system of reports from ships' logs which has been carried on since Christmas by the Meteorological Offices of France and this country, and to endeavour to improve it.

At the same time a proposal made by M. L. Teisserenc de Bort for the telegraphic transmission of a daily *résumé* of the weather in the New England States was considered. General Hazen expressed perfect readiness to furnish such reports, and it was resolved to procure such telegrams provided the cost of the service could be guaranteed by the European offices which would participate in it.

It was decided to recommend that barometrical observations should be corrected for the force of gravity at lat. 45°.

A letter from General Hazen respecting the reduction of barometer readings to sea-level, which has been lately circulated, was considered, and two memoranda on the subject from Hamburg and St. Petersburg respectively were handed in and will be printed.

It was considered *desirable*, as absolute synchronism in weather observations appears to be unattainable in Europe, that the same hours of local time should be adopted in each country (which would mean a change from 8 a.m. to 7 a.m. in this country).

It was decided that each of the International Reduction Tables (proposed by the Committee at its meeting at Berne in 1880) as did not involve any question which is still in an undecided state (such as, *e.g.*, hygrometrical tables, or tables of sea-level reduction) should be published.

It was decided to recommend that the next Congress should not take place till 1889, and Prof. Mascart stated that probably the French Government would propose that it should be held in Paris.

THE BRITISH ASSOCIATION

JUDGED by the quantity of work which the sections have put through their hands the Aberdeen meeting has been successful almost beyond precedent. Moreover much of this work has been of the best quality. The addresses come up to a very high standard, and in the first four sections, at least, not a few of the papers were really important original contributions to science, while the discussions in Sections A and B on certain great questions in physics and chemistry were a marked and commendable feature—a feature which, it is hoped, will in time become common to all the sections. Mr. Murray's lecture on deep-sea research has been justly considered one of the leading events of the meeting; a full report will appear in our columns.

At the concluding general meeting a deservedly hearty vote of thanks was accorded to the Aberdonians for their abundant hospitality. Birmingham seems determined to make next year's meeting a memorable one; and we may remind our readers that Sir William Dawson, of McGill College, Montreal, will be the President.

The total number of persons who attended the Aberdeen meeting was 2203.

The following is a synopsis of grants of money appropriated to scientific purposes by the General Committee at the Aberdeen meeting. The names of the members who would be entitled to call on the General Treasurer for the respective grants are prefixed:—

A—Mathematics and Physics	
*Foster, Prof. G. Carey—Electrical Standards	£40
*Stewart, Prof. Balfour—Solar Radiation	20
*Stewart, Prof. Balfour—Meteorological Observations at Chepstow	25
Darwin, Prof. G. H.—Instructions for Tidal Observations	50
*Stewart, Prof. Balfour—Comparing and Reducing Magnetic Observations	40
*Forbes, Prof. G.—Standards of Light	20
*Brown, Prof. Crum—Ben Nevis Observatory	100
*Armstrong, Prof.—Physical and Chemical Bearings of Electrolysis	20
B—Chemistry	
M'Leod, Prof.—Silent Discharge of Electricity into Atmosphere	£20
*Williamson, Prof. A. W.—Chemical Nomenclature ...	5
C—Geology	
*Blanford, Mr. W. T.—Fossil Plants of the Tertiary and Secondary Bed	20
Hughes, Prof. T. McK.—Caves of North Wales ...	25
*Etheridge, Mr. R.—Volcano Phenomena in Japan ...	50
*Grantham, Mr. R. B.—Erosion of Sea Coasts	20
*Bannerman, Mr. H.—Volcanic Phenomena of Vesuvius	30
*Evans, Dr. J.—Geological Record	100
*Etheridge, Mr. R.—Fossil Phyllopoda	15
D—Biology	
*Stanton, Mr. H. T.—Zoological Record	100
*Murray, Mr. J.—Marine Biological Station at Granton..	75
*Lankester—Prof. Ray—Zoological Station at Naples ...	50
Cleland, Prof.—Researches in Food Fishes at St. Andrew's	75
*Cordeaux, Mr. J.—Migration of Birds	30
Cleland, Prof.—Mechanism of Secretion of Urine ...	10
E—Geography	
Walker, General J. T.—New Guinea Exploration ...	150
Walker, General J. T.—Investigation into Depth of Permanently Frozen Soil in Polar Regions	5
F—Economic Science and Statistics	
Sidgwick, Prof.—Regulation of Wages under Sliding Scales	10
G—Mechanics	
Barlow, Mr. W. H.—Effect of Varying Stresses on Metals	10
H—Anthropology	
Garson, Dr.—Investigation into a Prehistoric Race in the Greek Islands	20
*Tylor, Dr. E. B.—Investigation into North-Western Tribes of Canada	50
*Galton, Mr. F.—Racial Characteristics in British Isles..	10
	£1195

* Reappointed.

REPORTS

Report of the Committee, consisting of Mr. Robert H. Scott (Secretary), Mr. J. Norman Lockyer, Prof. G. G. Stokes, Prof. Balfour Stewart, and Mr. G. J. Symons, appointed for the purpose of co-operating with the Meteorological Society of the Mauritius in their proposed publication of Daily Synoptic Charts of the Indian Ocean from the year 1861. *Drawn up by Mr. R. H. Scott.*—The Committee forward, for the inspection of the members of the Association, a copy of the charts for the month of March, 1861, with some specimens for January of the same year, and the complete number for February which appeared some years ago. These documents have recently arrived from the Mauritius. As the work has now made decided progress the Committee have applied for and obtained the grant of 50*l.* placed at their disposal by the General Committee. As soon as the requisite documents are received from Dr. Meldrum the Committee will submit a formal account of their expenditure with the necessary vouchers.

Second Report of the Committee, consisting of Prof. Schuster (Secretary), Prof. Balfour Stewart, Prof. Stokes, Mr. G. Johnstone Stoney, Prof. Sir H. E. Roscoe, Capt. Abney, and Mr. G. J. Symons, appointed for the purpose of considering the best methods of recording the direct Intensity of Solar Radiation.—The Committee have come to the following conclusions:—(1) It seems desirable to construct an instrument which would be a modification of Prof. Stewart's actinometer adapted for self-registration—the quantity to be observed being, not the rise of temperature of the enclosed thermometer after exposure for a given time, but the excess of its temperature when continuously exposed over the temperature of the envelope. (2) As the grant to the Committee will not admit of the purchase of a heliostat, it will no doubt be possible to procure the loan of such a instrument, and, by making by its means sufficiently numerous comparisons of the instrument proposed by the Committee with an ordinary actinometer, to find whether the arrangement suggested by the Committee is likely to succeed in practice. The Committee would therefore confine their action for the present to the carrying out of such a series of comparisons. (3) The size of the instrument might be the same as that of Prof. Stewart's actinometer. (4) The instrument should have a thick metallic enclosure, as in the actinometer above-mentioned, and in this enclosure there should be inserted a thermometer to record its temperature. Great pains should therefore be taken to construct this enclosure so that its temperature shall be the same throughout. (5) The interior thermometer should be so constructed as to be readily susceptible of solar influences. It is proposed to make it of green glass (a good absorber), and to give it a flattened surface in the direction perpendicular to the light from the hole. (6) It seems desirable to concentrate the sun's light by means of a lens upon the interior thermometer, as in the ordinary instrument. For if there were no lens the hole would require to be large, and it would be more difficult to prevent the heat from the sky around the sun from interfering with the determination. Again, with a lens there would be great facility in adjusting the amount of heat to be received by employing a set of diaphragms. There are thus considerable advantages in a lens, and there does not appear to be any objection to its use.

