THURSDAY, SEPTEMBER 7, 1899.

THE LITTLE NEGROES OF THE EAST.

The Negritos: the Distribution of the Negritos in the Philippine Islands and Elsewhere. By A. B. Meyer. Pp. 92. (Dresden: Stengel and Co.)

DR. A. B. MEYER, the distinguished Director of the Royal Zoological, Anthropological and Ethnographical Museum at Dresden, has issued as a separate volume a translation of two chapters, brought up to date, of his sumptuous folio monograph "Die Philippinen: II. Negritos" (1893). The Negritos of the Philippines are a dwarf,[1] frizzly-haired people with a black or dull copper-coloured skin. The head is on the lower limit of brachycephaly (average index about 80). The forehead is retreating, the concave nose is broad and flat, the projecting jaw is provided with thick lips and prominent teeth. The slender body is almost entirely smooth. They are a happy, lively people to whom care seems a stranger, their greatest anxiety being the procuring of food, which consists of all things edible—fruits, roots, honey, snakes, &c. When they have provided for their wants they care for no further exertion, and love to lie in laziness and ease. Their intelligence is stated to be of a low type, and they are not able to count above five. Their songs consist of monotonous, endless unison chants. Tattooing is apparently universal, the patterns being quite simple. They are without exception monogamists.

There is no doubt that these interesting little people, about whom much more information is greatly needed, are closely allied to the pygmy blacks of the Malay Peninsula and to those of the Andaman Islands. They represent an ancient race of mankind, and thus it is important to trace their present and past geographical distribution. The name of "Aëta," "Aita," "Ita," &c., generally applied to these people, is derived from the Tagal adjective *ita*, *itim* "black" (Malay, *itam*); they were known to the Chinese under the name of "Haitan" at the beginning of the thirteenth century.

The headquarters of the Negritos are the island of Luzon and the small islands in its immediate vicinity; here many have crossed with the Tagals, and constitute a half-bred population called Dumagates. There can be little doubt that they are the true aborigines of the Philippines. It may be taken as certain that Negritos are found, not only in Luzon, but also in Panay, Negros, Cebu, North-east Mindanao, and Palawan, not to mention smaller neighbouring islands. It is questionable whether they occur in Guimaras, Bohol, Samar, Mindoro, and the Calamianes.

There has been much speculation on slender data concerning the distribution of the Eastern pygmy negros, of which, as we have seen, the Semangs and allied tribes of the Malay Peninsula, the Andamanese and the Aëtas form distinct groups. De Quatrefages, for example, held that traces of Negritos are found nearly everywhere from India to Japan and New Guinea, and that Negritos and Papuans live together in New Guinea, crossed and intermixed ("Négrito-Papous"), differing from the true Papuans. Dr. Meyer submits these assertions to a careful criticism, which is a valuable corrective to specious generalisation.

Theoretically, one would expect to find Negritos in Borneo; the only evidence is the account of a similar people given by Captain Brownrigg to Mr. Earl in 1845 of his shipwreck during the previous year on the east coast of Borneo, and a decorated skull described by de Quatrefages and Hamy in "Crania Ethnica." The district visited by the Captain has not been properly explored, and till that is done the question must remain in abeyance. No other white man has seen a Negrito in Borneo, and it is certain that none have been heard of in Sarawak; Mr. Charles Hose, who probably knows more about the natives of the interior than any one else, disbelieves in their existence. I have myself seen low-caste natives in the interior of the Baram district of Sarawak, whose hair was wavy and almost curly; the contrast between these and their nearly straight-haired companions could easily lead to exaggeration, but this does not necessarily indicate Negrito blood. Dr. Meyer discusses the *provenance* of the decorated skulls from Borneo in European museums; at present our information is too meagre for accurate generalisation. There appears to be no evidence that the skull in question came from the "interior of Borneo," and it is by no means incredible that the skull, or the person when alive, was imported into Borneo; slaves have probably been imported at different times, and we know that various peoples have migrated into Borneo from all quarters.

There is no evidence of Negritos in Celebes, Timor, the Moluccas and Lesser Sunda Islands. The same applies to Java (the Kalangs are not Negritos); but in Sumatra and the neighbouring islands there is still some doubt whether such a population once existed. There is less evidence for an early Negrito stock in Formosa, Japan and China. The evidence for the Mergui Archipelago is doubtful, and that for the Nicobar Islands is more so. More evidence is required for Annam, Cochin China, and Cambodia.

A good deal has been written about the occurrence of a short, dark, frizzly-haired people in India, but of these there is no evidence whatever. Curly hair is characteristic of the "Dravidian" peoples, but this is never woolly. Prof. Keane figures[1] a "Panyan woman" as a "Negrito type, India"; but a reference to the original photograph published by Thurston[2] will prove that the hair is distinctly curly, which feature is unfortunately lost in Keane's reproduction. Thurston[3] gives the average height of twenty-five Paniyan men as 1·574 m. (5 feet 2 inches), with a cephalic index of 74; these are not Negritos.

The affinities of the Australians, more or less, with the "Dravidians" is now generally accepted, but a Negrito element has not yet been proved for them. Some hold that the Tasmanians belonged to that stock, and in his

[1] Average height for males 1442 mm., 4 ft. 8¾ in.; for females 1385 mm., 4 ft. 6½ in.

[1] "Man Past and Present," 1899, Pl. II. Fig. 3.
[2] *Bulletin* Madras Govt. Mus., 1897, ii. Pl. X.
[3] *Loc. cit.*, p. 29.

recent presidential address to Section F of the Australasian Association for the Advancement of Science, "On the Origin of the Aborigines of Tasmania and Australia," Mr. A. W. Howitt believes that

"the Tasmanians were the autochthonous inhabitants of Australia, and that their preservation in Tasmania was due to isolation by the formation of Bass Strait. The occupation of the continent by the Australians who, it may be reasonably held, were in a higher state of culture, must have resulted in the amalgamation of the two races, or by the extirpation of the former inhabitants, so far at least as regards the males."

He also suggests that a later wave of Papuan migration was virtually stopped by Torres Straits. He also puts forward

"the following tentative hypothesis: An original Negrito population, as represented by the wild tribes of Malaysia; a subsequent offshoot represented by the Andamanese and Tasmanians, and another offshoot in a higher state of culture originating the Melanesians."

Whatever Mr. Howitt writes is worthy of the careful attention of anthropologists, and it would be well to direct future research with this hypothesis well in view. As Garson, Ling Roth and others have expressed the opinion that the Tasmanians were of Negrito origin (using that term in a general sense), it is rather a pity that Dr. Meyer has not discussed this point.

Finally Meyer discusses the relationship of the Negritos to the natives of New Guinea; he, with Micluko-Maclay, asserts the unity of origin of the Negritos and Papuans, and at the same time insists that the Papuans are diversified and show various types.

"Does it point to a crossing of different elements, or does it simply reveal the variability of the race? I [Meyer] incline to the latter assumption as the simplest and as provisionally sufficient, particularly as in the still so limited state of our knowledge it will be labour lost to try to resolve a race like the Papuan into its various elements."

