karel Wessel van), Handbook of Cinchona Culture, 287
 Göttingen, Royal Society of Sciences of, 48, 452
 Gould (Dr. B. A.), Comet 1882 *b*, 267
 Gout and Rheumatic Patients, an Algerian Winter Resort for, 113
 Govi (Prof.), Expansion of Bulbs of Liquid Thermometers, 239
 Granite, Metamorphic Origin of, Duke of Argyll, 578
 Gratings, Concave, Prof. Rowland's, 95
 Gravels, Palæolithic River: C. Evans, 8 ; Wm. White, 53 ; T. K. Callard, 54 ; Worthington G. Smith, 102
 Gray (Andrew), on the Graduation of Galvanometers for the Measurement of Currents and Potentials in Absolute Measure, 32, 105, 319, 339
 Gray (Prof. Asa), Natural Selection and Natural Theology, 291, 527
 Gray and Milne's Seismographic Apparatus, 547
 Greece, Earthquake in, 248
 Greek Archipelago, Volcanic Phenomena in, 445
 Green (A. H.), Geology, Part I., 98
 Green (John Richard), Obituary Notice of, 462
 Greenland, New Danish Expedition to, 446 ; Nordenskjöld's Expedition, 424, 496 ; Dr. O. Dickson on, 541
 Grenfell (J. G.), Geological Traces of Great Tides, 222 ; Intelligence in Animals, 292 ; Holothurians, 508
 Gre-ham Funds, the, 292
 Grey, (F. W.), Snow Rollers, 507
 Griffith (J. W.) and A. Henfrey, Micrographic Dictionary, 603
 Griffith (R. W. S.), a Meteor, 434
 Grocers' Company, the Scheme of the, for the Encouragement of Original Research in Sanitary Science, 574
 Groneman (Dr. H. J. H.): Remarks on and Observations of the Meteoric Auroral Phenomenon of November 17, 1882, 296 ; the Auroral Meteoric Phenomena of November 27, 1882, 388
 Ground, Observations of Periodic Movements of the, 300
 Gudvangsören, Landslip at, 423
 Guns, Wire, James A. Longridge, 11, 35, 53
 Guppy (Surgeon-Major H. B.): Coral-eating Habits of Holothurians, 7 ; Habits of Scypho-Medusæ, 31 ; Anthropological Notes in the Solomon Islands, 607
 Gwynne (Bertram), Curious Case of Ignition, 580
 Gyrostatics, Sir William Thomson, 548
 Hagen (Dr. H. A.), Invertebrate Casts, 173
 Håkonson-Hansen's (Herr), Observations of the November Auroral Displays, 347
 Halo, a Curious, Father Marc Dechevrens, 30 ; Rev. W. Clement Ley, 53 ; Rev. Gerard Hopkins, 53 ; a Lunar Halo, J. Rand Capron, 78 ; T. P. Barkas, 103 ; S. A. Good, 180
 Hammam R'irha, an Algerian Winter Resort for Gout and Rheumatic Patients, 113
 Hammond, Kinetic Theory of Chemical Actions, 183
 Hannay (J. B., F.R.S.), Natural Selection and Natural Theology, 362
 Harding (C. W.), "Weather Forecasts," 79
 Hare, Baird's, and its Habits, 241 ; T. Martyr, 266
 Harkness (Prof.), on the Transits of Venus, 114
 Harnett (W. L.), Meteor, 103
 Harrison (J. Park), Projection of the Nasal Bones in Man and the Ape, 266, 294
 Hart (Samuel): the Late Transit of Venus, 483 ; a Remarkable Phenomenon—Natural Snowballs, 483
 Harting (J. E.), Incubation of the Ostrich, 480
 Hatton (Frank), Death of, 515
 Hawk Moth Larva, Surgeon-Major Johnson, 126
 Heating by Acetate of Soda, 344
 Heen (De), Relations between Physical and Chemical Properties of Simple and Compound Bodies, 422
 Heilprin (Prof. Angelo), on the Value of the "Nearctic" as one of the Primary Zoological Regions, 606
Helix pomatia, L., Rev. L. Blomefield, 553
 Heloderm, the Sonoran, 153
 Hensley (W. Botting): Botany of the *Challenger* Expedition, 462 ; on the Relations of the Fig and the Caprifig, 584
 Henfrey (A.), J. W. Griffith and, Micrographic Dictionary, 603
 Henson (Samuel), on a Fine Specimen of Apatite from Tyrol, lately in the possession of, 608
 Herdman (W. A.), a Dredging Implement, 54
 Heresies, Scientific, in China, 342
 Heroes of Science, Prof. P. Martin Duncan, F.R.S., 76
 Herring and Salmon Fisheries, 442
 Herschel (Prof. A. S.), The Matter of Space, 458, 504
 Herschel (Major J.): the Comet 1882 *b*, 4, 101 ; Soda Flames in Coal Fires, 78 ; the Magnetic Storm and Aurora, 87 ; Ignition by Sunlight, 531
 Hertz (Dr.): Pressure of Saturating Vapour of Mercury, 183 ; Light-Phenomena of Electric Discharges, 403
 Hesehus (N.): *Elastische Nachwirkung*, 183 ; Physics in Russia during the last ten years, 567
 Hicks (Henry), Metamorphic Rocks of Ross and Inverness, 474
 Hickson (Sydney J.), "Zoological Record," 366
 Higgins (C.) and E. W. W. Edwards, "The Electric Lighting Act 1882," 410
 Higgins (Henry), Natural Enemies of Butterflies, 338
 Higginson (Walter), the Comet 1882 *b*, 29
 High Tides, Abnormal, River Thames, J. B. Redman, 6
 Hildebrandsson (Prof.), on the Ben Nevis Observatory, 18
 Hippopotamus, Death of the old Female, 247
 Histology, Vegetable, Tables of D. P. Penhallow, 458
 Hofmann (Prof.), Lecture Experiments, 301
 Holden (Prof.), Figure of Nucleus of Comet of 1882 (Gould), 246
 Holley (G. W.), "Niagara and other famous Cataracts," 146
 Holothurians, Coral-eating Habits of, Surgeon Major H. B. Guppy, 7 ; W. Saville Kent, 433 ; J. G. Grenfell, 508
 Holub's (Dr. Emil), New African Expedition, 542
 Home (D. M.), Ben Nevis Observatory, 411
 Hong Kong, Geology of, F. W. Eastlake, 177 ; Observatory Scheme, 565
 Hopkins (B. J.), the Comet 1882 *b*, 62, 78
 Hopkins (Rev. Gerard), a Curious Halo, 53
 Hopkinson (Dr. John, F.R.S.), Some Points in Electric Lighting, 592
 Hornblende and other Schists of the Lizard District, Prof. T. G. Bonney, F.R.S., 71
 Hornstein (Dr. Carl), Death of, 247

- Horse, Anatomy of the, Prof. Flower's Lecture on, 372
 Hosack (James), Waterspout on Land, 79
 Houghton (Rev. W.), "The Microscope," 173
 Hovering of Birds, 366; Dr. Hubert Airy, 294, 336, 388, 412;
 Rev. W. C. Ley, 412; Duke of Argyll, 312, 387; H. T.
 Wharton, 312; Henry Cecil, 388; David Cunningham, 336;
 Dr. J. Rae, F.R.S., 337, 434; William Galloway, 337;
 C. T. Middlemiss, 337; W. Larden, 337; the Soaring of
 Birds, Lord Rayleigh, F.R.S., 534; Dr. Hubert Airy, 590
 Hubrecht (Prof. A. A.), Hypothesis of Accelerated Develop-
 ment by Primogeniture and its Place in the Theory of Evolu-
 tion, 279, 301
 Huggins, (Dr. William, F.R.S.), Photographing the Corona,
 199
 Human Morphology, vol. i., H. A. Reeves, 124
 Hungary: Silk Culture in, 209; Earthquake in, 423, 540
 Hunterian Oration, the Biennial, 298
 Hutton (F. W.), "Catalogues of New Zealand Diptera, Orthop-
 tera, and Hymenoptera," 399
 Huxley (Prof. T. H., F.R.S.), on Education, 396
 Hydraulic Experiments, Major Allan Cunningham, 1
 Hydra, Chlorophyll Corpuscles of, Prof. E. Ray Lankester,
 F.R.S., 87
 Hydrocarbon Flame Spectrum, Origin of, Profs. Liveing and
 Dewar, 257
 Hydrocarbons, Pollution of the Atmosphere by, J. J. Murphy,
 241
 Hydrogen Whistles, Francis Galton, F.R.S., 491
 Hygienic Dress, &c., Exhibition, 443
 Hygrometer of Saussure, 616
 Hypophysis in *Petromyzon Planeri*, Genesis of, 91
 Ignition, a Curious Case of, 509; Bertram Gwynne, 580
 "Im Fernen Osten," A. H. Keane, 170
 Implements, Palæolithic, of North-East London, Worthington
 G. Smith, 270
 Incandescent Lamp, Changy, 209
 Incandescent Electric Light, the Inventor of, W. M. Williams,
 240
 Incubation of the Ostrich, J. E. Harting, 480; Geo. J. Romanes,
 480
 India: The Indian Survey, Major Allan Cunningham, 97;
 Episodes in the Life of an Indian Chaplain, 99; Destruction
 of Life by Poisonous Snakes in, Sir J. Fayrer, F.R.S., 205;
 Destruction of Life by Wild Animals in, Sir J. Fayrer, F.R.S.,
 268; Indian Archegosaurus, R. Lydekker, 411
 Indiarubber, Action of Light on, Prof. H. McLeod, F.R.S., 312
 Industrial Education, J. Norman Lockyer, F.R.S., 248
 Industrial Society of Berlin, Prizes offered by, 346
 Infra-Red of the Spectrum, Work in the, Capt. W. de W.
 Abney, F.R.S., 15
 Infusoria, a Manual of the, &c., W. Saville Kent, Prof. E. Ray
 Lankester, F.R.S., 601
 Ingersoll (Ernest), Oyster Industry of the United States, 39
 Ingleby (Miss C. R.), the Magnetic Storm and Aurora, 86
 Ingram (William), the Recent Cold Weather, 530
 Inhibition (of Structural Functions) and Action of Drugs thereon,
 Dr. T. L. Brunton, F.R.S., 419, 436, 467, 485
 Inland Sea Canal, Lessep's Surveying Expedition for, 516
 Insects, Common British, Rev. J. G. Wood, 124
 Insects Injurious to Field Crops, Diagrams of, Miss E. A.
 Ormerod, 146
 Insects Visiting Flowers, 498
 "Insekten nach ihren Schaden und Nutzen, Die," Prof.
 Taschenberg, 172
 Institution of Civil Engineers, 95, 143, 168, 355, 403, 428, 475,
 595
 Institution of Mechanical Engineers, 247, 351, 611
 Institution of Naval Architects, 469
 Intelligence in Animals: J. G. Grenfell, 292; J. Birmingham,
 337; Dr. J. Rae, F.R.S., 366; D. Pidgeon, 366
 Intermittent Spring in the Jachère, 63
 International Electrical Conference, 18
 International Fisheries Exhibition, 322, 389, 468, 496, 516, 536
 Invertebrate Casts *versus* Algae in Palæozoic Strata, 46; Dr. H.
 A. Hagen, 173
 Irish Antiquities, Forged, W. J. Knowles, 54
 Iron, Schirmwirkung of, Prof. Stephan, 325
 Iron and Steel Institute, Annual Meeting of, 587
 Irving (Rev. A.): Solar Radiation and Glacier Motion, 553;
 the Föhn, 605
 Iserlohn, Fall of Meteorite at, 423
 Isomerism of Albuminous Bodies, on the, Shigetaké Sagiura, 103
 Isomerism, Physical, 301
Isanemones, 20
 Italy: Exploration of the Mediterranean, Dr. J. Gwyn Jeffreys,
 F.R.S., 35; Biology in, 46; Earthquake in, 540
 Ivens (R.) and H. Capello, Central and West Africa, 391
 Izvestia, Caucasian, 517
 Jablockhoff's New Element for Electro positive Plate, 114
 Jachère, Intermittent Spring in the, 63
 Jackson (B. Daydon), Translation of Gorkom's "Cinchona
 Culture," 287
 Jamaica, Annual Report of Public Gardens and Plantations of,
 90
 Janssen (M.): Papers at the Academy of Science, 62; sent to
 Oran to observe the Transit of Venus, 89
 Japan: Report on the Mint for 1882, 323; Japanese Students,
 565; Education in, 617
 Jeffreys (Dr. J. Gwyn, F.R.S.): Italian Explorations of the
 Mediterranean, 35; "Der Norske nord-hass-expedition,"
 1876-78, Hermann Friele, 457
 Jevons Memorial, 113
 Jews, Anthropology of the, B. Blechmann, 113
 Johns Hopkins University Circulars, 180
 Johnson (Surgeon-Major), Hawk Moth Larva, 126
 Johnstrup (Prof.), Glacial Phenomena in Denmark, 373
 Jones (Prof. T. Rupert), Catalogue of Fossil Foraminifera in
 British Museum, 173
 Jones (W. M.), the Magnetic Storm and Aurora, 87
 Joule (Dr., F.R.S.), Lime as a Purifier of Products of Com-
 bustion of Coal Gas, 496
 Journal of Anatomy and Physiology, 93, 426
 Journal of the Asiatic Society of Bengal, 71, 473
 Journal of the Franklin Institute, 143, 281, 306, 473
 Journal of Physiology, 23
 Journal de Physique, 24, 143, 282, 353
 Journal of the Royal Microscopical Society, 47, 426
 Journal of Royal Society of New South Wales, 93
 Journal of the Russian Chemical and Physical Society, 143, 521
 Jupiter: Herr Kortazzi's Observations on, 209; Markings on,
 W. F. Denning, 365
 Käfer Westfalens, Die, F. Westhoff, 239
 Kayser (Dr.), Death of, 447
 Keane (Prof. A. H.): Kreitner's "Im Fernen Osten," 170;
 Krao, the "Human Monkey," 245; "Wanderings in Balu-
 chistan," Major-General Sir C. M. MacGregor, K.C.B., 359;
 "Travels and Adventures East of the Caspian during the
 years 1879-81," Edmond O'Donovan, 359; Aino Ethnology,
 389; Dr. G. Nachtigal's "Sahara und Sudan," 408
 Keller (Prof.), Animal Migrations due to Suez Canal, 181
 Kenin (Mt.), Expedition, Departure of Geographical Society's,
 161
 Kent (W. Saville), Supposed Coral-eating Habits of Holothu-
 rians, 433; "A Manual of the Infusoria," &c., Prof. E.
 Ray Lankester, F.R.S., 601
 Kentucky, Ferns of, John Williamson, 336
 Kew Gardens, Visitors to, 230; Report for 1881, 322
 Kharkoff, Science at, 213
 Kiel Observatory, the Centre for Astronomical Telegrams, 276
 Kinematics, Prof. G. M. Minchin, 239
 Kinetic Theory of Chemical Actions, Hammond, 183
 Kittler, "Normal Element" of Electromotive Force of Galvanic
 Combination, 325
 Knörlein (Josef), Death of, 422
 Knowles (W. J.), Forged Irish Antiquities, 54
 Kobell (Dr. F. Ritter von), Death of, 88
 Koenig (Dr.), on the Leukoscope, 95
 Kohrausch (Prof.), Electric Conductivity of Salts of Silver
 Haloid, 182
 Kortazzi (Herr), Observations of Jupiter, 209
 Kraevitch (M.): Electricity of Air, 183; Action of Gravity on
 Gases, 324
 Krao, the "Human Monkey," 579; A. H. Keane, 245
 Krause (Drs. Arthur and Aurel), Return of, 92
 Kreitner's "Im Fernen Osten," A. H. Keane, 170
 Kum-bum, the Sacred Tree of, W. T. Thiselton Dyer, F.R.S.
 224
 Lakes, Pelagic Fauna of Freshwater, 92

- Lake Dwellings, Ancient Scottish, by Dr. Munro, Sir John Lubbock, F.R.S., 145
 Lambeth Field Club, Report of, 90
 Lamp, Lever's Arc, 274
 Lamprey, the Skeleton of the, Prof. W. K. Parker, F.R.S., 330
 Lampridæ, the Tracheæ in, 231
 Landslip at Gudvangsören, 423
 Langdon (Prof. R.), Transit of Venus, 159; Transit of Venus, 1882, British Expeditions, 179
 Lankester (Prof. E. Ray, F.R.S.): Chlorophyll Corpuscles in Hydra, 87; "A Manual of the Infusoria," W. Saville Kent, 601
 Larden (W.), Hovering of Birds, 337
 Latent Heats, Specific Heats, and Relative Volumes of Volatile Bodies, on a Relation existing between, F. Trouton, 292
 Lavoisier, Priestley, and the Discovery of Oxygen, G. F. Rodwell, 8, 100; C. Tomlinson, F.R.S., 53, 147
 Layard (Consul E. L.), a Correction, 78; a Blue Meteor, 531; Lead Ore discovered in China, 588
 Leaves, the Shapes of, Grant Allen, 439, 464, 492, 511, 552; F. O. Bower, 552
 Leaves and their Environment, Grant Allen, 604
 Leaves, Forms of, Sir John Lubbock, Bart., 605
 Ledger (Rev. E.), the Sun, its Planets, and their Satellites, 309
 Lecky (James), Singing, Speaking, and Stammering, 580
 Lemström (Prof.), Experiments with Aurora Borealis, 322; Auroral Experiments, 389
 Lena, Meteorological Expedition to the Mouth of the, 43, 372, 423, 496
 Lenz (Prof. R.): Electric resistance of Mercury, 182; on Cosmical Dust collected by Marx, 422
 "Lepidoptera of Ceylon," L. Reeve and Co., 150
 Lepidoptera, Porritt's Yorkshire List of, 540
 Leprosy in Norway, 423
 Lessepé's Surveying Expedition for Inland Sea Canal, 516
 Leukoscope, Dr. Koenig on the, 95; Prof. Helmholtz's, 277
 Lever's Arc Lamp, 274
 Ley (Rev. W. Clement): "Weather Forecasts," 29; Magnetic Arrangement of Clouds, 53; A Curious Halo, 53; Hovering of Birds, 412
 Libraries, Reference Catalogue of Derby Free, 62
 "Light," Lewis Wright, 75
 Lighting, Electric, Sir C. W. Siemens, F.R.S., 67
 Lightning Conductors, the Efficacy of, 48
 Liquid Films, Condensation of, on Wetted Solids, J. W. Clark, 370
 Liquids, Arrangement for Measuring the Refractive Index of, Pittschikoff, 325
 Lime as a Purifier of Products of Combustion of Coal Gas, Dr. Joule, F.R.S., 496
 Lime water, Protection of Oxygen, &c., from Admixture of Acid Vapour by, Dr. Loewe, 373
 Linnean Society, 94, 118, 167, 259, 354, 379, 451, 523, 547, 619
 Linnean Society of New South Wales, Proceedings of, 93, 215
 Lion at Rest, 584
 Listing (Prof. Johann Benedict): Death of, 228; Obituary Notice of, 316
 Lithium Lines, on the Reversibility of, Profs. Living and Dewar, 499
 Liveing (Prof.) and Prof. Dewar on the Origin of the Hydrocarbon Flame Spectrum, 257; on the Reversibility of the Lithium Lines, 499; Absorption of Ultra-Violet Rays by Various Substances, 521
 Lockyer (J. Norman, F.R.S.), Recent and Coming Total Solar Eclipses, 185; Hermann W. Vogel on Lockyer's "Dissociation Theory," 233; the Education of our Industrial Classes, 248
 Lodes, Origin of Metalliferous, Prof. Sandberger, 90
 Lodge (Prof. Oliver), the Ether and its Functions, 304, 328
 Loewe (Dr.), Protection of Oxygen, &c., from Admixture of Acid Vapour by Lime-water, 373
 London Mathematical Society (*see* Mathematical Society)
 London, Palæolithic Implements of North-East, Worthington G. Smith, 270
 London School Board, 346
 Longridge (James A.), Wire Guns, 11, 35
 Lovett, Lieut.-Col. Beresford, Itinerary Map from Teheran to Astrabad, &c., 323
 Lubbock (Sir John, F.R.S.), The Senses of Bees, 46; Munro's "Ancient Scottish Lake Dwellings," 145; Forms of Leaves, 605; on the Sense of Colour amongst some of the Lower Animals, 618
 Lunar Halo, A, J. Rand Capron, 78; T. P. Barkas, 103; S. A. Good, 150
 Lupton (Sidney), Elementary Chemical Arithmetic, 76
 Lydekker (Richard): Siwalik Carnivora, 293; Indian Archæogosaurus, 411
 Macdonald (Rev. D.), "Africana," 526
 McEwen (J. P. M.), Comet 1882 *b*, 52
 MacGregor (General Sir C. M.), "Wanderings in Baluchistan," Prof. A. H. Keane, 359
 McIntosh (J. G.), Waterspouts on Land, 103
 McKendrick (Prof. J. G.), Lectures on Physiological Discovery, 496
 McLachlan (R., F.R.S.): Early Coltsfoot, 266; Obituary Notice of Prof. P. C. Zeller, 535
 McLeod (Prof. H., F.R.S.), the Aurora, 99; Action of Light on Indiarubber, 312
 Madan (H. G.), Complementary Colours at Niagara Falls, 174
 Madras, Agriculture in, 607
 Madrid, Snow in, 161, 229
 Magnetic Arrangement of Clouds, C. H. Romanes, 31
 Magnetic Storm and Aurora: W. H. M. Christie, F.R.S. Prof. C. P. Smythe, G. M. Whipple, J. R. Capron, R. H. Tiddeman, J. E. Clark, H. C. Gill, H. Robinson, A. M. Worthington, T. G. Elger, Miss C. R. Ingleby, C. H. Romanes, E. Brown, F. Stapleton, Rev. S. H. Saxby, Dr. H. Airy, Major J. Herschel, A.S.P., H. D. Taylor, T. P. Barkas, W. M. Jones, G. R. Vicars, Prof. O'Reilly, J. L. Dobson, 82 to 87
 Magnetical Perturbations, Earth Currents during, 89
 Magnetisation of Metals, M. Berson, 183
 Main (Dr. J. F.), Practical Mechanics, John Perry, 456
 Maklay (M. Mikluho), Russian Imperial Grant to, 92
 Malay Archipelago, Meteorology of the, 79
 Mammoth Skeleton found at Belgrade, 63
 Man, Primitive, Prof. Owen on, Grant Allen, 31
 Man and the Ape, Projection of Nasal Bones in, J. Park Harrison, 266, 294
 Manchester Literary and Philosophical Society, 234, 524
 Manilla, Hurricane at, 63
 Manning (B.), the Comet, 29
 Manures, Inquiry into Degree of Solubility requisite in, 325
 Marcel-Deprez System, Transmission of Energy on, 372
 Marcat (Prof. Francis), Death of, 587
 Marine Surveying, a Treatise on, Rev. J. L. Robinson, 289
 Mars, Rev. T. W. Webb, 203
 Marshall (A. Milnes), New Dredging Implement, 11
 Marshall (Major) and Nicéville (L. A.), Butterflies of India, Burmah, and Ceylon, 50
 Marsipobranch Fishes, the Skeleton, W. K. Parker, F.R.S., 330
 Martin (H. Newell) and W. A. Moule, Handbook of Vertebrate Dissections, 335
 Martini (Signor), Sound produced by Outflow of Water, 183
 Martyr (J.), Baird's Hare, 266
 Marx, on Cosmical Dust Collected by, 422
 Masheder (Thomas), Meteors, 483
 Masters (Dr. M. T.), Double Flowers, 126
 Mathematical Society, 71, 190, 282, 379, 499, 595; Annual Meeting, 19
 Mathematical Society of Edinburgh, 346, 500, 595
 Mathematics in America, J. W. L. Glaisher, F.R.S., 193
 Mathematics in Scandinavia, 343
 Matter of Space: Charles Morris, 349; Prof. A. S. Herschel, 458, 504
 Maxwell (J. Clerk), the Life of, with a Selection from his Correspondence and Occasional Writings, and a Sketch of his Contributions to Science, Lewis Campbell, LL.D., 26
 Mechanical Subjects, Papers on, Sir J. Whitworth, 208
 Mechanics, Practical, John Perry, Dr. J. F. Main, 456
 Mechanics, Teaching of Elementary, 580
 Medicine in Russia, Popular, Dr. Slunin, 62
 Medioscribed Circle, 607
 Mediterranean, Italian Exploration of, Dr. J. Gwyn Jeffreys, F.R.S., 35; New Deep-Sea Fish from, Dr. Glioli, 198
Megalania prisca, Prof. Owen, F.R.S., 118
 Melbourne, Observatory at, 497
 Melde (Prof.), Electric Reaction, 183
 Meldola (R.), Difficult Cases of Mimicry, 481
 Meliola, on the Genus, H. Marshall Ward, 234
 Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux, 362