Third Report of the Committee, consisting of Profs. G. H. Darwin and J. C. Adams, for the Harmonic Analysis of Tidal Observations. Drawn up by Prof. G. H. Darwin.—"Record of Work during the past Year." The edition of the computation forms referred to in the second report is now completed, and copies are on sale with the Cambridge Scientific Instrument Company, St. Tibbs' Row, Cambridge, at the price of 2*s.* 6*d.* each. Some copies of the first report, in which the theory and use of these forms are explained, are also on sale at the same price. A few copies of the computation forms have been sent to the librarians of some of the principal scientific academies of Europe and America. In South Africa, Mr. Gill, at the Cape, and Mr. Neison, at Natal, are now engaged in reducing observations with forms supplied from this edition. A memorial has been addressed to the Government of the Dominion of Canada, urging the desirability of systematic tidal observation, and the publication of tide-tables for the Canadian coasts. There seems to be good hope that a number of tide-gauges will shortly be set up on the Atlantic and Pacific coasts, and in the Gulf of the St. Lawrence. The observations will probably be reduced according to the methods of the British Association, and the predictions made with the instrument of the Indian Government. Major Baird has completed the reduction of all the tidal results obtained at the Indian stations to the standard forms proposed in the Report of 1883, and Mr. Roberts has similarly reduced a few results read before the Association by Sir William Thomson and Capt. Evans in 1878. All these are now being published in the *Proceedings* of the Royal Society, in a paper by Major Baird and myself. A large number of tidal results have been obtained by the United States Coast Survey, and reduced under the superintendence of Prof. Ferrel. Although the method pursued by him has been slightly different from that of the British Association, it appears that the American results should be comparable with those at the Indian and European ports. Prof. Ferrel has given an assurance that this is the case; nevertheless, there appears to be strong internal evidence that, at some of the ports, some of the phases should be altered by 180°. The doubt thus raised will probably be removed, and the paper before the Royal Society will afford a table of reference for all —or nearly all—the results of the harmonic method up to the date of its publication. The manual of the tidal observation promised by Major Baird is now completed, and will be published shortly. This work will explain fully all the practical difficulties likely to be encountered in the choice of a station for a tide-gauge, and in the erection and working of the instrument. Major Baird's great experience in India, and the success with which the operations of which he has had charge have been carried out, render his advice of great value for the prosecution of tidal observation in other countries. The work also explains the method of measuring the tide diagrams, entering the figures in the computation forms, and the subsequent numerical operations.

Second Report of the Committee, consisting of Prof. Balfour Stewart (Secretary), Mr. J. Knox Laughton, Mr. G. J. Symons, Mr. R. H. Scott, and Mr. Johnstone Stoney, appointed for the purpose of cooperating with Mr. E. J. Lowe in his project of establishing a Meteorological Observatory near Chepstow on a permanent and scientific basis.—Since their re-appointment in 1885 this Committee have met twice, and have placed themselves in correspondence with Mr. Lowe, to whom the following letter was written by their Secretary: "The Committee request me to point out to you that the main feature of your proposal, which interests the British Association and the scientific public generally, is the prospect which it holds out of the establishment of a *permanent* institution, by means of which meteorological constants could be determined, and any secular change which may take place therein in the course of a long period of years be ascertained. It will be for you and the local authorities to decide what amount of work of *local interest* should be contemplated, and on this will the scale of the observatory mainly depend. The Committee are therefore unable to say what amount of capital would be required. They would point out four conditions which they hold to be indispensable:—(1) The area of ground appropriated should be sufficient to ensure freedom from the effects of subsequent building in the neighbourhood. (2) A sufficient endowment fund of at least 150*l.* annually should be created. (3) The control should be in the hands of a body which is in itself permanent as far as can be foreseen. (4) The land for the site shall be handed over absolutely to the above-mentioned governing body. Until the precise amount of the local meteorological requirements is ascertained and further progress is made in the scheme the Committee consider that they would not be justified in any more prominent action than that which they have already taken.

Report of the Committee, consisting of Profs. A. Johnson (Secretary), J. G. MacGregor, J. B. Cherriman, H. T. Bovey, and Mr. C. Carpmael, appointed for the purpose of promoting Tidal Observations in Canada.—The Committee, in order to strengthen their representation to the Canadian Government on the necessity of establishing stations for continuous tidal observations, deemed it well to get the opinions of Boards of Trade and ship-owners and ship-masters. On inquiry it appeared that the Montreal Board of Trade were at the very time considering the question, which had been brought independently before them. On learning the object of the Committee they gave it their most hearty support, and addressed a strong memorial on the subject to the Dominion Government. The Boards of Trade of the other chief ports of the Dominion also sent similar memorials. The ship-owners and masters of ships, to whom application was made, were practically unanimous in their testimony as to the pressing need for knowledge on the subject. The representations were made through the Minister of Marine, with whom an interview was obtained, at which a memorial was submitted. Copies of the answers of the ship-masters (a large number of which had been received) were submitted at the same time. The reply of the Minister of Marine stated that, owing to the large outlay on the Georgian Bay Survey and on the expedition to Hudson's Bay during the past summer (1885), the Government did not propose to take action in the matter of tidal observations at present. The Committee have reason to believe that if the financial prospects improve by next session of Parliament the Government will take the matter into earnest consideration; they therefore suggest that the Committee be reappointed.

Seventeenth Report of the Committee, consisting of Profs. Everet and Sir W. Thomson, Mr. G. J. Symons, Sir A. C. Ramsay, Dr. A. Geikie, Mr. J. Glaisher, Mr. Pengelly, Prof. Edward Hull. Prof. Prestwich, Dr. C. Le Neve Foster, Prof. A. S. Herschel, Prof. G. A. Lebour, Mr. Galloway, Mr. Joseph Dickinson, Mr. G. F. Deacon, Mr. E. Wethered, and Mr. A. Strahan, appointed for the purpose of investigating the Rate of Increase of Underground Temperature downwards in various Localities of Dry Land and under Water. Drawn up by Prof. Everett (Secretary).—The present Report is for the two years since the summer of 1883. Observations have been taken in a deep bore at Richmond, Surrey, by Mr. Collett Homersham, C.E., the engineer of the boring, on the premises of the Richmond Vestry Waterworks, on the right bank of the Thames, and about 33 yards from high-water mark. The surface is 17 feet above Ordnance datum. The upper part consists of a well 253 feet deep, with an internal diameter of 7 feet at top and 5 feet at bottom, which was sunk in 1876 for the purpose of supplying water to the town of Richmond, and carried down to the chalk. From the bottom of the well a 24-inch bore-hole was sunk to the total depth of 434 feet, thus penetrating 181 feet into the chalk. This portion of the work was completed in 1877. Above the chalk were tertiaries, consisting of 160 feet of London clay, 60 feet of the Woolwich and Reading beds, and some underlying sands. The water yielded at this stage was about 160 gallons a minute, and, when not depressed by pumping, was able to rise 4 or 5 feet above the surface. Its ordinary level, owing to pumping, was about 130 feet lower. In 1881 the Richmond Vestry determined to carry the bore-hole to a much greater depth, and the deepening has been executed under the direction of Mr. Homersham. The existing bore-hole was first enlarged and straightened, to enable a line of cast-iron pipes, with an internal diameter of $16\frac{1}{2}$ inches, having the lower end driven water-tight into the chalk at a depth of 438 feet, to be carried up to the surface. The total thickness of the chalk was 671 feet. Below this was the upper greensand, 16 feet thick; then the gault clay, $201\frac{1}{2}$ feet thick; then 10 feet of a sandy rock, and a thin layer of phosphatic nodules. Down to this point the new boring had yielded no water. Then followed a bed $87\frac{1}{2}$ feet thick, consisting mainly of hard oolitic limestone. Two small springs of water were met with in this bed at the depths of 1203 and 1210 feet, the yield at the surface being $1\frac{1}{4}$ gallons a minute, with power to rise in a tube and overflow 49 feet above the ground. A partial analysis of this limestone rock showed it to contain 2·4 per cent. of sulphide of iron in the form of pyrites. At the depth of 1239 feet this limestone rock ended, and hard red sandstone was found, alternating with beds of variegated sandy marl or clay. After the depth of 1253 feet had been attained, the yield of water steadily increased as the boring was deepened, the overflow at the surface being 2 gallons a minute at 1254 feet, 8 gallons at 1363 feet, and 11 gallons at 1387 feet. It rose to the top of a tube carried 49 feet above the surface, and overflowed; and a pressure-gauge showed that it had power to rise 126 feet above the surface. The diameter of the bore was $16\frac{1}{4}$ inches in the chalk, $13\frac{1}{2}$ inches in the gault, $11\frac{1}{4}$ inches in the oolitic limestone, and at the depth of 1334 feet it was reduced to a little under 9 inches. At 1337 feet the method of boring was changed, and, instead of an annular arrangement of steel cutters, a rotary diamond rock-boring machine was employed. The bore-hole, with a diameter of $8\frac{1}{2}$ inches, was thus carried down to $1367\frac{1}{2}$ feet, at which depth, lining tubes having to be inserted, the diameter was reduced to $7\frac{1}{4}$ inches, and this size was continued to 1447 feet, at which depth the boring was stopped. The bore-hole was lined with strong iron tubes down to the depth of 1364 feet; and those portions of the tubes that are in proximity to the depths where water was struck were drilled with holes to admit the water into them. Three observations of temperature taken with an inverted Negretti maximum at the depth of 1337 feet, when the bore-hole was full of water, recorded $75\frac{1}{2}°$ F. In the first observation, March 25, 1884, the thermometer was left for an hour and a quarter at the bottom of the bore-hole, and three weeks had elapsed since the water was disturbed by boring. The second observation was taken on March 31, when the thermometer was $5\frac{1}{2}$ hours at the bottom. In the third observation special precautions were taken to prevent convection. The thermometer was fixed inside a wrought-iron tube, 5 feet long, open at bottom. The thermometer was near the lower end of the tube, and was suspended from a water-tight wooden plug, tightly driven into the tube. There was a space of several inches between the plug and the thermometer, and this part of the tube was pierced with numerous holes to allow the escape of any cold water which might be carried down by the tube. The tube was one of a series of hollow boring-rods used in working the diamond drill-machine. By means of these it was lowered very slowly, to avoid disturbance of the water as much as possible; and the tube containing the thermometer was gradually worked through the sand at the bottom of the bore-hole. The lowering occupied five hours, and was completed at noon on Saturday, June 7. Cement, mixed with sugar, for the purpose of slow setting, was immediately lowered on to the surface of the sand, and above this a mixture of cement and sand, making a total thickness of 3 or 4 feet of cement plugging. The thermometer was left in its place for three full days, the operation of raising being commenced at noon of Tuesday, June 10, and completed at 5 p.m. The thermometer again registered $75\frac{1}{2}°$ F., exactly the same as in the two previous observations which were taken without plugging. It would therefore appear that the steady upflow of water in the lower part of the bore prevents any downward convection of colder water from above.