This is not the place to enter into a discussion on this difficult problem; for the present I can only say that I am inclined to adopt the former view. I certainly have not seen or heard of any trace of Negritos as such, the brachycephals I encountered in New Guinea were no shorter than the dolichocephals, nor had they more Negritic affinities than the latter. Meyer makes the following emphatic statement:

"A Negritic race side by side with the Papuan race nobody has been able to discover, just because it does not exist, and it does not exist because the Papuan race, in spite of its variability, is on the one hand a uniform race, and on the other as good as identical with the Negritos."

A careful perusal of Dr. Meyer's critical study leaves one fact strongly imprinted on the mind, and that is the urgent need for further evidence. There can be no doubt that observation in the field is by far the most important branch of anthropological work at the present time, and all our energies should be employed in this direction. The time is fast approaching when it will be too late.

A. C. HADDON.

BACTERIA.

Bacteria; especially as they are related to the Economy of Nature, to Industrial Processes, and to the Public Health. By George Newman, M.D., F.R.S. (Edin.), D.P.H. (Camb.), &c. Pp. xvi + 351. (London: John Murray, 1899.)

THE author in his preface says that the book is "an attempt, in response to the editor (F. E. Beddard, F.R.S.) of the series (the Progressive Science Series), to set forth a popular statement of our present knowledge of bacteria." "Popular science," continues the author, "is a somewhat dangerous quantity with which to deal. On the one hand it may become too popular, on the other too technical. It is difficult to escape the Scylla and Charybdis in such a voyage."

It may be said at the outset that Dr. Newman has accomplished a very difficult task in a manner which does him credit. Nevertheless, it is to be hoped that in future editions the writer will judiciously curtail certain sections and expand others, and will exercise more caution in laying down doctrines which, in some cases, might mislead the lay reader, and which occasionally even show a wrong conception of the present state of our bacteriological knowledge. That a further edition will be called for at no distant date need hardly be doubted, considering the general excellence of the work.

The first thirty-eight pages deal with the biology of bacteria. This portion of the book might well be curtailed; it contains little information that is new, and much that is old and contained in every text-book of bacteriology.

The second chapter deals with the bacteria in water, and includes much valuable information. It contains a useful reference to *B. enteritidis sporogenes* (Klein), a virulent anærobe apparently causally related to diarrhœa. The biological treatment of sewage might usefully have been discussed more fully and in a separate chapter. The statement, "The cultivation beds also have an inimical effect upon infective bacteria. Hence the final effluent is practically germ-free as regards pathogenic organisms," must be accepted with caution.

The chapter on bacteria in the air is well and concisely written, but the author quotes an experiment of his own which is a little difficult of comprehension. To quote his own words:

"The writer recently obtained some virulent typhoid excrement, and placed it in a shallow glass vessel under a bell-jar, with similar vessels of sterilised milk and of water, all at blood heat. So long as the excrement remained moist, even though it soon lost its more or less fluid consistence, the milk and water remained uninfected. But when the excrement was completely dried it required but a few hours to reveal typhoid bacilli in the more absorptive fluid, milk, and at a later stage the water also showed clear signs of pollution."

Shattock's interesting experiments are quoted, showing that sewer air does not necessarily exalt the virulence of a strain of lowly virulent diphtheria bacilli. It is to be noted that this does not affect the question of the possibility of sewer air depressing the vitality of the individual, and so allowing lowly virulent bacilli, either already present in the throat or subsequently gaining entrance, to develop and display their full power of pathogenicity.

The chapter on fermentation is a good one and is

more in touch with the original scope of the work as outlined by Dr. Newman himself in the preface. Moreover, there is much that is suggestive to the mind of the bacteriologist seeking for new avenues of research in a most important and imperfectly explored field.

The subject of bacteria in the soil is well dealt with in Chapter v., and the author records some of his own interesting experiments on nitrogen-fixing bacteria.

Chapters vi., vii. and viii. treat respectively of bacteria in milk, milk products and other foods; the question of immunity and antitoxins; and bacteria and disease. There is much in these chapters which will repay careful perusal. Dr. Newman very properly draws attention to Dr. D. S. Davies' persevering and instructive investigation of the late epidemic of typhoid fever at Bristol. It is a little difficult to measure the author's meaning when he says:

"Though the typhoid bacillus appears not to have the power of multiplying in milk, it has the faculty of existing and thriving in milk."

Dr. Newman states that the cause of scarlet fever is unknown. Perhaps it would be fairer to say that some bacteriologists consider that the proof that Klein's streptococcus is the causal agent rests on insufficient grounds.

The last chapter is devoted to disinfection, and the subject is well treated.

The book is rendered attractive with twenty-four good micro-photographs. There are seventy other illustrations; many of these are, as the author admits, diagrammatic. In a future edition some, at all events, of these might be usefully replaced by micro-photographs.

In conclusion, it may be said that Dr. Newman has successfully accomplished a very difficult task. It is true that the author has not altogether fulfilled his original intention of eliminating technical matters, and that exception may be taken to certain statements as being too dogmatic to please the cautious reader and thinker. Yet, judging the book as a whole, it may be said that it is certain to enhance the writer's reputation, and will surely be welcomed by the numerous readers of the publications of the Progressive Science Series. It is to be hoped that a demand for this volume may speedily call for a second edition. A. C. HOUSTON.

OUR BOOK SHELF.

Leitfaden der Kartenentwurfslehre. Von Prof. Dr. Karl Zöppritz. Second edition. By Dr. Alois Bludau. Erster Theil. *Die Projektionslehre.* Pp. x + 178. (Leipzig: Teubner, 1899.)

DR. BLUDAU, who has devoted much attention to map-projections, and has written some noteworthy papers on the subject, has lately published the first part of his new edition of the well-known work on cartography by Karl Zöppritz. The book has been thoroughly revised and recast; and the additional matter is so large as to render publication in two parts, issued separately, desirable. The first part deals only with the various projections of portions of the sphere that have from time to time been proposed. Dr. Bludau's object has been to produce a work which should meet the requirements of the present day, and be of real service to cartographers. With this view those projections which are of practical use are fully described, whilst those that may be termed "fancy" projections are only briefly discussed. Every effort has been made to ensure clearness and distinctness, and only those mathematical propositions and formulæ that are absolutely requisite are given. Dr. Bludau has successfully carried out his programme. The book is well written, and will be of great value and assistance to those who are practically engaged in the production of maps. Every important projection is mentioned with its date and the name of its author; and full use has been made of the researches of Tissot, and the published works of Profs. Fiorini of Bologna, Hammer and others. Dr. Bludau gives a list of the authorities whose writings he has consulted, and it may be noted that it does not include the name of any Englishman. The subject has been much neglected in this country, and nothing of any importance has been published since the papers of Airy and Clarke, and the well-known little book on the construction of maps by Hughes, the last edition of which appeared in 1864. Dr. Bludau gives almost without alteration the useful hints on drawing which appeared in the original "Leitfaden" of Zöppritz; and there are some tables for the construction of projections. Part ii. is to deal with topography and cartometry, and to contain a number of additional tables. C. W. WILSON.