- Memory, Diseases of, R. Ribot, Dr. G. J. Romanes, 169
 Mendeléeff (Prof.), "Principles of Chemistry," 113
 Mensuration, Useful Rules and Tables, W. J. M. Rankine, F.R.S., 431, 483
 Mercury, Electric Resistance of, R. Lenz, 182
 Mercury, Pressure of Saturating Vapour of, Herr Hertz, 183
 Merian (Prof. Peter), Death of, 372
 Meridian, Universal, 247, 516
 Merrifield (Mary P.), Rabenhorst's "Kryptogamen-Flora von Deutschland, Oesterreich, und der Schweiz," 385
 Metalliferous Lodes, Origin of, Prof. Sandberger, 90
 Metals, Magnetisation of, M. Berson, 183
 Metamorphic Rocks of Ross and Inverness, Henry Hicks, 474
 Metamorphic Origin of Granite—Prehistoric "Giants," Duke of Argyll, 578
 Metamorphism of Rocks, Recent Researches in, Dr. A. Geikie, F.R.S., 121
 Meteorite: Fall of, at Iserlohn, 423; near Brescia, 469; the Alfianello, 511
 Meteorology: the Observatory on Ben Nevis, 18, 39, 175, 399, 411, 487; Meteorology of the Malay Archipelago, 79; Meteorological Society, 95, 234, 307, 452, 570, 619; Proposed Exhibition of Meteorological Instruments, 373; Congress of Meteorologists, 539; Meteorological Observations at Mouth of Lena, Yurgens, 423; Meteorological Society of France, 497; Elementary Meteorology, R. H. Scott, F.R.S., 575
 Meteors: the November, 43; W. L. Harnett on a, 103; an Extraordinary, B. R. Branfill, 149; Meteoric Auroral Phenomenon of November 17, 1882, 173; Remarks on and Observations of the, Dr. H. J. H. Groneman, 296; Rev-Stephen H. Saxby, 338; H. Dennis Taylor, 365; T. W. Backhouse, 412; A. Batson, 412; H. D. Taylor, 434; Great Meteor in Sweden, 423; a Meteor, R. W. S. Griffith, 434; the Large Meteor of March 2, 1883, W. F. Denning, 461; Meteors, Thos. Masheder, 483; Henry Cecil, 483; Meteor, E. Brown, 508; in Sweden, 517; a Blue, Consul E. L. Layard, 531; at Carlsruhe, 540; Instructions for Observing, 615; Number of, observed at Prossnitz, 617
 "Mexico to-day," T. U. Brocklehurst, 503
 Meyer (Dr. A. B.), Stockholm Ethnographical Exhibition, 371
 "Micrographic Dictionary," J. W. Griffith and A. Henfrey, 160, 603
 Microphone, the, 588
 Microscope, a Search for "Atlantis" with the, Dr. Arch. Geikie, F.R.S., 25
 "Microscope, the," Rev. W. Houghton, 173
 Middlemiss (C. S.), Hovering of Birds, 337
 Midland Boulders, Rev. W. Tuckwell on the, 346
 Mikluho-Maclay (Baron) on New Guinea, 137, 184, 371
 Milan, Intemperance in, Prof. Verga on, 347
 Millar (W. J.): the Comet, 29; Rankine's "Rules and Tables," 483
 Milne (Mr.), Proposed Study of Earthquakes in Japan, 463
 Mimicry in Moths: Duke of Argyll, 125; Comm. D. Stewart, 314
 Mimicry, Difficult Cases of: Alf. R. Wallace, 481; R. Meldola, 481; Dr. P. H. Stokoe, 508; H. J. Morgan, 531
 Minchin (Prof. G. M.), Kinematics, 239
 Mineralogical Society, 24, 451
 Minor Planets, 148
 Mint, Japanese, Report for 1882, 323
 Mitchell Library, Glasgow, 615
 "Mittheilungen der deutschen Gesellschaft," of Yokohama, 41
 Mollusks, Edible, Acclimatisation of, Dr. J. G. Jeffreys, F.R.S., 511
 Montgolfer Anniversary, 90, 517
 Monuments, Ancient, Worthington G. Smith, 102
 Moons, Mock, F. T. Mott, 606
 Morgan (Augustus de), Memoir of, R. Tucker, 217
 Morgan (C. L.), Suicide of Scorpions, 313, 530
 Morgan (H. J.), Mimicry, 531
 "Morphologisches Jahrbuch," 94, 473
 Morphology Human, vol. i., H. A. Reeves, 124
 Morris, New Method of Producing Aluminium, 183
 Morris (Chas.), the Matter of Space, 349
 Morse, Memorial in Rome, 445
 Moseley (Prof. H. N., F.R.S.): Von Graff's Monograph on the Turbellarians, 227; Incubation of Ostrich, 507
 Moths, Mimicry in: Duke of Argyll, 125; Commander Duncan Stewart, 314
 Motion, Optical Illusions of, Dr. Bowditch, 183
 Motion, Laws of, Prof. P. G. Tait, 283
 Mott (F. T.): the Sea Serpent, 293; Mock Moons, 606
 Moule (W. A.) and H. Newell Martin, Handbook of Vertebrate Dissection, 335
 Mouse-Mill Dynamo (Thomson's), J. T. Bottomley, 78
 Mudballs, Formation of, Prof. G. H. Darwin, F.R.S., 507
 Muirhead (Dr. H.), Aurora of Nov. 17, 1882, 315
 Müller (Dr. Fritz): Animal Intelligence, 240; Two Kinds of Stamens with Different Functions in the same Flower, 364
 Müller (Dr. Hermann), Two Kinds of Stamens with Different Functions in the same Flower, 30
 Mundella (A. J., M.P.), on Education, 276
 Munro (Dr.), "Ancient Scottish Lake Dwellings," by Sir John Lubbock, M.P., F.R.S., 145
 Munro (J.), Swan Lamp Spectrum and Aurora, 173
 Murcia, Earthquake at, 277
 Murphy (J. J.): Complementary Colours, 8; Pollution of the Atmosphere, 241; Aurora, 434
 Museum, New Natural History, 54
 Museum, Warwick, 539
 Muybridge's Zoetrope Pictures of Animals in Motion, 42; New Work on Motion in Man and Animals, 539
 Myxinoidea, the Skeleton of, Prof. W. K. Parkes, F.R.S., 330
 Nachtigal (Dr. G.), "Sahara und Sudan," A. H. Keane, 408
 Naphtha Wells at Ferghana, 445
 Naples, Zoological Station in, J. T. Cunningham, 453
 Napoli on Electricity as a Motive Power for Balloons, 517
 Nasal Bones in Man and the Ape, Projection of, J. Park Harrison, 266, 294
 Natural History Museum, The New, 54; Removal of Collections, 538
 "Natural History, Another Book of Scraps principally relating to," Chas. Murray Adamson, 480
 "Natural" Experiment in Complementary Colours, Chas. T. Whitmell, 266
 Natural Selection and Natural Theology, Prof. Asa Gray, 291, 529; Dr. Geo. J. Romanes, F.R.S., 362, 528; J. B. Hannay, F.R.S., 364
 Natural Science in Civil Service Examinations, 321
 Navy, The British, Sir Thomas Brassey, 549
 "Nearctic" as one of the Primary Zoological Regions, on the Value of the, Alfred R. Wallace, 482; Prof. Angelo Heilprin, 606
 Nebulæ, Meridian Observation of, 324; New, 400, 446
 Nervous System, Influence of, on the Regulation of Temperature in Warm-blooded Animals, 469
 "Neurologie, Lehrbuch der," Dr. Schwalbe, 196
 Newall (H. Frank), Internal Reflections in the Eye, 376
 Newcastle-upon-Tyne Free Library, 615
 Newcomb (Prof.), "Popular Astronomy," 373
 New Guinea, Dr. Otto Finsch's Expedition to, 43; Baron Mikluho-Maclay on, 137, 184
 New South Wales, Linnean Society of, 215
 New Zealand: Earthworm in, 91; Pott's "Out in the Open," 172; Astronomy in, 276; Catalogues of New Zealand Diptera, Orthoptera, and Hymenoptera, F. W. Hutton, 399
 "Niagara and other Famous Cataracts," G. W. Holley, 146
 Niagara Falls, Complementary Colours at, H. G. Madan, 174
 Nicéville (L. de) and Major Marshall, Butterflies of India, Burmah, and Ceylon, 50
 Nicols (Arthur), Zoological Notes on the Structure, Affinities, Habits, and Mental Faculties of Wild and Domestic Animals, Dr. Geo. J. Romanes, F.R.S., 333
 Niger: W. A. Forbes's Zoological Expedition up the, 14; Col. Desbordes on Banks of, 424
 Nilson, Thorium, 184
 "Nomenclator Zoologicus," Sam. H. Scudder, 28
 Nordenskjöld (Baron): Discovery of North-East Passage, 422; his Proposed Greenland Expedition, 446, 496
 Nordland, "Naturen" on the Old Silver Mines of, 347
 "Normal Element" of Electromotive Force of Galvanic Combination, Kittler, 325
 Norske nord-hass-expedition, 1876-78, Herman Friele, Dr. J. Gwyn Jeffreys, F.R.S., 457
 North-East Passage, Discovery of, Baron Nordenskjöld, 422
 Norway: Leprosy in, 423; Dust Showers, 496; Norwegian Geodetical Operations, 224, 341; Norwegian Science Grants, 444
 Nouveaux Mémoires de la Société Helvétique des Sciences Naturelles, 282

- November Meteors, 43
 Numbers, Theory of, Prof. H. J. S. Smith on, 564
 Numerical Estimate of the Rigidity of the Earth, G. H. Darwin, F.R.S., 22
- Observatories: Meteorological Observatory on Ben Nevis, 18, 39, 175, 399, 411, 487; Brussels, 445; Melbourne, 497
 O'Donovan (Edmond), "Travels and Adventures East of the Caspian," Prof. A. H. Keane, 359
 Olsa, Remarkable Mirage seen at, 616
 Osmanney (Admiral), Aurora of Nov. 17, 139
 Optical Illusions of Motion, Dr. Bowditch, 183
 Optics, Physical, R. T. Glazebrook, F.R.S., 361
 Opuntia Ficus-Indica, Abnormal Fruit of, Dr. A. Ernst, 77; Dr. M. T. Masters, 126
 Ordnance Survey of Scotland, Completion of, 92
 O'Reilly (Prof.), the Magnetic Storm and Aurora, 87
 Orientalists, Congress of, 565
 Ormerod (Miss E. A.), "Diagrams of Insects injurious to Field Crops," 146
 Ornithologist in Siberia, 560
 "Ornitologia della Papuasia e delle Molucche," Tommaso Salvadori, 577
 Ostrich, Incubation of the: J. E. Harting, 480; George J. Romanes, 480; Prof. Moseley, F.R.S., 507
 Oswald (Felix L.), "Zoological Sketches," Dr. Geo. J. Romanes, F.R.S., 333
 "Out in the Open," T. H. Potts, 172
 Owen (Prof. R., F.R.S.), on Primitive Man, Grant Allen, 31; *Megalania Prisca*, 118; on the Affinities of Thylacoleo, 354
 Owl, Scream of Young Burrowing, S. Garman, 174
 Oxygen, Discovery of, Lavoisier, Priestley, and the, G. F. Rodwell, 8, 100
 Oyster Culture, 180
 Oyster Fisheries of Denmark, Decline of, 346
 Oyster Industry of the United States, 39
- Palæolithic Implements of North-East London, Worthington G. Smith, 270
 Palæolithic River Gravels: C. Evans, 8; William White, 53; T. K. Callard, 54; Worthington G. Smith, 102
 Palæontology, Carboniferous Vertebrate, Notice of some Discoveries recently made in, T. Stock, 22
 Palæozoic Strata, Invertebrate Casts *versus* Algae in, 46
 Palestine, Physical History of, Prof. Hull, F.R.S., 520
 Palmieri (Prof. Marino), Death of, 41
 Panama, Earthquake at, 248
 Papuan Ornithology, Tommaso Salvadori, 577
 Parallax, Stellar, 210
 Paris: Academy of Sciences, 24, 48, 72, 95, 119, 144, 168, 192, 229, 235, 260, 283, 308, 332, 356, 380, 404, 428, 475, 548, 572, 596, 620; Annual Meeting of, 538; Disposal of the Sewage of Paris, 133; Second Inundation at, 247; Comptes Rendus of the Geographical Society, 401; Bulletin of, 424; Electric Lighting in, 516
 Parker (Rev. Dr. G. W.), Umdhleb Tree of Zululand, 7
 Parker (W. K., F.R.S.), The Skeleton of Marsipobranch Fishes, 330
 Parkes Museum, First General Meeting, 19
 Parrakeet, The Uvaan, 417
 "Pathological Anatomy and Pathogenesis, a Text-Book of," Ernst Ziegler, 477
 Patent Bill, the New, 587
 Pavy (Dr. F. W., F.R.S.), Physiology of the Carbo-hydrates in the Animal System, 618
 Pedara in Sicily, Earthquake at, 567
 Pelagic Fauna of Freshwater Lakes, 92
Peripatus Cofensis, Prof. F. M. Balfour on, 215
Peronospora, New Vine Parasite, 133
 Perry (John), Practical Mechanics, Dr. J. F. Main, 456
 Persia, Colonel Lovett's Itinerary Map from Teheran to Astrabad, 323
 Perugia, Earthquake in, 90
 Petermann's "Mittheilungen," 92, 401, 541
 Petrie (W. M. F.), Aurora of Nov. 17th, 1882, 139, 315
 Petromyzon Planeri, Genesis of Hypophysis in, 91
 Pfandler (Prof.), The Inventor of Continuous-Induction Dynamo-Electric Machine, 517
 Philippine Islands, Typhoon in, 181
 Phillips (H. A.), Pollution of the Atmosphere, 127, 266
 Phosphorus, the Glowing of, 301
 Photographing with the Electric Light, Priority in, 276
 Photography, Astronomical, Edward C. Pickering, 556
 Photometer, Wedge and Diaphragm, 201
 Photometric Measurements of Sun, Moon, Cloudy Sky, and Electric and other Artificial Lights, Sir William Thomson, 277
 Phylloxera Destruction, Commission of the French Academy, 89
 Phylloxera in Chambery, 133
 Physical Notes, 324
 "Physical Optics," R. T. Glazebrook, F.R.S., 361
 Physical Society, 95, 143, 215, 354, 427, 452, 500
 Physical and Chemical Properties of Simple and Compound Bodies, Relation between, De Heen, 422
 "Physics of the Earth's Crust," Rev. O. Fisher, 76
 "Physics in Pictures," &c., 551
 Physics in Russia during the last Ten Years, N. Hesehus, 567
 Physiological Discovery, McKendrick Lectures on, 496
 Pickering (Edward C.), Astronomical Photography, 556
 Picou (R. W.), "Manuel d'Electronomie Industrielle," 146
 Pietet (Raoul), his "Rapid Vessel," 398
 Pidgeon (D.), Intelligence in Animals, 366
 File-dwellings, Bobenhausen, 160
 Piltshikoff, Arrangement for Measuring the Refractive Index of Liquids, 325
 Pitt-Rivers Collection, the, 346, 461.
 Planet, Inter-Mercurial, A. F. Goddard, 148
 Planets, Minor, 248; No. 228, 518
 Planté and Faure Accumulators, Chemistry of the, Dr. J. H. Gladstone, F.R.S., and Dr. A. Tribe, F.R.S., 583
 Plants, Influence of "Environment" on, W. T. Thiselton Dyer, F.R.S., 82; Howard Fox, 315
 Plants, Formic and Acetic Acid in, 91
 Plants, Cultivated, Origin of, A. de Candolle, 429; Vilmorin Andrieux, 429
 Pogge (Dr.), German African Expedition, 92
 Poggendorff's "Fall Machine," 300
 Polakoff's Explorations, 424
 Polar Exploration, Swedish and Dutch Expeditions, 299
 Pollock (E.), Aurora of Nov. 17, 141
 Pollution of Rivers, &c., German Society for Prevention of, 89
 Pollution of the Atmosphere, H. A. Phillips, 127
 Porritt's Yorkshire List of Lepidoptera, 540
 Porro (Francis), Professor Schiaparelli on the Great Comet 1882*b*, 533
 Port (Dr. Arnold Dodel-), "Atlas der Physiologischen Botanik," 617
 Post-Tertiary, a Possible Cause of the Extinction of the Bones of the, S. Garman, 313
 Potato Disease, A. S. Wilson, 523
 Potts (T. H.), "Out in the Open," 172
 Praxinoscope, Reynaud's New Projection, 60
 Preece (W. H., F.R.S.), the Progress of Telegraphy, 390; "Effects of Temperature on Electromotive Force and Resistance of Batteries," 426
 Prehistoric Animals, Remains of, discovered at Andernach, 445
 Prehistoric "Giants"—Metamorphic Origin of Granite, Duke of Argyll, 578
 Prejevalsky (Colonel), Exploration of Central Asia, 133
 Preston (S. Tolver), "Ether and its Functions," 579
 Priestley, Lavoisier, and the Discovery of Oxygen, G. F. Rodwell, 8, 100; C. Tomlinson, F.R.S., 53, 147
 Primitive Man, Professor Owen on, Grant Allen, 31
 Primogeniture, the Hypothesis of Accelerated Development by, and its Place in the Theory of Evolution, Prof. A. A. Hubrecht, 279, 301
 Prince (E. E.), Threatened Extinction of the Elephant, 509
 Princeton School of Science, Longitude of, 248
 Princeton Scientific Expedition (No. 3), 323
 Pringsheim (Dr. N.), "Jahrbücher für wissenschaftlich Botanik," Prof. W. R. McNab, 502
 Prisms, Direct-vision, 182
 Projection Praxinoscope, Reynaud's New, 60
 Prossnitz, Number of Meteors observed at, 617
 Putnam (Prof. F. W.), on American Antiquities, 277
- Quain's Elements of Anatomy, 196
 Quarterly Journal of Microscopical Science, 47, 353
 Quiberon, Discovery of Dolmen at, 540
- Rabenhorst's "Kryptogamen-Flora von Deutschland, Oesterreich, und der Schweiz," Mary P. Merrifield, 385
 Rabies, New Facts concerning, 192

- Radiation, Terrestrial, Prof. Tyndall's Observations, Dr. A. Woelfel, 460
- Radiometer, Prof. Ravelli on Educational Uses of, 444
- Rae (Dr. J., F.R.S.): Hovering of Birds, 336; Intelligence in Animals, 366; the Sea Serpent, 366; British Circumpolar Expedition, 508
- Railway Geology—a Hint, 8
- Railways, Electric, 338; Prof. W. E. Ayrton, 255
- Rainfall, British, G. J. Symons, F.R.S., 149
- Rains, Heavy, in France and Algeria, 229
- Rankine (W. J. M.), Useful Rules and Tables Relating to Mensuration, 431; W. J. Miller, 483
- Ransome (Arthur), Fertilisation of the Speedwell, 149, 223
- "Rapid Vessel," Pictet's, 398
- Rattlesnake, Scream of Young Burrowing Owl like Warning of, S. Garman, 174
- Ratzel (Dr. F.), *Anthropo-Geographie*, 125
- Ravelli (Prof.), Educational Uses of Radiometer, 444
- Rayleigh (Lord, F.R.S.): the Soaring of Birds, 534; Distribution of Energy in the Spectrum, 559
- Rays, Ultra-Violet, Absorption of, by various Substances, Professors Liveing and Dewar, 521
- Reaction, Electric, Prof. Melde, 183
- Reale Istituto Lombardo di Scienze e Lettere, 282, 354, 473, 521
- Redman (J. B.), River Thames Abnormal High Tides, 6
- Reeve and Co. (L.), "*Lepidoptera of Ceylon*," 150
- Reeves (H. A.), *Human Morphology*, vol. i., 124
- "Réforme" Telegraphic Communication with London, 469
- Refractive Index of Liquids, Arrangement for Measuring, Piltchikoff, 325
- Regel (Dr. L. E.), Central Asian Exploration, 446
- Rein (Dr. J. J.), Aino Ethnology, 365
- Reinhardt (Prof. J. R.), Death of, 41, 61
- Rendiconto dell' Accademia delle Scienze di Bologna, 189
- Riviera, Unprecedented Cold in the—Absence of Sunspots, C. J. B. Williams, 551
- Revue Internationale des Sciences, 426
- Revue Internationale des Sciences Biologiques, 189
- Reymond (Prof. E. du Bois), Darwin and Copernicus, 557
- Reynaud's New Projection Praxinoscope, 60
- Rheolyzer, Wartmann's, E. von Fleische, 127
- Rhone Glacier Studies, Prof. Forel on Recent, 183
- Rhytina Stelleri*, Crania of, 347
- Ribot's Diseases of Memory, Dr. G. J. Romanes, F.R.S., 169
- Rigidity of the Earth, Numerical Estimate of the, G. H. Darwin, F.R.S., 22
- Riley (Dr. C. S.), Hibernations of *Aletia xyliina* in United States, 214
- Rip van Winkle, a Modern, Saltburne, 127
- Ristori (E.), Orbit of Comet 1882 *b*, 388
- River Gravels, Palæolithic, C. Evans, 8
- Rivista Scientifico-Industriale e Giornale del Naturalista, 94, 282, 521
- Robert's Tide Tables, 230
- Roberts (Prof. W. Chandler, F.R.S.), Hardening and Tempering Steel, 594
- Robinson (H.), the Magnetic Storm and Aurora, 85
- Robinson (Dr.), Memorial of, 112
- Robinson (John), Flora of Essex County, Massachusetts, 173
- Robinson (Rev. J. L.), a Treatise on Marine Surveying, 289
- Robinson (W. H.), the Zodiacal Light (?), 605
- Rocks, Recent Researches in Metamorphism of, Dr. A. Geikie, F.R.S., 121
- "Rockies, Camps in the," W. A. Baillie-Grohman, 551
- Rodwell (G. F.): Lavoisier, Priestley, and the Discovery of Oxygen, 8, 100; Notes of Travel in Sardinia, 342
- Rogers (Prof. James E. T.), Ensilage in America, Prof. J. Wrightson, 479
- Romanes (C. H.): Magnetic Arrangement of Clouds, 31; the Magnetic Storm and Aurora, 86
- Romanes (Dr. G. J., F.R.S.), Ribot's Diseases of Memory, 169; on Natural Selection and Natural Theology, Prof. Asa Gray, 291, 362, 528; "Zoological Sketches," Felix L. Oswald, 333; "Zoological Notes on the Structure, Affinities, Habits, and Mental Faculties of Wild and Domestic Animals," Arthur Nicols, 333; "The Vampire Bat," 412; Incubation of the Ostrich, 480; Benevolence in Animals, 607
- Roorkee Hydraulic Experiments, Major Allan Cunningham, 1
- Rowan (Vice-Admiral), Elements of the Great Comet 1882 *b*, 226
- Rowan (D. J.), the Zodiacal Light (?), 605
- Rowell (G. A.), Experiments on Aurora, 443
- Rowland (Prof.), Concave Gratings for giving a Diffraction Spectrum, 95
- Royal Asiatic Society (North China Branch), Journal of, 161
- Royal Commissioners for Technical Education, visit to Birmingham, 469
- Royal Geographical Society, 323
- Royal Horticultural Society, 119, 523, 570
- Royal Institution, Lecture Arrangements, 112, 495
- Royal Society, 118, 143, 189, 215, 234, 257, 330, 354, 376, 426, 450, 473, 499, 521, 568, 618; Names Nominated for the Council, 40; Award of Medals, 61; Address of the President, 134, 162; New Fellows, 615
- "Rules and Tables," Rankine's, 431; W. J. Millar, 483
- Russell, H. C., the Comet 1882 *b*, 56
- Russia, Popular Medicine in, Dr. Slunin, 62; Forest Fires in, 113; Russian Chemical and Physical Society, 444; Russian Geographical Society, 424; Glacial Formations of, 497; Physics in, during the last ten years, N. Heschus, 567
- Russow (E.), on Sieve-Tubes, 366
- Rye, E. C., "The Zoological Record" for 1881, 310
- Sabine's New Wedge and Diaphragm Photometer, 201
- Sachs (Julius), "Text-Book of Botany, Morphological and Physiological," Prof. E. P. Wright, 263
- Safety Lamp, Prize offered for New, 496
- Sagiura (Shigetaké), on the Isomerism of Albuminous Bodies, 103
- "Sahara und Sudan," by Dr. G. Nachtigal, Prof. A. H. Keane, 408
- Saharanpur Botanical Gardens, Report, 588
- St. Petersburg Society of Gardening, 19
- Salz, Discovery of Bronze Hatchet at, 540
- Salmon and Herring Fisheries, 442
- Salvadori (Tommaso), "Ornitologia della Papuasie e delle Molluche," 577
- Sampson (Commander), the Comet 1882 *b*, 108, 266
- Sandberger (Prof.), Origin of Metalliferous Lodes, 90
- Sanitary Associations, Reports of, 423
- Sanitary Research, Grocers' Company Scheme, 495, 515
- Sanitary Science, the Scheme of the Grocers' Company for the Encouragement of Original Research in, 574
- Sap-flow, F. M. Burton, 530
- Saporta (Marquis de), Fossil Algae, 514
- Sardinia, Notes of Travel in, G. F. Rodwell, 342
- Sarepta, the Stones of, 231
- Saturn's Ring, Cassini Division of, 374
- Saxby (Rev. S. H.): the Magnetic Storm and Aurora, 86; the Aurora, 100; Meteor of November 17, 338
- Scale, Equal Temperament of the, C. B. Clarke, 240
- Scandinavia, Mathematics in, 343
- Schliemann (Dr.), Proposed Excavations at Athens, 276
- Schaeberle (Prof.), Method for Observing Artificial Transits, 67
- Schiaparelli (Prof.), on the Great Comet 1882 *b*, Francis Porro, 533
- Schirmwirkung of Iron, Prof. Stephan, 325
- Schmidt's Cometary Object, 20; Variable Star near Spica, 617
- Schriften der Physikalisch-Ökonomischen Gesellschaft zu Königsberg, 521
- Schwalbe (Dr.) "*Lehrbuch der Neurologie*," 196
- Schwarz (Herr), Action of Zinc on Sulphur, 184
- Science and Theology, 337
- Scientific Heresies in China, 342
- Scientific Renown, the Thirst for, 285
- SCIENTIFIC WORTHIES:—William Spottiswoode, P.R.S. (*with a Portrait*), 597
- Sclater (P. L., F.R.S.), the High Springs of 1883, 529
- Scorpions, Suicide of, C. L. Morgan, 313, 530
- Scotch Universities Bill, 565, 573
- Scotland: Completion of Ordnance Survey of, 92; New Appointments of Geological Survey, 246
- Scott (R. H., F.R.S.), "*Elementary Meteorology*," 575
- Scott (Major-Gen. H. G. D., F.R.S.), Death of, 587
- Scottish Lake Dwellings, Ancient, by Dr. Munro; Sir John Lubbock, F.R.S., 145
- Scottish Review, 399
- Scottish Meteorological Society, Half-yearly General Meeting, 469
- Scudder (Sam. H.), "*Nomenclator Zoologicus*," 28
- Scypho-Medusa, Habits of, Surgeon-Major H. B. Guppy, 31
- Sea Lion, the Cape, 415
- Seabroke (Geo. M.), Comet 1882 *b*, 4, 52

- Sea Serpent, the, F. T. Mott, 293; Joseph Sidebotham, 315; W. Barfoot, 338; Prof. W. Steadman Aldis, 338; Dr. J. Rae, F.R.S., 366
- Seals in the Baltic, 133
- Sea-shore, Apparent Bird Tracks by, 91
- Secchi (Father), Monument to, 298
- Seebohm (Henry), Ornithologist in Siberia, 560
- Seismographic Apparatus, Gray and Milne's, 547
- Selenka (Prof. E.), Sipunculacea, 133
- Serpieri (Prof. A.), "Il Potenziale Elettrico nell' Insegnamento dell' Elettrostatica," 312
- Serravallo, Double Action Mercury Air-pump, 324
- Sextants, New Apparatus for Testing, 473
- Shadows after Sunset, E. D. Archibald, 77; Prof. Dier, 150; J. Rand Capron, 182
- Sheep, Blanford's, 415
- Sheffield Free Libraries Report, 19
- Shetland, Severe Weather in, 443
- Shulachenko's Experiments with Telephones, 445
- Siberia, Proposal for Publication of General Description of, 182
- Siberian Aborigines, Yadrintseff, 541
- Siberian Flora, 445
- Siberia in Europe, Henry Seebohm, 560
- Sidebotham (Joseph), the Sea Serpent, 315
- Siemens (Sir Charles W.), Electric Lighting, the Transmission of Force by Electricity, 67; and Dr. Percy, Presented with the Freedom of the Company of Turners, 276; Electrical Transmission of Force and Storage of Power, 518
- Sieve Tubes, E. Russow, 366
- Silesia, Earthquake in, 400
- Silk Culture in Hungary, 209
- Silver, Electric Conductivity of Haloid Salts of, Prof. Kohlrausch, 182
- Simondsia paradoxa*, Dr. Cobbold, 547
- Singing, Speaking, and Stammering, W. H. Stone, 509, 531, 558, 580; James Lecky, 580
- Sipunculacea, Prof. E. Selenka, 133
- Siwalik Carnivora, Richard Lydekker, 293.
- "Skin-vision" of Animals, 399
- Skobelev (General), the Weight of his Brain, 347
- Slunin (Dr.), Popular Medicine in Russia, 62
- Smith (C. S.), Sir G. B. Airy on the Forth Bridge, 99
- Smith (F. S.): a Modification of the Gold Leaf Electroscope, 102
- Smith (Prof. H. J. S.): Death of, 371; Obituary Notice of, Dr. W. Spottiswoode, F.R.S., 381; his Mathematical Papers and Memoirs, 443; and the Representation of a Number as a Sum of Five Squares, 538, 564, 565, 587
- Smith (Leigh), Gift to the Royal Geographical Society, 323
- Smith (Robert H.), "Cutting Tools Worked by Hand and Machine," 577
- Smith (Worthington G.), Ancient Monuments, 102; Palæolithic Gravels, 102; Palæolithic Implements of North-East London, 270
- Smoke Abatement, Dr. E. Frankland, F.R.S., 407
- Smoke Abatement Institution, National, 443
- Smyth (Prof. C. Piazz), the Magnetic Storm and Aurora, 83; the Peak of Teneriffe Active again, 315
- Snake Bite, Death from, in Bombay, Sir Joseph Fayrer, F.R.S., 556
- Snakes, Destruction of Life in India by Poisonous, Sir J. Fayrer, F.R.S., 205
- Snowballs, Natural, a Remarkable Phenomenon, S. Hart, 483
- Snow Rollers, G. J. Symons, F.R.S., 507; F. W. Grey, 507
- Soda Flames in Coal Fires, Major J. Herschel, 78
- Soda, Heating by Acetate of, 344
- Soda Industry, Present Condition of, Walter Weldon, F.R.S., 401
- Sodium as New Element for Electro-positive Plate, Jablockhoff, 114
- Solar Corona, on Photographing the, Dr. Wm. Huggins, F.R.S., 199
- Solar Eclipses, Recent and Coming Total, J. Norman Lockyer, F.R.S., 185
- Solar Eclipse on May 6, Total, 248; English Expedition, 398, 567
- Solar Radiation and Glacier Motion, Rev. A. Irving, 553
- Solomon Islands, Anthropological Notes in the, H. B. Guppy, 607
- Sound-vibrations of Solid Bodies (glass cylinders) in Contact with Liquids, Auerbach, 325
- Space, the Matter of, Charles Morris, 349; Prof. A. S. Herschel, 458, 504
- Spatzier (Johann), Death of, 422
- Speaking, Singing, and Stammering, W. H. Stone, 509, 531, 558, 580; James Lecky, 580
- Spectrum Analysis: Work in the Infra-Red of the Spectrum, Capt. Abney, F.R.S., 15; Prof. Rowland's Concave Gratings for giving a Diffraction Spectrum, 95; Profs. Living and Dewar on the Origin of the Hydrocarbon Flame Spectrum, 257; Distribution of Energy in the Spectrum, Lord Rayleigh, F.R.S., 559
- Speedwell, Fertilisation of, A. M. Stanley, 127, 174; A. Ransom, 149, 223
- Speke and Grant's Zebra, Sir J. Fayrer, F.R.S., 604
- Spencer's (Herbert) Philosophy, Section founded for Study of, at Birmingham, 587
- Spica, Schmidt's Variable Star near, 617
- Spider, New Species of African, 348
- Spitzbergen, Swedish Expedition to, 243
- Spitzbergen Geological and Palæontological Collections, 588
- Sponges, Australian Freshwater, 91
- Spörer (Prof.) on the Transit of Venus, 284
- Sportsman's Handbook to Practical Collecting, &c., Rowland Ward, 146
- Spottiswoode (Dr. W., F.R.S.): Address to Royal Society, 134, 162; Obituary Notice of Prof. Henry Smith, 381; "Scientific Worthies," 597
- Spring in the Jachère, Intermittent, 63
- Spring (M. W.), Thunderstorms, 133
- Springs, High, of 1883, P. L. Sclater, F.R.S., 529
- Squares, Prof. H. J. S. Smith and the Representation of a Number as a Sum of Five, 538, 564, 565, 587
- Stamens, Two Kinds of, with Different Functions in the same Flower, Dr. Hermann Müller, 30; Dr. Fritz Müller, 364
- Stammering, Singing, Speaking and: W. H. Stone, 509, 531, 558, 580; James Lecky, 580
- Stanley (A. M.), Fertilisation of Speedwell, 127
- Stanley (W. F.), Causes of Elevation and Subsidence of Earth's Surface, 523
- Stapleton (F.): the Magnetic Storm and Aurora, 86; the Comet, 151
- Stapley (A. M.), Fertilisation of Speedwell, 174
- Stars: Star Maps in *Glasgow Evening Times*, 89; Measures of Double, 182; Binary, 518; ζ Cancri, 424; ρ Eridani, 589; Variable, 324, 400, 540; S Virginis, 424; U Cephei, Ceraski's, 424, 445; Supposed Variable μ Doradus, 498; Schmidt's Variable near Spica, 617
- Steel Plate Manufacture, 405
- Steel, Hardening and Tempering, Prof. W. Chandler Roberts, F.R.S., 594
- Stellar Parallax, 210
- Stephan (Prof.), Schirmwirkung of Iron, 325
- Stevenson (Thos.), Observations of Increase of Velocity of Wind with Altitude, 432; E. D. Archibald, 506
- Stewart (Comm. D.), Mimicry in Moths, 314
- Stock (T.), Notice of some Discoveries recently made in Carboniferous Vertebrate Palæontology, 22
- Stockholm Ethnographical Exhibition, Dr. A. B. Meyer, 371
- Stokoe (Dr. P. H.), Mimicry, 508
- Stone Age in Japan, Weapons and Implements from the, 616
- Stone (Prof. E. J.), Transit of Venus, 1882, British Expeditions, 177
- Stone (W. H.), Singing, Speaking, and Stammering, 509, 531, 558, 580
- Storage Batteries, Chemistry of, Dr. E. Frankland, F.R.S., 568
- Strahan (A.), Movements of Air in Fissures and the Barometer 375, 461
- Stress, Clerk Maxwell on, 314
- Strong (George), Improved Feed-water Heater and Purifier, 90
- Suez Canal, Animal Migrations due to, Prof. Keller, 181
- Sulphur, Action of Zinc on, Schwarz, 184
- "Sun: its Planets and their Satellites," Rev. E. Ledger, 309; Eclipse of the, 346; Ignition by Sunlight, Major Herschel; E. H. Verney, 531; Shadows after Sunset, E. D. Archibald, 77; J. Rand Capron, 102; Prof. Dier, 150; a Mock Sunset, 78; Wolf and Faye on Periodicity of Sunspots, 235; Absence of Sunspots—Unprecedented Cold in the Riviera, C. J. B. Williams, 551
- Sunflowers at Night, the Reversion of, C. A. White, 241
- Surveying, Marine, a Treatise on, Rev. J. L. Robinson, 289
- Svanberg (Dr. Gustav), Death of, 132