The boring has since been carried to the depth of 1447 feet, with a diameter reduced to $7\frac{1}{8}$ inches, and Mr. Homersham lowered the thermometer to the bottom without plugging. It remained down for six days (February 3 to 9, 1885), and gave a reading of $76\frac{3}{4}°$ F. The water overflowing at the surface had a temperature of 59° F. To deduce the mean rate of increase downwards, we shall assume a surface temperature of 50°. This gives for the first 1337 feet an increase of $25\frac{1}{2}°$, which is at the rate of 1° F. in 52·4 feet, and for the whole 1447 feet an increase of $26\frac{3}{4}°$, which is at the rate of 1° F. in 54·1 feet. These results agree well with the Kentish Town well, where Mr. Symons found in 1100 feet an average increase of 1° in 55 feet.

Mr. Galloway has furnished observations taken during the sinking of a shaft to the depth of 1272 feet in or near the Aberdare valley, Glamorganshire. The position of the shaft is on the slope on the east side of the valley, about midway between the bottom of the valley and the summit of the hill which separates it from the Merthyr valley. The mouth of the shaft is about 800 feet above sea-level. Observations were taken at four different depths—546 feet, 780 feet, 1020 feet, and 1272 feet—the thermometer being in each case inserted, and left for twenty-four hours, in a hole bored to the depth of 30 inches at a distance not exceeding $2\frac{1}{2}$ yards from the bottom of the shaft for the time being. About eight hours elapsed between the completion of the hole and the insertion of the thermometer. The strata consist mainly of shales and sandstone, with a dip of 1 in 12, and the flow of water into the shaft was about 250 gallons per hour. The first of the four observations was taken in the fireclay under the Abergorkie vein; the second in strong "clift" (a local name for argillaceous shale) in disturbed ground; the third in bastard fireclay under a small rider of coal previously unknown; the fourth in "clift" ground two yards above the red ash vein, which overlies the 9-foot seam at a height of from 9 to 12 yards. The observations were as follow:—At 546 feet, 56° F.; 780 feet, $59\frac{1}{2}°$ F.; 1020 feet, 63° F.; 1272 feet, $66\frac{1}{2}°$ F. Comparing consecutive depths from 546 feet downwards, we have the following increments of temperature:—$3\frac{1}{2}°$ in 234 feet, giving 1° for 67 feet; $3\frac{1}{2}°$ in 240 feet, giving 1° for 69 feet; $3\frac{1}{2}°$ in 252 feet, giving 1° for 72 feet; showing a remarkably regular rate of increase. A comparison of the first and fourth observations gives an increase of $10\frac{1}{2}°$ in 726 feet, which is at the rate of 1° F. in 69·1 feet. As a check upon this result we find that this rate of *decrease* reckoned *upwards* from the smallest depth (546 feet) would give a surface temperature of (56 − 7·9 =) 48°·1, which, as the elevation is 800 feet, is probably very near the truth.

Mr. Garside has sent an observation of temperature taken by himself in the roof of the Mersey tunnel in August, 1883. The temperature was 53°, the depth below Ordnance datum being 92 feet. A great quantity of water from the river was percolating through the sides of the tunnel. On August 13, 1854, he verified his previous observation in Denton Colliery (15th Report). The second observation was made at the same depth as the first (1317 feet), in the same pit and level, and under the same circumstances, except that the thermometer was allowed to remain fourteen days in the hole bored for it, instead of only six hours. The temperature observed was the same as before—namely 66°. Mr. Garside has also supplemented his previous contribution to our knowledge of the surface temperature of the ground in the East Manchester coal-field (16th Report) by two more years' results from the same observing stations. The difference between them agrees well with the generally accepted rate of 1° for 300 feet, and indicates about 48° as the surface temperature at small elevations, such as 30 feet. The pits in the East Manchester coal-field from which we have observations—namely, Astley Pit (Dukinfield), Ashton Moss, Bredbury, Denton, and Nook Pit, are all sunk in ground at elevations of between 300 and 350 feet. It would therefore appear that the assumption of a surface temperature of 49°, which we made in reducing these observations, is about 2° in excess of the truth. A very elaborate paper on "Underground Temperature" has recently been communicated to the Royal Society by Prof. Prestwich. He is disposed to adopt 1° F. in 45 feet as the most probable value of the normal gradient.

Report of the Committee, consisting of Mr. W. T. Blanford and Mr. J. S. Gardner (Secretary), on the Fossil Plants of the Tertiary and Secondary Beds of the United Kingdom. Drawn *up by Mr. J. S. Gardner, F.G.S., F.L.S.*—The report opens with a list of all the principal works on the British Tertiary flora down to the year 188Ↄ. The number of species that had been more or less described were :—From the Thanet beds, 3 ; from the Reading beds, 9 ; from Sheppey, 108 ; from Alum Bay, &c., 43 ; from Bournemouth (deducting those not peculiar), 11 ; Bovey Tracy, 50 ; Upper Eocenes, 13 ; Mull, 9 ; Antrim, about 16 ; making a grand total of 262 species, not a tenth part of which, Mr. Gardner anticipates, would survive a rigorous examination. The study of only one group of plants—the Gymnosperms—has been the serious business of the past three years ; for not only have I had to study, but in the majority of cases to find the specimens as well. I trust that the results attending the expenditure of the grant I have been favoured with may be considered satisfactory, and these I now proceed to detail.

Bracklesham Flora.—Two visits have been made to Selsey. The beds, it is well known, are marine, but a few terrestrial fruits are from time to time procured from them. I was able to make a large collection of fossil shells while looking for plants, which, being from the highest beds, are less known, and are interesting as illustrating the passage from the Bracklesham to the Barton fauna, which is more gradual, I think, than is supposed. The surface of one of these beds is dotted over with fossil *Posidonias*, a marine monocotyledonous plant identical with the species now inhabiting the Mediterranean. It had not been previously recorded as a British fossil, though another species is abundant in the contemporary beds of the *Calcaire grossier* of the Paris basin. In our species the rhizomes radiate from a centre, whilst in the French and other European fossil species they are long and branching. They are found among beautiful *Tellina* shells, preserving, to a large extent, their banded colours. The only other fossil plant to record here is a *Nipadites*, which, unlike those of the Bournemouth beds, is large, flattened, and oval.