The Dog, its External and Internal Organisation. Edited by A. C. Piesse, M.R.C.V.S.; with Anatomical Description by W. S. Furneaux. With five plates and text cuts. Pp. 31. (London and Liverpool: G. Philip and Son.)

THIS is an oblong work of 28 pp. of the puzzle-book order, with five plates, the parts of which are cut out and so arranged in super-position that the reader first skins his dog and then works through its skeletal, circulatory, and muscular apparatus and viscera, until a median longitudinal section is reached. The latter is conspicuous for the delineation *in situ* of the central nervous system, but the entire peripheral system has been mysteriously overlooked.

The first 14 pp. of the text are devoted to a consideration of the history of the dog and of the leading breeds, illustrated by six woodcuts, the remaining 14 pp. to a so-called "Anatomical Description"—in reality an attempt at a general *résumé* of the anatomy and physiology of the vertebrate organism with especial reference to the dog, the whole concluding with a detailed explanation of the plates, the organs and structures represented being indicated by numbers. The work is of too thin and amateurish a character to merit detailed comment in these pages, but while fairly trustworthy so far as it goes, it is wanting in balance and accuracy of detail; and in attempting to express scientific facts in non-scientific terms the authors at times lapse into a looseness of expression apt to mislead.

To define the "Dogs (*Canina*)" as belonging "to the family of Mammalia," and to indulge in feebly stated generalities about the structure of ganglia and the orders of nerve-fibres, to the exclusion of an adequate description of the course and nature of the leading nerve tracts, is but to confuse the mind. We do not know for what class of persons the book is intended. It will be useless to the serious student, and of little avail to the lay reader, as conveying an accurate idea of the most elementary facts. The small modicum of anatomy which it contains, interspersed with passing allusions to habit and to appearances indicative of disease, will doubtless be attractive to some persons, but by those who desire full information, such as can alone be of real service educationally or otherwise, access must be had to well-known authoritative works such as Ellenberger and Baume's "Anatomie des Hundes." The volume before us may perhaps do something to encourage a love of the dog and an appreciation of the beautiful in its construction, leading thus up to the study of the more directly useful; and for this reason we regret the more that a bibliographic list of the afore-mentioned authoritative treatises should not have been given. Without one the present work fails in its most useful purpose.

LETTERS TO THE EDITOR.

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

A Hertz Wave Receiver.

DURING a visit to Adelaide in December last year, I was asked to suggest some sort of apparatus whereby Hertz wave disturbances might be observed. The result may perhaps be of some interest at the present time. The spark at the oscillator was small, as only a small induction coil was available. From previous experiments made by me, I had discovered that in order to obtain great sensitiveness the distance between the poles of a Branly detector should be as small as possible; also the amount of current employed should be very small. After many experiments, I devised an apparatus in which only about 1/16 of a milliampere gave excellent results. The apparatus was simple and easily constructed. In Fig. 1, M N are the poles of the permanent magnet of a D'Arsonval galvanometer; K, its coil having a resistance of 500 ohms; H, the internal soft iron

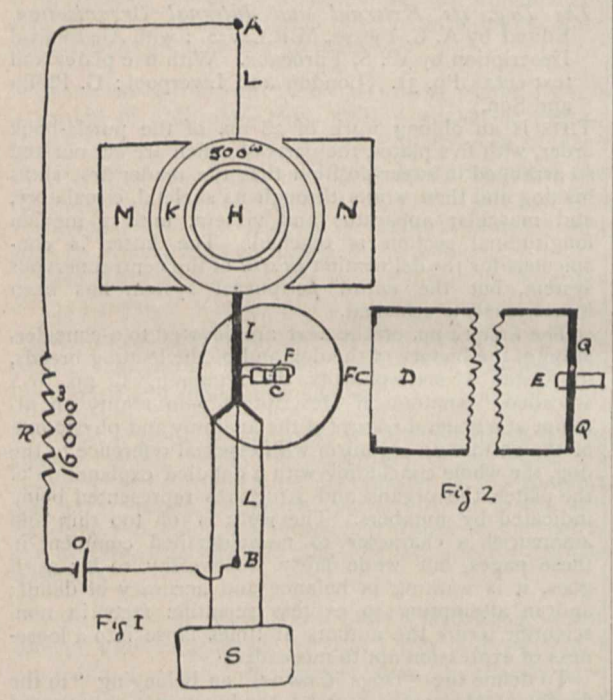

Fig 1

Fig 2.

Hertz wave receiver

fixed core; L L, the flat suspension wires; I, a rod of ebonite to the side of which a minute Branly receiver is attached. This forms a part of the galvanometer circuit, and moves with the coil through its angle of displacement, the ends A B of the suspension wires being fixed to the frame (not shown). The circuit included one small dry cell, O, and a non-inductive resistance of 16,000 ohms. In order that the Branly receiver C, after being made a conductor by the influence of a Hertz wave, may be restored to its condition of high resistance, it is brought up against a point F, when deflected (a side view of this is shown in Fig. 2). This point is kept in a state of vibration by means of a jet of water, thus, D E is a tube furnished with an elastic disc, to which a projection, F, is attached; a jet is so fixed at E that its discharge impinges on the centre of the disc, the jet is fixed to the tube by a bar, Q Q, the discharge is affected by the tube as a resonator, and hence F vibrates. The suspended coil, K, is furnished with a mirror and a pointer, whereby its movements are easily seen. This form of decohering instrument was used to avoid the evil effects due to electromagnetic vibrators, which act on a receiver if very sensitive. The induction wings were connected to the apparatus at A and B. On repeating the experiment at home, I found that 1/16 milliampere is by no means the smallest current that might be used; all that is required is a current sufficient to move the suspended coil. I am informed that the current used in wireless telegraphy is usually about one milliampere; it is obvious that by increasing the sensitiveness of the galvanometer a more sensitive coherer may be used. By means of a vane, S, moving in liquid the movements of the coil are damped. The cohering substance was an 8 per cent. alloy, made in the oxyhydrogen flame, and then reduced to filings in the usual way; it was found to be exceedingly sensitive, much more so than the mechanical mixture of filings of the two metals.

I made several attempts to use a sensitive galvanometer for closing the circuit; the results were unsatisfactory, the closing of the circuit was uncertain, and when it was closed the tendency to stick, due to the contacts, was a source of much trouble. It then occurred to me that the whole difficulty would be obviated by attaching the coherer to the moving axis of the galvanometer coil itself, for by this means the contact is entirely avoided, while the coherer is brought within the range of a constantly vibrating projection which causes immediate decoherence. A vibrating reed was tried as a decoherer, but abandoned owing to the trouble of feeding it with air under pressure. Another form of decohering vibrator was also tried, which consisted of a long tube, part of which was glass; each end was furnished with a tympan, one of which was placed as F and D, Fig. 2; the other tympan was led into a metal box containing an electromagnetic vibrator, the hammer of which beat upon the tympan remote from the galvanometer. The electromagnet may thus be placed at a great distance from the apparatus, while its impulses are communicated through the column of air in the tube.