- Swan Lamp Spectrum and the Aurora, J. Rand Capron, 149; J. Munro, 173
- Sweden, Auroræ in, 113, 496; Darwin Memorial and the People of, 275; Great Meteor in, 423, 517; Swedish Expedition to Spitzbergen, 1882, 243; New Swedish Arctic Expedition, 400
- Sweden and Norway, Electrical Disturbances in, 89
- Switzerland: Swiss Geological Society, 132; Avalanche in Western, 181; Earthslip in, 209; Heavy Rain in, 209; Fishes of Switzerland, Dr. Victor Fatio, 220; Electric Lighting in, 497
- Sydney: Linnean Society of New South Wales, 308, 355, 475, 571, 619
- Symons (G. J., F.R.S.), British Rainfall, 149; Snow Rollers, 507
- Szechenyi's Travels, A. H. Keane, 170
- Tacchini (Prof.), Aurora of November 17, 139
- Tait (Prof. P. G.), Paper on the Laws of Motion, 283
- Talisman Deep-Sea Expedition, 587
- Tapetum, the, in the Retina of Mammals, 216
- Tapir, the Malayan, 151
- Taschenberg (Prof.), "Die Insekten nach ihren Schaden und Nutzen," 172
- Tashkend, Earthquake at, 617
- Tawney (E. B.): Death of, 247; Obituary Notice of, 295
- Taylor (Rev. C. J.), the Aurora, 99
- Taylor (H. D.): the Magnetic Storm and Aurora, 87; Aurora of November 17, 140; Meteor of November 17, 365
- Taylor (J. E.), Origin of our Vernal Flora, 7
- Teaching of Elementary Mechanics, 580
- Technical College, the Finsbury, 181, 318, 425, 495
- Technical Education Commission, 516
- Telegraph Extension in China, 588
- Telegraphy, Advance in Use of, for French Newspapers, 372; the Progress of, W. H. Preece, F.R.S., 390
- Telephones, Shulachenko's Experiments with, 445
- Telephonic Communication, Novelty in, 515
- Temperament, Equal, of the Scale, C. B. Clarke, 240
- Teneriffe, the Peak of, active again, Prof. C. Piazzì Smyth, 315
- Terrestrial Radiation, Note on, John Tyndall, F.R.S., 377; Dr. A. Woeikof, 460
- Tertiary History of the Grand Cañon District, Dr. Arch. Geikie, F.R.S., 357
- Thames, River—Abnormal High Tides, J. B. Redman, 6
- Thames, Monthly Means of the Temperature of the Water of, Sir G. B. Airy, F.R.S., 189
- Theology, Natural Selection and Natural, Prof. Asa Gray, 291, 529; Dr. Geo. J. Romanes, F.R.S., 362; J. B. Hannay, F.R.S., 364
- Theology, Science and, 337
- Thermometer, an Improved Air, 43
- Thermometers, Liquid, Expansion of Bulbs of, Prof. Govi, 209
- Thibet, Native Exploration of, Sir H. Rawlinson, 323
- Thomson's Mouse-Mill Dynamo, J. T. Bottomley, 78
- Thomson (Sir William, F.R.S.): Photometric Measurements of Sun, Moon, Cloudy Sky, and Electric and other Artificial Lights, 277; Gyrostatics, 548
- Thorium, Nilson, 184
- Thunderstorms, M. W. Spring, 133
- Thylacoleo, on the Affinities of, Prof. Owen, F.R.S., 354
- Ticks, 531; Dr. T. Spencer Cobbold, F.R.S., 552; Rev. L. Blomefield, 552
- Tiddeman (R. H.), the Magnetic Storm and Aurora, 84
- Tides, Abnormal High—in the Thames, J. B. Redman, 6
- Tides, Great, Prof. R. S. Ball, F.R.S., 201
- Tides, Geological Traces of Great, J. G. Grenfell, 222
- Timehri, 539
- Times, the, on Science, 566
- Timor-Laut, H. O. Forbes's Visit to, 159
- Tissandier's Electromagnetic Engine for directing Balloons, 343
- Tomlinson (C., F.R.S.), Priestley and Lavoisier, 53, 147
- Torpedo, Prof. Fritsch on the, 453
- Trall (D.), Transit of Venus, 159
- Tram-car, Electrically moved, Successful Trial of a, 470
- Transactions of the New York Academy of Sciences, 330
- Transit of Venus, 112, 154, 253, 540; Prof. Harkness on the, 114; British Expeditions, E. J. Stone, F.R.S., Prof. Langley, John Birmingham, 177; C. J. B. Williams, F.R.S., 197; in Algiers, 208; French Expedition, 208; German Expedition, 208; Prof. Edgar Frisby, 266; Samuel Hart, 483
- Transits, Method for Observing Artificial, Prof. Schaeberle, 67
- Transmission of Energy on the Marcel-Deprez System, 372
- "Travels in India," 21
- Tresca (M.), Papers on Electrical Measures, 62
- Tribe (Dr. A., F.R.S.), Chemistry of the Planté and Faure Accumulators, 583
- Tricycles propelled by Electricity, 19
- Tromholt (Sophus), on the Aurora Borealis, 394
- Trotting Horse, Evolution of the American, W. H. Brewer, 609
- Trouton (F.), on a Relation existing between the Latent Heats, Specific Heats, and Relative Volumes of Volatile Bodies, 292
- Trouvé's Batteries, 41, 42
- Tubercle, Bacillus of, 492, 563
- Tucker (R.), Memoir of Augustus de Morgan, 217
- Tuckwell (Rev. W.), on the Midland Boulders, 346
- Tunnel, Channel, Prof. W. Boyd Dawkins, F.R.S., 338
- Turbellarians, Prof. von Graff's Monograph on the, Prof. H. N. Moseley, F.R.S., 227
- Tylor (Dr. E. B., F.R.S.), "The Burman," 6
- Tympanic Membrane, Function of Membrana Flaccida of, J. M. Crombie, 129
- Tyndall (Prof. John, F.R.S.), "Note on Terrestrial Radiation," 377; Dr. A. Woeikof on, 460
- Typhoon in Philippine Islands, 181
- Ultra-Violet Rays, Absorption of, by various Substances, Professors Living and Dewar, 521
- Umdhlebi Tree of Zululand, W. T. Thistelton Dyer, F.R.S., 7; Rev. Dr. G. W. Parker, 7, 32
- United States, Oyster Industry of the, 39; New Aquarium at Wood's Hole, 347
- Universities Bill, Scotch, 565, 573
- University and Educational Intelligence, 47, 71, 93, 117, 142, 167, 214, 233, 281, 306, 330, 353, 402, 449, 472, 498, 547, 594, 618
- Ural Mountains, Russian Exploration of, 446; Severe Weather in, 539
- Uric and Hippuric Acid, Dr. A. B. Garrod, F.R.S., 451
- Uvæan Parrakeet, 417
- Valais, Earthquake in the, 181
- "Vampire Bat": Thos. Workman, 411; A. W. Auden, 411; G. J. Romanes, F.R.S., 412
- Variable Stars, 324, 400, 540; S Virginis, 424; U Cephei, Ceraski's, 424, 446; Supposed Variable μ Doradus—a Spurious Star, 498; Schmidt's Variable Star near Spica, 617
- Varna, Alleged Wreck of the, 114
- Vega Expedition, the Fossil Plants collected by, 347
- Vega Gold Medal conferred on Mr. Stanley, 422
- Vegetable History, Tables of, D. P. Penhallow, 458
- Velocity of the Wind, Diurnal Variations in the, E. Douglas Archibald, 461
- Venus, Transit of, 112, 154, 253, 541; Observations in Paris of the, 113; Prof. Harkness, 114; W. de Fonvielle's "Periodical," 132; Duke of Argyll, Dr. R. S. Ball, F.R.S., Dr. W. Doberck, J. L. E. Dreyer, C. L. Wragge, W. F. Denning, D. Traill, H. Cecil, R. Langdon, 154 to 159; British Expedition, E. J. Stone, F.R.S., 177; Prof. Langley, 179; John Birmingham, 180; Prof. Cacciato, 180; C. J. B. Williams, 197; German Expedition, 208; French Expedition, 208; United States Expedition, 246, 539; Prof. Edgar Frisby, 266; Prof. Spörer on the, 284; Samuel Hart, 483
- Verhandlungen der Naturforschenden Gesellschaft in Basel, 71
- Verhandlungen der Naturhistorischen Vereines der Preussischen Rheinlande und Westfalens, 521
- Vernal Flora, Origin of our, J. E. Taylor, 7
- Verney (E. H.), Ignition by Sunlight, 531
- Vertebrate Dissection, Handbook of, H. Newell Martin and William A. Moule, 335
- Vertebrate Palæontology, Carboniferous, Notice of some Discoveries recently made in, T. Stock, 22
- Vicars (G. Rayleigh), the Magnetic Storm and Aurora, 87
- Victoria (Philosophical) Institute, 143, 260, 283
- Vienna Geographical Society, 229, 401
- Vienna Imperial Academy of Sciences, 72, 95, 120, 260, 284, 524, 572
- Vienna International Electrical Exhibition, 373, 444
- Vilmorin-Andrieux, "Les Plantes Potagères," 429
- Vine Parasite, New, 133
- Vivisection, Facts and Considerations relating to, 542

- Vivisection Bill, 549
 Vogel (Hermann W.), Lockyer's Dissociation Theory, 233
 Volatile Bodies, on a Relation existing between the Latent Heat, Specific Heats, and Relative Volumes of, F. Trouton, 292
 Volcanic Ashes, Showers of, 516
 Volta Prize, 89
 Von Graff's Monograph on the Turbellarians, Prof. H. N. Moseley, F.R.S., 227
 Volcanic Eruption in the Caucasus, 63
 Volcanic Phenomena in Greek Archipelago, 445
 Von Siebold, Memorial to, 41
- Walker (C. V., F.R.S.), Death of, 228
 Wallace (A. R.): Difficult Cases of Mimicry, 481; on the Value of the "Nearctic" as one of the Primary Zoological Regions, 482
 Ward (H. Marshall), on the Genus *Meliola*, 234
 Ward (Rowland), the Sportsman's Handbook to Practical Collecting, &c., 146
 Warmbrunn, Fulgurite found near, 540
 Wartmann's Rheolyzer, E. von Fleische, 127
 Warwick Museum, 539
 Washington Observatory, U.S., 300
 Waste Paper in China, 588
 Watch and Clockmaker's Handbook F. J. Britten, H. Dent Gardner, 76
 Water, Sounds produced by Outflow of, Signor Martini, 183
 Water-Analysis, American Researches on, 211, 231
 Waterspouts on Land: James Hosack, 79; J. G. McIntosh, 103
 Watson (Sir Thos.), Death of, 160
 Watts (Arthur), the Comet, 5
 Waves, Mountainous, in Channel in Calm Weather, 540
 Weather, the: J. M. Fountain, 32; J. R. Capron, 198; Altitude and, Dr. Woeikoff, 223; the Recent Cold Weather, W. Ingram, 530; "Weather Forecasts," the Bishop of Carlisle, 4, 51; Rev. W. Clement Ley, 29; C. W. Harding, 79
 Webb (Rev. T. W.), "Anomalous" Tail to the Comet 1882 *b*, 89; Mars, 203
 Weights and Measures, 131
 Weldon (Walter, F.R.S.), Present Condition of Soda Industry, 401
 Westhoff (F.), "Die Käfer Westfalens," 239
 Wetted Solids, Condensation of Liquid Films on, J. W. Clark, 370
 Wharton (H. J.), Hovering of Birds, 312
 Whipple (G. M.): the Magnetic Storm and Aurora, 83; New Apparatus for Testing Sextants, 473
 Whistles, Hydrogen, Francis Galton, F.R.S., 491
 White (Wm.), Palæolithic River Gravels, 53
 White (W. H.), "The British Navy: its Strength, Resources, and Administration," Sir Thomas Brassey, 549
 Whitehouse (F. Cope), "Is Fingal's Cave Artificial?" 285
 Whitmell (Chas. T.), "Natural" Experiment in Complementary Colours, 266
 Whitworth (Sir Joseph), Papers on Mechanical Subjects, 208
 Whitworth (W. A.), the Churchman's Almanac for Eight Centuries, 386
 Wiedmann (E.), Pre-fusional Chemical Transposition of Heated Water containing Salts, 183
 Williams (Dr. C. J. B.): the Comet, 29, 110; Transit of Venus, 197; the Comet 1882 *b* during last Month, 197; Unprecedented Cold in the Riviera—Absence of Sunspots, 551
 Williams (W. M.), the Inventor of the Incandescent Electric Light, 240
 Williamson (John), Ferns of Kentucky, 336
- Wilson (A. S.), Potato Disease, 523
 Wind, Increase in Velocity of, with Altitude, E. D. Archibald, 243, 506; Thos. Stevenson, 432; Diurnal Variation of, on Open Seas and near and on Land, A. Buchan, 413; E. Douglas Archibald, 461
 Winlock (W. C.), the Comet, 128
 "Winners in Life's Race," Arabella Buckley, 51
 Winter (Karl), Death of, 229
 Wire Guns, James A. Longridge, 11, 35, 53
 Wissmann and Pogge's African Expedition, 92, 341, 401
 Woeikoff (Dr.): Altitude and Weather, 223; Terrestrial Radiation and Prof. Tyndall's Observations, 460
 Wolf on Methods employed in Astronomical Physics, 372
 Wolga, Discovery of Remains of Diluvial Mammals on, 373
 Women Students, College Hall and Residence for, 229
 Wood (Rev. J. G.), Common British Insects, 124
 Workman (Thos.), The "Vampire Bat," 411
 Worms, Charles Darwin on, 20
 Worthington (A. M.), the Magnetic Storm and Aurora, 85; Influence of Vacuum on Electricity, 434
 Wragge (Clement L.): Ben Nevis Observatory, 39, 487; Aurora, 54; Transit of Venus, 158
 Wright (Prof. E. P.), "Text-Book of Botany, Morphological and Physiological," Julius Sachs, 263
 Wright (Lewis), "Light," 75
 Wrightson (Prof. J.), Ensilage in America, Prof. James E. T. Rogers, 479
- Yadrintseff, Siberian Aborigines, 541
 Yarrell (Wm.), A History of British Birds, Part xv., 98
 Year-Book of Pharmacy, 1882, 361
 Yoe (Shway), "The Burman," 5
 Yorkshire College Students' Association, 19
 Yorkshire List of Lepidoptera, Porritt's, 540
 Yurgens' Meteorological Observations at Mouth of Lena, 423.
- Zebra, Speke and Grant's, Sir J. Fayrer, F.R.S., 604
 Zeitschrift für wissenschaftliche Zoologie, 94, 189, 426
 Zeller (Philip Christoph), Obituary Notice of, R. McLachlan, F.R.S., 535
 Ziegler (Ernst), "A Text-Book of Pathological Anatomy and Pathogenesis," 477; Death of, 566
 Zinc on Sulphur, Action of, Schwarz, 184
 Zodiacal Light (?), 580; W. H. Robinson, 605; Robert Dwarries Gibney, 605; E. Brown, 605; D. J. Rowan, 605
 Zoological Expedition up the Niger, W. A. Forbes's, 14
 Zoological Gardens, Additions to, 20, 63, 78, 91, 114, 133, 161, 182, 210, 230, 248, 277, 300, 324, 348, 374, 400, 424, 445, 470, 497, 517, 540, 589, 617
 "Zoological Notes on the Structure, Affinities, Habits, and Mental Faculties of Wild and Domestic Animals," Arthur Nicols, Dr. Geo. J. Romanes, F.R.S., 333
 "Zoological Record for 1881," the, E. C. Rye, 310; Sydney J. Hickson, 366
 Zoological Regions, Primary, on the Value of the "Nearctic" as one of the, Prof. Angelo Heilprin, 606
 "Zoological Sketches," Felix L. Oswald, Dr. Geo. J. Romanes, F.R.S., 333
 Zoological Society, 94, 190, 282, 307, 379, 451, 499, 570, 619
 Zoological Society's Living Collection, Illustrations of New and Rare Animals of, 151, 415
 Zoological Station in Naples, J. T. Cunningham, 453
 Zoology in Japan, 614
 Zululand, Umdhlebi Tree of, W. T. Thielton Dyer, F.R.S., 7; Rev. G. W. Parker, 7, 32



A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE

"To the solid ground

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

THURSDAY, NOVEMBER 2, 1882

HYDRAULIC EXPERIMENTS

Roorkee Hydraulic Experiments. By Major Allan Cunningham, R.E. Three vols. (Roorkee: Thomason College Press, 1880-81.)

UNDER the direction of the Indian Government there have been constructed a number of canals, which, while reaching in transverse dimensions a size not much inferior to the Suez or North Sea Canal, have a far greater length and ramify into smaller channels of enormous total extent. Besides these, reservoir and river works have been carried out of the greatest magnitude. Hence the Indian Government has a most direct interest in the advancement of the knowledge of hydraulics. Not only must hydraulic formulæ be used in the design of hydraulic works, but also in regulating the distribution of a valuable commodity—irrigation water—on which large revenues depend. Yet down to a recent period the Indian Government has been content to avail itself of researches carried out in Europe, and chiefly in France, and has made no use of its splendid opportunities for scientific hydraulic experiments. When at last hydraulic experiments on a large scale were sanctioned, involving a large expenditure it was very fortunate that the direction of them was intrusted to so very competent an officer as Major Cunningham. "Beaucoup de personnes croient que tout homme intelligent et instruit peut faire, sans grand travail de bonnes expériences; c'est une erreur qui a fait perdre beaucoup de temps et d'argent." So says M. Boileau, who is himself one of the most careful of hydraulic experimenters. Major Cunningham certainly does not think lightly of his work. He has enormous industry; he repeats his observations again and again; he studies every detail of his methods; he notes the opinions of all his predecessors in work of a similar kind, and discusses his results with great lucidity. If his experiments have furnished no strikingly novel laws, the fault is not his.

"The general result of this work may perhaps be considered in some ways disappointing, in that there are no brilliant results, no simple laws of fluid motion disco-

vered, not even a new formula for mean velocity proposed" (p. 4).

It is certainly true that when Major Cunningham passes from discussing the details of practical methods, where he is always instructive, to purely scientific questions, to generalising laws from the results obtained on verifying accepted rules, he has a rather exceptional number of purely negative conclusions to state. It is almost amusing to find caution carried to the extent involved in printing as a general result of a considerable discussion that the value of the coefficient in the formula for the discharge of a stream "depends *probably* on the nature of the banks and bed, as well as on the hydraulic mean depth and slope." But nevertheless we believe the practical objects of the experiments have been obtained, and the outlay usefully incurred. Less of thoroughness at all events would have rendered the experiments useless, and although considering the scale of the experiments they seem at present rather less fruitful of definite results than might have been expected, yet it may be hoped that Major Cunningham has not made the most that can be made of his results. In time the new suggestion will come which will reduce to order the discordant observations. In the establishment of any new general conclusions or formulæ in the hydraulics of streams, this store of data will certainly be of the greatest value.

Of the magnitude of the work undertaken by Major Cunningham, it is difficult to give an adequate idea. It lasted over four years. The results include 565 sets of vertical velocity curve observations, each set including velocities taken three times at each foot of depth; 545 sets of rod float observations, each including six measurements of velocity; 581 sets of mean velocity observations, each including three measurements of velocity at ten to twenty points; 440 measurements of surface slope; besides many others. In addition to all this, the tabulation and computation of the results involved enormous labour. The printing of the results at Roorkee, whilst it must have involved greater trouble and responsibility than similar work in this country, seems to have been most efficiently and accurately done.

From the practical point of view Major Cunningham's book may be regarded as an exhaustive treatise on Float Gauging. All the more important observations were

made by floats, and he has used these simple instruments in all their known forms, as surface floats, sub-surface floats, twin floats, and rod-floats. Every detail of the construction and use of these floats has been studied, their form, the length of run, the mode of marking the sections and float paths, and the precautions in taking the time. The sources of error are weighed, and in some degree the limits assigned beyond which the methods become unreliable. There will always be cases where the methods of float-gauging must be used, and no one who has work of this kind to do can afford to neglect Major Cunningham's directions. A few observations were, in fact, made with current meters. But the instruments used were of a type which must now be regarded as antiquated, and as to these Major Cunningham suggests no improvement which has not already been tried by the German engineers, who have, in fact, converted the current meter into a new instrument of precision.

It is not at all to be regretted that Major Cunningham adopted floats in his experiments. Even from the scientific point of view, if floats are at best a rough means of determining velocities, yet they are not liable as more complicated instruments are, if used without sufficient care or knowledge, to large and concealed errors. Hence float observations may always be used advantageously to check observations made in other ways. The progress of hydraulics suggests questions, for the solution of which float methods are inadequate, and the results obtained by Harlacher and Wagner seem to show that floats will be superseded by instrumental means of greater complication, but of far greater delicacy. But in truth in hydraulics no one method is free from objections and researches carried out by all methods, when sufficient care is exercised, will prove useful.

We may now pass to consider briefly the bearing of these experiments on some points of theory. Major Cunningham devotes Chapter VI. to a discussion of the unsteadiness of the motion of the water in ordinary streams. At each point the velocity varies in direction, and magnitude from instant to instant. The float-velocities taken on 50-feet runs, which are themselves mean velocities for a certain time and distance, vary from 10 to 30 per cent., so that to obtain the true mean velocity over any given float-path, something like fifty float observations are necessary. Recent current-meter observations show this variation of velocity still more clearly. The essential unsteadiness of the motion of water in streams was pointed out with the greatest clearness by M. St. Venant (1872), and the still more important fact that the motion is periodically unsteady, that is, that the variations occur periodically about a constant mean value, so that the average velocity for a sufficient but not very great length of time is sensibly constant. It is only this last fact which has rendered it possible, to apply the equations for steady motion to the actual motion of streams, and it is a pity that Major Cunningham has not adopted St. Venant's convenient term, mean local velocity, for the sensibly constant average velocity at each point of a stream. It is not the "interlacing of the stream lines" (p. 107), but the destruction of stream-line motion by eddying motions of quite another character, to which the unsteadiness seems to be due.

In Chapter VII. the observations of the surface-slope

at different periods of the experiments are discussed, and it is here that we think may be discovered the one matter in which the conditions of the experiments were unsatisfactory, and in which they are markedly inferior to Bazin's small-scale experiments. Taking the Solani embankment and Solani aqueduct sites, at which the largest amount of work was done, we find that the experiments were made at about the centre of a ten-mile reach, terminated at the upper end by a regulator controlling the water-supply, and at the lower end by a fall where, by artificial means, the water-level was kept up to any desired height. The bed of the canal between these limits had originally the uniform slope of about a foot per mile. This original level is maintained at five points by masonry works, but between these the bed is irregularly scoured out to an extent which must have made very sensible variations of velocity within distances of a mile. At the tail of the reach is a weir standing five feet above the level of the bed, the crest of which was further raised by temporary obstructions of a height sometimes reaching five feet more. Hence the whole height of obstruction was often greater than the whole depth of water at the site of the gaugings. Under these circumstances the slope of the water surface varied, being generally quite different in the part of the reach above the site of the experiments from that in the part below, where the influence of the tail weir was felt. Further, the difference of slope in the parts of the reach above and below the site of the experiments differed widely in different conditions of the water supply. The local surface slope, that is the slope of the water surface in the neighbourhood of the gauging site, varied irregularly with the variation of the slopes above and below, being apparently, as might be expected, most affected by the obstruction at the tail of the reach. Now as the velocity at a given site does not exclusively depend on the surface slope at the site, but to a certain extent on the slope above and below, the conditions of the site were initially to some extent unfavourable, and that in a degree which, although it may be small, is difficult to appreciate. The local surface slope itself can only be measured on a considerable length of stream (1000 to 4000 feet). But in that length the surface slope appeared to vary, the slope in 2000 feet being as much as 25 per cent. different from that in 4000 feet, and the slope at one bank being 50 per cent. different from the slope at the other. It is obvious, therefore, that the local surface slope is a quantity which, under the conditions of these experiments, was not ascertainable with any great accuracy. But the whole comparison of the experimental results with formulæ of discharge involves the accurate knowledge of this quantity. All inferences from these experiments as to the reliability of formulæ must be weakened in proportion as the slope measurement is doubtful.

It is not in Major Cunningham's experiments alone that this difficulty in determining the surface slope has been found. It is to the uncertainty of this quantity mainly, to this *fons et origo malorum*, that the discordances of large-scale experiments are due. The roughest small-scale experiments, those, for instance, discussed by Eytelwein and Prony, have furnished coefficients more useful in practice and more generally applied than any large-scale experiments hitherto carried out. The advan-

tage of regular canals over natural rivers for hydraulic experiments almost disappears when the canal bed is scoured out to an irregularity similar to that of a natural stream, and the canals are at a disadvantage when artificial control at the tail of the reach modifies the conditions of flow to an extent sensibly felt at the site of the experiments. It is, of course, in the lower states of the water in the canal in the Roorkee aqueduct reach, that the effect of the tail control is most sensible, but then experiments made in these conditions are an essential part of the data necessary for generalisation.