Reading Beds.—A considerable portion of the grant has been expended in working these beds with, I am pleased to report, the happiest results. The flora is found in the Katesgrove pit, on the banks of the Kennet, immediately beneath the mottled clay. The matrix is a fine porcelainous fuller's earth interstratified with sand, and the beds seem very local. The limit of the pit being reached, it is not probable that any part of the beds will be exposed for long. I have illustrated a beautiful specimen—one of several—of *Anæmia subcretacea*, Sap., from these beds. This fern is highly characteristic of the lower Eocenes in France, but had only previously been found in the middle Bagshot beds of Bournemouth in this country. I have also illustrated another fern (?) from these beds, of which I have only as yet found a small fragment. The figures are therefore taken from specimens found many years ago by Prof. Prestwich. Other valuable additions to the Reading flora are some splendid specimens of a conifer, which I can see no ground for distinguishing from *Taxodium heterophyllum* of China. Another interesting specimen from Reading is a pine leaf of two needles, about the size and substance of those of *P. maritima*, the first pine foliage, I believe, ever found in the English Eocene. One leaf bed is almost wholly made up of leaves of *Plutonis*, and a bed above is fairly sprinkled with fruits of the same. Fruits are very abundant, and include four kinds of leguminous pods, and there are many flowers. As a result of this work the Reading flora no longer appears so completely distinct from that of Bournemouth.

Woolwich Beds.—I regard these as thoroughly distinct in age from those of Reading. I have not found, in the course of two visits paid for the purpose, any bed worth collecting from, though I think such must exist at Lewisham.

Studland Beds.—We were able to reach a leaf bed in the Lower Bagshot at Studland, and to obtain a great number of specimens, nearly all of whieh are quite new to me. They are mostly dycotyledonous leaves and fruits, which will require time to determine. There are no Coniferæ among them, and I am only able to add one fern—a *Lygodium*, very near to that of Bournemouth—to the *Chrysodium langæanum*, procured abundantly by me ten years ago in a different bed at the same locality.

Hordwell Beds.—I have to add *Salvinia* to the flora, not previously found fossil in England, and exclusively confined to the Miocene in Austria and Switzerland.

Barton Beds.—A new species of pine from Highcliff was discovered quite unlike those hitherto found at Bracklesham. The beds are rapidly assuming an angle of repose, and becoming deeply buried under *débris*, so that some of them are no longer visible except by making excavations. Though the Barton series is one of the most interesting of our Eocene formations, the detailed bedding has not been worked out like that of the Bracklesham series below and the Headon series above, and the greatest misconceptions seem to prevail as to the number of species of fossils that it contains.

Bournemouth Beds.—Five series of leaves were obtained this year by Mr. Keeping and myself, the most noteworthy of which are some specimens of *Godoya* which exceed any I had previously seen. I have illustrated a new and very distinct species of *Adiantum*, a fragment of what may be *Gymnogramma*, and a trifid group of *Polypodium* leaves, which seem to be different from either of the species previously recorded.

The London Clay.—Mr. Shrubsole has kindly sent me some of the best of the fruits that have been found. I have not made any complete studies of them yet, but they promise to afford results of the highest value. Among a few recognised is the very unmistakable seed of *Verschaffeltia*, a genus of palms from Seychelles quite new to fossil floras.

Gurnet Bay Beds.—I have been able to ascertain that another fern rivals *Anæmia subcretacea* in range, *Chrysodium langæanum*, which extends from the town of Bagshot upwards into the Bembridge beds. The plants are as a rule dreadfully macerated and chopped up. Among them are small fragments of a *Gleichenia*, which, though not very beautiful, is a very important fern, coming from the horizon. By far the most important discovery, however, is that of *Doliostrobus*, the first really extinct conifer that I have met with in British Eocenes. It belonged to the tribe of *Araucarieæ*, and its identification has been thoroughly confirmed by correspondence and the interchange of specimens with Dr. Marion, the well-known botanist of Marseilles. It is certain that during the Eocene period, as the temperature increased from the base upward to the Middle Bagshot, when the maximum of heat seems to have prevailed, there was a tendency for the plant world to move northward. It is equally certain that in the later half of the Eocene, as the temperature began to decrease, the movement was in the opposite direction, and we find in the European Miocenes of Switzerland and Italy a number of plants that at an earlier period were growing in the far north.

Report of the Committee, consisting of H. Bauerman, F. W. Rudler, and Dr. H. Johnston Lavis, for the Investigation of the Volcanic Phenomena of Vesuvius, by H. Johnston Lavis, M.D., F.G.S., Reporter.—The unfortunate outbreak of cholera in Naples and the stringent local quarantine measures prevented work on Vesuvius being carried out during the autumn of 1884. Nevertheless, daily observations were made of the variations in the activity of the volcano, of which a careful record has been kept. All important changes of the crater-plain, and in the cone of eruption, have been photographed. Descriptions of the small eruption of May 2 of 1883 have already been given in NATURE, and the results of a microscopical examination of the sides of the remarkable hollow dyke then formed will soon be published. The Naples section of the Italian Alpine Club have generously undertaken to publish a journal of Vesuvius, which will contain reproductions of the photographs exhibited. The third sheet of the geological map of Vesuvius and Monte Somnia (scale 1 : 10,000) has been completed by the reporter, and is exhibited at the meeting. The relationship of the varying activity of a volcano in a Strombolian state of activity to barometric pressure, the lunar tides, and rainfall, cannot but be regarded as important in solving some questions of vulcanology. Instrumental means of measuring such present so many practical difficulties that a scale of activity has been drawn up, which requires only a few minutes to learn, can be practised by any one with good eyesight and moderate intelligence who is within visual range of the volcano, and, above all, requires no further outlay than pen, ink, and paper. The objections will be mentioned after describing the process. 1st degree, a faint red glimmer above the main vent interrupted by complete darkness ; 2nd degree, the glimmer is continuous, but the ejection reaches hardly above the central crater rim at the most ; 3rd degree, glimmer continuous and well marked ; the ejections are distinctly discernible as they rise and then fall on the slopes of the cone of eruption and roll down its slopes ; 4th degree, the ejections reach a considerable height, are brilliant, and light up the top of the great cone ; 5th degree, verging on an actual paroxysmal eruption, the ejections are shot up very high, being only very slightly or not at all influenced in their course by a strong wind. Each explosion follows with much rapidity, and corresponds with the "boati" heard all around the west, south, and south-east slopes of the mountain. The objections to this method of registering the variations in the activity of a volcano are: (*a*) cloud-cap, which may for days cut off the view; (*b*) after a great eruption, resulting in a deep crater, the changes of activity would be invisible from the neighbourhood of the mountain; (*c*) it is only applicable after dark, so that usually only one observation a day can be made; (*d*) should lava be flowing from a lateral outlet, as is often the case, the level of the fluid in the chimney would vary as the outflow took place with greater or less rapidity, dependent on its blocking the passage more or less. The reporter thinks it desirable to introduce a description of this method into the report, so that it may be made use of in the case of other suitable volcanoes.

Report of the Committee, consisting of Prof. Ray Lankester, Mr. P. L. Sclater, Prof. M. Foster, Mr. A. Sedgwick, Prof. A. M. Marshall, Prof. A. C. Haddon, Prof. Moseley, and Mr. Percy Sladen (Secretary), appointed for the purpose of arranging for the occupation of a Table at the Zoological Station at Naples.—In the Report read last year at Montreal it was announced that a scheme was on foot for the building of a large physiological laboratory in connection with the Zoological Station at Naples, and for the purchase of a new sea-going steamer, to be equipped as a floating laboratory. Your Committee are now able to report that both these projects are steadily advancing towards attainment. For the physiological laboratory the Municipality of Naples has made a grant of 400 square metres of ground, and the Italian Parliament has voted the sum of 50,000 lire towards the cost of building. In addition to this assistance from the Italian Government, a union of the maritime provinces of South Italy is about to be formed for the purpose of contributing towards the cost of the new laboratory, and of maintaining two tables there for the use of natives of the provinces concerned. The new steamship, which it is hoped will shortly be in the possession of the station, will form a further addition to the capabilities of the establishment This undertaking is in the hands of an influential committee in Germany, organised for the purpose of collecting subscriptions, and by whom the vessel will be presented to the station. It is intended that the steamer should be of 300 to 400 tons burden, with engines of 150 to 200 horse-power, and be fitted up in all respects as a floating laboratory. With such a vessel it will be perfectly practicable to remain weeks or months in any desired locality, and distance from home will be no obstacle, as naturalists will live and work on board. Concurrent with these strides of the Zoological Station, improvements in the general management, in methods of work, and in instruments of research are constantly being made. The general efficiency of the establishment is so well known that it will suffice to say that the whole organisation of the station is in a state of active and prosperous vitality. The best evidence of this is furnished by the accompanying lists:—(1) of the naturalists who have occupied tables during the past year, and (2) of the publications resulting from work carried out at the station.