Since my return home, I have used the Wehnelt circuit breaker for producing Hertz waves with ordinary oscillators; the effects appear to be perfect, although the space at my disposal, about two miles, is for the most part covered with houses and high buildings. F. J. JERVIS-SMITH.

Oxford, August 26.

Is Insusceptibility to Vaccine produced by Small-pox?

IF vaccination inhibits, arrests, modifies, or mitigates variola because it is one with variola, if the attenuated virus and local eruption interfere with the more virulent and generalised eruption, may not a reciprocal antagonism be expected? If the minor malady interfere with the major malady, how much more should the major malady, within a reasonable period of years, confer some degree or manner of general constitutional protection in respect of the minor, if not a modification of the local result of vaccination? If I mistake not, systematic investigation on this point, on the human subject, has been strangely neglected in England, if not elsewhere.

CHARLES G. STUART-MENTEATH.

23 Upper Bedford Place, W.C., August 14.

SMALL-POX does leave behind it an insusceptibility to vaccinia. If the writer of the letter will refer to Dr. Monckton Copeman's article on "Variola and Vaccinia, their Manifestations and Inter-relations in the Lower Animals: a Comparative Study" (*Journ. of Path. and Bact.*, vol. ii., 1894, p. 408, *et seq.*), he will find references to the protection conferred by variola against vaccinia. That systematic investigation has not been carried out is probably due to the fact that the subject has so little practical interest.

From the point of view of those interested in the general question of immunity, this subject is well worth careful and systematic study. G. SIMS WOODHEAD.

THE DOVER MEETING OF THE BRITISH ASSOCIATION.

IN addition to the arrangements described previously, an installation of Marconi's system of wireless telegraphy will be set up in the Maison Dieu Hall of the Town Hall. There will be constant communication with the South Foreland, the East Goodwin Lightship and Wimereux-Boulogne. By means of this arrangement visitors will be kept fully informed of the proceedings of the French Association at Boulogne.

In Section A (Mathematics and Physics) the President's address is intended to be taken at 10 a.m. on Thursday, September 14. On Friday, September 15, papers connected with mathematical physics and electricity will be read. On Saturday, September 16, on the occasion of the French visit, it is hoped that Prof. J. J. Thompson and Prof Oliver Lodge will communicate papers. On Monday, September 18, the section will subdivide into a meteorological and a mathematical section. On Tuesday, Prof. Threlfall will exhibit and describe his gravity metre, and a discussion on platinum thermometry will be opened by Prof. Callendar and Drs. Harker and Chappuis.

The President's address in Section B (Chemistry) will be given on Thursday, September 14, immediately after the address to Section A. It will deal with the assimilation of carbon by the higher plants, and will be mainly descriptive of new work carried out during the past two years in the Jodrell Laboratory at Kew. In addition to papers of a specially technical character several discussions of interest have been arranged for the meeting. On the occasion of the visit of the French Association on the Saturday, there will be a joint meeting of Sections B and K to discuss the question of symbiotic fermentation, both in its chemical and biological aspects. This discussion will be opened by Prof. Marshall Ward; Dr. Calmette and Prof. Armstrong, amongst others, will take part in it.

Prof. Armstrong has undertaken to open a discussion on a subject of importance and interest in organic chemistry, under the title of "Laws of substitution, especially in benzenoid compounds"; whilst a prominent place in the programme will also be given to inorganic chemistry in a discussion on "Atomic Weights," to which Prof. F. W. Clarke, of Washington, will communicate a statement of his views. Amongst the special papers already promised, Prof. Dewar hopes to be able to communicate the results of his most recent investigations on the solidification of hydrogen, and the liquefaction of helium. Prof. Ladenburg, of Breslau, will read a paper on "The development of chemistry in the last fifteen years," and Mr. H. J. H. Fenton will give an account of his recent researches on "Oxidation in the presence of iron."

In Section D (Biology) the following papers of general interest to biologists will be read, amongst others: Mr. J. J. Lister will describe a remarkable new type of calcareous sponge discovered by Dr. Willey during his expedition to New Britain. Mr. J. J. Budgett, who has just returned from the River Gambia, will give an account of the zoological results of his expedition. Messrs. Gamble and Keeble will communicate an account of their experiments on the colour changes of prawns. Prof. Poulton will describe a new series of experiments on the protective value of form and colour in insects. Mr. W. Garstang will give an account of the methods and results of a periodic survey of the plankton and physical condition of the English Channel. Dr. C. G. Petersen will describe the plaice culture in the Limfjord (Denmark). Valuable communications on special morphological problems will also be submitted. Mr. Graham Kerr will discuss the origin of the paired limbs of vertebrates; and Dr. Willey the process of cephalisation in mollusca and vertebrata. In the marine excursion, if weather permits, the plankton collecting apparatus will be demonstrated; and Dr. Petersen and Mr. Garstang will exhibit their new forms of net for opening and closing under water.

In Section E (Geography) the following papers are promised: Presidential address by Sir John Murray on the floor of the ocean; Mr. J. J. Buchanan, F.R.S., on the physical and chemical work of an Antarctic expedition; Dr. H. O. Forbes, on a visit to Sokotra; Mr. A. W. Andrews, on the use of lantern slides in geographical education; Mr. O. H. Howarth, explorations in Oaxaca, Mexico; Dr. G. Schott (Homburg) on the oceanographical work of the *Valdivia* Expedition; Mr. H. N. Dickson, on the oceanography and meteorology of the North Atlantic; Mr. H. N. Dickson will also read a paper on the temperature of the sea water round the British Islands; Sir John Farquharson, on twelve years' work of the Ordnance Survey; Mr. Vaughan Cornish, on the sand dunes of Lower Egypt; Mr. George Murray, F.R.S., on the distribution of plants in the oceans; Mr. Robert Irvine, on the distribution of nitrogen in the sea; Mr. C. W. Andrews, on oceanic islands; Sir John Murray and Mr. F. Pullar, on the bathymetrical survey of the Scottish Lakes; Mr. W. R. Rickmens, on a journey in Transcaucasia; Dr. H. R. Mill, on the terminology of the forms of ocean floor; and Mr. E. Heawood on the discovery of Australia.

In Section H (Anthropology) the President will deliver his address on Thursday morning at eleven, and the remainder of the day will probably be devoted to the discussion of reports and papers on physical anthropology. The subject of finger-prints as means of identification will be examined in important papers by Dr. Francis Galton, and Mr. E. R. Henry, who has used the method with success in police work in India. Other anthropometric points will be discussed by Dr. J. G. Garson. Mr. J. Gray contributes a paper on the population of East Aberdeenshire; and Mr. D. McIver on his recent work on the early inhabitants of Egypt.

Friday will be devoted to an important series of papers and exhibits arising out of Prof. Haddon's recent expedition to Torres Straits. Communications are promised from Prof. Haddon himself, and from Dr. Seligmann and Messrs. Ray and Rivers, who took part in the expedition. Some, if not all, of the archæological papers will be taken on Saturday. Monday and Tuesday will be occupied with papers on Ethnography and kindred subjects. Wednesday will, as usual, be reserved for overflows and late arrivals.