Major Cunningham spent a good deal of time in verifying a supposed theory that the surface of a stream should be convex. The theory is probably a capital instance of the frequent mistake of importing the principles of theoretical hydrodynamics into practical hydraulics. In a stream flowing from a reservoir, in such a way that the tangential forces on the surface of the elementary streams are absent or negligible, the energy per pound of fluid is uniformly distributed. It follows that in parts where the velocity is greater, the pressure is less. A stream may be regarded as a bundle of horizontal filaments coming from a common reservoir. If in such a stream the central filaments have a greater velocity than those nearer the sides, their pressure will be less. Consequently, for equilibrium there must be a greater depth of stream towards the centre, and the transverse water-line will be convex upwards. Such is the theory which Major Cunningham has taken a great deal of trouble to test, and to which he attaches weight in spite of his observations. From preliminary calculations he shows that the known differences of velocity would give a difference of level, between the centre and sides of the Ganges Canal, of 3 inches. After the most careful measurements, it was found that the difference of level varied from $+0.018$ foot to -0.095 foot, the average difference being almost exactly zero. Obviously the theory is outrageously wide of the truth, and the reason is not far to seek. The differences of velocity to which the supposed differences of pressure are due, are created by exactly those tangential actions of the filaments which the theory neglects. There is no reason for assuming equal distribution of energy along a filament, part of the energy of which is being destroyed by lateral frictional actions between the filaments. As to the observations in Chapter V., with a gauge giving still water-level, it is not clear that the small difference of level observed was not due to the position of the mouth of the tube which communicated with the canal.

The discussion of the vertical velocity parabolas in Chapter XI. is extremely interesting, and the method adopted for finding the most probable curve by the method of least squares, is laborious and conscientious. The method of weighting the observations seems, it is true, rather artificial, especially as the observations at great depth best define the form of the parabola. The general conclusion arrived at is, that while all the observations can be fairly well expressed by parabolic curves, no formula can be found expressing the dependence of the variation of velocity on the slope, and dimensions of the channel. It would be interesting to see if a parabola with axis on the water-line would not agree better with the results, the observations above the line of maximum velocity being of course discarded. So far as there is any theory of the

mutual action of the filaments, it leads to the result that the parabolic axis should be at the surface; and that is not inconsistent with one possible explanation of the reduction of velocity near the surface.

In ordinary streams, the velocity is greater towards the surface and centre, and less towards the bottom and sides. But the greatest velocity is not found at the surface, but at a variable depth below it, amounting very often to one-fourth of the whole depth. The Mississippi observers attributed this to the friction of the water against the air. In accordance with this they found the depression of the line of maximum velocity to depend quite directly on the direction of the wind, and they logically introduced into their formulæ of flow, the free surface, as forming part of the frictional wetted border. Major Cunningham retains the Mississippi observers' explanation, while his experiments disagree with theirs on all the points which directly support the explanation. He finds, for instance, that the depression of the line of maximum velocity is entirely independent of the direction and force of the wind. Now excepting one suggestion to be referred to presently, no kind of retarding action between the air and water has been stated which is not of the nature of a frictional resistance. The Mississippi observers and some others who adopt the explanation of the depression of the line of maximum velocity we are now criticising, state explicitly that they consider the resistance between the air and water to be of the same nature as the resistance between the water and its solid bed. If so, since the line of maximum velocity is ordinarily depressed to one-fourth the depth at the centre, and generally still more towards the sides, the friction between the water and air must be something like one fourth as great as the friction between the water and solid bed. But is it conceivable that the friction between the level water surface and mobile air should have anything like one-fourth the value of the resistance of the water impinging on all the immovable roughnesses of the stream bed? Further, any resistance of this kind must depend on the relative velocity of the water and air. But the air is most commonly in motion, and on the average must as often and as long blow down stream as up stream. Blowing down stream, it should accelerate the stream to the same extent as blowing up stream it retards it. But it is known from Boileau's experiments and others that the depression is still persistent with a wind blowing down stream at a velocity greater than that of the water. To this Major Cunningham's only answer is that "the time required for the penetration of change of velocity of the surface current caused by wind to any considerable depth appears to be very great. It has been estimated that it would take one week for half change of surface velocity to penetrate three feet." The evidence for this is not given, but if it is so, is it not because the friction between air and water is extremely small, and it is only in those cases where the persistence of the wind action for a long time allows an accumulation of effect, that that effect is sensible.

A wind blowing on the surface of a lake is long in producing a current merely because the friction is small, but it does produce a current in time, because the action is cumulative. On a river it produces no sensible effect at all, as Major Cunningham's experiments show. But if

the friction between air and water is as great as he supposes, it ought to produce a sensible effect, and since winds blow as often and as long down-stream as up-stream, the water-surface should as often be accelerated as retarded, and the vertical velocity parabola should as often have its axis above the water-surface as below it. Boileau does indeed suggest that the absorption of air by the water and the evaporation of the water cause a loss of energy near the surface, but here again the cause seems as inadequate as air-friction. The experiment of Francis, quoted on p. 107, is admitted by Major Cunningham, to prove that "there is a continual transfer of water from the bed towards the surface, even in water in apparently tranquil motion," and his own float-observations (p. 269) show that "near the edge of a stream there is a persistent flow of the water at and near the surface from the edge towards the centre." Now the flow from the bottom and sides towards the top and centre brings water, stilled by impinging on roughnesses of the bed, to replace the quick moving surface-water. It is not true that the water so rising must acquire the velocity of the layers through which it passes, for it may rise in eddying masses, which are but little affected by the friction on their surface, or the motion of the water may be in horizontal spiral paths, which allow the bottom water to reach the surface without passing through the quicker moving central parts of the stream. At all events the transfer of the bottom water to the surface is a known phenomenon, and it is adequate as an explanation of the diminution of surface velocity.

In Chapter XVI. is given a somewhat elaborate theory of the motion of a rod-float, which leads to the result that the rod-velocity is slightly less than the true mean velocity of the water past the immersed portion of the rod. Quite apart from the question of the general unsteadiness of the motion of the water, it may be pointed out that the relative velocity $v-u$ of the streams impinging on the rod must for the most part fall below the limit for which the pressure due to impact or friction can be assumed to vary as the square of the velocity. Hence the calculation that the rod-length should be 0.94 of the depth of the water to give a true mean velocity, seems an extremely doubtful one.

In criticising thus two or three points of theory, it must be pointed out that these matters do in fact lie somewhat outside the main objects of the experiments, and an error on these points detracts nothing from the practical value of Major Cunningham's work.

W. C. U.

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. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

"Weather Forecasts"

WILL you permit me to call attention to the apparently complete failure of the Forecasts of Weather given in the daily papers with respect to the storm of Tuesday, October 24? The matter seems to me to be one of much practical moment. Here is an extract from the "Weather" article in the *Times*, which I presume agrees with that given in each of the daily papers:—

Forecasts of Weather for Tuesday, October 24 (issued at 8.30 p.m. on the previous day).

- o. SCOTLAND, N.—South-westerly breezes, fresh or moderate; showery.
1. SCOTLAND, E.—South-westerly breezes, moderate; some showers, with bright intervals.
2. ENGLAND, N.E.—Same as No. 1.
3. ENGLAND, E.—Same as No. 5.
4. MIDLAND COUNTIES.—Same as No. 1.
5. ENGLAND, S. (London and Channel).—Westerly and south-westerly breezes, light to fresh; fine and cold at first, some local showers later.
6. SCOTLAND, W.—Same as No. o.
7. ENGLAND, N.W. (and N. Wales).—Same as No. o.
8. ENGLAND, S.W. (and S. Wales).—South-westerly winds, fresh to strong; showery.
9. IRELAND, N.—Wind returning to south-west, and freshening; weather showery.
10. IRELAND, S.—Same as No. 9.

Warnings.—None issued.

By order,
ROBERT H. SCOTT, Secretary.

Notice particularly the concluding words: "Warnings; none issued;" and then remember what took place. It is curious to compare in this respect the *Times* of October 24 with that of October 25. In the latter issue we read as follows:—

"Yesterday morning a violent gale of wind, accompanied by a heavy downpour of rain, visited London. The previous night was beautiful, but at three o'clock yesterday morning the sky became overcast, and from half-past four o'clock up to ten o'clock there was an incessant downpour of rain. At half-past nine o'clock the upper part of 19, Windmill Street, King Street, New Cut, was stripped off, and the occupiers of the upper floors had a narrow escape. At ten o'clock a sign-board was carried away from the frontage of a house in Jewry Street, Aldersgate Street. Although the street was crowded, no one was reported hurt. At Five Fields, Dulwich, the grass was strewn with broken arms from the trees, and a large elm at Norwood was blown down. A portion of a large shed situated near the Surrey Gardens Estate was unroofed. The trees in the various metropolitan parks have suffered severely from the gale. The River Thames at ten o'clock resembled a small sea, and much damage was done to the shipping below London Bridge."

And much more to the same purpose.

I feel desirous of knowing, both on general and scientific grounds, and also for obvious practical reasons, whether any explanation can be given of this absolute breakdown of weather science. It would seem to be possible that a storm can visit our coasts, and do immense destruction both by sea and land, and yet not give the faintest notice to our weather prophets of the impending danger; and it really almost makes one smile to perceive that on the day of the storm no warnings were issued, and that on the day after "the South Cone was hoisted this morning in Nos. 2, 3, 5, 7, and 8."

If no mistake has been made in the observations, and a mistake seems scarcely possible, we seem to be driven to the conclusion that a storm of the first magnitude can come upon us unawares; and if this be so, the conclusion is discouraging and very strange as regards science, and it is very serious as affecting the value of forecasts of the weather to fishermen and others.

I write this letter with the hope that some light may be thrown upon the subject to which it refers. H. CARLISLE
Rose Castle, Carlisle, October 26

The Comet

I BEG that you will allow me space for a few lines of comment upon the letters and drawings of the comet in your last issue, my own included. While thanking the engraver for the generally accurate reproduction of my sketch, it is clear that wood-engraving scarcely admits of a perfect rendering of stumped shading. A few words of correction will serve all the purpose of preserving for possible future use the evidence which I wished to put on record. The chief defect is in the *isolation* given to the "wisp," described by another correspondent as a "horn." It seemed rather to be an inclined elongation of the brightest part. The inclination too is exaggerated: its prolongation should have passed *within* the star on the northern¹ border, but

¹ The tail lies nearly along a parallel of declination.

clear of the head. The only other alteration I should desire would be the strengthening of the brightness all along the middle or axis of the tail, and the smoothing away of all other features such as now seem indicated in the body of it. Trivial as these changes may seem, the ultimate value of the drawing, if it should ever have any, must depend on its accuracy. The feebleness of the feature which attracted my attention may at the same time be inferred from its absence in the adjoining contemporaneous sketch accompanying Mr. Seabroke's letter, while its reality is proved by the descriptions in the two letters which follow. As regards the "rift" or "shadow," on which stress is laid by Mr. Williams at Cannes, one cannot help suspecting that this impression was the effect of contrast *only*—contrast between the complete absence of tail in that quarter, and the unrecognised presence of exceedingly feeble luminosity due to the extension and diffusion of cometic matter roundabout. It would require very strong evidence indeed to establish the real presence of *shadow* in the ordinary sense of that term.

One other point deserves notice. You have three contemporaneous accounts, from Rugby, Hawkhurst (Kent), and Cheltenham, all referring to the morning of October 23. Considering how rude and unsettled the weather has been for weeks past, so extensive a clearance was rather remarkable.

The brightness of this comet's tail may be inferred from an observation which I made during the current week, and which will perhaps excite as much surprise, if not incredulity, in others as it did in me. Sunday night was clear and bright, with a moon four days past the full. I was at an hotel in London, and on the stroke of three I stole into a vacant room in the third floor, the window of which looked south-east. Here I stood for a full hour looking for the comet, scarcely able to credit my senses, as the morning drew on without my seeing it. With the naked eye I could see stars of the 5th, and with a binocular, stars in Hydra of the 7th or even 8th magnitude; but no comet. At first I was uncertain, for this very reason, as to the identity of α Hydra, although if I had not been seeing the comet flaring below it so frequently during the last three or four weeks, no such doubt would have occurred to me. At last, as all the small stars of Hydra gradually settled themselves in my recollection in their right places, and I knew *exactly* where the whole length of the comet *must* be, and the whole being then well above the opposite roof, I fancied at times that I could make out a faint illumination in the proper place; but not even then, with the binocular, could I find the head; nor could I, without previous knowledge, have been able to testify confidently to the presence of the tail.

I regret that I cannot condense this account without sacrificing some of the conditions which help to make so strange a disappearance credible. If anyone had told me on the 23rd that the object I was then drawing would be invisible to me a week later, in London, *by reason of moonlight only*—for the visibility of small stars proves the clearness of the atmosphere—how could I have credited it? I feel, therefore, that I cannot expect to be believed unless the whole circumstances are told, even though they betray my uncertainty about stellar configurations when deprived of the aid of a map. J. HERSCHEL

ON Wednesday, the 25th instant, at 6.10 a.m., Mr. Hodges and I again obtained two measures of position of the nucleus with the equatorial, after correcting for instrumental errors and refraction, the mean of the readings comes out R.A. 10h. 6m. 48s., Dec. 17° 2' 55". But owing to flexure of the instrument and to the fact that the circles read only to 20" and 2s. respectively, these figures are open to correction. Daylight, with a little haze, had so far advanced when the measures were completed, that only the nucleus was distinguishable in the telescope; but with the filar micrometer I measured its length; the mean of two readings came out to 41"·5, but owing to the gradual shading off of the nucleus, one's readings might vary 5" according to its assumed limits. The width I made about 10". I was rather surprised at these results, as I had estimated its length two days before at about 10" only; but I had then used an eye-piece to which I am not accustomed, and my estimate was probably an error. The position angle of the major axis of the nucleus was 108° 7'.

Though the comet was fainter by reason of the bright moon, still we could trace the tail as far as on Monday, the 23rd.

We viewed the comet at 5 a.m., but owing to buildings in the line of sight, we got no reliable readings until 6 a.m.

In my sketch of the nucleus in your last issue, the engraver

has made it round, with a fainter elongation. It appeared of nearly the same brightness throughout. GEO. M. SEABROKE
Temple Observatory, Rugby, October 30

I SEND herewith two sketches of the comet made by me on the mornings of October 23 and 31, and a few brief particulars which may be of some value.

October 23, 1882, at 4.30 a.m., the first sketch was made. At 4 o'clock the atmosphere was exceptionally clear, and the sky continued cloudless until 5 o'clock, when a few light clouds appeared. The comet was not brilliant, although clearly seen. Nucleus with coma presented an indistinctly outlined disc a few degrees above the horizon, and obliquely upwards was a tail which stretched more than 15° across the sky. I compared the extent of tail at the time with the distance between α and β Orionis, and the tail had decidedly the best of it. Whilst glancing from the comet to Orion, I saw in the intervening sky-space, in little over three minutes, no less than *five* meteors, one of which left a long luminous trail visible several seconds. The extremity of the tail was broad. Its *under* boundary was a well-defined line about 40° from the horizontal, and was slightly convex downwards. The *upper* boundary was about 45° from the horizontal, was nearly straight, but very ill-defined, the light fading away into darkness very gradually upwards. The fanning out of the tail was very rapid towards the far end. The termination was somewhat fish-tail shaped, since there was centrally a deepish concavity between the extreme limits, which projected horn-like. The light of the tail was broken into two unequal areas by an obscure streak. The inclosed lower area was the smaller and decidedly brighter, and on its lower side contained a still brighter area, that, starting from the upper part of the coma, gradually passed into the lower boundary.

October 31, 1882, at 5.30 a.m., the second sketch was made. The atmosphere was again very clear, but the moon's light dimmed the comet greatly, and exactly at 6 o'clock it and the coming dawn rendered it indistinguishable. The naked eye could distinguish none of the features observed on the 23rd, but the general outline had somewhat changed, and the comet had changed its position relatively to the stars. ARTHUR WATTS.

Manor House, Shincliffe, Durham, October 31

MAY I beg the readers of NATURE, who possess good measures of the course of the great comet, kindly to publish them in NATURE? I would also be very much obliged for good measures of the distances of different envelopes of the head from the nucleus. The measures are desirable in two directions—*towards* the sun, and *perpendicularly* to this direction. Of the greatest scientific interest would be a complete series of measures during the whole period of visibility of the comet, and especially in the first and last days of this period. B.

"The Burman"

MR. E. B. TYLOR, in his review of "The Burman" in NATURE (vol. xxvi. p. 593), has fallen into an error which it may be well to correct. He says that the tattooing on the body of the "Greek nobleman," Georgios Konstantinos, "was evidently done by Burmese tattooers, and is a masterpiece of their unpleasant craft." This is a mistake into which even a man who had seen many specimens of Burmese tattooing, might fall. But it could never be made by a Burman. The general resemblance to the decorations on the Burman's thighs is close enough, but each separate figure, when done by the Burmese Sayah, is surrounded by a border of Burmese letters, in many cases as a mere ornament, but in not a few with a special cabalistic meaning. Still, however blurred with age, they can always be recognised as Burmese characters. I went down and examined the "tattooed nobleman," which he was good-natured enough to allow me to do very closely, and the result was to convince me that it was no native of Burma who so cruelly victimised the poor man. The frames of the figures might have been lettered, but if so, they were of some language with which I am unacquainted. Moreover, many of the figures themselves were such as a Burman Sayah never uses; such as especially the birds and serpentine creatures, while the elephants were of a very inferior character. The Beeloes (ogres) and Kyah-Beeloes (tiger-ogres), moreover, which appear on every Burman's legs, were absent, and, most conclusive of all, there was not a single inn, not one cabalistic square. No Say-Sayah I ever knew would have had self-control enough to have omitted the e signs of his wisdom in magic. Mr. Tylor says the story of Konstantinos is "mostly

fictional." That may be, but if he was not tattooed in Central Asia, it is difficult to say where it could have been done. I may also mention that the "nobleman" did not understand a single word of Burmese, and did not recognise a Burman, which could hardly have been the case if he had suffered his "punishment" in Burma. The pain, by the way, is not nearly so great as it is represented to be, and even when a man is tattooed all over the head, I cannot understand his dying or going mad, as Konstantinos's companions are said to have done. When I was tattooed, I had nearly twenty figures done at a sitting, and felt no particular inconvenience, though the actual operation is no doubt "unpleasant."

SHWAY YOE

The opinion that the "tattooed man" was decorated in Burma has been generally received by anthropologists, and so far as I know, not hitherto contradicted. In addition to Mr. Franks' paper I may now refer to the *Transactions of the Berlin Anthropological Society*, in the *Zeitschrift für Ethnologie*, vol. iv. 1872 p. 201, for an account of an examination of him by Prof. Bastian, who, as an authority on Burmese matters, has been already mentioned in connection with "Shway Yoe's" book. Prof. Bastian says, "as to the Burmese character of the tattooing there can be no doubt. The letters rather point to the Shans, to whose district many treasure-diggers resorted," &c. It appears, also, that Konstantinos, when questioned as to the mode in which he was operated on, described the instrument as a split point carried in a heavy metal handle, which agrees with the Burmese method.

As the "tattooed man" is in part inscribed with actual letters, a copy of these would probably settle the question at once. It is a pity that for some reason photographs of him, which one would think were profitable articles from the exhibitor's point of view, are not (or lately were not) to be had. E. B. TYLOR

River Thames—Abnormal High Tides

The normal high water in the Pool, or the average of all the tides of the year, is a constant quantity, and is the same now as half a century back, the mean level being 12 inches below the Metropolitan datum of high water of spring tides called "Trinity standard." High water of spring tides averages 12 inches above, and high water of neaps 3 feet 6 inches below that datum. Whilst, however, the ordinary high water is a constant quantity, exceptional tides rise now very much higher than they did a quarter of a century back; on October 18, 1841, a tide occurred which rose 3 feet 6 inches above Trinity, and it was the highest recorded for half a century; eleven years afterwards, on November 12, 1852, 3 feet 7 inches were marked. The land flood of that year is popularly known as the Duke of Wellington's flood, from the demise of the great captain having occurred at that period; no such tide recurred for seventeen years nearly, until March 28, 1869, when 3 feet 7 inches was again reached. Five years afterwards the tide rose, on March 20, 1874, higher than ever before recorded, reaching an excess of 4 feet 4 inches.

These exceptional metropolitan tides arise from the rare concurrence of three causes, viz. an exceptionally heavy land flood meeting an equinoctial spring tide, and these accompanied by a great westerly gale heaping up the Channel sea, suddenly veering to north-west, and driving the tidal wave before it from the North Sea up the Thames estuary. Four reasons may be specified for these results. The first is the greatly increased rate of discharge of floods from the catchment basin. This, however, is questioned by many; but we find Stevenson giving the ordinary discharge as 102,000 cubic feet per minute; Beardmore 100,000 as the annual mean at Staines, and 400,000 as the maximum, whilst O'Connell, in the "Encyclopædia Metropolitana," states it at from 475,000 to 600,000 and Prof. Unwin, of Cooper's Hill College, obtained results during the winter of 1875, at the Albert Bridge, Windsor Home Park, equivalent to from 701,280 to 845,640, or one-third more than any previous estimate.

Secondly, the low-water régime of the river has been greatly developed by increased scour and removal of shoals by dredging, so that the head of the low-water prism ascending from seaward, with 20 feet minimum depth, which a quarter of a century back was below the Arsenal at Woolwich, is now above the Dock Yard, two miles higher. Thirdly, the removal of old London, Blackfriars, and Westminster bridges, by raising high water above-bridge 6 to 12 inches, and lowering low water 3 to 4 feet, brings up about 33 per cent. more tidal water above-bridge than half a century back.

Fourthly, the Thames Embankments have added a few inches to the range, by narrowing, straightening, and regulating the channel by which the tidal momentum has been increased. Now, assuming that the high water of a spring tide is raised from 4 to 6 inches, this, from London Bridge to Twickenham, would amount to 700,000 tons of water, but the additional quantity, due to the removal of the old bridges within the same limits, would amount to six times that quantity, or to 4,200,000 tons.

In an essay by me, recently published by the Institution of Civil Engineers, the proportion of land water as compared with tidal water was estimated at 1-18th of the latter, and that of the 14 inches excess of range over any previously recorded tide in November, 1875, only from 3 to 3½ inches might be due to land water. The Embankment Commissioners of 1861 took the hitherto standard maximum height for quays of 4 feet above Trinity, and this proved a safe elevation until March, 1874; but the tide on November 15, 1875, was 6 inches higher, and forcibly directed public attention to the question, and again on January 2, 1877, the tide rose as high as in March, 1874, and in January, 1881, reached a height of 4 feet 8 inches at the London Docks, and 5 feet here in Westminster, the maximum yet experienced.

The Admiralty Tide Tables of the last twenty years show that 2 feet and 2 feet 1 inch are the maxima to be expected during the equinoxes, but the computers direct attention to the fact that gales of wind will add at times materially to the estimated heights; indeed north-north-west gales will add 1 yard vertically to the computed heights in the Port of London, as the surface of the water at high water will be at times 5 feet higher than at sea with a good spring tide, the tidal column rising upwards at a tolerably uniform rate of 1½ inch per mile in the forty-eight miles from Sheerness to London.

From 1860 to 1863, 6 inches was the calculated maximum above Trinity standard and that observed 3 feet and 6 inches in December 1863.

From 1864 to 1866, 6 inches was again the estimated excess, and 3 feet and 6 inches again the actual result in November 1866.

For 1867-1868 they were relatively 4 inches and 3 feet, the last in February 1868.

After this due to the altered condition of the river brought about by the causes just referred to, we have the following results as regards maxima, viz. :-

	Estimated height above Trinity.	Observed height above Trinity.	
1869—March	"	3 "	
October	1 8	—	
1870—February	—	3 0	
March	2 0	—	
1871—April	1 8	—	
1872—April	—	2 10	
September	1 7	—	
1873—February	—	3 3	
October	2 0	—	
1874—March	2 1	4 4	Westminster.
1875—April	1 10	—	
November	—	4 9	"
1876—September	1 5	—	
June-Dec.	—	1 11	
1877—January	—	4 4	"
March-Sept.	1 11	—	
1878—March	2 1	—	
November	—	3 1	"
1879—March	1 10	—	
April	—	3 6	
1880—March	1 6	—	
November	—	2 9	"
1881—January	—	5 0	"
September	1 11	—	
1882—February	—	4 6	"
Aug.-Sept.	2 1	—	

During the recent springs we have the following results (at Westminster) :-

	1882.	Estimated excess.	Actual excess.	Excess.	Wind.
Tuesday, Sept. 26, p.m.	0 5	0 12	0 7	E.S.E.	
Wednesday, ,, 27, ,,	1 4	1 9	0 5	W.	
Thursday, ,, 28, ,,	1 11	2 0	0 1	W.N.W.	
Friday, ,, 29, ,,	2 0	2 6	0 6	W.N.W.	

The early morning tide marked about 2 inches higher. During the past springs we have

Tuesday, Oct. 10, p.m.	11 below ...	6 below ...	5 ...	E.
Wednesday, ,, 11, ,,	5 ,, ...	6 above ...	11 ...	S.S.E.
Thursday ,, 12, ,,	1 ,, ...	12 ,, ...	13 ...	W.N.W.
Friday ,, 13, ,,	3 above ...	9 ,, ...	6 ...	N.N.W.

The comparatively quiet autumnal weather sufficiently accounts for the slight variations.

The tide ebbed as low as 23 feet 6 inches below Trinity in October last year at the London Docks Shadwell entrance, yielding a total tidal vertical oscillation of fully 28 feet in the Port of London.

J. B. REDMAN

6, Queen Anne's Gate, Westminster, S.W., October 19

P.S.—The springs succeeding those described in my letter show a greater difference, influenced doubtless by the great gale of Tuesday, October 24, when the barometer fell as low as in the gales of October 28 and November 16, 1880, on these three occasions reading a tenth under 29 inches. The tide of October 28, 1880, was a low neap, but on November 19, 1880, at the top of the springs estimated at 6 inches under Trinity high water it was 2 feet 9 inches above, or 3 feet 3 inches excess three days after the gale.

The excessive amount of land water now meeting the tide adds to the increase, together with the northerly gales.

	Estimated.	Observed.	Excess.	
Tues. Oct. 24, noon	0 9 below ...	0 6 below ...	0 3	{ S.S.W. gale.
Wed. ,, 25, p.m.	0 5 above ...	0 12 above ...	0 7	W.S.W.
Thurs. ,, 26, ,,	1 1 ,, ...	2 9 ,, ...	1 8	S.
Fri. ,, 27, ,,	1 6 ,, ...	4 3 ,, ...	2 9	E.N.E. ¹
Sat. ,, 28, ,,	1 7 ,, ...	5 0 ,, ...	3 5	N.N.E. ¹

In effect the last tide is identical with that of January 18, 1881.—J. B. R.

Note.—The estimate of excess due to wind over and above the forecasts is somewhat overstated in this letter, as the Admiralty heights are for London Bridge and those observed are for Westminster, where the reading will be quite 2 inches higher.

Umdhlebi Tree of Zululand

THE following note has been communicated to us by the Rev. Dr. Parker, a well-known missionary in Madagascar. The story reminds one of the old myth about the Upas in Java. No light can be thrown upon it at Kew, but perhaps in the pages of NATURE it might meet the eye of some person who could give some more information about it. W. T. THISELTON DYER

There are two species, in both the leaf is lanceolate, dark green, glossy, hard, and brittle, and from both a thick milky juice exudes, while the fruit is like a long black pod, red at the end. One species is a tree with large leaves, and peculiar looking stem, the bark hanging down in large flakes, showing a fresh growth of bark underneath: in the words of my informant, "What a villainous-looking tree! nasty, rough, ugly!" The other species is a shrub, with smaller leaves, and the bark not peeling off the stem. Both species are said to possess the power of poisoning any living creature which approaches it; the symptoms of poisoning by it being severe headache, blood-shot eyes, and delirium, ending in death. The person affected dies either in delirium, or instantaneously without any delirium. A superstition is connected with this plant. Only a few persons in Zululand are supposed to be able to collect the fruits of the Umdhlebi, and these dare not approach the tree except from the windward side. They also sacrifice a goat or a sheep to the demon of the tree, tying the animal to, or near the tree. The fruit is collected for the purpose of being used as the antidote to the poisonous effects of the tree from which they fall—for only the fallen fruit may be collected. As regards habitat, these trees grow on all kinds of soil, not specially on that which exudes carbonic acid gas, but the tree-like species prefers barren and rocky ground. In consequence of this superstition, the country around one of these trees is always uninhabited, although often fertile.

G. W. PARKER

The Origin of our Vernal Flora

It is usual to assign an Arctic origin to our mountain flora, and floral comparisons and statistics fully bear out this brilliant generalisation. It is formulated that height above the sea-level is climatically equivalent to northern latitude. This is an

¹ Gales.

assumption that flowering plants are largely conditioned by heat. Thus latitude and oreographical habitats are more or less equal.

Might I introduce another element into this question? Seeing that temperature is so largely influential in explaining the distribution of flowering plants, it occurs to me that not only may height above the sea-level answer to northern distribution, but seasonal occurrence as well.