The General Collections.—Additions have been again received from Capt. Chierchia, who has, since the last Report, sent two collections of specimens from the Pacific and Indian Oceans. Other collections have been likewise received from Lieut. Cercone, Lieut. Orsini, and Lieut. Colombo, from the Atlantic, the Red Sea, and the Mediterranean respectively. Some of the material previously obtained by Capt. Chierchia has already been utilised by Count Béla Haller in a paper on the molluscan kidney, recently published; and the same author is at present preparing a monograph on the Patellæ. In like manner the Pteropoda have been investigated by Dr. Boas, of Copenhagen, whose monograph upon the subject is now in the press. Since the last Report the British Association table has been occupied by Mr. Wm. E. Hoyle, who, although limited in time, was enabled to prosecute researches on the embryology of the Cephalopoda, and to collect material from which important results may be expected. The report forwarded by Mr. Hoyle is appended:—

Report on the Occupation of the Table, by Mr. William E. Hoyle.—I reached Naples on April 6, 1885, and left on the 28th of the same month. In so short a time it was obviously impossible to make anything of the nature of a complete investigation in a subject of such magnitude and difficulty as the embryology of the Cephalopoda; it seemed, herefore, that the opportunities afforded me could best be utilised by collecting material for subsequent examination. Of this I had an abundant and immediate supply, thanks to the kindly forethought of your secretary, who had given notice to the authorities of the station of the nature of the work I had undertaken, so that they had a quantity of ova ready for my use. The greater part of my time was spent in extracting embryos from the egg and preserving them in various fluids, and a fairly complete series of developmental stages of *Loligo* and a good many embryos of *Sepia* were thus obtained. When the young Cephalopods have reached a stage at which the rudiments of the arms are clearly visible, it is moderately easy, after a little practice, to extricate them by making an incision into the egg-membrane with a fine scalpel; but previously to this period they so nearly occupy the whole interior of the egg that it is almost impossible to obtain them uninjured. A quantity of such eggs I preserved whole by a method suggested to me by Dr. Jatta, who is at work upon a monograph of the Cephalopoda of the Bay of Naples. The strings of eggs are placed whole in weak solution of chromic acid (about 0·25 per cent.) for a few hours, and then in distilled water for twenty-four hours, after which they are preserved in alcohol. The embryos can then be extracted much more readily than when fresh. Some time was devoted to examining and drawing the embryos in the fresh condition, and in watching the process of segmentation in *Loligo* and *Sepia*. I observed the presence of the "Richtungsbläschen" in the former, which, so far as I am aware, has only been noted in a Russian memoir on the development of *Sepiola* by Ussow. A number of blastoderms in process of segmentation were preserved according to a method proposed by Ussow, for the knowledge of which I am indebted to Dr. Edward Meyer, who kindly translated it for me from the original. The egg, without removal of the membranes, is placed in 2 per cent. solution of chromic acid for two minutes, and then in distilled water, to which a little acetic acid (one drop to a watchglassful) has been added, for two minutes longer. If an incision be now made into the egg-membrane the yolk flows away and the blastoderm remains; if any yolk still cling to it, it may be removed by pouring away the water and adding more. The blastoderms thus prepared show, when appropriately stained, fine karyokinetic figures, of which I hope shortly to publish an account. The reduction of the collected embryos to serial sections and their examination will of course occupy some time, but I hope in a few months to prepare some account of the results obtained from them.

Report of the Committee, consisting of Prof. Huxley, Mr. Sclater, Mr. Howard Saunders, Mr. Thiselton Dyer, and Prof. Moseley (Secretary), appointed for the purpose of promoting the Establishment of Marine Biological Stations on the Coast of the United Kingdom.—The Committee has received the sum granted (150*l.*) from the Treasurer of the Association, and has paid it to the funds of the Marine Biological Association of the United Kingdom, as the most direct means of promoting the speedy establishment of a marine laboratory in a most favourable situation on the British coast—namely, Plymouth. An excellent site for a laboratory has been granted to the Marine Biological Association by Government, at Plymouth. A sum of 8000*l.* has been raised by subscriptions and donations, the Government has promised to aid the working of the laboratory by an annual subsidy, and there is every prospect of success. It is probable that the building of the laboratory will commence in November.

Report of the Committee, consisting of the Rev. Canon Tristram, the Rev. F. Lawrence, and Mr. James Glaisher (Secretary), appointed for the purpose of promoting the Survey of Palestine.—The Survey of Eastern Palestine has been carried on during the last year privately by Herr G. Schumacher, C.E., assisted by Mr. Laurence Oliphant, who has also furnished the Committee with valuable notes of personal exploration in the district now called Junlau—the ancient Gaulanitis. The portion surveyed by Herr Schumacher consists of about 200 square miles, and covers an area previously quite unknown. The map, which is now in the hands of the Committee, is accompanied by voluminous memoirs and a great number of sketches, drawings, and plans of ruins figured for the first time, which it is proposed to publish, with the memoirs, in October. The map of the Wady Arabah has been laid down in the Society's sheets; the geological memoirs compiled by Prof. Hule after his expedition of 1883-84 are nearly ready, and will be issued before the end of the year; and the Society has been enabled to secure Mr. Chichester Hart's

Natural History memoir, made from new observations during the same journey. In addition the Committee have received from Mr. Guy Le Strange, and published, observations and notes made by him during a recent journey east of Jordan. The results of the survey, so far as it has been completed, will appear in a map reduced to a scale of about three miles to an inch, showing the country on both sides of the river Jordan, instead of on the western side only. This portion of the work is under the direction of Col. Sir Charles Wilson, K.C.M.G., F.R.S. The Society has also issued during the last year a popular account, by Prof. Hule, of his recent journey, called "Mount Seir," and reprints of Capt. Conder's popular books, "Tent Work in Palestine" and "Heth and Moab." Finally, the Committee have completed the issue of their great work, the "Survey of Western Palestine," with the last volumes of "Jerusalem," the "Flora and Fauna," and a portfolio of plates showing the excavations and their results.

SECTION H

ANTHROPOLOGY

OPENING ADDRESS BY FRANCIS GALTON, F.R.S., ETC., PRESIDENT OF THE ANTHROPOLOGICAL INSTITUTE, PRESIDENT OF THE SECTION

THE object of the Anthropologist is plain. He seeks to learn what mankind really are in body and mind, how they came to be what they are, and whither their races are tending; but the methods by which this definite inquiry has to be pursued are extremely diverse. Those of the geologist, the antiquarian, the jurist, the historian, the philologist, the traveller, the artist, and the statistician, are all employed, and the Science of Man progresses through the help of specialists. Under these circumstances, I think it best to follow an example occasionally set by presidents of sections, by giving a lecture rather than an address, selecting for my subject one that has long been my favourite pursuit, on which I have been working with fresh data during many recent months, and about which I have something new to say.

My data were the Family Records entrusted to me by persons living in all parts of the country, and I am now glad to think that the publication of some first-fruits of their analysis will show to many careful and intelligent correspondents that their painstaking has not been thrown away. I shall refer to only a part of the work already completed, which in due time will be published, and must be satisfied if, when I have finished this address, some few ideas that lie at the root of heredity shall have been clearly apprehended, and their wide bearings more or less distinctly perceived. I am the more desirous of speaking on heredity, because, judging from private conversations and inquiries that are often put to me, the popular views of what may be expected from inheritance seem neither clear nor just.

The subject of my remarks will be "Types and their Inheritance." I shall discuss the conditions of the stability and instability of types, and hope in doing so to place beyond doubt the existence of a simple and far-reaching law that governs hereditary transmission, and to which I once before ventured to draw attention, on far more slender evidence than I now possess.

It is some years since I made an extensive series of experiments on the produce of seeds of different size but of the same species. They yielded results that seemed very noteworthy, and I used them as the basis of a lecture before the Royal Institution on February 9, 1877. It appeared from these experiments that the offspring did *not* tend to resemble their parent seeds in size, but to be always more mediocre than they—to be smaller than the parents, if the parents were large; to be larger than the parents, if the parents were very small. The point of convergence was considerably below the average size of the seeds contained in the large bagful I bought at a nursery-garden, out of which I selected those that were sown.