Among the reports, that on the education of defective children deserves particular notice, and those on excavations at the lake village of Glastonbury, the Roman site at Silchester, and elsewhere in this country, will afford interesting material for comparison with those of our French visitors.

The President's address in Section K (Botany) will be delivered at 10.30 on Thursday, September 14. On Friday afternoon a lecture—of a semi-popular nature—will be delivered by Mr. Harold Wager, on sexuality of the fungi. Saturday morning will be given up to a joint discussion with Section B on fermentation, which will be opened by Prof. Marshall Ward. On Saturday afternoon the members of Section K propose to have a botanical excursion to the sand dunes between Deal and Sandwich. The contributions to be made to the Section include papers on fungi by Prof. M. Ward, Prof. Potter, Mr. Wager, Dr. Darbishire and others; on physiological botany, by Mr. Francis Darwin; on latex of india-rubber, by Mr. Biffen and Mr. Barkin. Prof. Campbell contributes a paper on studies in Araceæ; and Mr. Willis deals with the morphology and life-history of the Indo-Ceylonese Podostemaceæ. Miss Sargant promises a demonstration of vermiform nuclei in the fertilised embryo-sac of *Lilium Martagon*. Prof. Bower will read a paper dealing with the sporangia of ferns. Prof. Bertrand, of Lille, communicates a paper on *Sigillaria*. Prof. Weiss sends a contribution dealing with *Lepidophloios*, and Mr. Seward and Miss Gowan deal with the botany and geology of the maiden-hair tree. There are also other papers expected on fossil botany. Mr. Lloyd Williams will give an account of further work on the Brown Algæ. There are also to be contributions to the Section on local botany and on other subjects of general botanical interest.

THE FORECAST OF THE MONSOON.[1]

THE brief telegrams that have lately been published from India concerning the amount of rainfall have given a very uncertain note. Favourable and unfavourable accounts have followed in rapid succession, and at the moment of writing it seems doubtful whether to expect a normal amount of precipitation, or to dread a recurrence of one of those calamitous famines, which drain so severely the resources of India, and from the last of which she has barely recovered. In these circumstances, it is of more than usual interest to turn to the official forecast, to see how the causes, which in the opinion of the best-informed meteorologists affect the climate of India, are operating for and against the prospects of a successful harvest.

At the outset we meet with a grave disappointment. The Simla authorities distinctly express their inability to make a forecast, on any scientific ground, of two very important factors which affect the agricultural value of the monsoon rainfall. These are the possibility of the occurrence of a protracted break in the rainfall during the months of July and August, even after the season has opened favourably, and of an unusually early termination of the rains in the North and Central Provinces of India and in Bengal. For fifteen years the Meteorological Office has deplored the want of the necessary data that would warrant a prediction on these important topics, and there are no signs that the information will be forthcoming at an early date. As a matter of fact, the authorities go little further than an examination of the conditions under which the south-west monsoon currents will arrive on the coasts of the peninsula. It is true that the probable amount of rainfall in the various provinces of India is considered at some length, but it is expressly declared that this "forecast is a statement of probabilities, and not of certainties, and that it is liable to error from the limitation and uncertainty of part of the data on which it is based."

Similar words accompany all the forecasts that prudent men venture to make, and it must be admitted that the continual repetition is wearisome and distressing. Such a caution may be necessary, but if it produces on the mind of an impatient public the impression that little or no advance is being made in meteorology, and particularly Indian meteorology, a great injustice is done to a body of highly-skilled observers, who have not spared themselves to benefit science, to improve the lot of the agriculturist, and to strengthen the hands of the Government in dealing with a misfortune they are eager to alleviate, but powerless to avert.

But it is not difficult to see some of the reasons that compel the staff to halt at the result of this preliminary investigation. Forecasting, as understood in England, and which practically rests on the capacity of the telegraph to outrun the storm or the weather it announces, would be valueless in India. Away from the coasts and outside shipping interests, there is no necessity for daily forecasting, nor for the study of those ephemeral fluctuations which go to make up our weather. On the other hand, the meteorological conditions that result from the movement of enormous masses of the air attract greater scientific attention, owing to their periodic character and the effect likely to be produced on agriculture and the well-being of large masses of the population. It would be wrong, however, to forget that in late years, and mainly under the energetic direction of Mr. Eliot, barometric variations, however small in amount, have been studied with good effect, and have revealed the probable existence of cyclical variations which can have considerable influence in promoting or checking the general oscillatory motion of the air across the equator, to which motion the south-west and north-east monsoon winds are mainly due.

But in the forecast before us, though the variations of pressure from the normal, and the effect such fluctuations have on the local weather existing in India immediately preceding the advance of the monsoon, are treated as a factor in the problem, two other conditions have naturally great weight. These are the amount and time of occurrence of the snowfall in the mountain districts adjacent to Northern India, and the behaviour of the south-east trades in the preceding season, as investigated at Seychelles, Mauritius, the Cape of Good Hope, and the logs of ships passing over the area affected. Such latter information is of necessity incomplete, but is likely to be of great importance in proportion as it covers a larger area, for the greater the district brought under review, the greater the probability of tracing the true physical cause on which important variations rest. It may not be out of place, as showing the wide extent over which meteorological phenomena extend themselves, and the consequent necessity for the examination of all remote causes to which they may be traced, to recall the apparent connection existing between the barometric oscillations in the Indo-Malayan region on the one hand, and Russia and Siberia on the other. Further, we have some evidence of connection between the south-east rains of South Africa and the amount of the rainfall at the time of the summer monsoon, while the overflow of the Nile seems to participate in similar periodic variations. Such general disturbance tends to point to a common cause, and it is gratifying to know that the possibility of the connection has been pointed out by the Indian meteorological officers, who are fully alive to the importance of discovering the origin of these effects, which demonstrate themselves periodically. In basing the forecast on more or less local appearances, we seem to recognise the weak point in long-period forecasting. We are in the position of a physician who deals with the symptoms rather than the origin of a disease.

This difficulty of trusting to appearances may be illustrated in many ways. For instance, how are we going to estimate the relative importance of the two operating factors we have mentioned above, the snowfall on the Himalayas and the behaviour of the south-east trades? And how are we going to act if we find the indications from the two sources discordant? Some time since we believe that the snowfall was regarded as the one important item in the making up of the forecast. Scanty rain was anticipated as the consequence of heavy snow, but greater experience has somewhat discredited the notion. Late snow in April or May, or the cause which produces the late snowfall, no doubt does exercise very considerable influence locally on the distribution of the monsoon winds; but when we have to deal (as already pointed out) with the effects produced by the circulation of an atmosphere covering an entire hemisphere, such local results play but an insignificant part. Nevertheless, we find Mr. Eliot, who doubtless is glad to avail himself of every source of information, carefully tabulating the time and amount of the snowfall from Afghanistan on the West to Assam in the East. But, in drawing his conclusion, he does not leave out of sight the local character of the indication, and a distinction is drawn between the conditions that should follow the reports from Western India and those received from the Eastern Himalayas. In the former case, the signs point to an early and strong monsoon with beneficial results to the utmost limits of the Punjab. The conclusions to be drawn from the accounts from the eastern portion are more uncertain both on account of deficiency in the data received and greater doubt in the interpretation of the sign, but it is expected that the rainfall in North-east India will be diminished,

[1] "Memorandum on the Snowfall in the Mountain Districts bordering Northern India, and the Abnormal Features of the Weather in India; with a Forecast of the S.W. Monsoon Rains of 1899." By John Eliot, Meteorological Reporter to the Government of India, and Director-General of Observatories in India. (Simla: June 1899.)

while heavier rain will be prevalent in the north-west. It will be interesting to compare this prediction with actual results; but at present we are more concerned to point out the care that .s taken in preparing the forecast, the difficulty in the collection of exact data, and the manful determination to make the best use of all available sources.