All botanists must have been struck by the fact that the earliest plants to bloom among our vernal flora are genera peculiarly Arctic and Alpine. In some instances (as with *Chrysosplenium oppositifolium* and *C. a'ternifolium*) the species are identical. These latter plants blossom with us in March or April; within the Arctic circle not until June and July, and even so late as August. Thus, with them, seasonal blossoming is equivalent to northern latitude, as regards the thermal conditions under which they flower. The generic names of all our early flowering plants are those pre-eminently Alpine and Arctic in their distribution—*Potentilla*, *Stellaria*, *Saxifraga*, *Chrysosplenium*, *Draba*, *Ranunculus*, *Cardamine*, *Alsine*, &c. I contend, therefore, that our vernal flora is explained by the fact that their seasonal occurrence, as regards temperature, is equivalent both to height above the sea-level and northern latitude. In every instance it will be found that the blossoming of the species of the above genera necessarily takes place in Great Britain two or three months earlier than within the polar circle. May we not therefore contend that we owe our English vernal flora to the same causes as distributed our English Alpine plants; and that they are as much protected by being able to flower earlier in the year, as if they had been located on the tops of high hills and mountains?

The power to endure cold and wet displayed by many members of our vernal flora is very remarkable. Thus *Ranunculus bulbosus* and *R. acris*, *Stellaria media*, &c., are frequently found in flower all through the winter, unless the season be extra cold. Many other early bloomers among our common flowers are also remarkable for their durability, whilst the late flowering plants are equally noticeable for the short space during which they bloom. This indicates a hardihood on the part of our vernal flora which cannot be explained except by reference to the climatal experience of the species. Some of them, as the groundsel and chickweed, may have exchanged an entomophilous for an anemophilous habit, or have become self-fertilised by the change.

Again, it must have been observed that many of our early flowering plants display a tendency towards a seasonal division of labour. All of them either flower before they leaf, or show a tendency to do so, as with the Coltsfoot (*Tussilago farfara*), the Crocus (*C. vernus*), the Snow-drop (*Galanthus nivalis*), &c. Even the violets (*Viola odorata* and *V. canina*), the Daffodil, Primrose, Cowslip, &c., although they in part leaf when they flower, develop leaves much more abundantly after flowering than before, thus showing an inclination towards dividing the period of active life into two distinct stages—the reproductive and the vegetative. Everyone knows how completely this has been effected by the Meadow Saffron (*Colchicum autumnale*). My impression is that this early flowering tendency is a survival of the habit these plants had to blossom under more rigorous climatal conditions. In short, that our vernal flora must have the same origin assigned to it as an Alpine; that it has survived through being able to bloom at an early period of the year at low levels, instead of flowering at a later season higher up, above the sea-level; protection and advantage being secured in both instances.

J. E. TAYLOR

Ipswich

On Coral-eating Habits of Holothurians

BEING struck with a remark of Mr. Darwin in his work on "Coral Reefs," where it is stated on the authority of Dr. J. Allan, of Forres, that the Holothurians subsist on living coral, and that by these and other creatures which swarm on coral reefs, an immense amount of coral must be yearly consumed and ground down into mud (p. 14), I determined to commence a series of observations on this subject, in order to ascertain the rate at which these animals void the coral sand from their intestinal canal, and "ergo" the amount of coral an individual would yearly transform into sand.

I have by no means satisfied myself that the Holothurians do subsist on living coral. This may be due, however, to my field of observation being confined to the fringing reefs around Santa Anna, and the neighbouring coast of the large island of St. Christoval—where living coral occurs only in scanty patches, the greater portion of the coral "flats" being formed of coral detritus

cemented into a more compact rock. I carefully watched the habits of the two species most numerous on the "flats," and in no case did I observe a single individual browsing on the patches of living coral. In truth it was on the dead coral rock forming the "flats" of these reefs that these two species of *Holothuria* subsisted; and it appeared to me that they selected those feeding-grounds where the attachment of molluscs, zoophytes, and stony algae had to some degree loosened the surface of the rock.

The particular species, on which my observations were made to determine the amount of coral sand daily discharged, possessed a bluish-black body, from 12 to 15 inches in length when undisturbed, and with a circle of 20 palatine tentacles around the mouth. Without going into all the details of my methods of investigation, it will be sufficient to state that from three independent observations on this species of *Holothuria* I have placed the amount of coral sand daily voided by each individual at not less than two-fifths of a pound (*avoirdupois*). At this rate some fifteen or sixteen of these animals would discharge a ton of sand from their intestinal canals in the course of a year, which represents about 18 cubic feet of the coral rock forming the "flat" on which these creatures live. In order to illustrate this point more clearly, I will assume that every rood of the surface of the "flat" supports some fifteen or sixteen *Holothuria*, a number which errs rather on the side of deficiency than of excess. In the course of a year 18 cubic feet of coral rock will be removed in the form of sand from the surface of each rood, which is equal to the removal of $\frac{1}{60}$ of a foot per annum, or 1 foot in about 600 years.

Although this estimate can be only regarded as of a tentative character and as applicable to but one species of the *Holothuria*, it nevertheless throws some light on what I may term the "organic denudation" of coral reefs, and it is not unreasonable to suppose that where a fringing reef is undergoing a very gradual up-heaval, the combined operation of the fish, the mollusc, the annelid, and the echinoderm, may prevent it from ever attaining an elevation above the level of the sea at high water.

H. B. GUPPY

H.M.S. *Lark*, St. Christoval, Solomon I-lands, June 30

Railway Geology—a Hint

It must often have occurred to others as well as to myself when making a long journey by rail, and being whirled along all too fast through section after section of the greatest interest to the eye that can see in them something more than mere railway "cuttings," how valuable would be some handbook giving the geological features of the country traversed by the principal railway lines, and illustrated by clearly drawn maps and sections.

To give an instance—I have occasion pretty often to travel by the South Western line from Waterloo Station to Exeter, a route along which my untrained eye can take note of a succession of instructive pictures, in the course of a five hours' journey—the recent gravels, &c., covered by pine wood in the neighbourhood of Woking, broken abruptly at Basingstoke station by a section of the chalk, to be succeeded from here onwards to Salisbury by undulating downs of the same formation, bare of trees, and but sparsely inhabited; next, at the Yeovil junction, a sandstone quarry, riddled by martin's nests, presumably of oolitic age; then, between Axminster and Honiton the greyish blue of a cutting through the lias; to be finally succeeded, as I approach the term of my journey, by the rich red earths and loams of the new red sandstone.

Any other line, for instance, the Great Western, which runs parallel to that just instanced, would give equally varied pictures; and a copiously illustrated handbook, with notes explanatory, but as brief as possible—not only of the ground immediately bordering the line of rail, but of the general features of the neighbouring country within the range of the eye of the traveller, should surely, I venture to think, have a large circulation.

Will no geologist—a member of the Government Survey, for instance—undertake the task?

J. C. G.

New University Club, October 27

[We noticed a Guide of this kind for American railways in vol. xix. p. 287, and then suggested the utility of a similar handbook for England.—ED.]

Complementary Colours

I HAVE often noticed the complementary purple on the foam of the bluish-green waters of Alpine rivers. The waters of the

Lake of Geneva, and of the Rhone at Geneva, as is well known, are not bluish-green, but greenish-blue; but there also I have noticed what to my eye is exactly the same tint of purple on the foam.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, October 28

Palæolithic River Gravels

THE recent articles and reports in your columns on the subject of Palæolithic river gravels bring three points strongly forward, viz. :—

1. The great number of "flint implements" and "flint flakes" found in the river gravels.

2. The presence in the same deposits of bones of recent and extinct Mammalia.

3. The entire absence of the bones of man.

Such being the uniform results of persevering researches extending now for more than twenty-four years, it is surely time to request anthropologists to give (1) some explanation of the remarkable absence of human remains in deposits containing so many objects considered to be of human manufacture, and (2) some proof that it is absolutely impossible for these so-called "flint implements" and "flint flakes" to have been formed by natural causes.

C. EVANS

Hampstead, October 18

LAVOISIER, PRIESTLEY, AND THE DISCOVERY OF OXYGEN

IT is a matter of very little importance whether Lavoisier actually obtained oxygen gas a few weeks or days before Priestley. The bare bald discovery of the gas is a very minor matter when placed in juxtaposition with the astounding revolution produced in chemistry by Lavoisier; with the admirable series of experiments, the acute reasoning, the elegant logical penetration, which enabled him to overthrow the theory of Phlogiston when literally all Europe supported it. The discovery of oxygen dims and pales before the development of the theory of combustion, the theories of acidification, of calcination, of respiration, and the introduction of exact quantitative processes and instruments of precision into chemistry.

But it matters much whether the fair fame of one of the noblest and wisest men in the long roll of illustrious natural philosophers is to remain with a grievous slur cast upon it. It matters much whether his reputation is to be blasted by the reproach that he claimed the discovery of oxygen, knowing well that Priestley had preceded him.

It is with a view of removing this slur upon the memory of the founder of modern chemistry, and certainly not with any thought of adding one iota to his long list of greater triumphs, that we have examined into the true bearings of the question.

First as to the accusations. Dr. Thomas Thomson, in his "History of Chemistry," 2nd edit., 1830, vol. ii. p. 19, writes: "Lavoisier, likewise, laid claim to the discovery of oxygen gas, but his claim is entitled to no attention whatever, as Dr. Priestley informs us that he prepared this gas in M. Lavoisier's house in Paris, and showed him the method of procuring it in the year 1774, which is a considerable time before the date assigned by Lavoisier for his pretended discovery." Again, p. 106: "Yet in the whole of this paper the name of Dr. Priestley never occurs, nor is the least hint given that he had already obtained oxygen gas by heating red oxide of mercury. So far from it, that it is obviously the intention of the author of the paper to induce his readers to infer that he himself was the discoverer of oxygen gas. For after describing the process by which oxygen gas was obtained by him, he says nothing further remained but to determine its nature, and 'I discovered with much surprise that it was not capable of combination with water by agitation,' &c. Now why the expression of surprise in describing phenomena which had been already shown? And why the omission of all mention of Dr. Priestley's name? I confess that this seems to me capable of no other explanation

than a wish to claim for himself the discovery of oxygen gas, though he knew well that that discovery had been previously made by another."

Had Dr. Thomson been better acquainted with the character of Lavoisier; had he known what manner of man he was in all his dealings with his contemporaries and with the work of those who had gone before, he would never have made such an assertion as the above.

Prof. Huxley in his Birmingham address on Priestley (August 1, 1874) also accuses Lavoisier of unfairness: "though Lavoisier," he writes, "undoubtedly treated Priestley very ill, and pretended to have discovered dephlogisticated air, or oxygen, as he called it, independently, we can almost forgive him, when we reflect how different were the ideas which the great French chemist attached to the body which Priestley discovered."

Starting, as we confess, with the complete belief that Lavoisier did not discover oxygen, we are compelled to assert that a careful perusal of the various memoirs bearing upon the subject and the consistent attitude of Lavoisier throughout, has led us to the firm conviction that he has as much right to be regarded as the discoverer as either Priestley or Scheele.

Let us examine Dr. Thomson's statements. The year 1774 he asserts "is a considerable time before the date assigned by Lavoisier to his pretended discovery."

Lavoisier ("Traité élémentaire de Chimie," 1789, part 1, Chap. III.) says in speaking of oxygen: "Cette air que nous avons découvert presque en même temps, M. Priestley, M. Scheele, et moi, a été nommé, par le premier air déphlogistiqué; par le second, air empyréal. Je lui avais d'abord donné le nom d'air éminemment respirable; depuis on y a substitué celui d'air vital." Evidently "presque en même temps" is a very loose statement. Scheele's treatise, "Chemische Abhandlungen von der Luft und Feuer," was published in Upsala in 1777, and he certainly did not discover oxygen before 1775. Lavoisier is therefore speaking in quite general terms when he says that oxygen was discovered almost at the same time by Priestley, Scheele, and himself. He at least puts himself on a level with Scheele as to date, and it is universally admitted that Scheele procured the gas after Priestley. And this general expression is the only claim to the discovery we can anywhere find in the writings of Lavoisier.

Now what are the facts in favour of Lavoisier? On November 1, 1772, he deposited with the secretary of the Academy a note, which was opened on May 1 following, in which he stated that he had discovered that sulphur and phosphorus, instead of losing weight when burnt, actually gained it, without taking into account the humidity of the atmosphere. He traced this to the fixation of air during the combustion, and surmised that the gain of weight by metals during calcination was due to the same cause. He reduced litharge in close vessels "avec l'appareil de Hales," and observed the disengagement of a great quantity of air. "This note leaves no doubt," says Dr. Thomson, "that Lavoisier had conceived his theory, and confirmed it by experiment, at least as early as November, 1772. . . . "Il est aisé de voir," writes Lavoisier, just before his death, "que j'avais conçu, dès 1772, tout l'ensemble du système que j'ai publié depuis sur le combustion."

Early in 1774 he published experiments in his "Opuscules physiques et chimiques," to prove that lead and tin, when heated in closed vessels, gain weight, and cause a diminution in the volume of air. "J'ai cru pouvoir conclure," he writes, "de ces expériences, qu'une portion de l'air lui-même, ou d'une matière quelconque, contenue dans l'air, et qui y existe dans un état d'élasticité, se combinait avec les métaux pendant leur calcination, et que c'était à cette cause qu'était due l'augmentation de poids des chaux métalliques." Later in the year he read before the Academy ("à la rentrée publique de la

Saint Martin, 1774"); a memoir "On the calcination of tin in closed vessels," in which he proved that when tin was calcined in hermetically sealed vessels, it absorbed a portion of the air equal in weight to that which entered the retort when it was unsealed, so as to admit air. He states as his conclusion that only a part of the air can combine with metals or be used for purposes of respiration, and that hence the air is not a simple body as generally believed, but composed of different substances; and he adds that his experiments on the calcination of mercury, and the revivification of the calx, singularly confirm him in this opinion.

At the Easter Meeting of the Academy in 1775, Lavoisier read a memoir, "Sur la nature du principe qui se combine avec les métaux pendant leur calcination et qui en augmente en poids." In a footnote we are informed that the first experiments described in the memoir were made more than a year previously, while those relating to the mercury *precipitatus per se*, "ont d'abord été tentées au verre ardent dans le mois de Novembre, 1774." Having heated calx of mercury with carbon, he found that fixed air soluble in water was given off, while when he heated it alone he observed *avec beaucoup de surprise* that an air was produced insoluble in water, readily supporting combustion, serving for the calcination of metals; incapable of precipitating lime water, and incapable of being absorbed by alkalies.

Priestley obtained a gas from mercury, *calcinatus per se*, on August 1, 1774, and finding it insoluble in water, and capable of readily supporting combustion, concluded that the mercury during calcination had absorbed *nitrous particles* from the air. He did not discover the real nature of the gas till March, 1775. In October, 1774, Priestley visited Paris, and mentioned to Lavoisier, Leroi, and others the production of gas from the mercury *calcinatus per se*. Probably the properties were not demonstrated. Lavoisier says he observed "with much surprise" that the gas was not absorbed by water, &c., was not in fact fixed air. He had expected to find the air given off by calx of mercury when heated alone, the same as that evolved when he tested it with charcoal, and was surprised to find it a different air. He enumerates the principal properties of the new gas as we know it. He burns it in a candle, charcoal, and phosphorus. He calls it air *éminemment respirable*, and *air pur*; and says it alone is concerned in respiration, combustion, and the calcination of metals.

Lavoisier constantly quotes Priestley and Scheele in connection with oxygen; again and again he speaks of that air which Mr. Priestley calls *dephlogisticated*, M. Scheele *empyreal*, and I *highly-respirable*,³ but we can find no distinct claim to its discovery save the sentence quoted above, in which he states that it was discovered *almost at this same time* by Priestley, Scheele, and himself.

In his next memoir, "On the Existence of Air in Nitrous Acid" (read April 20, 1776), he says: "Je commencerai, avant d'entrer en matière, par prévenir le public qu'une partie des expériences contenues dans ce mémoire ne m'appartiennent point en propre; peut-être même, rigoureusement parlant, n'en est-il aucune dont M. Priestley ne puisse réclamer la première idée." And again: "Je terminerai ce mémoire comme je l'ai commencé, en rendant hommage à M. Priestley de la plus grande partie de ce qu'il peut contenir d'intéressant." Moreover, in giving an account of ammonia, sulphurous acid, and several other gases, he writes: "Les expériences dont je vais rendre compte appartiennent presque toutes au docteur Priestley; je n'ai d'autre mérite que de les avoir répétées avec soin, et surtout de les avoir rangées dans un ordre propre à présenter des conséquences." Thus it must be admitted that Lavoisier was always ready to acknowledge the merits of Priestley.

Even supposing that Priestley had demonstrated the

production of oxygen to Lavoisier before he had himself obtained it, which, however, does not appear probable, Lavoisier investigated its chief properties before Priestley knew any more of it, than it was a gas containing nitrous particles. "Till this first of March, 1775," writes Priestley, "I had so little suspicion of the air from *mercurius calcinatus* being wholesome, that I had not even thought of applying to it the test of nitrous air." Again, in speaking of an experiment made on March 8, 1775, he says: "By this I was confirmed in my conclusion that the air extracted from *mercurius calcinatus*, &c., was at least as good as common air; but I did not certainly conclude that it was any better." At this time Lavoisier had proved the principal properties of the new gas, as we now know them. No wonder he expresses surprise. Did Paracelsus discover hydrogen? or did Boyle? or Mayow? or Cavendish? Lavoisier saw with much surprise, not that a gas was produced by heating calx of mercury, but that the gas was different from fixed air.

Let us finally examine Dr. Thomson's criticism of the "Opuscules Physiques et Chimiques":—

"Nothing in these essays," he writes, "indicates the smallest suspicion that air was a mixture of two distinct fluids, and that only one of them was concerned in combustion and calcination; although this had been already deduced by Scheele from his own experiments, and though Priestley had already discovered the existence and peculiar properties of oxygen gas. It is obvious, however, that Lavoisier was on the way to make these discoveries, and had neither Scheele nor Priestley been fortunate enough to fit upon oxygen gas, it is exceedingly likely that he would himself have been able to have made that discovery."

Now these essays were published "au commencement de 1774," at which time we have abundant evidence from other memoirs that Lavoisier had more than suspicion "that air was a mixture of two distinct fluids, and that only one of them was concerned in combination and calcination." Moreover, this had not "been already deduced by Scheele from his own experiments; neither had Priestley "already discovered the existence and peculiar properties of oxygen gas."

We do not the least press the following point. We trust we have made out our case without the necessity of resorting to it; but we venture to ask upon what authority Dr. Thomson asserts that "Dr. Priestley informs us that he prepared this gas in M. Lavoisier's house in Paris, and showed him the method of procuring it in the year 1774." In our edition of Priestley's works (3 vols. 8vo. "Being the former six volumes abridged and methodised with many additions." Birmingham: Thomas Pearson, 1790), Priestley, after telling us that he visited Paris in October, 1774, says, "I frequently mentioned my surprise at the kind of air which I had got from this preparation to M. Lavoisier, Mr. Le Roy, and several other philosophers, who honoured me with their notice in that city" (p. 109). And again, "as I never make the least secret of anything I observe, I mentioned this experiment also, as well as those with the *mercurius calcinatus*, and the red precipitate to all my philosophical acquaintances at Paris and elsewhere; having no idea at that time, to what these remarkable facts would lead." It is of course a very different thing to mention an experiment to an acquaintance, and to actually perform it before him. But suppose, as Dr. Thomson asserts, that Priestley had prepared the gas from *mercurius calcinatus* in Lavoisier's house in October 1774, it is abundantly manifest by his own confession that he had no idea at that time of the nature of the gas; and more than five months afterwards that he had "so little suspicion of the air from *mercurius calcinatus* being wholesome, that I had not even thought of applying to it the test of nitrous gas"; and even so late as March 8, 1775, he did not conclude that the new gas was any better than common air!

Who is the discoverer? Is it the man who obtains a new body for the first time without recognising that it is different from anything else, or is it the man who demonstrates its true nature and properties? If the former Eck de Sulzbach discovered oxygen in 1489, and Boyle in 1672 not only procured hydrogen but proved its inflammability. If the latter, assuredly Lavoisier discovered oxygen.

But whatever the verdict may be, the memory of Lavoisier shall be saved from any imputation of unfairness. He was the most generous of men. His noble character stands out clearly and luminously in all his actions. He was incapable of any meanness.

We cannot for one moment compare the work of Priestley with that of Lavoisier. The elegant methods and admirable diction of the latter contrast strangely with the clumsy manipulation and prosy phlogistianism of the former. "From an ounce of red lead," writes Priestley, "heated in a gun-barrel, I got about an ounce measure of air, which altogether was worse than common air, an effect which I attribute in great measure to phlogiston discharged from the iron. The production of air in this case was very slow." Then he heated, without method or reason, as Hales had done before him, "flowers of zinc, chalk, quicklime, slacked lime, tobacco-pipe clay, flint, and muscovy talck, with other similar substances, which will be found to comprise almost all the kinds of earth that are essentially distinct from each other, according to their chemical properties," in the hope of getting some phlogisticated air from them. What a tarrago! John Mayow, a century earlier, wrote more scientifically: "Si ad flammæ naturam serio attendamus, et nobiscum cogitemus, qualem demum mutationem particulæ igneæ subeunt, dum eadem accenduntur: nihil aliud certe concipere possumus, quam particularum ignearum accensionem in motu earum perniciosissimo consistere. Quidni ergo arbitremur, particulas salinas ad ignem constandum præcipue idoneas esse? Quæ cum maxime solidæ, subtiles, agilesque sint, motui velocissimo, igneque obeundo multo aptiores esse videntur, quam particulæ sulphuræ, crassiores mollissimæque."

Priestley's observations read like the writings of the seventeenth century, Lavoisier's like those of the nineteenth. Compare with the extract given above about the "phlogiston discharged from the iron" the following, "I have," writes Lavoisier, "a salt of unknown composition: I put a known weight in a retort, add vitriolic acid and distil. I obtain acid of nitre in the receiver, and find vitriolated tartar in the retort, and I conclude that the substance was nitre. I am obliged in this reasoning to suppose that the weight of the bodies employed was the same after the operation as before, and that the operation has only effected a change." "J'ai donc fait mentalement une équation dans laquelle les matières existantes avant l'opération formaient le premier membre, et celles obtenues après l'opération formaient le second, et c'est réellement par la résolution de cette équation que je suis parvenu au résultat. Ainsi, dans l'exemple cité, l'acide du sel que je me proposais d'examiner était une inconnue, et je pouvais appeler x . Sa base m'était également inconnue, et je pourrais l'appeler y ; et puisque la quantité de matière a du être la même avant et après l'opération, j'ai pu dire $x + y + \text{acide vitriolique} = \text{acide nitreux} + \text{tartre vitriolé} = \text{acide nitreux} + \text{acide vitriolique} + \text{alcali fixe}$; d'où je conclus que $x = \text{acide nitreux}$, $y = \text{acide fixe}$, et que le sel en question est du nitre."

There is nothing in Priestley's scientific writings which exhibits so masterly a treatment as this. Priestley ignored Lavoisier's brilliant conclusions. He died defending the theory of Phlogiston. He denied the decomposition of water. He worked without method or order; and without the balance; and reasoned upon facts which lacked verification by quantitative means.

His conclusions were frequently hasty and ill founded. Lavoisier's work requires no praise in this place. Priestley's discoveries may be compared to the mingled chaos of *ἁπομειψία* of Anaxagoras; Lavoisier was the *Noûs*, the designing intelligence which set them in order, and put each in its appointed place. Not without reason, said M. Wurtz, "La Chimie est une science française. Elle fut instituée par Lavoisier d'immortelle mémoire."

G. F. RODWELL

A NEW DREDGING IMPLEMENT

HAVING recently visited Oban, in company with a friend for the express purpose of obtaining living specimens of Pennatulida, and of testing the powers of an instrument devised for their capture, I send you a note of our experiences which may perhaps be of interest to your readers.

The ordinary dredge, though well adapted to obtaining most animals that dwell on the sea-bottom, will clearly not do for all, and for no animal form is it less suited than for the one we were most anxious to obtain—*Funiculina quadrangularis*. This giant Pennatulid consists of a tall fleshy rod-like axis, three to five feet or more in length, and about half an inch in diameter, which bears along its sides the individual polypes of the colony, and is traversed throughout its entire length by a flexible calcified stem. *Funiculina* lives erect, with the lowermost six or eight inches planted as a stalk in the mud of the sea-bottom, and the major portion of its length projecting up freely into the water.

For such a form the dredge is clearly very unsuitable. Indeed unless the dredge be of very great size it must be a pure accident if specimens ever get into it at all. The tangles give a better chance, and yet for such a purpose they are but a clumsy and haphazard contrivance; and even should they by chance entangle and draw out a *Funiculina* there is a danger, amounting almost to certainty that it will drop off again during the process of hauling in.

The instrument we employed was a modification of one originally devised by Dr. Malm of Göteborg, and used by him with considerable success in dredging for *Funiculina* in Gullmarn Fjord, Bohuslän. Dr. Malm's apparatus, of which he has kindly furnished us with a description and drawings, consisted of three poles, each nine feet long, connected together at their ends, so as to form a triangle; the poles were armed with large-sized fish-hooks, and the dredging-rope attached at one angle, the whole apparatus strongly resembling that used by the Philippine Islanders for dredging *Euplectella*, as described and figured by Moseley (*Naturalist on the Challenger*, p. 407).

Our instrument, as we first used it, consisted of two poles six feet long, connected together in the form of a letter A by a cross-bar four feet long. The rope was fastened to the apex of the A, and lead weights to the lower ends of the side poles. Attached along the cross-bar at intervals of six inches were cords four feet in length, each armed with five or six fish-hooks and having a small lead weight tied to its lower end. The theory of the machine was that the whole instrument would be dragged along at an angle of about 30° to the sea-bottom, steadied by the weights at the ends of the side poles; the cross-bar being a foot or so above the ground, and the cords armed with fish-hooks trailing behind, with their ends kept on the bottom by the small weights attached to them.

The machine was subsequently modified by lengthening the cross-bar to nine feet, and attaching the fish-hooks not singly, but in threes, like grappling irons. We also connected the cords together by horizontal strings, in order to obviate their tendency to become entangled with one another.

The instrument yielded excellent results: a large number of specimens of *Funiculina quadrangularis* were obtained, four or five, and in one case as many as seven being brought up at a single haul; the specimens were also in perfect condition, the injury inflicted by the hook being quite imperceptible. Several of the specimens were of large size; and one dredged in Ardmucknish Bay, and measuring no less than sixty-five inches in length, appears to be the largest specimen hitherto obtained alive from any locality, being a foot longer than the largest recorded by Kölliker in his monograph on the Pennatulida. Even this, however, does not appear to be the limit of growth, for a dead stem obtained at Glaesvae, in the Bergen Fiord, and now in the Hamburg Museum, is more than seven feet in length.

Funiculina quadrangularis is generally considered a rare species. It is certainly a very local one; but our Oban experience would lead us to infer that where it does occur it is to be found in quantity, an inference borne out by Sir Wyville Thomson, who speaks of passing over a "forest of *Funiculina*" when dredging in Raasay Sound during the *Porcupine* expedition. It appears to have been hitherto obtained at Oban only in small numbers, a result we believe to be due entirely to the use of instruments ill-adapted to its capture.

Four or five specimens of *Pennatula phosphorea* were obtained with the same instrument, which further proved its utility by bringing up several fine specimens of Hydrozoa. The instrument in its present form is clearly capable of improvement; still the results of a first trial have been so good, that we may possibly be rendering a service to other naturalists by making them known through your columns.

A. MILNES MARSHALL

Owens College, October 27

WIRE GUNS

IT will no doubt surprise many of our readers to be told that after nearly a quarter of a century of experiment and investigation, and the expenditure of millions upon millions of money, the nation is so imperfectly armed that we are again entering upon a period of reconstruction of our heavy ordnance, the outcome of which it is not easy to foresee. From the old cast-iron 68 pounder, weighing from 4 to 5 tons, we have arrived at the 80 ton gun of Woolwich, but only to learn that such guns are already obsolete, and must give place to others of a new type developing greater power with less weight. Till very recently we have been constantly told by the highest authorities in this department of the Government that the English guns were the finest, the strongest, and the most powerful in the world, and it is no doubt somewhat startling to learn that all this has been a delusion.

It is not our intention to dwell upon the causes of this, nor to inquire whether it has been due to departmental conservatism or to the uncertainty incidental to the progress of an art carried on by a tentative method, and modified from time to time by new discoveries in physical science. Our purpose is rather to give some information about a system of gun making, which is at last obtaining the attention of gunmakers, we allude to what is termed the wire system of construction.