The experiments showed further that the mean filial regression towards mediocrity was directly proportional to the parental deviation from it. This curious result was based on so many plantings, conducted for me by friends living in various parts of the country, from Nairn in the north to Cornwall in the south, during one, two, or even three generations of the plants, that I could entertain no doubt of the truth of my conclusions. The exact ratio of regression remained a little doubtful, owing to variable influences; therefore I did not attempt to define it. After the lecture had been published, it occurred to me that the grounds of my misgivings might be urged as objections to the general conclusions. I did not think them of moment, but as the inquiry had been surrounded with many small difficulties and matters of detail, it would be scarcely possible to give a brief and yet a full and adequate answer to such objections. Also, I was then blind to what I now perceive to be the simple explanation of the phenomenon, so I thought it better to say no more upon the subject until I should obtain independent evidence. It was anthropological evidence that I desired, caring only for the seeds as means of throwing light on heredity in man. I tried in vain for a long and weary time to obtain it in sufficient abundance, and my failure was a cogent motive, together with others, in inducing me to make an offer of prizes for family records, which was largely responded to, and furnished me last year with what I wanted. I especially guarded myself against making any allusion to this particular inquiry in my prospectus, lest a bias should be given to the returns. I now can securely contemplate the possibility of the records of height having been frequently drawn up in a careless fashion, because no amount of unbiassed inaccuracy can account for the results, contrasted in their values but concurrent in their significance, that are derived from comparisons between different groups of the returns.

An analysis of the records fully confirms and goes far beyond the conclusions I obtained from the seeds. It gives the numerical value of the regression towards mediocrity as from 1 to $\frac{2}{3}$ with unexpected coherence and precision, and it supplies me with the class of facts I wanted to investigate—the degrees of family likeness in different degrees of kinship, and the steps through which special family peculiarities become merged into the typical characteristics of the race at large.

The subject of the inquiry on which I am about to speak was Hereditary Stature. My data consisted of the heights of 930 adult children and of their respective parentages, 205 in number. In every case I transmuted the female statures to their corresponding male equivalents and used them in their transmuted form, so that no objection grounded on the sexual difference of stature need be raised when I speak of averages. The factor I used was 1·08, which is equivalent to adding a little less than one-twelfth to each female height. It differs a very little from the factors employed by other anthropologists, who, moreover, differ a trifle between themselves; anyhow it suits my data better than 1·07 or 1·09. The final result is not of a kind to be affected by these minute details, for it happened that, owing to a mistaken direction, the computer to whom I first entrusted the figures used a somewhat different factor, yet the result came out closely the same.

I shall explain with fulness why I chose stature for the subject of inquiry, because the peculiarities and points to be attended to in the investigation will manifest themselves best by doing so. Many of its advantages are obvious enough, such as the ease and frequency with which its measurement is made, its practical constancy during thirty-five years of middle life, its small dependence on differences of bringing up, and its inconsiderable influence on the rate of mortality. Other advantages which are not equally obvious are no less great. One of these lies in the fact that stature is not a simple element, but a sum of the accumulated lengths or thicknesses of more than a hundred bodily parts, each so distinct from the rest as to have earned a name by which it can be specified. The list of them includes about fifty separate bones, situated in the skull, the spine, the pelvis, the two legs, and the two ankles and feet. The bones in both the lower limbs are counted, because it is the average length of these two limbs that contributes to the general stature. The cartilages interposed between the bones, two at each joint, are rather more numerous than the bones themselves. The fleshy parts of the scalp of the head and of the soles of the feet conclude the list. Account should also be taken of the shape and set of many of the bones which conduce to a more or less arched instep, straight back, or high head. I noticed in the skeleton of O'Brien, the Irish giant, at the College of Surgeons, which is, I believe, the tallest skeleton in any museum, that his extraordinary stature of about 7 feet 7 inches would have been a trifle increased if the faces of his dorsal vertebræ had been more parallel and his back consequently straighter.

The beautiful regularity in the statures of a population, whenever they are statistically marshalled in the order of their heights, is due to the number of variable elements of which the stature is the sum. The best illustrations I have seen of this regularity were the curves of male and female statures that I obtained from the careful measurements made at my Anthropometric Laboratory in the International Health Exhibition last year. They were almost perfect.

The multiplicity of elements, some derived from one progenitor, some from another, must be the cause of a fact that has proved very convenient in the course of my inquiry. It is that the stature of the children depends closely on the average stature of the two parents, and may be considered in practice as having nothing to do with their individual heights. The fact was proved as follows :—After transmuting the female measurements in the way already explained, I sorted the children of parents who severally differed 1, 2, 3, 4, and 5, or more inches into separate groups. Each group was then divided into similar classes, showing the number of cases in which the children differed 1, 2, 3, &c., inches from the common average of the children in their respective families. I confined my inquiry to large families of six children and upwards, that the common average of each might be a trustworthy point of reference. The entries in each of the different groups were then seen to run in the same way, except that in the last of them the children showed a faint tendency to fall into two sets, one taking after the tall parent, the other after the short one. Therefore, when dealing with the transmission of stature from parents to children, the average height of the two parents, or, as I prefer to call it, the "mid-parental" height, is all we need care to know about them.

It must be noted that I use the word parent without specifying the sex. The methods of statistics permit us to employ this abstract term, because the cases of a tall father being married to a short mother are balanced by those of a short father being married to a tall mother. I use the word "parent" to save a complication due to a fact brought out by these inquiries, that the height of the children of both sexes, but especially that of the daughters, takes after the height of the father more than it does after that of the mother. My present data are insufficient to determine the ratio satisfactorily.

Another great merit of stature as a subject for inquiries into heredity is that marriage selection takes little or no account of shortness or tallness. There are undoubtedly sexual preferences for moderate contrast in height, but the marriage choice appears to be guided by so many and more important considerations that questions of stature exert no perceptible influence upon it. This is by no means my only inquiry into this subject, but, as regards the present data, my test lay in dividing the 205 male parents and the 205 female parents each into three groups—tall, medium. and short (medium being taken as 67 inches and upwards to 70 inches)—and in counting the number of marriages in each possible combination between them. The result was that men and women of contrasted heights, short and tall or tall and short, married just about as frequently as men and women of similar heights, both tall or both short; there were 32 cases of the one to 27 of the other. In applying the law of probabilities to investigations into heredity of stature, we may regard the married folk as couples picked out of the general population at haphazard.

The advantages of stature as a subject in which the simple laws of heredity may be studied will now be understood. It is a nearly constant value that is frequently measured and recorded, and its discussion is little entangled with considerations of nurture, of the survival of the fittest, or of marriage selection. We have only to consider the mid-parentage and not to trouble ourselves about the parents separately. The statistical variations of stature are extremely regular, so much so that their general conformity with the results of calculations based on the abstract law of frequency of error is an accepted fact by anthropologists. I have made much use of the properties of that law in cross-testing my various conclusions, and always with success.

The only drawback to the use of stature is its small variability. One-half of the population with whom I dealt varied less than 1·7 inch from the average of all of them, and one-half of the offspring of similar mid-parentages varied less than 1·5 inch from the average of their own heights. On the other hand, the precision of my data is so small, partly due to the uncertainty in many cases whether the height was measured with the shoes on or off, that I find by means of an independent inquiry that each observation, taking one with another, is liable to an error that as often as not exceeds $\frac{2}{3}$ of an inch.

It must be clearly understood that my inquiry is primarily into the inheritance of different degrees of tallness and shortness. That is to say, of measurements made from the crown of the head to the level of mediocrity, upwards or downwards as the case may be, and not from the crown of the head to the ground. In the population with which I deal, the level of mediocrity is $68\frac{1}{4}$ inches (without shoes). The same law, applying with sufficient closeness both to tallness and shortness, we may include both under the single head of deviations, and I shall call any particular deviation a "deviate." By the use of this word and that of "mid-parentage," we can define the law of regression very briefly. It is that the height-deviate of the offspring is, on the average, two-thirds of the height-deviate of its mid-parentage.