This scrupulous care is well illustrated in the second class of information incorporated into the weather prediction, and which rests on the abnormal features of the recent meteorology of India. To discuss these with any prospect of success, it is first necessary to determine correct normals. The work that this involves can only be appreciated by those who have been actually concerned in a similar inquiry, but it is a method of investigation into which Mr. Eliot and his predecessor, Mr. Blandford, have thrown themselves with signal success. The volumes of the Indian Meteorological Memoirs bear witness to the ability and zeal with which the work has been carried on throughout some twenty-five selected observatories. We may well express the hope that so much work is now yielding abundant fruit.

THE PRESENT POSITION OF THE INVESTIGATION OF THE MALARIAL PARASITE.

THE *rôle* played by the mosquito as a carrying agent of the malarial parasite from man to man seems to be restricted to one genus, the Anopheles. Major Ross, of the Liverpool School of Tropical Diseases, in a telegram from Sierra Leone, announces the fact that he has found the Anopheles there, and that it may be the intermediary host of the quartan malarial fever.

Many observers in different countries, noticing the fact that malaria is most prevalent at the most active period of mosquito life, have attributed malaria to the agency of this insect. Dr. Patrick Manson, in 1894, first brought the subject forward in England, and, acting on his suggestion and advice, Major Ross undertook an investigation in India.

In 1897, by using two species of Anopheles, Ross traced the malarial parasite into the wall of the stomach of the mosquito after it had fed on patients whose blood contained the crescentic gametocytes; the next year he succeeded in tracing the complete life-history of the proteosoma *Grassii Labbé* of sparrows, and showed that its intermediary host was one particular kind of mosquito, the *Culex pipiens*. The gametocytes contained in the red blood corpuscles of the vertebrate host pass with the blood into the stomach of the mosquito, and passing through the stomach-wall bulge into the body-cavity; here a sexual process takes place, zygotoblasts are eventually formed, which pass into the insects' blood, and finally find their way into the salivary gland and to the duct leading from this to the extremity of the stylet; from here they escape into the blood of the vertebrate host when the insect bites. A full account of the process is given by Ross in NATURE of August 3.

Following on these results, Grassi in Italy attacked the problem from another point of view; he studied the mosquitoes prevalent in the different parts of the country where malaria occurs. The results were interesting. He found there was no indigenous malaria where the *Culex pipiens* was common, but it did occur where the large mosquito Anopheles was found.

Bignami and Bastianelli, who had been trying unsuccessfully to infect a man by allowing mosquitoes to bite him, attributing their want of success to the use of the wrong kind of mosquito, and, acting on the observations of Grassi, tried again with some mosquitoes imported from a malarious district. This time they succeeded in infecting the man with malaria of the same type that prevailed in the district from which the mosquitoes came. Moreover, they have shown that the development of the human form of parasite in the body of Anopheles is identical with the development of the proteosoma of birds in *Culex pipiens*, as observed by Ross.

According to these observers, the species *Anopheles claviger* is the most common intermediary host of the parasite of malaria in Italy, the tertian and summer-autumn types.

It is evident that the next step in the study of malaria should be to hunt for the different species of Anopheles and see if these are the intermediary hosts of the different types of malaria throughout the world, and what particular species is most concerned in transferring the parasite from man to man. Grassi has done this for Italy, and now we hear that Ross has found a species of Anopheles to be concerned in the transference of quartan fever; thus all the types of malarial fever are now referred to the Anopheles as their intermediary host. His full report on return from Africa will be read with interest.

Whether the Anopheles can be extirpated from a locality, and by what means, will be the problem for scientific workers resident abroad to settle; fortunately they seem to be confined to small areas, so the suggestion of Ross to draw off the water from stagnant pools may not be so hopeless a task as it would at first appear.

NOTES.

THE following men of science have been elected fellows of the Reale Accademia dei Lincei. As ordinary fellows: for mathematics, P. Tardy, G. Veronese; for mechanical science, G. Favero, G. Colombo, V. Volterra; for agricultural science, A. Targioni-Tozzetti. As corresponding fellows: for mathematics, G. Ricci; for mechanics, G. A. Maggi; for physics, G. Grassi, A. Battelli; for crystallography and mineralogy, A. D'Achiardi; for botany, F. Delpino; for agriculture, A. Borzì; for pathology, E. Marchiafava. As foreign fellows: for mathematics, G. Mittag-Leffler, J. Weingarten; for physics, E. Mascart, W. Kohlrausch; for chemistry, Ludwig Mond, E. Fischer; for crystallography and mineralogy, C. Klein, F. Fouqué, F. Zirkel; for geology and palæontology, O. Torell, A. De Lapparent, R. Lepsius; for botany, W. Pfeffer; for zoology and morphology, E. Haeckel, E. van Beneden; for physiology, E. Pflüger, E. Hering.

THE Berlin correspondent of the *Times* reports that the Imperial Government has ordered Prof. Kossel, of the Board of Health, to proceed to Lisbon and Oporto to study the plague and the methods adopted to combat it. Prof. Kossel will be accompanied by Prof. Frosch, of the Berlin Institute, for the Study of Infectious Diseases, who is being despatched on the same mission by the Prussian Government. Drs. Calmette and Salinbeni are already investigating the outbreak, and will report upon it to the Paris Pasteur Institute.

PRINCE KROPOTKIN sends us a note which suggests that the movements of sea-gulls along the British coasts may indicate forthcoming weather changes. On Saturday, August 26, while off Broadstairs, he noticed several flocks of gulls flying along the coast towards Dover. The wind was then blowing from the north-east, as it had been doing throughout August, and there was little indication of a change; but an old fisherman remarked that the gulls which had stayed on the coast at Margate and to the west of it were moving to the south coast to meet a south-west wind, which was sure to come. As is known, the change occurred on the following day, and the wind veered round to the south-west. In connection with this observation, it is worth remark that Mr. Inwards, in his "Weather Lore," says: "The arrival of sea-gulls from the Solway Firth to Holywood, Dumfriesshire, is generally followed by a high wind and heavy rain from the south-west."

THE death is announced of M. Henri Lévêque de Vilmorin, first vice-president of the Paris Société d'Horticulture, and officer of the Legion of Honour.