Twenty-seven years ago this system was brought before the then existing Ordnance Committee by the writer who has from that time to this persistently advocated its merits, proving, not only by the construction of guns but also by mathematical analysis, its great advantage over other systems; but it is only within the last two or three years that it has been regarded with tolerance by practical gun makers.

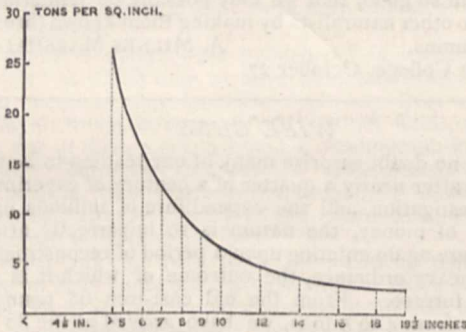
In France the system has been applied under the superintendence of Capt. Schultz, of the École Polytechnique, and in this country Sir Wm. Armstrong and Co. have made one or two guns, the latest and largest of

which is now under trial at Woolwich. So far as these guns have been tried they have given very exceptionally good results, both in France and England, and they promise to excel all others in strength, facility of construction, and economy as regards cost. Let us then attempt to explain in a popular manner the principles and methods of this system of construction.

A gun is a machine the object of which is to send heavy bodies to a great distance at a very high velocity. The motive power acts on the body for a very short time, a fraction of a second only, it must therefore be of great intensity, and consequently the machine must have very great strength. Formerly all guns were made of cast-iron or bronze; after this wrought iron and steel came into use or a combination of the two, Krupp and Whitworth adopted steel, Armstrong and Woolwich a combination of wrought-iron and steel, Palliser again, a combination of cast and wrought-iron.

In making a vessel to resist great internal pressure, it was natural to conclude that by increasing the thickness of the vessel, its resisting strength could be proportionately increased, but as was first pointed out by the late Prof. Barlow, it was found that the limit in this direction was very soon reached, and that no vessel, whatever the thickness, could resist an internal pressure greater than the tensile strength of the material of which it was made.

If the cylinder be composed of a material whose tensile strength is 10 tons per square inch, and if the internal pressure be 10 tons per square inch, and if the cylinder be conceived as to be divided into a great number of



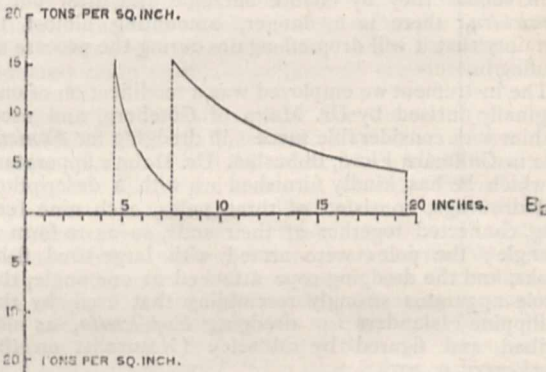
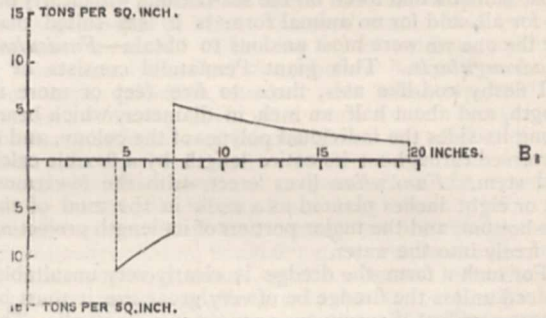
successive indefinitely thin layers, then, whatever be its thickness, the first of these layers will be strained to 10 tons, its maximum strength, the next layer will be strained less, and the strains will go on decreasing according to a fixed law as we proceed outwards. Now these outer layers cannot exert any more force, except it be transmitted from the innermost one, and consequently any further assistance can only be got from them by increasing the strain of the innermost layer, which, being already strained to its maximum strength, must necessarily give way.

In order to meet this radical defect in all homogeneous cylinders the principle of initial tension was adopted. This was done by building up the cylinder of several concentric rings, or hoops, each of which was put on the one below it with an initial strain, thus compressing all those below. If now, by this method, the innermost hoop or tube be put into a state of compression of, say, 5 tons per square inch, it is evident that the first thing the internal pressure has to do, is to remove this compression to zero. This will absorb 5 tons per square inch of pressure. It has then to overcome the tensile strength of the material, or 10 tons per square inch, which requires an additional pressure of 10 tons per square inch. Thus the resisting force of the cylinder has been increased from 10 to 15 tons per square inch.

Now the greater the number of the hoops in a given thickness of cylinder, the greater is the additional strength imparted, provided that each hoop is put on with the

proper initial strain, and if the hoops were infinite in number and therefore infinitely small in thickness, we could obtain the maximum strength for the thickness of cylinder, and each ring would, at the moment of rupture, be strained to its maximum tensile force. In such a cylinder the strength would increase in the exact ratio of the increase of thickness, and when it burst every layer would give way at the same time, but as there is no limit to the possible increase of thickness, there is also no limit to the possible increase of the internal pressure. Of course this theoretical construction is practically impossible, but we can approach to it very closely by making the hoops very numerous and very thin. The limit of the number of hoops is however very soon reached in the system of hoop construction.

Sir Wm. Armstrong's 100-ton gun is built up of a steel tube and three wrought-iron hoops on it. The Woolwich 81-ton gun has a steel tube and two wrought-iron hoops. Sir Wm. Armstrong's gun is therefore a better gun than



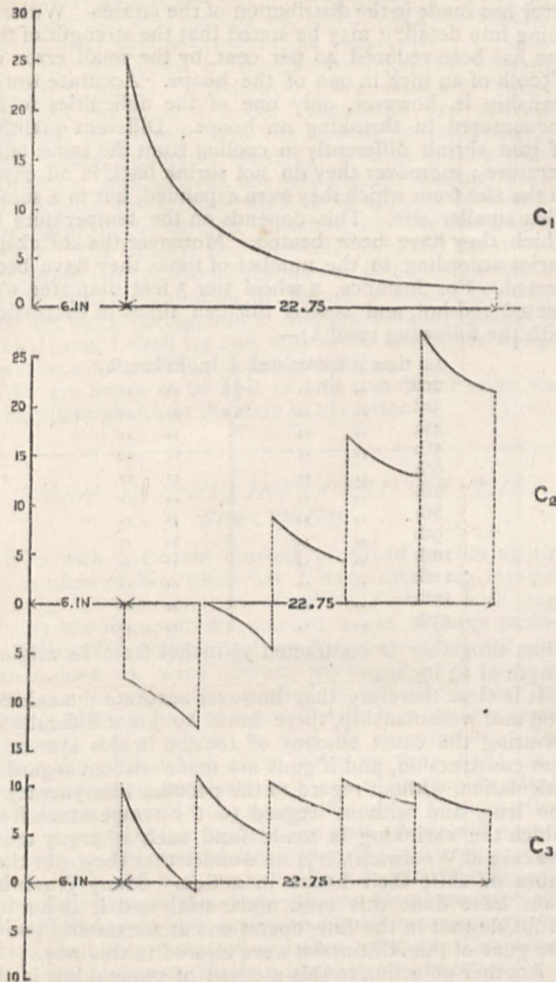
the Woolwich, assuming in both cases that the initial tensions are correctly adjusted, but if in either case the number of hoops had been doubled, the total thickness remaining the same, both guns would have been greatly increased in strength. The practical difficulties of increasing the number of rings are, however, very great, and the expense would be enormous. The proper initial tension, or *shrinkage* as it is called, depending on extreme accuracy of workmanship, would be extremely difficult of attainment, and Sir Wm. Armstrong has probably gone nearly as far as is practically possible in this direction.

The regulation of the initial tension in guns of the hoop construction is so important that it is necessary to go somewhat more into detail, in order that our readers may thoroughly understand its importance, and be in a position to appreciate the advantages attendant on the use of wire.

We therefore introduce to their notice a series of diagrams showing the distribution of the strains throughout the thickness of a gun. The first is the case of a

homogeneous gun, such for instance as a solid cast-steel gun as formerly made by Krupp, and we will assume it to be 9 inches calibre, and 15 inches thick at the breech end, and that it is subjected to an internal pressure of 24 tons per square inch. Now it is evident that the total strain to be resisted is 9 times 24 tons, or 216 tons, one half of which, or 108 tons must be borne by each side of the gun, or by a thickness of 15 inches of steel. If therefore the strain could be uniformly distributed, it would not exceed $\frac{108}{15}$, or 7.2 tons per square inch, but in reality the strain at the inside circumference would be nearly 27 tons per square inch, whilst at the exterior of the gun it would be only $2\frac{1}{2}$ tons per square inch.

The subjoined diagram (A) represents the condition of



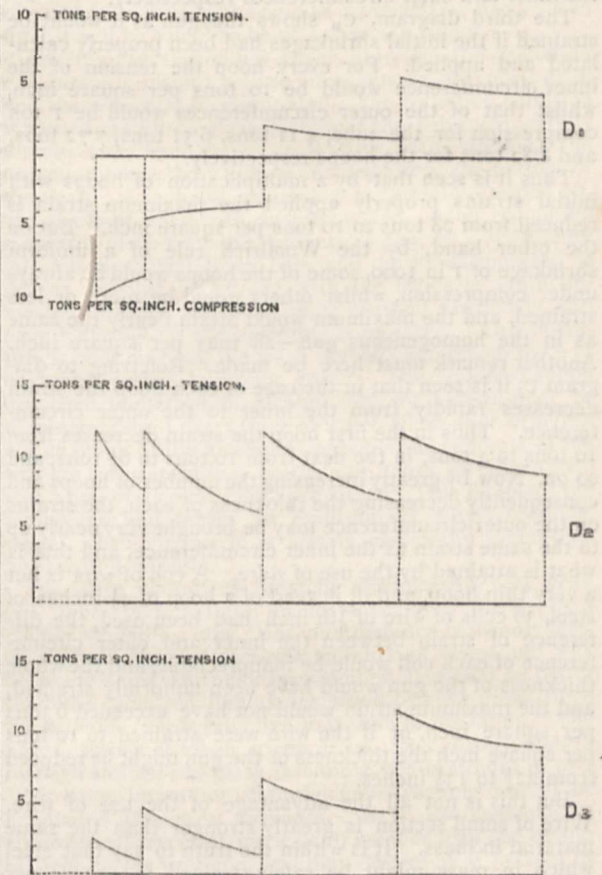
strain of such a gun under these circumstances. The abscissæ denote the distances from the centre of the bore, whilst the corresponding ordinates denote the strains in tons per square inch at these distances.

In the next place let us examine the condition of strain of a gun of the same calibre, but composed of an internal steel tube $3\frac{1}{8}$ inches thick upon which is shrunk a wrought-iron hoop $12\frac{1}{4}$ inches thick with a shrinking of 1 in a thousand. This was the Woolwich construction for all guns up to 9-inch calibre up to 1869.

Subjected to an internal pressure of 24 tons per square inch, the diagram B_2 shows the induced strains. Previous to the internal pressure being applied, diagram B_1 shows that the steel tube would be compressed by the outer wrought-iron hoop. The compression would

be 11.7 tons per square inch at the inner and 7.86 tons at the outer circumference; on the other hand the wrought-iron hoop would be in a state of tension, 5.19 tons per square inch at the inner and 1.38 tons at the outer circumference. When the internal pressure of 24 tons per square inch is applied, the diagram B_2 shows the condition of strain. The steel tube would be strained to 15.53 tons per square inch at the inner, but only to 2.67 tons at the outer circumference, whilst for the wrought iron hoop the strains would be 15.09 and 4 tons respectively per square inch. Thus it appears that comparing this gun with the homogeneous gun of the same size and under the same conditions the maximum strain has been reduced from 27 tons to 15.83 tons per square inch.

Pursuing the matter further let us examine the conditions of Sir Joseph Whitworth's 12-inch gun, built up of a steel tube 4.35 inches thick, on which are placed four successive steel hoops, each of 5.55 inches thick, the



total thickness of the gun being thus $22\frac{1}{2}$ inches. Before proceeding to the examination of the strains in this gun, it is desirable to devote a moment or two to the very important question of the amount of initial strains with which hoops should be put on. The Woolwich practice is to adopt a uniform shrinkage of 1 in a 1000, that is to say, the internal diameter of each hoop is $\frac{999}{1000}$ ths of the external diameter of the hoop below it. The outer hoop is expanded by heat, placed over the inner one, and then in cooling grips it with the force due to a contraction of $\frac{1}{1000}$ th of its size. This is a fundamental error in the Woolwich practice, and it is mainly from their persistence in this error that so many Woolwich guns have failed. The proper amount of shrinkage is not a fixed amount. It depends on the thickness of the rings, their position in the structure, and the modulus of elasticity of the material, and it is only by a due regard to these

elements of the problem that the advantages of the hoop system can be properly developed.

In illustration of this we refer to three diagrams of Sir Joseph Whitworth's 12-inch steel gun. The first, C_1 , shows the strains, if the hoops are put in with no initial strain, that is to say, if each hoop is an exact fit to the one below it, which is Sir Joseph's present practice. The gun in this state is in the same condition under internal pressure as a homogeneous or solid gun of steel. The tensions with an initial pressure of 24 tons per square inch would be 28.18 tons and 2.3 tons per square inch at the inner and outer circumference respectively. The second diagram, C_2 , would be the state of the strains, if the Woolwich rule of a uniform shrinkage of 1 in 1000 were adopted. The inner tube and the first hoop would never be out of compression, the second hoop would be strained to 8.44 tons and 3.85 tons, the third ring to 17.40 tons and 12.84 tons, and the fourth ring to 27.64 tons and 22.82 tons at the inner and outer circumferences respectively.

The third diagram, C_3 , shows the gun as it would be strained if the initial shrinkages had been properly calculated and applied. For every hoop the tension of the inner circumference would be 10 tons per square inch, whilst that of the outer circumferences would be 1 ton compression for the tube, 4.11 tons, 6.51 tons, 7.72 tons, and 8.82 tons for the hoops respectively.

Thus it is seen that by a multiplication of hoops with initial strains properly applied the maximum strain is reduced from 28 tons to 10 tons per square inch. But on the other hand, by the Woolwich rule of a uniform shrinkage of 1 in 1000, some of the hoops would be always under compression, whilst others would be more or less strained, and the maximum would attain nearly the same as in the homogeneous gun—28 tons per square inch. Another remark must here be made. Referring to diagram C_3 it is seen that in the case of each hoop the strain decreases rapidly from the inner to the outer circumference. Thus in the first hoop the strain decreases from 10 tons to 4 tons, in the next from 10 tons to 6½ tons, and so on. Now by greatly increasing the number of hoops and consequently decreasing the thickness of each, the strains on the outer circumference may be brought very nearly up to the same strain as the inner circumference, and this is what is attained by the use of *wire*. A coil of wire is but a very thin hoop, and if, instead of a hoop of 4½ inches of steel, 36 coils of wire of ¼th inch had been used, the difference of strain between the inner and outer circumference of each coil would be inappreciable, and the whole thickness of the gun would have been uniformly strained, and the maximum strain would not have exceeded 6 tons per square inch, or if the wire were strained to 10 tons per square inch the thickness of the gun might be reduced from 22¾ to 13½ inches.

But this is not all the advantage of the use of wire. Wire of small section is greatly stronger than the same material in mass. It is within the truth to say that steel which in mass might be safely strained to 15 tons per square inch, might in the form of wire be strained to 30 tons per square inch. Consequently the wire gun would be as safe under a strain of 20 tons as the hoops under 10 tons, and therefore the thickness of a wire gun of equivalent strength to that represented in diagram C_3 might be reduced to 6¾ inches instead of 22¾ inches.

From the preceding remarks and the diagram of Whitworth's 12-inch gun, it will be seen how very important is the question of the degree of shrinkage in built up guns. It is worth while to dwell a little longer upon this question, and to illustrate it we now give diagrams showing how the strength of a gun may be reduced by a small difference in the shrinking such as would be caused by a slight error in the dimensions of one of the hoops, due either to miscalculation, imperfect workmanship, or irregular contraction in cooling. The diagrams D_1 and D_2 represent the strains on the hoops of an 8-inch gun, built

up of an inner tube and three concentric hoops of iron having an elastic limit of 12 tons per square inch. D_1 shows the strains when the gun is completed and free from internal pressure, on the hypothesis that the shrinkages are correctly calculated and accurately worked too. The tube and first hoop are in compression, the two outer rings in tension. D_2 represents the strain when subjected to internal pressure, so as to make the maximum strain 12 tons per square inch, and it is seen that all the hoops are equally strained up to the elastic limit. D_3 shows the strain in the same gun on the hypothesis that either from miscalculation or inaccurate workmanship the outer hoop has been made 1/500th of an inch too small, and when by internal pressure the maximum strain reaches 12 tons per square inch.

It is apparent at a glance what a great difference this error has made in the distribution of the strains. Without going into detail, it may be stated that the strength of the gun has been reduced 40 per cent. by the small error of 1/500th of an inch in one of the hoops. Accurate workmanship is, however, only one of the difficulties to be encountered in shrinking on hoops. Different qualities of iron shrink differently in cooling from the same temperature; moreover they do not shrink back in all cases to the size from which they were expanded, but to a somewhat smaller size. This depends on the temperature to which they have been heated. Moreover the shrinkage varies according to the number of times they have been heated. For instance, a wheel tier 7 feet diameter was heated red-hot, and cooled thirteen times in succession with the following results:—

Ist time it contracted	in. in length.
2nd	1 1/10
3rd	1 1/10
4th	1 1/10
5th	1 1/10
6th	1 1/10
7th	1 1/10
8th	1 1/10
9th	1 1/10
10th	1 1/10
11th	1 1/10
12th	1 1/10
13th	1 1/10

Thus altogether it contracted 5 3/8 inches from its original length of 22 inches.

It is clear therefore that however accurate the calculation and workmanship, there must be great difficulty in ensuring the exact amount of tension in this system of gun construction, and if guns are made without regard to calculation, without regard to the peculiar idiosyncrasy of the iron, and without regard to the temperature from which the shrinking is made (and such is pretty much the case at Woolwich), it is no wonder that they split their tubes or shift their hoops in action. Many Woolwich guns have done this even under trial, and it is not improbable that in the late operations at Alexandria two of the guns of the *Alexandra* were injured in this way.

Another objection to this method of gunmaking is the possibility of latent defects in the hoops. It is impossible always to detect a flaw, even of considerable magnitude, in a hoop of iron or steel 10 to 18 inches thick such as are used in the large Woolwich guns, and such latent flaws may prove fatal to the gun even if in other respects it were properly constructed.

JAMES A. LONGRIDGE

(To be continued.)

MR. FORBES' ZOOLOGICAL EXPEDITION UP THE NIGER

MR. W. A. FORBES writes from Lokoja, on the Niger, at the confluence with the Binué (September 9) as follows:—I have been here on and off

about a fortnight, and have been up the Binué as far as Loko, about 100 miles, where I got some birds. Altogether up to the present I have seen or got about 80 species of birds, including *Scopus*, *Plotus*, *Indicator*, and *Rynchops*; as yet no *Podica*, *Irrisor*, or *Musephagidae*. Of Hornbill I have seen 3 or 4 species, but they are very shy, and as yet I have not shot one. Ploseine birds are the feature here; about 1-3rd of the species are of that family, and some I have are good ones, especially *Estrelida nigricollis* and *E. rara*, both of them discovered by Heuglin. These and other things make me fancy that we are out of the true West African region here; the antelopes seem also eastern. There are 4—5 here, including a brown *Hippotragus*, and what I fancy is *Alcelaphus tora*. I have skins and horns of these, and shall get others. *Bos brachyceros* is common here, but as yet I have only seen spoor, not the beast itself. We saw lots of Hippopotamuses coming up, and I killed the second I shot at, but could not recover the body.

I have also killed a large crocodile, 15 feet long, apparently *C. acutus*. I have also a few fishes and reptiles, and shall get more I hope. Butterflies are not very numerous at present, and the country is too open for them, being, generally speaking, a large grassy plain, with lots of isolated trees, not very big, and bushes. There is no regular thick forest up here at all, and even in the lower river, in the delta, it is nothing like the Neotropical forests. The weather has been very dry, and the river is still rising. After leaving Bidda our plans are uncertain. Mr. M. talks of going on to Sokoto, if he can get away from his stock-taking, and if he goes I shall probably go too. If not, I shall try and stay some time at Ischungu, a station a little off the river above Egga.

We are happy to be able to add that Mr. Forbes was in excellent health at the date of his letter.

WORK IN THE INFRARED OF THE SPECTRUM

IT is with a certain amount of dread of boring the readers of NATURE, that I have taken up my pen to write on the method of photographing with rays of very low refrangibility, since it ought to have passed the limits of novelty. And yet I suppose it has not altogether done so, since almost weekly, I have inquiries made as to where the method is described, and am questioned as to how to succeed with it, when my correspondents know where to find its description. The Editor, also, has asked me to write on the subject, so I propose to put as concisely as I can what plan to adopt. It is almost too well worn a scientific adage to repeat that unless you can obtain a sensitive salt which will absorb the rays to be used photographically, you cannot hope for success; and the method which I shall describe presently fully secures this desideratum. To photograph the red and dark rays then a sensitive salt must be procured which shall absorb the red and ultra-red rays. The colour of the salt to aim at then is a bluish green, which gives a continuous absorption at the least refrangible end of the spectrum. The salt employed is bromide of silver in a modified molecular state, a state I may say which is very easy to obtain when the formula below is strictly carried out, but very easily missed if the experimenter is self-inspired to make improvements in the method of procedure. I don't know whether it is something peculiar to photographic minds that there is in them such a large amount of self-assurance, but my frequent experience is that those who try a formula for a photographic preparation invariably try to improve on it before giving the original one a chance of success: and then when failure occurs they blame everything and everybody except their own conceptions. May I ask those who read this and endeavour to prepare the sensitive compound alluded to,

to follow out strictly the directions as I described them in the Bakerian Lecture for 1880.

The following is the mode of preparation. A normal collodion is first made according to the formula below:—

Pyroxyline (any ordinary kind) . . .	16 grains
Ether ('725 Sp.)	4 oz.
Alcohol ('820)	2 oz.

This is mixed some days before it is required for use, and any undissolved particles are allowed to settle, and the top portion is decanted off. 320 grains of pure zinc bromide are dissolved in $\frac{1}{2}$ oz. to 1 oz. of alcohol ('820) together with 1 drachm of nitric acid. This is added to 3 ozs of the above normal collodion, which is subsequently filtered. 500 grains of silver nitrate are next dissolved in the smallest quantity of hot distilled water, and 1 oz. of boiling alcohol '820 added. This solution is gradually poured into the bromized collodion, stirring briskly while the addition is being made. Silver bromide is now partially suspended in a fine state of division in the collodion, and if a drop of the fluid be examined by transmitted light it will be found to be of an orange colour.

Besides the suspended silver bromide, the collodion contains zinc nitrate, a little silver nitrate, and nitric acid, and these have to be eliminated. The collodion emulsion is turned out into a glass flask, and the solvents carefully distilled over with the aid of a water bath, stopping the operation when the whole solids deposit at the bottom of the flask. Any liquid remaining is carefully drained off, and the flask filled with distilled water. After remaining a quarter-of-an-hour the contents of the flask are poured into a well-washed linen bag, and the solids squeezed as dry as possible. The bag with the solids is again immersed in water, all lumps being crushed previously, and after half-an-hour the squeezing is repeated. This operation is continued till the wash water contains no trace of acid when tested by litmus paper. The squeezed solids are then immersed in alcohol '820 for half-an-hour to eliminate almost every trace of water, when after wringing out as much of the alcohol as possible the contents of the bag are transferred to a bottle, and 2 ozs. of ether ('720) and 2 ozs. of alcohol ('805) are added. This dissolves the pyroxyline and leaves an emulsion of silver bromide, which when viewed in a film is essentially green-blue by transmitted light.

All these operations must be conducted in very weak red light—such a light, for instance, as is thrown by a candle shaded by ruby glass, at a distance of twenty feet. If a green light of the refrangibility of about half way between E and D could be obtained it would be better than the faint red light transmitted by ruby glass, since the bromide is less sensitive to it than to the latter. The light coming through green glass after being filtered through stained red glass is almost the best light to use. It is most important that the final washing should be conducted almost in darkness. It is also essential to eliminate all traces of nitric acid, as it retards the action of light on the bromide, and may destroy it if present in any appreciable quantities. To prepare the plate with this silver bromide emulsion all that is necessary is to pour it over a clean glass plate, as in ordinary photographic processes, and to allow it to dry in a dark cupboard.

It has been found advantageous to coat the plate in red light, and then to wash the plate and immerse it in a dilute solution of HCl, and again wash, and finally dry. These last operations can be done in dishes in absolute darkness; the hydrochloric acid renders innocuous any silver sub-bromide which may have been formed by the action of the red light, and which would otherwise cause a heated image.

Let me here give warning, that the emulsion formed will be very grainy in appearance, and requires vigorous shaking to cause it to emulsify proper. If it requires a little plain pyroxyline, say about two grains to the

fluid ounce should be added to give greater consistency. One thing is certain, if it be not coarse-grained under the microscope it will not be sensitive to the required region, and moreover it will be found that on an average it should be about twice as coarse as the average form of bromide which is generally obtained in collodion emulsion. Here let me interpolate a remark. It has been assumed that because an emulsion in gelatine has a bluish colour after it has been boiled, that in this case we have the same form of bromide as that described above. It is a very different form: let me show how. Suppose we throw a spectrum on a gelatine plate it will be found that G requires about $\frac{1}{2}$ of a second with a very narrow slit, whereas to obtain B it will require the best part of a minute, and to obtain rays of lower refrangibility very much more; and that any amount of exposure will not make an impression much below A. With the blue-green bromide in collodion to obtain an impression about G will take some eight or ten seconds, and it will be found that at the same time we have an impression of B. A minute's exposure to the prismatic spectrum will under similar circumstances give an impression as much below A as D is above it, measured not in wave-lengths but along the

photograph. I point out this because a leading continental photographic experimentalist has expressed himself satisfied as to the identity of the two forms of sensitive salt. They are totally distinct as if he tried to work with a gelatine plate in the infra-red region he will soon own. Now in reference to the coarseness of grain it is right to call attention to its disadvantages. Its advantage we know. In spectrum work we often come across close pairs of lines. Now suppose each pair happened not to be separated by a larger interval than the grain of the sensitive salt, we shall be unable to resolve such a pair, for the action of either component of the pair, and much more both, if they fell on it would be to cause, on development, a reduction to metallic silver of the whole grain. Thus evidently such a close pair would be unresolved.

When a photograph of the spectrum on the finest grained plate is examined under the microscope it will be found that the metallic image is composed of grains of silver and nothing else; and that instead of the lines having sharp edges as seen by the eye that they shade. Part of this shading is due to the grain, though the greater part is due to proper absorption, which the eye is incap-

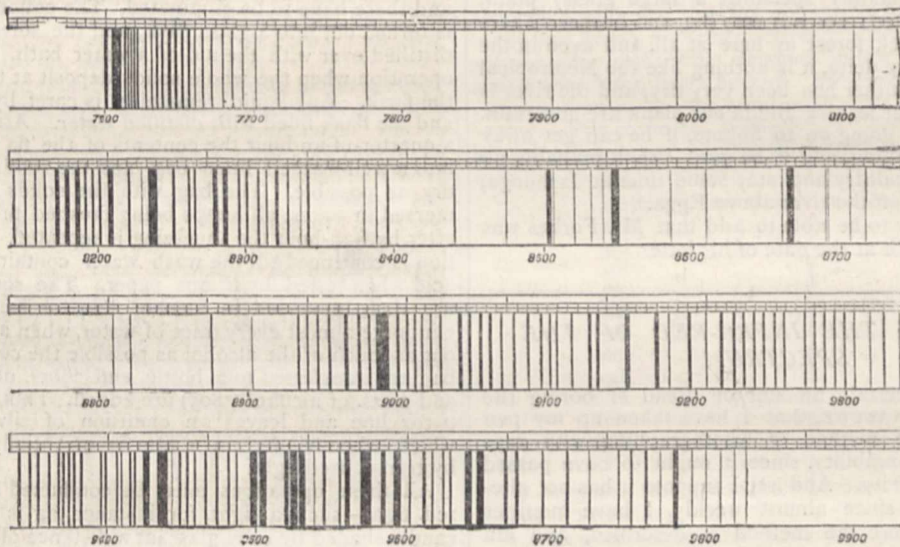


FIG. 1.

able of distinguishing. The fineness of grain given by the different processes we may class as follows, in the order of coarseness, the coarsest grain being first:—

1. Wet plate developed by iron.
2. Special bromide emulsion, as before described.
3. { Ordinary collodion emulsion.
3. { Wet plate developed by pyrogalic acid.
4. Gelatino-bromide plates.