If this remarkable law had been based only on experiments on the diameters of the seeds, it might well be distrusted until confirmed by other inquiries. If it were corroborated merely by the observations on human stature, of which I am about to speak, some hesitation might be expected before its truth could be recognised in opposition to the current belief that the child tends to resemble its parents. But more can be urged than this. It is easily to be shown that we ought to expect filial regression, and that it should amount to some constant fractional part of the value of the mid-parental deviation. It is because this explanation confirms the previous observations made both on seeds and on men, that I feel justified on the present occasion in drawing attention to this elementary law.

The explanation of it is as follows. The child inherits partly from his parents, partly from his ancestry. Speaking generally, the further his genealogy goes back, the more numerous and varied will his ancestry become, until they cease to differ from any equally numerous sample taken at haphazard from the race at large. Their mean stature will then be the same as that of the race; in other words, it will be mediocre. Or, to put the same fact into another form, the most probable value of the mid-ancestral deviates in any remote generation is zero.

For the moment let us confine our attention to the remote ancestry and to the mid-parentages, and ignore the intermediate generations. The combination of the zero of the ancestry with the deviate of the mid-parentage, is that of nothing with something, and the result resembles that of pouring a uniform proportion of pure water into a vessel of wine. It dilutes the wine to a constant fraction of its original alcoholic strength, whatever that strength may have been.

The intermediate generations will each in their degree do the same. The mid-deviate of any one of them will have a value intermediate between that of the mid-parentage and the zero value of the ancestry. Its combination with the mid-parental deviate will be as if, not pure water, but a mixture of wine and water in some definite proportion had been poured into the wine. The process throughout is one of proportionate dilutions, and therefore the joint effect of all of them is to weaken the original wine in a constant ratio.

We have no word to express the form of that ideal and composite progenitor, whom the offspring of similar mid-parentages most nearly resemble, and from whose stature their own respective heights diverge evenly, above and below. He, she, or it, may be styled the "generant" of the group. I shall shortly explain what my notion of a generant is, but for the moment it is sufficient to show that the parents are not identical with the generant of their own offspring.

The average regression of the offspring to a constant fraction of their respective mid-parental deviations, which was first observed in the diameters of seeds, and then confirmed by observations on human stature, is now shown to be a perfectly reasonable law which might have been deductively foreseen. It is of so simple a character that I have made an arrangement with one movable pulley and two fixed ones by which the probable average height of the children of known parents can be mechanically reckoned. This law tells heavily against the full hereditary transmission of any rare and valuable gift, as only a few of many children would resemble their mid-parentage. The more exceptional the gift, the more exceptional will be the good fortune of a parent who has a son who equals, and still more if he has a son who overpasses him. The law is even-handed; it levies the same heavy succession-tax on the transmission of badness as well as of goodness. If it discourages the extravagant expectations of gifted parents that their children will inherit all their powers, it no less discountenances extravagant fears that they will inherit all their weaknesses and diseases.

The converse of this law is very far from being its numerical opposite. Because the most probable deviate of the son is only two-thirds that of his mid-parentage, it does not in the least follow that the most probable deviate of the mid-parentage is $\frac{3}{2}$, or $1\frac{1}{2}$ that of the son. The number of individuals in a population who differ little from mediocrity is so preponderant, that it is more frequently the case that an exceptional man is the somewhat exceptional son of rather mediocre parents, than the average son of very exceptional parents. It appears from the very same table of observations by which the value of the filial regression was determined, when it is read in a different way, namely, in vertical columns instead of in horizontal lines, that the most probable mid-parentage of a man is one that deviates only one-third as much as the man does. There is a great difference between this value of $\frac{1}{3}$ and the numerical converse mentioned above of $\frac{3}{2}$; it is four and a half times smaller, since $4\frac{1}{2}$, or $\frac{9}{2}$, being multiplied into $\frac{1}{3}$, is equal to $\frac{3}{2}$.

Let it not be supposed for a moment that these figures invalidate the general doctrine that the children of a gifted pair are much more likely to be gifted than the children of a mediocre pair. What it asserts is that the ablest child of one gifted pair is not likely to be as gifted as the ablest of all the children of very many mediocre pairs. However, as, notwithstanding this explanation, some suspicion may remain of a paradox lurking in these strongly contrasted results, I will explain the form in which he table of data was drawn up, and give an anecdote connected with it. Its outline was constructed by ruling a sheet into squares, and writing a series of heights in inches, such as 60 and under 61, 61 and under 62, &c., along its top, and another similar series down its side. The former referred to the height of offspring, the latter to that of mid-parentages. Each square in the table was formed by the intersection of a vertical column with a horizontal one, and in each square was inserted the number of children out of the 930 who were of the height indicated by the heading of the vertical column, and who at the same time were born of mid-parentages of the height indicated at the side of the horizontal column. I take an entry out of the table as an example. In the square where the vertical column headed[1] 69– is intersected by the horizontal column by whose side 67– is marked, the entry 38 is found; this means that out of the 930 children 38 were born of mid-parentages of 69 and under 70 inches, who also were 67 and under 68 inches in height. I found it hard at first to catch the full significance of the entries in the table, which had curious relations that were very interesting to investigate. Lines drawn through entries of the same value formed a series of concentric and similar ellipses. Their common centre lay at the intersection of the vertical and horizontal lines, that corresponded to $68\frac{1}{4}$ inches. Their axes were similarly inclined. The points where each ellipse in succession was touched by a horizontal tangent, lay in a straight line inclined to the vertical in the ratio of $\frac{2}{3}$; those where they were touched by a vertical tangent, lay in a straight line inclined to the horizontal in the ratio of $\frac{1}{3}$. These ratios confirm the values of average regression already obtained by a different method, of $\frac{2}{3}$ from mid-parent to offspring, and of $\frac{1}{3}$ from offspring to mid-parent. These and other relations were evidently a subject for mathematical analysis and verification. They were all clearly dependent on three elementary data, supposing the law of frequency of error to be applicable throughout; these data being (1) the measure of racial variability, (2) that of co-family variability (counting the offspring of like mid-parentages as members of the same co-family), and (3) the average ratio of regression. I noted these values, and phrased the problem in abstract terms such as a competent mathematician could deal with, disentangled from all reference to heredity, and in that shape submitted it to Mr. J. Hamilton Dickson, of St. Peter's College, Cambridge. I asked him kindly to investigate for me the surface of frequency of error that would result from these three data, and the various particulars of its sections, one of which would form the ellipses to which I have alluded.

I may be permitted to say that I never felt such a glow of loyalty and respect towards the sovereignty and magnificent sway of mathematical analysis as when his answer reached me, confirming, by purely mathematical reasoning, my various and laborious statistical conclusions with far more minuteness than I had dared to hope, for the original data ran somewhat roughly, and I had to smooth them with tender caution. His calculation corrected my observed value of mid-parental regression from $\frac{1}{3}$ to $\frac{6}{17 \cdot 6}$, the relation between the major and minor axis of the ellipses was changed 3 per cent., their inclination was changed less than 2°. It is obvious, then, that the law of error holds throughout the investigation with sufficient precision to be of real service, and that the various results of my statistics are not casual determinations, but strictly interdependent.

In the lecture at the Royal Institution to which I have referred, I pointed out the remarkable way in which one generation was succeeded by another that proved to be its statistical counterpart. I there had to discuss the various agencies of the survival of the fittest, of relative fertility and so forth; but the selection of human stature as the subject of investigation now enables me to get rid of all these complications, and to discuss this very curious question under its simplest form. How is it, I ask, that in each successive generation there proves to be the same number of men per thousand who range between any limits of stature we please to specify, although the tall men are rarely descended from equally tall parents, or the short men from equally short? How is the balance from other sources so nicely made up? The answer is that the process comprises two opposite sets of actions, one concentrative and the other dispersive, and of such a character that they necessarily neutralise one another, and fall into a state of stable equilibrium. By the first set, a system of scattered elements is replaced by another system which is less scattered; by the second set, each of these new elements becomes a centre whence a third system of elements are dispersed. The details are as follows:—In the first of these two stages, the units of the population group themselves, as it were by chance, into married couples, whence the mid-parentages are derived, and then by a regression of the values of the mid-parentages the true generants are derived. In the second stage each generant is a centre whence the offspring diverge. The stability of the balance between the opposed tendencies is due to the regression being proportionate to the deviation; it acts like a spring against a weight.