THE tenth annual general meeting of the Institution of Mining Engineers will be held at Sheffield on September 19-21, under the presidency of Mr. J. A. Longden. Among the subjects of papers to be read or taken as read are:—Instantaneous outbursts of fire-damp and coal at Broad Oak Colliery, by Mr. John Gerrard; Castleton: history, geology, minerals and mining, by Mr. A. H. Stokes; the Peak Cavern, by the Rev. J. M. Mello; the mining districts near Kamloops Lake, British Columbia, by Mr. G. F. Monckton; the Devonian iron-ores of Asturias, Spain, by Mr. J. A. Jones; alternating currents and their possible applications to mining (Part i.), by Mr. Sydney F. Walker.

A TEACHER of science with a successful career before him has been lost by the death of Mr. O. G. Jones, who was killed in an accident on the Dent Blanche on August 30. Mr. Jones was appointed to the post of physics master in the City of London School in 1892, when a science side was being organised. He received his training at the Finsbury Technical College and at the Central Technical College, South Kensington, at both of which institutions he held scholarships. He was a B.Sc. of the University of London, where he took first class honours in physics. He possessed high qualities as a teacher, and his sad death will be much regretted.

THE New York *Nation* publishes a few particulars referring to the Danish northern-lights expedition which has just left Copenhagen for Iceland. The headquarters will be at Akureyri, a prettily situated little town on Iceland's northern coast. The expedition has been for several months under preparation, and its members have been carefully practised in the use of the instruments, all of the latest construction, which it carries with it. While the headquarters will remain at Akureyri, an auxiliary station will be established on a high hill not far away, and the two stations will be connected both by telephone and by an optical telegraph. The Director of the Danish Meteorological Office, Dr. Adam Paulsen, is at the head of the expedition. He will test his own published theories on the aurora, as well as others advanced by various investigators. Among the instruments to be used are photographic ones, and others of a novel character for the measurement of aerial electricity. Dr. La Cour and Dr. Jantzen are the two chief assistants to Dr. Paulsen, while Count Harold Moltke is attached to the party as its artist. The expedition will return in May 1900.

FROM Schwaz in Tirol to Gloggnitz in Lower Austria the southern boundary of the northern Dolomites and the central zone of the Eastern Alps is marked by a distinct depression, corresponding to a band of palæozoic schists, and evidently produced by denudation. This depression may have been a longitudinal valley, perhaps even in Tertiary times, but it is now drained by five channels which have been eroded across the whole of the northern Dolomites, the valleys of the Inn, the Lake Chiem Ache, the Saalach, the Salzach, and the Enns. In a short but valuable paper, contributed to the current number of the *Mittheilungen* of the Vienna Geographical Society, Prof. C. Diener discusses the relation of each of these valleys to the structure of the rocks through which it has been cut. He finds that in their present form all five are simply results of the erosive action of running water, and their position is practically independent of the complex tectonic structure of the region.

THE scientific aspects of the question of musical pitch were described in last week's NATURE by Mr. A. J. Hipkins. A book has now been published containing letters, articles, and comments which have appeared in the press with reference to the proposal to adopt the low pitch throughout the pianoforte trade. The following agreement has been signed by the leaders of the pitch movement in the pianoforte trade:—"The vexed question of a suitable pitch for pianofortes should be settled, and believing that the time has arrived when it can be done effectually, we, the undersigned, after due deliberation, have decided to adopt the Paris diapason normal, but with the allowance for a higher temperature in orchestral performance, accepted since 1896 by the Philharmonic Society—namely, A 439 (C 522) at 68° Fahrenheit. From September 1, 1899, we intend to adopt this pitch as a standard for pianofortes both for retail and wholesale purposes, and will regard the late Philharmonic pitch A 454 (C 540) when required, as an exception, and not, as has been for many years in this country, the rule."

IN commemoration of the centenary of the discovery of the galvanic pile, and in connection with the International Exposition at Como, a statue of Volta has been erected on the Piazza Volta, by public subscription. The accompanying view

of this monument to the pioneer of electrical science is given in *La Nature*. Upon the pedestal of the statue the following words appear:—

OMAGIO
DEI TELEGRAFISTI
D'OGNI NAZIONE
NEL PRIMO CENTENARIO
DELL' INVENZIONE
DELLA PILA
MDCCCXCIX.

As already announced, a National Electrical Congress will be held at Como, in connection with the Volta centenary celebra-

tions, on September 18–23. The congress is being organised by the Associazione Elettrotecnica Italiana and the Società Italiana di Fisica, and the leading foreign scientific authorities have been invited to attend.

THE report of the Director of the Botanical Survey of India, for the year 1898-99, shows that every advantage has been taken of the funds placed at the disposal of the survey for exploration in Burma, Assam and Bengal. A report by Mr. J. F. Duthie, Director of the Botanical Department of Northern India, states that the two parties of plant collectors who left Saharanpur in March 1898 to collect botanical specimens in the forest tracts of the Rohilkhand, Northern Oudh and Gorakhpur districts, collected between them about 1000 species; and also seeds of a large number of trees and shrubs for sowing in the Saharanpur Garden The collections include several very interesting plants, for many of them had not been previously recorded for that part of India, whilst some had not been collected since they were originally discovered by Buchanan-Hamilton and others many years ago.

A BLUE-BOOK just issued, on the number of persons employed, and accidents in mines and quarries in the United Kingdom in 1898 contains several noteworthy points. During the year, 990 separate fatal accidents occurred in and about the mines and quarries, causing the loss of 1075 lives. Compared with the previous year, there was a decrease of twenty-five in the number of fatal accidents and a decrease of twenty-seven in the number of lives lost. When these numbers are considered in relation to the number of persons engaged in the mining industry, it is found that the death-rate in 1898 was the lowest hitherto recorded, viz. 1·28 per thousand as compared with 1·49 for the preceding five years. The improvement commenced in 1895, and has continued steadily down to the present time. It is pointed out that the use of naked lights—always the principal source of danger—is responsible for 147 out of the 163 explosions which occurred, and for sixteen of the twenty-seven deaths. In one of the worst explosions in 1898, it was conclusively proved that the explosion was one of coal-dust alone, and that it was caused by a shot of gunpowder illegally fired in a place which was very hot and dusty. As usual, gunpowder caused far more accidents than any other explosive, and nitroglycerine compounds were responsible for more accidents than nitrate of ammonia compounds.

THE Physical Atlas which has been for about ten years in preparation at the Edinburgh Geographical Institute, under the direction of Mr. J. G. Bartholomew, will be the most comprehensive of its kind ever attempted. A draft prospectus just issued shows that the work will comprise seven volumes and more than two hundred plates. The subjects of these volumes will be geology; orography, hydrography, and oceanography; meteorology; botany; zoology; ethnography and demography; general cosmography and terrestrial magnetism. Berghaus's "Physikalischer Atlas" has been used as the basis of the undertaking; but the present work is much more extensive, and comprises entirely new and original material. Mr. Bartholomew's aim has been to produce a cartographic unification of natural science at the present time, and neither pains nor expense have been spared to make the Atlas a standard one to which men of science may turn with confidence. The meteorology section, with over 400 maps on thirty-four plates, will shortly be published.