It will thus be seen that for delicate work the dispersion with the wet plate process and the special bromide emulsion must be larger than when using a gelatine plate if equal resolving power be wished for. The above plate is an instance of this. In it we have the solar spectrum in approximate wave-lengths from $\lambda\lambda$ (7,600) to about λ 10,500. The general impression to the eye is the extraordinary width of the lines compared with those in the visible spectrum. No doubt they are as a rule broader, but their breadth is also to be accounted for in other ways. First, the slit used was not quite as fine as might have been when the photographs were taken. Secondly, the dispersion used was the first order of a Rutherford grating 17,200 lines (about) to the inch, and a camera lens of a focus of about fifteen inches. In later photographs nearly all the broad lines have been resolved into pairs or triplets, as have also some of the lines of medium

breadth. There are lines, however, like the 3 broad lines between 8500 and 8700 which remain unchanged whatever dispersion was used. This resolution was effected by using a finer slit and dispersion of the second order, the fine slit alone will not give it. If we take an example in the visible spectrum, and examine the B line with the eye, it will be found to be made up of a series of doublet flutings, each component being apparently of equal intensity. These pairs it is impossible to secure on the photographic plate, unless the second order of the grating spectrum is used; but when secured it will be found that the more refrangible component is more intense, as is the case in certain hydro-carbon flutings. The sole reason why the first order is useless to cause resolution is that the pairs are so close they can both fall on the diameter of the grain of the sensitive compound. On the other hand, with a gelatine plate I have been able to see on one inch and a half every line and more than given in Angström's map from G to F. In this case the grain is almost invisible.

The development of the plate is greatly more difficult than the preparation of the emulsion. A strong developer it will not stand, and I may say also that a very new one is also inadmissible when using the ferrous oxalate development. To make the developer a saturated solution of neutral oxalate of potash is saturated in the cold, with

ferrous oxalate : and then the deep red solution decanted off. When freshly prepared it is useless to attempt to develop a plate with it unless the precaution be taken of adding to it an equal part of a saturated solution of ferric oxalate in the oxalate of potash. Such a mixture may be employed by adding to it immediately before all an equal volume of a solution of potassium bromide (twenty grains of the salt to thirteen of water). The plate may then develop without fog or it may not : if it does fog, the developer must have more bromide solution added to it, and another trial made. On some days a clean picture seems an impossibility, whilst on others, every one will be perfect. It is not the emulsion that is in fault, since, on a "clear day" and on a "foggy day" the identical emulsion may be used, showing that the developer is at fault. This year this trouble seems to have increased, and I can only lay it down to the different preparations of the oxalates. Of one thing care should be taken, viz., that the developer never shows alkalinity ; a drop of dilute sulphuric acid or nitric acid may be added to the developing cup just before development with advantage.

With prisms the photography with the rays of low refrangibility is simple, with one great drawback, and that is the difficulty of obtaining a true focus for the plate. This must all be done by guess-work, and plates exposed till the focus is obtained. When once obtained it is a good plan to mark the camera to show the focus, and at the same time accurately to mark the table on which it stands, so that the *same portion of the lens receives the same rays*. This is more particularly necessary to attend to when using an achromatic lens. I believe it to be easier to use the uncorrected lens than a corrected one, provided always that the camera has a proper horizontal swing back, which can be shifted through a very large angle at least 30° when using three prisms. If a spherical mirror be used in the collimator and in the camera instead of the lenses, the same difficulties of focusing do not present themselves. The disadvantage of this method is that the edges of the spectrum based are diffused and not straight, and this is awkward when making comparison of different spectra. With a grating nearly the same difficulty arises when using lenses, but not quite to such a degree. If "a" and A be got in focus at the end of the plate, the swing back being used till this results, and if the lens be placed close to the grating, the whole of the infra-red region will be fairly in focus. This of course only applies to my own grating which may have a slight curvature. In using the grating we must not forget that the second order overlaps the first order, and the third order the second order and so on : and if a plate were exposed without any artifice being adopted to get rid of this overlap the plate would show two or three spectra. There are several methods of accomplishing this separation, the simplest being to use the absorbing medium in front of the slit. At first I used stained red glass which cuts off all radiation above the green, leaving thus the tails of the different spectra intact. At present, when wishing to go no further down the scale than $\lambda 10,000$, I have found that a deep coloured solution of iodine of potassium in water about one-tenth of an inch in thickness is very excellent. The objection to the red glass is that it exercises a certain amount of general absorption in the infra-red region, but with the white glass of the cell holding the solution, and the solution itself, this general absorption is minimized. To get down still further, very thin stratum of a blue dye in tetrachloride of carbon is efficacious in conjunction with the iodine solution. With the above solutions $\lambda 13,000$ can be reached. Beyond this limit it is necessary to use other means of eliminating the higher orders of the spectrum. The simplest plan is to place behind the collimator a couple of prisms, and some two feet from the prisms, the grating so that it only receives those rays which it may be desired to im-

press. Thus one side of the grating may catch the limit of the red whilst the rest will be filled with the dark rays. The most difficult plan is to place a prism according (as Fraunhofer did) in front of the grating, in such a way that the axis of the prism is at right angles to the ruling and parallel to the plane of the grating ; this causes a complete separation of all the different orders of spectra. But the resulting photographs are inconvenient to measure, since they are curved, and the position of the camera is also awkward. Another plan is to use a prism in front of the slit, but this too, I have found inconvenient for the same reasons as given above. For ordinary work the absorption method is decidedly the most elegant, but then it limits the operations with the spectrum. It was from photographs obtained in this manner that the above map of the solar spectrum was obtained, and as it is before us, it may be well to make a few remarks on it. As to

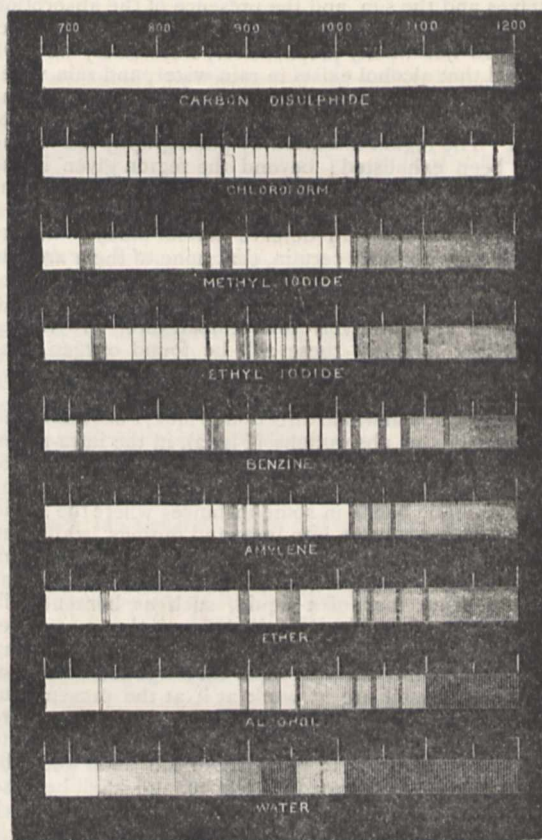


FIG. 2.

what the lines are due to we are at present absolutely uninformed, except as to some very few. A notable exception to this is the line lettered about 8600, which is one of the strongest lines in this part of the spectrum. Colonel Festing and myself found that this line coincided absolutely in position with what we call the radical absorption band of benzene, that is to say, that by diminishing a thin layer of benzene placed between the slit of the spectroscope and a source of light giving a continuous spectrum, this absorption-band, amongst many others, was the last to disappear, and that it also was the key-note as it were of the absorptions of all benzene derivatives.

A coincidence of this kind would not be fortuitous any more than that the vapour of sodium gives lines coincident with the D lines ; and hence we were forced to ascribe this line to benzene or some of its derivations. When first we made this announcement it was facetiously

remarked that we had been photographing London smoke; and no doubt had not other localities for photographing the spectrum been chosen, the reproach (for such it was) might have been just. My visit last June to the Riffel, 8,500 feet high, showed that not only was this said line present, but that it was more intense even than at the level of the sea. There was more unfolding of the spectrum at that high altitude, and lines faint indeed, which had almost escaped registration below, were well marked on the photographs obtained there. The brilliancy of this infra-red spectrum can scarcely be surpassed. When examined at an elevation of 10,000 feet, the general absorption due to water almost vanishes, and with the exception of two congeries of lines which lie beyond those given in the diagram, the whole of the lines shown are stronger than I have ever had them before.

Colonel Festing and myself have also shown the presence of some alcohol derivative, somewhere between ourselves and the sun, and the presence of the absorption lines at a high altitude place it outside our atmosphere. This I was not wholly prepared for, since lately we have been told that alcohol exists in rain water, and rain water can only derive it from the air. The fact, however, remains that it probably exists beyond the limits of our atmosphere. The region disclosed by photography has by no means been exhausted; beyond the region given in the diagram lies one in which we have a breadth of continuous spectrum, and beyond that again beautiful groups of lines, all of which require and deserve careful study. Of one thing we may be fairly certain, that none of them are due to metallic vapours, but are probably due to vapours of non-metallic compounds in some form or another, and these at a comparatively low temperature. It is not unlikely that amongst these will be found oxygen compounds, and if so it would be interesting in more ways than one.

As a suggestion in which direction to look, I have annexed a diagram of the absorptions (Fig. 2), in the infra-red of a few liquids, by which it will be seen, that by a study of these we may perhaps throw some light on the solar spectrum. The bands in some instances where the liquid is vaporized are split up into lines and flutings, whilst the radical bands, to which I have already drawn attention, seem to remain constant. When it is remembered that one-tenth of an inch of a liquid, such as benzene, will give a definite absorption, it will be seen that a manageable length of vapour may be placed between the slit and the source of light, for its proper investigation. Colonel Festing and myself are at work at it at the present, but the field of investigation is so large that it requires more workers before any general theory can be brought to bear on the subject. It is partly to aid such would-be workers that I have penned the above, and shall be glad if it stirs up some few to aid in this research, which not only has a bearing on solar physics, but even still more largely on physical chemistry.

W. DE W. ABNEY

NOTES

WE have received a communication from Prof. Hildebrandson, director of the Meteorological Observatory, Upsala, so well known for his researches into the upper currents of the atmosphere, in which, with reference to the proposed observatory on Ben Nevis, he remarks that "the erection on Ben Nevis of a permanent meteorological observatory is of the utmost importance for the development of modern meteorology. No better situation for a mountain observatory can be imagined. I have for a special purpose discussed the few observations published from Puy de Dôme. They are of great importance, but unfortunately this mountain, as well as the station of Gen. Nansouty on the Pic-du-Midi, has a bad situation in relation to storm tracks, being almost constantly placed on the north-westerly or south-easterly

slope of a high pressure. On the contrary, Ben Nevis is situated almost in the middle track of the depressions or storms of north-western Europe. Hence observations made there must be of far greater importance in their relation to the theory of cyclones than the mountain observations in the south of France. I hope the Scottish Meteorological Society will find the means of carrying on this work." With these views of Prof. Hildebrandson we heartily concur, and hope that the Council of the Scottish Meteorological Society will succeed in the patriotic effort we understand that they are now making to raise the necessary funds, viz. 5000*l.*, for the erection and partial endowment of this truly national observatory.

WE are glad to learn that Sir Edward Reed is so far recovered that he may be able in the course of a few days to give occasional attendance in Parliament.

THE International Electrical Conference which has been sitting in Paris for the last fortnight, has, after passing several resolutions, adjourned to the first Monday of October, 1883. In regard to electrical units it was resolved that at present there is not a sufficient concord of view to enable the numerical value of the "ohm" in the mercurial column to be definitely fixed, and that all governments be appealed to by France to encourage further research on the subject. The section for "Earth Currents and Lightning Conductors" resolved that Government should be requested to favour regular and systematic observations of atmospheric electricity upon their telegraphic systems; that it is important for the study of storms to be extended to every country; that wires independent of the telegraphic system should be provided for the special study of earth currents; and that, so far as possible, the great subterranean telegraphic lines, particularly those running north and west, should be utilised for the same purpose, observations being instituted on the same day in the various countries. The section for fixing a standard of light expressed the opinion that the light emitted by a square centimetre of melting platinum would furnish an absolute standard. In closing the Conference M. Cochery, the Postal Minister, assured the Members that the French Government would endeavour to give effect to their Resolutions by representations to the various Governments concerned. It is hoped that the twelve months for which the Conference is adjourned will be sufficient for the searches in the various departments in question to be completed. England is indebted solely to the private enterprise and spirit of Sir William Thomson for being represented at all. Between the French Government, the Foreign Office, and the Science and Art Department a sad mess has been made. The Post Office Telegraph Department was never asked to send a representative, nor have any of those who took such an active part in the Conference last year been asked to take any part in this. A more disgraceful muddle has never previously distinguished our "how not to do it" system.

M. MIGNET, Perpetual Secretary of the Academy of Moral Sciences, has just resigned the office held by him from the reorganisation of the Academy in 1835, up to the present time. Having been born at the end of the last century, his plea of old age may be said to be fully justified. It is stated on good authority that he will be succeeded by M. Jules Simon, who is now temporarily filling the office of *secrétaire perpétuel*.

THE annual meeting of the five French Academies, sitting as one body in the capacity of the French Institute, was held on October 25. M. Dumas, as director of the Académie Française, was in the chair. He opened the proceedings by an address, which quite fulfilled the expectations that had been raised. M. Dumas gave an elaborate history of the several academies of Paris, of their suppression in 1793, and their reopening in 1795 as the five classes of the Institute. The regu-

lations adopted at the time were altered by the several monarchical governments, but have gradually resumed their former provisions, so that the present Institute may be said practically to exist as it was at the end of the last century. The subject was treated with wonderful eloquence and expression. M. Dumas derived the origin of modern scientific societies from the *Accademia di Lincei*; he showed that the Academy of Sciences of Paris and the Royal Society of London came into existence about the same period, their meetings having been foreshadowed or instigated by the *conversazioni* held by the friends or followers of Descartes. M. Dumas insisted most on the grand spectacle exhibited by these institutions surviving monarchy, nobility, established churches, and finding in political revolutions a new field for their activity. He might have added, that even under the disorderly reign of the Commune, the sittings of the Academy were unmolested, and the editor of the *Journal Officiel de la Commune* did his best to report the sittings. The Academy of Sciences was represented in the addresses delivered by M. Alphonse Milne-Edwards, who gave a graphic account of the really good work done by the *Travailleur* in the Mediterranean and Atlantic. The large hall was crowded, and the whole proceedings were of high interest.

THE anniversary meeting of the London Mathematical Society will be held on the evening of Thursday, November 9, at 8 p.m., at 22, Albemarle Street. Mr. S. Roberts, F.R.S., has chosen as the subject of his valedictory address, "Some Remarks on Mathematical Terminology and the Philosophical Bearing of Recent Mathematical Speculations concerning the Realities of Space"; his principal aim will be to show that mathematics are neutral in philosophy. *Inter alia* he will report to the Society the fact of the establishment of the De Morgan memorial medal and the conditions of its being awarded. The following changes are proposed to be made in the Council for the ensuing session: Prof. Henrici, F.R.S., President, Sir J. Cockle, F.R.S., and Mr. Roberts, F.R.S., Vice-Presidents, and Messrs. E. B. Elliott and Dr. J. Hopkinson, F.R.S., to be new members in the place of Prof. Rowe and Mr. H. W. Lloyd Tanner, who retire.

THE *Japan Gazette* of August 21 contains a long and curious description of a bear festival among the Ainos. The writer, Dr. B. Scheube, is, we believe, the only European who has ever been actually present at this ceremony, the descriptions of it given by Miss Bird and other writers being derived from hearsay. The bear receives the title of *Kimui-Kamui*. The true derivation of this latter title—which is generally and incorrectly said to come from the Japanese *Kami*, a divinity—has been explained by Mr. Keane in *NATURE* (vol. xxvi. p. 525). The festival is now rarely held, and there is small reason to regret this, as it has degenerated to a brutal orgy. It commences with drink, every change in ceremony begins and concludes with drink, until finally every one in the village is intoxicated, while their hands, faces, and clothes are smeared with the gore of the sacrifice. Dr. Scheube says: "I had much difficulty in keeping off the drunken crowd that wanted me to partake of the blood and liver (the latter is eaten raw); and I can say that though hardened in these things by the practice of my profession, the sight of these drunken people with their bodies smeared over with blood filled me with a loathing that made me feel glad that the day and the feast were coming to an end together." Dances, many of them of an obscene nature, also form part of the ceremony.

A VERY business-like Annual Report from the Sheffield Free Libraries and Museum Committee has been sent us. Though complaint is made of the heavy cost of two of the branches, it is satisfactory to find that one of these rivals the Central Library in its number of volumes circulated. A new catalogue of the Central Library, which has been issued lately, we shall hope to notice more fully shortly. Besides the new branch of a musical department we may call attention also to the Observatory and

the Museum of Natural History, with the hope that, in all our large towns eventually, the Free Library will become the centre of instruction in all knowledge.

THE *Electrician* learns that the improvements in the storage of electric energy and in electromotors have so far advanced, that tricycles can not only be lighted, but also propelled solely by electricity, as was seen from the tricycle ridden last week by Prof. Ayrton in the city. The Faure accumulators in which the energy was stored for the lighting and drawing, were placed on the footboard of the tricycle, and the motion was produced by one of Professors Ayrton and Perry's newly-patented electromotors placed under the seat of the rider. Using one of these specially-made tricycle electromotors and the newest type of the Faure accumulators, the total dead weight to be added to a tricycle to light and propel it electrically, is only one and a half hundredweight, a little more than that of one additional person.

WE wish to call the attention of our readers to the "*Feuille des Jeunes Naturalistes*," published monthly in Paris, with a London agency at 110, Leadenhall Street. Founded at Mulhouse in Alsace in 1870, the young journal was hardly launched before the national troubles began; the publication was removed to Paris, where the two first editors both perished during the war at the age of about twenty. The object of the journal is to establish a medium of communication between young naturalists, to encourage them to publish their earliest essays in a serial where they will be sure to find readers to be instructed and competent judges to guide them in their future studies. Every kind of trustworthy observation is welcomed; and the editors undertake to translate communications sent to them in English. The Journal is believed to have been instrumental in the formation of several local natural history societies.

THE St. Petersburg Society of Gardening is taking the necessary steps to prepare the International Botanical and Gardening Exhibition and Congress, which will take place in the Russian capital. Professors Beketoff, Borodin, Famintzin, Marklin, and Maximowitsch, and Messrs. Annenkoff, Gobi, Iversen, Semenoff, and Wolkenstein are elected members of the scientific committee; three other committees—for the Exhibition, for the erection of buildings, and for the reception of guests—were appointed at the last meeting of the Society.

WE have received a copy of the syllabus of the Yorkshire College Students' Association. The society was founded in 1877, and is now in its sixth session. The number of members is large, and the meetings have hitherto been very successful. Attention is devoted to literature as well as to science. An excellent programme of papers is down for the present session which began on October 24 with an address by the president, Prof. Thorpe, on "The Story of the Origin of the Metric System."

THE German Ornithological Society held its annual meeting at Berlin recently under the presidency of Baron Homeyer. Mr. Schalow (Berlin) read a paper on the progress of ornithology during the last five years; Prof. Landois (Münster) on egg shells considered from a histological and a genetic point of view; Mr. Müttel (Berlin) on the call of the Tragopan; and Prof. Blasius the report of the stations for observing the migrations of birds in Germany.

TELEGRAMS from the south-east of Europe report that there was an earthquake in the northern part of the Balkan Peninsula on October 25. At 1.26 p.m. the shocks were felt severely at Preboi, in Bosnia. They lasted fully three seconds, the direction of the vibrations being from west to east.

THE first General Meeting of the Members of the Parkes Museum, since the incorporation of the Museum, was held on

Saturday last. Capt. Douglas Galton, C.B., was voted to the chair. It was unanimously resolved that H.R.H. Prince Leopold, Duke of Albany, who had graciously consented to accept the presidency, be formally elected to that office. Capt. Douglas Galton, in replying to a vote of thanks for presiding, said that the Museum had now entered on a fresh phase of existence, and had established itself as an independent institution in premises which, after necessary alterations had been completed, bid fair to serve its purpose, for the present at least, admirably. The Council contemplated making the sanitary arrangements necessary for the Museum itself as perfect as possible, and it was intended that all such arrangements should be useful for teaching purposes; the drainage, for instance, had been carefully considered by Prof. Corfield and Mr. Rogers Field, M. Inst. C.E., and the latter gentleman had generously undertaken to bear the whole expense of carrying it out. Mr. Twining had undertaken the whole trouble and cost of arranging, and for the most part of providing the Food Collection; the Warming, Lighting, and Ventilating have been referred to a Special Committee, whose endeavour it would be to insure that every appliance was the best of its kind. The general collection was to be carefully weeded and re-arranged, and it was hoped that the Museum would be opened to the public soon after Christmas.

THE name *isane-mones* has been recently applied by M. Brault to curves of equal velocity of wind, and he has made a drawing of such curves for the North Atlantic in summer, using for the purpose 240,000 observations on board ship. It is shown that an approximate numerical value may be attached to each of the ordinary terms used in ship's logs to denote the wind's force. M. Brault's map, which appears in *Comptes Rendus*, is remarkable in that it reproduces almost exactly the map of *mean isobars*. Thus, during summer, that is to say, when the atmosphere is most stable over the great North Atlantic basin, the *mean isane-mones* and the *mean isobars* are the same, presenting only differences that are nearly equal to possible errors of observation and of construction. It remains to be seen in what measure this important law is general; M. Brault believes it to be so for every surface of the globe which is under what he calls fundamental maxima and minima (such as the maximum and minimum of Asia, the maximum of the Azores), the fixity and permanence of which are such that they form together, and at six months' interval two distinct systems which suffice to define the two great phases of the annual circulation. (*Ephemeral* maxima and minima are such as appear and disappear daily in our latitudes; while *mobile* or *tempestuous* minima such as cyclones or squalls, are grouped as a third class.)

In his work on worms, Darwin has described some tower-like dejections which he never saw constructed in England, but which are attributed to an exotic species of *Perichata*, from Eastern Asia, naturalised in the environs of Nice. M. Trouessart has lately observed similar dejections in gardens near Angers. Having collected a large number of worms from where the towers were made, he found no species of *Perichata*, nor of any other exotic genus. In two or three cases he surprised the worms at work, and they were *Lumbricus agricola*. It was the anterior part of the body that was lodged in the tower. After the rainy period at the end of September all the tubular interior of each tower (forming a continuation of the subterranean gallery) was quite free; but a few days later it was obstructed by recent dejections. M. Trouessart supposes that the calotte or cap of the tower, getting hard in air, a time comes when the worm can no longer burst the upper wall as before, to place its dejections outside (so increasing the height of the tower), but deposits them within. Thus a long period of rain is necessary for these towers to rise regularly. The towers probably serve to protect the

galleries from rain, and to afford a breathing place for the worms, where they are not seen by birds.

WE learn from the *Rivista Scientifico-Industriale* that Baron V. Cesati has resolved to sell his botanical collection. This consists of a herbarium of about 32,000 phanerogamic species, also a special cryptogamic herbarium containing at least 17,000 species; altogether more than 350,000 plants. There is also a collection of autographs of 2500 botanists. Any one wishing to purchase is desired to apply to the owner, at the Botanical Gardens of Naples. Full particulars of the herbaria will be given.

IN the construction of a railway bridge recently over the Ticino, electric illumination has been used instead of that with stearine candles (previously preferred for the compressed air caissons). The hygienic conditions of the workmen in the caissons is thus greatly improved; as stearine candles impregnate the atmosphere with smoke. Eight lamps of the small Swan type are used to light the working chamber; a Siemens' dynamo of about 30 lamp-power supplying the current. A second dynamo is kept in reserve, to be used in case of breakdown or excessive heating. The additional cost of the system is regarded as largely compensated by the increased comfort in working.

THE additions to the Zoological Society's Gardens during the past week include a Vervet Monkey (*Cercopithecus lalandii* ♂) from South Africa, presented by Mr. G. H. Jones; nine Hairy-footed Jerboas (*Dipus hirtipes*), twenty-four — Gerbilles (*Gerbillus* —) from Arabia, presented by Lieutenant Paget, R.N.; a Laughing Kingfisher (*Dacelo gigantea*) from Australia, presented by Mr. H. G. Austin; a Ceylon Jungle Fowl (*Gallus stanleyi* ♂) from Ceylon, presented by Mrs. Dick Lauder; a Spinose Land Emyd (*Geomyda spinosa*) from Borneo, presented by Miss C. G. Robson; two Sharp-headed Lizards (*Lacerta oxycephala*) from Madeira, presented by Mr. H. J. Clements; three European Tree Frogs (*Hyla arborea*), European, presented by Miss L. Burness; a Rhesus Monkey (*Macacus erythraeus* ♂) from India, a Malbrouck Monkey (*Cercopithecus cynosurus*) from East Africa, deposited; two Canadian Beavers (*Castor canadensis*) from Canada, an Eyra (*Felis eyra* ♂), two Sun Bitterns (*Eurypyga helias*), a Brown Gannet (*Sula leucogastra*) from South America, two Globose Curassows (*Crax globicera* ♂ ♀) from Central America, a Razor-billed Curassow (*Mitua tomentosa*) from Guiana, a Greater Shearwater (*Puffinus cinereus*) from Lincolnshire, six Knots (*Tringa canutus*), a Lapwing (*Vanellus cristatus*), British, a Matamata Terrapin (*Chelys matamata*) from the Amazons, purchased; a Muscovy Duck (*Cairina moschata*) from South America, received in exchange.

OUR ASTRONOMICAL COLUMN

SCHMIDT'S COMETARY OBJECT.—We have received a circular (No. 48) of the Imperial Academy of Sciences of Vienna, containing a letter from Dr. Julius Schmidt, dated Athens, October 14, in which he notifies his discovery of a nebulous object not far from the head of the great comet, which will be best given in his exact words. He writes:—"Seit October 9, 16^h 5h. liegt in S.W. neben dem Kometen eine der Form nach stark variable cosmische Nebelmaterie, welche die scheinbare Geschwindigkeit des grossen Kometen zwar etwas übertrifft, doch im Ganzen der Bewegung desselben entspricht." Dr. Schmidt appends the following places, the first and last being from measures, the second deduced from a star-chart.—

1882.	M.T. at Athens.	Apparent R.A.	Apparent Decl.	Dist. from nu- cleus of principal comet.
	h. m.	h. m. s.		
Oct. 9	16 54 ...	10 15 53 ...	- 12 53 ...	3 24
10	16 36 ...	10 10 26 ...	- 12 43 ...	4 25
11	16 37 ...	10 5 51 ...	- 14 33 ...	5 21

On submitting these positions to calculation by the ordinary method of Olbers for a parabolic orbit, Mr. Hind has found the

following elements, the second set being the result of the corrected value for the ratio of the curvate distances at the extreme observations, though the representation of the middle place is not sensibly improved thereby:—

Perihelion passage, Sept. 24 ²⁷⁷⁸ G.M.T.	Sept. 24 ⁰⁹¹²
Long. of perihelion	234 42'6
„ ascending node	232 21'5
„ „	350 2'4
Inclination	354 50'9
Log. perihelion distance	29 11'5
	29 41'9
	8'26678
Motion—retrograde.	Retrograde.

The general resemblance of these elements to those of the great comet, will excite remark. The middle observation shows a difference from computation of $-6'2$ in right ascension, and $-3'2$ in declination by the second orbit; perhaps unavoidable error in an estimated place, or vagueness of the nebulousity may account for the differences, yet Dr. Schmidt speaks of having observed "Die Positionen des eines Kernes des seitlichen Nebels." Further, it may be observed that the orbit in which the great comet is now moving does not accord with the positions given by Dr. Schmidt; thus with the last elements published in NATURE, the observed and computed right ascension on October 9 will agree if the perihelion passage be assumed to have occurred September 13⁷³², but the calculated declination is north of that observed by $1^{\circ} 39'$, and for the observation on October 11, the calculation is in error $+1^{\circ} 56'$ in right ascension and $+2^{\circ} 31'$ in declination. Nevertheless the general similarity in the arrangement of the elements suggests a past connection of the two bodies, and it may be hoped that further light will be thrown upon the question, if either earlier or later observations of Dr. Schmidt's object are forthcoming.

COMET 1882 *b*.—In an unusually clear sky for the season on the morning of October 23, a fine view of this comet was obtained in the vicinity of London; the length of the more definite portion of the tail was about $16\frac{1}{2}^{\circ}$. At 5 a.m. on October 30, with strong moonlight and a somewhat vaporous sky, it was still conspicuous, notwithstanding the material diminution in the theoretical intensity of light. If the tail extended in the same direction from the nucleus on both dates, there was a large increase in its real dimensions in the course of the week. In fact, on October 30, if we assume the tail to have been a prolongation of the radius-vector, it would cover a space considerably greater than the mean distance of the earth from the sun, and with any reasonable assumption as to deviation from that line, its true length could hardly have been less than 70,000,000 miles.