A simple equation connects the three data of race variability, of the ratio of regression, and of co-family variability, whence, if any two are given, the third may be found. My observations give separate measures of all three, and their values fit well into the equation, which is of the simple form—

$$v^2 \frac{p^2}{2} + f^2 = p^2,$$

where $v = \frac{2}{3}$, $p = 1 \cdot 7$, $f = 1 \cdot 5$.

It will therefore be understood that a complete table of mid-parental and filial heights may be calculated from two simple numbers.

It will be gathered from what has been said, that a mid-parental deviate of one unit implies a mid-grandparental deviate of $\frac{1}{3}$, a mid-ancestral unit in the next generation of $\frac{1}{9}$, and so on. I reckon from these and other data, by methods that I cannot stop to explain, that the heritage derived on an average from the mid-parental deviate, independently of what it may imply, or of what may be known concerning the previous ancestry, is only $\frac{1}{2}$. Consequently, that similarly derived from a single parent is only $\frac{1}{4}$, and that from a single grandparent is only $\frac{1}{16}$.

The most elementary data upon which a complete table of mid-parental and filial heights admits of being constructed are (1) the ratio between the mid-parental and the rest of the ancestral influences, and (2) the measure of the co-family variability.

I cannot now pursue the numerous branches that spring from the data I have given, as from a root. I will not speak of the continued domination of one type over others, nor of the persistency of unimportant characteristics, nor of the inheritance of disease, which is complicated in many cases by the requisite concurrence of two separate heritages, the one of a susceptible constitution, the other of the germs of the disease. Still less can I enter upon the subject of fraternal characteristics, which I have also worked out. It will suffice for the present to have shown some of the more important conditions associated with the idea of race, and how the vague word "type" may be defined by peculiarities in hereditary transmission, at all events when that word is applied to any single quality, such as stature. To include those numerous qualities that are not strictly measurable, we must omit reference to number and proportion, and frame the definition thus:—"The type is an ideal form towards which the children of those who deviate from it tend to regress."

The stability of a type would, I presume, be measured by the

[1] A matter of detail is here ignored which has nothing to do with the main principle, and would only serve to perplex if I described it.

strength of its tendency to regress; thus a mean regression from 1 in the mid-parents to ⅔ in the offspring would indicate only half as much stability as if it had been to ⅓.

The mean regression in stature of a population is easily ascertained, but I do not see much use in knowing it. It has already been stated that half the population vary less than 1·7 inch from mediocrity, this being what is technically known as the "probable" deviation. The mean deviation is, by a well-known theory, 1·18 times that of the probable deviation, therefore in this case it is 1·9 inch. The mean loss through regression is ⅓ of that amount, or a little more than 0·6 inch. That is to say, taking one child with another, the mean amount by which they fall short of their mid-parental peculiarity of stature is rather more than six-tenths of an inch.

With respect to these and the other numerical estimates, I wish emphatically to say that I offer them only as being serviceably approximate, though they are mutually consistent, and with the desire that they may be reinvestigated by the help of more abundant and much more accurate measurements than those I have had at command. There are many simple and interesting relations to which I am still unable to assign numerical values for lack of adequate material, such as that to which I referred some time back of the superior influence of the father over the mother on the stature of their sons and daughters.

The limits of deviation beyond which there is no regression, but a new condition of equilibrium is entered into, and a new type comes into existence, have still to be explored. Let us consider how much we can infer from undisputed facts of heredity regarding the conditions amid which any form of stable equilibrium, such as is implied by the word "type," must be established, or might be disestablised and superseded by another. In doing so I will follow cautiously along the same path by which Darwin started to construct his provisional theory of pangenesis; but it is not in the least necessary to go so far as that theory, or to entangle ourselves in any questioned hypothesis.

There can be no doubt that heredity proceeds to a considerable extent, perhaps principally, in a piecemeal or piebald fashion, causing the person of the child to be to that extent a mosaic of independent ancestral heritages, one part coming with more or less variation from this progenitor and another from that. To express this aspect of inheritance, where particle proceeds from particle, we may conveniently describe it as "particulate."

So far as the transmission of any feature may be regarded as an example of particulate inheritance, so far (it seems little more than a truism to assert) the element from which that feature was developed must have been particulate also. Therefore, wherever a feature in a child was not personally possessed by either parent, but transmitted through one of them from a more distant progenitor, the element whence that feature was developed must have existed in a particulate, though impersonal and latent, form in the body of the parent. The total heritage of that parent will have included a greater variety of material than was utilised in the formation of his own personal structure. Only a portion of it became developed; the survival of at least a small part of the remainder is proved, and that of a larger part may be inferred by his transmitting it to the person of his child. Therefore the organised structure of each individual should be viewed as the fulfilment of only one out of an indefinite number of mutually exclusive possibilities. It is the development of a single sample drawn out of a group of elements. The conditions under which each element in the sample became selected are, of course, unknown, but it is reasonable to expect they would fall under one or other of the following agencies: first, self-selection, where each element selects its most suitable neighbour, as in the theory of pangenesis; secondly, general co-ordination, or the influence exerted on each element by many or all of the remaining ones, whether in its immediate neighbourhood or not; finally, a group of diverse agencies, alike only in the fact that they are not uniformly helpful or harmful, that they influence with no constant purpose—in philosophical language, that they are not teleological; in popular language, that they are accidents or chances. Their inclusion renders it impossible to predict the peculiarities of individual children, though it does not prevent the prediction of average results. We now see something of the general character of the conditions amid which the stable equilibrium that characterises each race must subsist.

Political analogies of stability and change of type abound, and are useful to fix the ideas, as I pointed out some years ago. Let us take that which is afforded by the government of a colony which has become independent. The individual colonists rank as particulate representatives of families or other groups in the parent country. The organised colonial government ranks as the personality of the colony, being its mouthpiece and executive. The government is evolved amid political strife, one element prevailing here and another there. The prominent victors band themselves into the nucleus of a party, additions to their number and revisions of it ensue, until a body of men are associated capable of conducting a completely organised administration. The kinship between the form of government of the colony and that of the parent state is far from direct, and resembles in a general way that which I conceive to subsist between the child and his mid-parentage. We should expect to find many points of resemblance between the two, and many instances of great dissimilarity, for our political analogy teaches us only too well on what slight accidents the character of the government may depend when parties are nearly balanced.

The appearance of a new and useful family peculiarity is a boon to breeders, who by selection in mating gradually reduce the preponderance of those ancestral elements that endanger reversion. The appearance of a new type is due to causes that lie beyond our reach, so we ought to welcome every useful one as a happy chance, and do our best to domicile and perpetuate it. When heredity shall have become much better and more generally understood than now, I can believe that we shall look upon a neglect to conserve any valuable form of family type as a wrongful waste of opportunity. The appearance of each new natural peculiarity is a faltering step in the upward journey of evolution, over which, in outward appearance, the whole living world is blindly blundering and stumbling, but whose general direction man has the intelligence dimly to discern, and whose progress he has power to facilitate.

THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE[1]

THE meeting of 1885 of the American Association for the Advancement of Science was held at the Ann Arbor University. The total attendance (according to *Science*) of members was not a large one, the number reaching only to 365; the number of papers was 176. Two changes in the organisation were made; by one, the section of histology and microscopy was abolished, as it has been urged for some time that a special science of microscopy does not exist, the microscope being rather a tool to be used by scientific men in various branches. The other change was in the name of the section of mechanics, the words "and engineering" being added to the title, that it may be more clearly understood by Americans that those interested in all branches of engineering are invited to take part in the proceedings. As this was the first meeting since the action of the Government in regard to the Coast Survey, the question was generally discussed. The matter was referred to a committee, which offered to a general session of the Association the following resolutions, which were unanimously accepted:—

WHEREAS, The attention of this Association has been called to articles in the public press, purporting to give—and presumably by authority—an official report of a Commission appointed by the Treasury department to investigate the condition of the U.S. Coast Survey Office, in which report the value of a certain scientific work is designated as "meagre."

AND WHEREAS, This Association desires to express a hope that the decision, as to the utility of such scientific work, may be referred to scientific men.

Resolved, That the American Association for the Advancement of Science is in earnest sympathy with the Government in its every intent to secure the greatest possible efficiency of the public service.

Resolved, That the value of the scientific work performed in the various departments of the Government can be best judged by scientific men.

Resolved, That this Association desires to express its earnest approval of the extent and high character of the work performed by the U.S. Coast Survey—especially as illustrated by the

[1] For early copies of the addresses and papers we are indebted to the editor of *Science*.