The place given by M. Cruls for the comet he found at Rio Janeiro on September 12 a.m., differs $5^{\circ} 43'$ in right ascension, and $1^{\circ} 25'$ in declination from that occupied by the great comet at the time.

From an observation at the Collegio Romano, in Rome, on the morning of October 25, kindly communicated by Prof. Millosevich, it appears that the elements last published in this column were in error $-2'4$ in right ascension and $-0'3$ in declination, small differences, considering that the last observation used in their determination was made on October 1, and a proof of the precision of the observations issued from the Collegio Romano.

GEOGRAPHICAL NOTES

THE Council of the Geographical Society have made the final arrangements for their new African expedition under Mr. Joseph Thomson. Mr. Thomson hopes to leave England in the end of November for Zanzibar, where he will stay some months getting together his retinue and goods, and making other provisions for his hazardous journey. He will probably leave the coast in April or May next. The field of the new expedition lies to the east and north-east of Lake Victoria Nyanza, and may include a running survey of the eastern shore of the lake. The expedition will probably start from Pangani, and ascend the river of that name as far as Kilima Nyaro, whence they will proceed direct to Victoria Nyanza. The route after that will depend much on circumstances, but Mr. Thomson hopes to visit the reputed Lakes Bahringo and Samburu, as also Mount Kenia. Probably Mounts Kenia and Kilima Nyaro will be more carefully examined than they have been, and beyond them the country to be traversed by the expedition is almost totally unexplored. On its borders we

meet with the names of such travellers as Denhardt, Krapf, New, Wakefield; but the field is practically virgin. A great part of the region is a wilderness, rendered so by roving Masai, whose depredations have scattered the population and rendered culture impossible. Besides the danger from these roving freebooters, the expedition will be compelled to carry its own provisions to a large extent, as there is no likelihood of getting a regular supply on the spot. Water, too, it is feared, will be scarce, so that on the whole Mr. Thomson will have a trying task before him. The expedition will be purely geographical, but it is almost certain that a naturalist will accompany Mr. Thomson as far as Kilima Nyaro, partly at the expense of the British Association. Mr. Thomson will, however, be in no way responsible for the safety or the conduct of the naturalist's party. It is probable that Dr. Aitchison, who did good work in natural history during the Afghan war, will be selected for this work, and his retinue and all his arrangements will be quite independent of those of Mr. Thomson; the two parties will simply go together so far as their route is in common.

MR. STANLEY has published separately a full report of the address he recently gave in Paris. From this we glean one interesting item of exploration. After he had launched his steamer on the upper waters of the Congo, above the cataracts, he proceeded up the river and entered the Kwango, the great southern tributary. One hundred miles from its mouth he came to where two large streams united to form the main river; a greyish-white stream from south by east, the other, of an inky colour, from east by south. Ascending the latter, much less rapid than the former, Mr. Stanley came, after steaming another 120 miles, to a large lake, into which the river widened. On circumnavigating it, he found it about seventy miles in length, and with a breadth varying from six to thirty-eight miles. The natives he found very wild, and naturally astonished at the puffing monster. A splendid country the shore seemed to be—dense, impenetrable—lofty forests, alternating with undulating grass lands. Mr. Stanley was altogether three years away from Vivi, and doubtless he has collected much information in the country around the Congo. If the five stations established on the banks—one at the mouth of the Kwango—are left unmolesed, much material of value to science may be collected; they are superintended by Europeans, who have all the apparatus for taking meteorological and other observations.

ON Sunday, October 29, the Paris Society of Topography distributed its medals in the large Hall of the Sorbonne. M. de Lesseps was in the chair. The three great medals were awarded to M. D. Brazza, M. Roudaire, and Commander Perier. One of the others to M. Triboulet, treasurer of the Academy of Aérostation Météorologique, for his continuous efforts in aerial photography and the success obtained nineteen days ago in photographing the horizon visible from a captive balloon, with an apparatus put in operation from the ground.

AT the last meeting of the Section of Physical Geography of the Russian Geographical Society, M. Grigorieff made a report on the results of Arctic exploration during last summer; W. E. Fuss read a communication on his visit to Novaya Zemlya, which was made to determine accurately the position of the new meteorological station; and M. Rykatcheff a communication on meteorological observations he made during an ascent in a balloon.

THE students of the Physical and Mathematical Faculty of St. Petersburg have presented M. Miklukho Maclay with an address of thanks for his valuable researches, and express the wish that the results may soon be given to the world.

A RECENT issue of the *North China Herald*, published at Shanghai, contains an article on a Chinese work entitled "Travels in India." The work is of interest as exhibiting the impression made on an intelligent Chinese traveller by the results of Western civilisation. The author, Huang Mao-ts'ai, is, it appears, a literary graduate of Kiangsi, who became impressed with the importance to China of knowing what is going on in neighbouring countries, and accordingly obtained, in 1878, a commission from the Governor of Szechuen to pass through Thibet to India. Arriving at Patang he was deterred by the hostility of the hill tribes from proceeding further in that direction, and he therefore retraced his steps, turning southward into Yunnan, whence he crossed into Burmah, and descending the Irawaddy to Rangoon, he took passage for Calcutta. He spent six months in India, returning to China by Singapore and Saigon.

His four volumes and maps were laid before the throne, and he was rewarded with an appointment in Yunnan. Around China he sees on all hands powerful and aggressive neighbours. To the ambitious schemes of these powerful neighbours and the means of check-mating them he devotes many pages. He dreams even of conquest, and suggests that by encouraging emigration to the southern seas, establishing consuls to look after the emigrants, opening schools to enlighten them in foreign science, and at the same time keep up the knowledge of their native language, the great islands of that region could be made to fall like ripe fruit into the lap of China. In the territorial acquisitions of other countries Mr. Huang finds three degrees of villainy, which he describes respectively as "stealthily beguiling," "enroaching by degrees," and finally "swallowing up." Notwithstanding the offensive discrimination of these terms, he exhibits a high appreciation of English rule in India. In the latter country, he says, there are no idle officers; each has his sphere, into which no other intrudes. The will of each high functionary is limited by his council. Salaries are sufficiently liberal to prevent extortion. All are animated by a regard for their own good name. The law is faithfully executed and public spirit prompts to efforts for the general good. He is struck by the magnificence of Calcutta and its great public works. On the subject of taxes, he says: "The ground is taxed, houses are taxed, shop-signs are taxed, all manner of beasts are taxed, all handicrafts are taxed, and even fire and water are taxed. There are other taxes more than I can mention; yet you do not hear one murmuring word from the people. Why is this? It is owing to two causes: Firstly, they regard the humane Government of the English as a great improvement on the oppressive cruelty of their native rulers: and secondly, they are aware that the revenue thus collected is expended for the good of the country—in making roads, founding schools, and so on." The author is so impressed by the railway system of India that he is extravagant in his advocacy of something similar in China. He wants a railway from the north-western frontier of China proper into Ili, as the only means of retaining that province and Kashgaria. In reply to objections on the score of the enormous expense of this undertaking, he exclaims with true Chinese vanity: "What other countries can do, China can do, as she is ten times richer, and a hundred times more populous."

NOTICE OF SOME DISCOVERIES RECENTLY MADE IN CARBONIFEROUS VERTEBRATE PALÆONTOLOGY

IN the course of my work upon the carboniferous rocks of the neighbourhood of Edinburgh, I have succeeded in obtaining several specimens which throw some additional light upon the little known Selachians of the Paleozoic age. It was considered a great step in advance when Prof. Kner, in Germany, and Sir P. Egerton in England, proved that the spine of the tooth known as *Diplodus*, which occurs frequently in Carboniferous rocks, was the equally well-known *Pleuracanthus*, a genus of not infrequent occurrence in the same beds. A very interesting slab from the ironstone of Burghdee, near Edinburgh, in the Carboniferous Limestone series, advances our knowledge another important stage. Upon it there are several teeth of the species *Diplodus parvulus*, Traq., associated with cranial cartilage, and a spine which is certainly not *Pleuracanthus*, but is totally unlike it, and one which does not appear to have been ever described. Upon showing it to my friend, Dr. Traquair, he said it confirmed an opinion at which he had long since arrived, that the *Diplodus* tooth would be found common to several genera of Selachian fishes. It certainly was a singular fact, and one which must have struck those palæontologists who have most carefully examined the fish-faunas of particular beds and horizons, that the number of the species of spines usually exceed those of teeth. Another important conclusion may be drawn from this discovery, viz. that spines are of very little value in relation to the affinities of sharks. Nothing can be more different than the spine of *Pleuracanthus* and that of *Diplodus parvulus*, Traq.

These conclusions are supported by another specimen in a nodule obtained from a much lower horizon, viz. on that of the Wardic Shales at Hailes Quarry, near Edinburgh. Here we have a *Hybodont* tooth associated with the spine known as *Tristychius*. The tooth, indeed, cannot be distinguished from *Hybodus*; it is deeply furrowed as in many of the Mesozoic species, and has the two depressed lateral cusps. This form of tooth is

very persistent, extending from the Lower Carboniferous to the Chalk. Germar was the first, I think, to point out the existence of a *Hybodont* tooth in rocks of Carboniferous age, but (though I have not yet carefully examined his figures and description) the spines appear to be different from those I find associated with the Hailes specimen, though they appear to me to be of the same general type. That a *Tristychine* spine, with its smooth surface and strongly arcuate shape, should be associated with a *Hybodont* tooth is certainly unexpected, and shows again the necessity of caution in dealing with spines, for the Mesozoic spines associated with *Hybodus* are very different from *Tristychius*. *Hybodus* and *Diplodus* are therefore generalised forms of teeth associated with spines known as *Tristychius*, *Pleuracanthus*, with one undescribed genus, probably with many others. Messrs. Hancock and Atthey, to whom British science is indebted for some of the most important ichthyological observations made since Agassiz' time suggested the possibility of *Cladodus* being the tooth of *Gyracanthus*. I have seen nothing to confirm or refute this suggestion. They also referred certain small tooth-like bodies with success to the dermal skeleton of that genus. I have obtained a nodule from the Wardic shales, which has these in a remarkably good state of preservation in connection with a large fragment of the fin of that powerful shark. These dermal denticles are so closely approximated to each other that they form a dense covering, through which however appear distinctly traces of the skeleton of the fin. The occurrence of the genus at so low a horizon is of itself deserving of record, and in addition to this fragment, I have found imperfectly preserved specimens of spines of the same genus at the same place.

The remains of Labyrinthodonts are exceedingly scarce below the Burdichoun horizon. I am not aware of more than one having been discovered, and that proves to be *Ophiderpeton*, or a closely allied genus. This specimen was discovered in the Wardic shales, low down in the Calciferous sandstone series. The position of the Wardic shales in the Carboniferous series has not yet been exactly defined. Owing to the confused nature of the rocks, and the fact that they are so deeply covered with drift in a good deal of the Edinburgh area, it has not been found possible to settle quite clearly the relative position of the different members of the Carboniferous series. Nevertheless the opinion appears to be universal that the shales along the shore between Seafield and Granton are very low in the Carboniferous system. All that I have seen confirms this conclusion. I was amused, indeed, to see them in an otherwise well got up map, lately published, coloured as the Millstone grit! Antiquated, surely! The fossils are generally identifiable with those which are everywhere found to underlie the marine limestones (in the Scotch beds, at any rate), and from all that the drift will let one see, there must be several thousand feet of such rocks with the Wardic and Granton beds near the base. This being so, the occurrence of this vertebrate so low down is of interest and importance, and helps to confirm Prof. Fritsch's view, arrived at in his case from anatomical considerations, that *Ophiderpeton* and its allies are the roots of the Amphibian genetic tree.

T. STOCK

A NUMERICAL ESTIMATE OF THE RIGIDITY OF THE EARTH¹

ABOUT fifteen years ago Sir William Thomson pointed out that, however it be constituted, the body of the earth must of necessity yield to the tidal forces due to the attraction of the sun and moon, and he discussed the rigidity of the earth on the hypothesis that it is an elastic body.

If the solid earth were to yield as much as a perfect fluid to these forces, the tides in an ocean on its surface would necessarily be evanescent, and if the yielding be of smaller amount, but still sensible, there must be a sensible reduction in the height of the oceanic tides.

Sir William Thomson appealed to the universal existence of oceanic tides of considerable height as a proof that the earth, as a whole, possesses a high degree of rigidity, and maintained that the previously received geological hypothesis of a fluid interior was untenable. At the same time he suggested that careful observation would afford a means of arriving at a numerical estimate of the average modulus of the rigidity of the earth's mass as a whole. The semi-diurnal and diurnal tides present phenomena of such complexity, that it is quite beyond the power

¹ Paper read by G. H. Darwin, F.R.S., at the British Association Southampton meeting.

of mathematics to calculate what these heights would be, if the earth's mass were absolutely unyielding. But the tides of long period are nearly free from the dynamical influences which render those of short period so intractable to calculation, and must in fact nearly follow the laws of the "equilibrium theory."

In 1867 it was not, however, even definitely known whether or not the tides of long period were of sensible height at any station. Although there has been a continual advance in the knowledge of tidal phenomena since that time, it is only within the last year that there is a sufficient accumulation of tidal observations, properly reduced by harmonic analysis, to make it possible to carry out Sir William Thomson's suggestion. The great advances in knowledge that have been recently made are principally due to the adoption of systematic tidal observation at a great number of stations by the Indian Government. The results of these observations are now being issued yearly by the Secretary of State for India in the form of tide-tables for the principal Indian ports. I have had the pleasure of carrying out the examination of the tidal records, and a detailed account of the work will appear at § 848 of the new edition of Thomson and Tait's "Natural Philosophy," now in the press.

The tides chosen for discussion were the lunar fortnightly declinational tide, and the lunar monthly elliptic tide. These tides must be free from the meteorological disturbances which make the heights of all the solar tides quite beyond prediction. The fortnightly and monthly tides consist in an alternate increase and diminution of the ellipticity of the elliptic spheroid of which the sea level (after elimination of the tidal oscillations of short period) forms a part. There are two parallels of latitude respectively north and south of the equator which are nodal lines, along which the water neither rises nor falls. When, in the northern hemisphere, the water is highest to the north of the nodal line of evanescent tide, it is lowest to the south of it, and *vice versa*; and the like is true of the southern hemisphere. If the ocean covered the whole earth, the nodal lines would be in latitudes $35^{\circ} 16' N.$ and $S.$ (at which latitudes $\frac{1}{2} - \sin^2 \text{lat.}$ vanishes); but when the existence of land is taken into consideration, the nodal latitudes are shifted. Now according to Sir William Thomson's amended equilibrium theory of the tides, the shifting of the nodal latitudes depends on a certain definite integral, whose limits are determined by the distribution of land on the earth's surface.

For the purpose of examining the tidal records, it was therefore first necessary to evaluate this integral. Approximation is of course unavoidable, and for that end the irregular contours of the continents were replaced by meridians and parallels of latitude, and the integral evaluated by quadrature. This procedure will give results quite accurate enough for practical purposes. It appeared as the result of the quadrature that, if we assume the existence of a large Antarctic continent, the latitude of evanescent tide is $34^{\circ} 40'$, and if there is no such continent it is $34^{\circ} 57'$. Hence the displacement of the nodal latitudes due to the existence of land is very small.

This point having been settled, the mathematical expressions for the fortnightly and monthly tides are completely determinate, according to the equilibrium theory, with no yielding of the earth's mass.

If there is yielding of the earth, either with perfect or imperfect elasticity, and with frictional resistance to the motion of the water, the height of tide and the time of high water must depart from the laws assigned by the equilibrium theory. This conclusion may also be stated in another way, which is more convenient for practical purposes; for we may say that at any station there must actually be a tide with a height equal to some fraction of the full equilibrium height, and with high water exactly at the theoretical time, and a second tide, of exactly the same nature, with a height equal to some other fraction of the equilibrium height, but differing in the time of high water by a quarter-period from the theoretical time, viz. about three-and-a-half-days for the fortnightly, and a week for the monthly tide. These two tides may, according to geometrical analogy, be called perpendicular component tides. According to the theory of the composition of harmonic motions, the two components may be compounded into a single tide, with time of high water occurring within a half-period of the theoretical time; and this is the way in which the results of elastic yielding and frictional resistance were first stated above. Thus the actual tide at any station involves two unknown fractions, x and y , being the factors by which two components, each of the full theoretical height, are to be multiplied in order to give the two components in proper amount to represent the reality.

If the equilibrium theory is fulfilled without sensible elastic yielding of the earth, the first component has its full value, or x is equal to one, and the second component vanishes, or y is zero. If fluid friction exercises a sensible influence, y will have a sensible value; and if the solid earth yields tidally, x will be less than unity. The amount of elastic yielding, and hence the average modulus of elasticity of the whole earth may be computed from the value of x . After rejecting the observations made at certain stations for sufficient reasons, I obtained from the Tidal Reports of the British Association and from the Indian Tide Tables, the results of thirty-three years of observation, made at fourteen different ports in England, France, and India.

These results, when properly reduced, gave thirty-three equations for the x and thirty-three for the y of the fortnightly tide, and similarly thirty-three for the x and thirty-three for the y of the monthly tide; in all 132 equations for four unknowns.

The x and y of the two classes of tide were in the first instance regarded as distinct, but the manner in which they arise shows that it is legitimate to regard them as identical, and thus we have sixty-six equations for x and sixty-six for y .

The equations were then reduced by the methods of least squares, with the following results:—

For the fortnightly tide—

$$x = \cdot 675 \pm \cdot 056, y = \cdot 020 \pm \cdot 055.$$

And for the monthly tide—

$$x = \cdot 680 \pm \cdot 258, y = \cdot 090 \pm \cdot 218.$$

The numbers given with alternative signs are the probable errors.

The very close agreement between the x and y for the two tides is probably somewhat due to chance.

The smallness of the two y 's is satisfactory; for, as above stated, if the equilibrium theory were true, they should vanish. Moreover, the signs are in agreement with what they should be, if friction is a sensible cause of tidal retardation. But considering the magnitude of the probable errors, it is of course more likely that the non-evanescence of the y 's is due to errors of observation or to the method of reduction.

I have already submitted to the British Association at this meeting a paper on a misprint, discovered by Prof. Adams, in the tidal report for 1872. This report forms the basis of the method of harmonic analysis which has been employed in the reduction of the tidal observations, and it appears that the erroneous formula has been systematically used. The large probable error in the value of the monthly tide may most probably be reduced by a correct treatment of the original tidal records.

It has been already remarked that it is legitimate to combine all the observations together, for both sorts of tide, and thus to obtain a single x and y from sixty-six years of observation. Carrying out this idea, I find:

$$x = \cdot 676 \pm \cdot 076, y = \cdot 029 \pm \cdot 065.$$

These results really seem to present evidence of a tidal yielding of the earth's mass, and the value of the x is such as to show that the effective rigidity of the whole earth is about equal to that of steel.

But this result is open to some doubt for the following reason:—

Taking only the Indian results (forty-eight years in all), which are much more consistent than the English ones, I find

$$x = \cdot 931 \pm \cdot 056, y = \cdot 155 \pm \cdot 068.$$

We thus see that the more consistent observations seem to bring out the tides more nearly to their theoretical equilibrium values with no elastic yielding of the solid.

It is to be observed however that the Indian results being confined within a narrow range of latitude give (especially when we consider the absence of minute accuracy in my evaluation of the definite integral) a less searching test for the elastic yielding than a combination of results from all latitudes.

On the whole we may fairly conclude that, whilst there is some evidence of a tidal yielding of the earth's mass, that yielding is certainly small, and that the effective rigidity is at least as great as that of steel.

SCIENTIFIC SERIALS

The Journal of Physiology, vol. iii. Nos. 5 and 6, August, 1882, with Supplement number. No. 11 contains:—Optical illusions of motion, by H. P. Bowditch and G. S. Hall.—On

reflex movements of the frog under the influence of strychnia, by G. L. Walton.—A contribution to our knowledge of the action of certain drugs upon bodily temperature, by H. C. Wood and E. T. Reichert.—Influence of Peptones and certain inorganic salts on the diastatic action of saliva, by R. H. Chittenden and J. S. Ely.—On cerebral localisation, by S. Exner.—The physiological action of methylcyanethine, by G. L. Walton.—On the influence of variations of intra-cardiac pressure upon the inhibitory action of the vagus nerve, by H. Sewell and F. Donaldson.—Preliminary observations on the innervation of the heart of the tortoise, by W. H. Gaskell.—Concerning the influence exerted by each of the constituents of the blood on the contraction of the ventricle, by S. Ringer (plate xix.).—The Supplement contains a list of works and papers on physiology published in 1881.

The American Naturalist for October, 1882, contains:—Sketch of the progress of North American Ichthyology in the year 1880-81, by W. N. Lockington.—On the methods of microscopic research in the zoological station at Naples, by C. O. Whitman.—On the homologies of the crustacean limb, by A. S. Packard, jun.—On the idols and idol worship of the Delaware Indians, by C. C. Abbott.

Journal de Physique, September.—Dynamo-electric machines with continuous currents, by M. Potier.—Influence of a metal on the nature of the surface of another metal placed at a very small distance, by M. Pellat.

SOCIETIES AND ACADEMIES
LONDON

Mineralogical Society, October 24.—Anniversary Meeting. W. H. Huddleston, F.G.S., president, in the chair.—Nine new Members were elected.—The officers and Council were elected for the ensuing year, the only changes being the election of Messrs. T. D. Gibb, T. M. Hall, Jas. P. Anson, and H. M. Plattnault, on the Council in place of Dr. Aitken, Professors Crum Brown and Hughes, and Mr. Louis, who retired in rotation.—It was resolved to hold the meetings of the Society at fixed dates for the ensuing year, viz., on December 13, 1882, February 15, May 15, and October 23 (Anniversary), the meeting for May to be held in Scotland.—The Report of the Council was read and adopted.

PARIS

Academy of Sciences, October 25.—M. Blanchard in the chair.—Herr Wiedemann presented the first volume of a new work by him, "Die Lehre von der Electricität."—On the effect of a stroke of an inclined cue on a billiard ball, by M. Résal.—Separation of gallium (continued), by M. Lecoq de Boisbaudran.—Contribution to the study of typhoid fever in Paris; the present epidemic, from September 22 to October 19, 1882, by M. de Pietra Santa. There have been 2225 deaths this year (up to the latter date), more than during the whole of last year (2130), (628 in the last four weeks). All the twenty arrondissements have been affected, and all the eighty quarters, except the four American and that of St. Fargeau in the west and Salpêtrière and Petit-Montrouge in the south. The seventh arrondissement has suffered most. M. Santa notices the unwholesome state of the houses.—On a bed of coal discovered in the province of Algiers, and on layers of white sand accompanying it, by M. Pinard. This is near Bou Saada. The coal is at least equivalent in illuminating power and yield of gas to the best French and English coal. The yield of coke varies between 62 and 66 per cent. The sand, which might be used for the finest glass, and is very abundant, is the product of disaggregation of immense banks of grit.—Results of modes of treatment adopted in 1881-82, in the Alpes-Maritimes, for destruction of phylloxera, by M. Laugier. More than 200 hectares have been treated with sulphide of carbon and sulphocarbonate of potassium, and the results are very satisfactory.—Observations of the great comet (Cruls) at the Observatory of Marseilles, by M. Borrelly.—Spectroscopic observations on the same comet, by MM. Thollon and Gouy. On October 9, the sodium lines seen on September 11, had disappeared; the four ordinary carbon bands were present; the nucleus gave a narrow continuous spectrum with many dark and bright lines. On the 16th the violet band was almost gone, and the continuous spectrum considerably weakened. The disappearance of the sodium lines and others observed by M. Loise shows that under ordinary conditions the spectroscope cannot give us a complete analysis of cometary

matter. If the temperature is sufficient to produce the emission spectrum of carbon compounds, it should be sufficient to produce that of sodium; but the facts are contrary. The authors incline to the electric theory of comets; in the case of a gaseous carburet traversed by the effluve from a Holtz machine, and holding fine metallic dust in suspension, the carbon bands appear, but not the metallic lines.—Relations between the residues of a function of an analytic point (x, y), which is reproduced, multiplied by a constant, when the point (x, y) describes a cycle, by M. Appell.—On the hyper-geometric functions of two variables, by M. Goursat.—Decomposition of a whole number N into its maximum n th powers, by M. Lemoine.—Lunar induction and its periods, by M. Quet.—On the automatic transmission and registration of messages of optic telegraphy, by M. de Brettes. A claim of priority.—On metallic thorium, by M. Nilson. He has reduced thorium by heating with sodium, the anhydrous double chloride of thorium and potassium, and adding to the mixture chloride of sodium; all in an iron crucible. The specific gravity of the pure metal is about 11.00; the substance, as prepared by Chydenius (density 7.657-7.795), was probably impure. For atomic volume, M. Nilson gets the value 21.1 (coinciding with the atomic volumes of zirconium (21.7), cerium (21.1), lanthanum (22.6), and didymium (21.5)).—Determination of the equivalent of thorium, by the same. The equivalent is equal to 58.10, if that of oxygen = 8 and that of sulphur = 16.—On benzylene orthotolidine and methyl phenanthridine, by M. Etard.—On the reduction of nitrates in arable land, by MM. Deherain and Maquenne. An earth loses the property of reducing nitrates when it has been heated or submitted to chloroform vapours. Earth that has lost the property through heat, reduces anew when a little normal earth is added.—On the convulsing action of curare, by M. Couty. Curare is not only a paralyzing poison, but also in the first place, slightly convulsing; nor merely a peripheric poison, but also, in certain measure, a poison of the nerve-centres.—On parasites of the blood in impaludism, by M. Laveran. He has observed them in 300 cases.—Isanemones of summer in the North Atlantic, by M. Brault. These curves of equal velocity of wind coincide with the isobars.—On turfiform constructions of earth-worms in France, by M. Trouessart. He observed them in gardens in Angiers, and found they were produced by *Lumbricus Agricola*. Darwin knew only of this production by a *Perichaeta* naturalised at Nice, from the east. M. Gautrelat, in a note, affirmed that M. Le Bon's glyceroborate of soda is not a definite salt, but a mixture of monoborine (monoboric ether of glycerine), sub-borate of soda, and glycerine.—A map, by M. Durand Claye was presented, showing the increase of population in the department of the Seine, and adjacent parts of the department of Seine-et-Oise. The variations of growth are indicated by means of curves called *isoplethes*.—Some documents from M. de Lesseps, relating to construction of the hospital of Panama, by the Canal Company, were presented.

CONTENTS

PAGE

HYDRAULIC EXPERIMENTS	I
LETTERS TO THE EDITOR:—	
" Weather Forecasts."—The BISHOP of CARLISLE	4
The Comet.—Major J. HERSCHEL; GEO. M. SEABROKE; ARTHUR WATTS	4
The Burman.—SHWAY YOE; Dr. E. B. TYLOR, F.R.S.	5
River Thames—Abnormal High Tides.—J. B. REDMAN	6
Umdhlebi Tree of Zululand.—W. T. THISELTON DYER, C.M.G., F.R.S.; Rev. Dr. G. W. PARKER	7
The Origin of our Vernal Flora.—J. E. TAYLOR	7
On Coral-eating Habits of Holothuriads.—Surgeon-Major H. B. GUPPY	7
Railway Geology—a Hint.—J. C. G.	8
Complementary Colours.—JOSEPH JOHN MURPHY	8
Palaeolithic River Gravels.—C. EVANS	8
LAVOISIER, PRIESTLEY, and the DISCOVERY of OXYGEN. By G. F. RODWELL	8
A NEW DREDGING IMPLEMENT. By A. MILNES MARSHALL	11
WIRE GUNS. By JAMES A. LONGRIDGE, C.E. (<i>With Diagrams</i>)	11
MR. FORBES' ZOOLOGICAL EXPEDITION OF THE NIGER	14
WORK IN THE INFRA-RED OF THE SPECTRUM. By Capt. W. DE W. ARNEY, R.E., F.R.S. (<i>With Diagrams</i>)	15
NOTES	18
OUR ASTRONOMICAL COLUMN:—	
Schmidt's Cometary Object	20
Comet 1882 <i>b</i>	21
GEOGRAPHICAL NOTES	21
NOTICE OF SOME DISCOVERIES RECENTLY MADE IN CARBONIFEROUS VERTEBRATE PALAEONTOLOGY. By T. STOCK	22
A NUMERICAL ESTIMATE OF THE RIGIDITY OF THE EARTH. By G. H. DARWIN, F.R.S.	22
SCIENTIFIC SERIALS	23
SOCIETIES AND ACADEMIES	24