AMONG the recent publications of the Deutsche Seewarte we would draw attention to a valuable discussion by Dr. W. Köppen, in vol. xxi. of *Aus dem Archiv*, upon recent determinations of the relation between wind velocity and Beaufort's wind-force scale (0–12). The relatively great expense of anemometers, and the difficulty of obtaining a good exposure for them, are obstacles to their general use, while the employment of the Beaufort scale is necessarily continued at the great majority of observing stations, and at sea. It is therefore important to determine satisfactorily the relation between wind velocity and force. The first serious attempt at this determination was made by Mr. R. H. Scott, in 1875, and the values then obtained still appear in text-books and instructions, although it is now admitted that the instrumental factor 3, which had hitherto been generally used for the conversion of the anemometrical records into miles per hour, is considerably too high. Since that time experiments have been made, notably by Köppen, Sprung, Mohn, Dines, Curtis and others, the general result of which has been to show that the factor in question should be reduced to about 2·2. This result is confirmed by Dr. Köppen's recent investigation, and we understand that, as a result of his inquiries, anemometrical records in all the publications of the Seewarte will in future be reduced to real velocities by this smaller factor. We recommend the careful perusal of Dr. Köppen's paper to all meteorologists.

WE have received from the Secretary to the British Association Committee on Zoological and Botanical Publication a notice to the effect that at the Bristol meeting of the Association the committee was reappointed, with the Rev. T. R. R. Stebbing as chairman, in succession to the late Sir W. H. Flower, and with the addition of Messrs. B. D. Jackson and A. C. Seward as representatives of Botany. It is now proposed to deal with botanical publications; and it is believed that the principles and proposals of the 1897 report will apply with equal force to botanical papers. It is hoped that they may be interpreted in that spirit. It will be well to remind our readers that the recommendations are as follows, viz.:—(1) "That each part of a serial publication should have the date of actual publication, as near as may be, printed on the wrapper, and, when possible, on the last sheet sent to press. (2) That authors' separate copies should be issued with the original pagination and plate-numbers clearly indicated on each page and plate, and with a reference to the original place of publication. (3) That authors' separate copies should not be distributed privately before the paper has been published in the regular manner. (4) That it is desirable to express the subject of one's paper in its title, while keeping the title as concise as possible. (5) That new species should be properly diagnosed, and figured when possible. (6) That new names should not be proposed in irrelevant footnotes or anonymous paragraphs. (7) That references to previous publications should be made fully and correctly if possible, in accordance with one of the recognised sets of rules for quotation, such as that recently adopted by the French Zoological Society."

AN account of the electric welding of tram-rail joints in the city of Buffalo, U.S.A., is given in the *Electrical Review* of August 25. This process of rail welding has been greatly improved, and the results now obtained are seemingly all that can be desired. In Buffalo the bar used for welding is 1 × 3 × 8, and this joining of steel to steel, and the increased carrying capacity owing to the bars at the joints, results in a joint being a place of least resistance. The plant in operation for the purpose of welding consists of five cars. One of these is a sand-blast car which runs in advance of the welding car, and prepares the joint. The other cars are the welding car, the transformer car, the motor and booster car, and a car that follows in the rear to smooth any rough places about the joint. After the welding bars are placed over the joint the jaws of the welder are applied to them, and a pressure of about 1400 lbs. applied by means of a hydraulic jack connected to the upper

end. The current is then turned on, and the metal becomes brighter and brighter until the weld is completed, after which the current is turned off and the pressure increased to about thirty-five tons. While under this pressure the weld is allowed to cool, after which the car is moved back about six inches and the jaws applied to the other end of the bar, where the process is repeated. The other end is treated in the same manner. In other words, the centre weld is made first, and then the end welds. Artificial means of cooling are used, and as the bars cool they exert a powerful influence in bringing the rail ends close, so as to make a tight joint. The current for the operation of the plant is taken from the regular trolley wire service. It would be expected, from considerations of the action of heat upon metals, that rails welded in this way would buckle when they experienced a considerable rise of temperature, or snap when the temperature was very low, but, as a matter of fact, welded rails neither buckle nor break. By applying immense pressure to the material during welding, the length of a continuous rail made by this process is said to have no limit except that of the line itself.

DR. FRANZ BOAS has made a mathematical study (*American Anthropologist*, N.S., i. p. 448) of the biological significance of the cephalic index on the lines suggested by Mr. Francis Galton, and fully developed by Prof. Karl Pearson. His conclusion is that while the cephalic index is a convenient practical expression of the form of the head, it does not express any important anatomical relation. On the other hand, the relation between capacity and head diameters is found to be of fundamental importance, and among these the relation between the transverse diameter and capacity is most significant. Since in measurements on the living we are unable to measure capacity of the head, it is necessary to find a substitute. It would seem that circumferences are the most available means for judging cranial size. Therefore such circumferences should be included in all anthropometrical schedules designed to investigate racial characters.

FROM the Field Columbian Museum we have received Nos. 3 to 6 of the first volume of its "Geological Series" (Chicago, 1899). No. 3 treats of the ores of the South American Republic of Colombia, the specimens being described by Mr. H. W. Nichols, from a collection made by Señor F. Pereira Gamba. The ores were obtained from the mountainous western portion of Colombia, in which the Andes entering from the south divides into three chains known as the eastern, central and western Cordilleras. Gold was first mined by Europeans in Colombia in 1537, and during the sixteenth and seventeenth centuries the country was the great gold producer of the world; now it is said to rank ninth in importance. Iron ore is worked and smelted at Amagá. The authors observe that the gold and silver ores occur either in the acid lavas, which have been erupted at intervals from the close of the Tertiary period to the present time, or in adjacent Archæan schists. In the early days of mining, the superficial weathered rocks, which are the richest, were worked with signal success; the mines are now sunk below this zone. The ores are found in quartz as fissure-veins in the schists, and also as segregations from the surrounding lavas. In the latter case, they appear to have come to the surface in the lavas, from which they have to some extent been leached by hot solfataric waters and by tropical rains.

MESSRS. NEWTON AND CO. inform us that the whole of the lantern exhibitions at the forthcoming meeting of the British Association at Dover are to be carried out by them.

MESSRS. PHILIP HARRIS AND CO., Birmingham, have just published a diary which should be of service to science teachers. The diary covers the year from September 1, 1899, to August 31, 1900; and, in addition to the usual blank pages, contains seventy-six pages of tables and definitions frequently required in physical and chemical laboratories. The book is thus similar to an engineer's pocket-book, and its publication in the form of a diary will make it a constant companion of many science teachers.

MESSRS. R. FRIEDLÄNDER AND SON, Berlin, have issued in a single volume the numbers of *Naturae Novitates* published by them during 1898. It is well known to collectors of scientific books that Messrs. Friedländer's publication contains a useful classified list of current literature on all branches of science, compiled from catalogues in many languages. It is convenient to have these bibliographical lists in volume form, and a full index at the end increases their value.

THE additions to the Zoological Society's Gardens during the past week include a Sykes's Monkey (*Cercopithecus albigularis*, ♂) from South Africa, presented by Mr. W. P. Peyton; a Common Camel (*Camelus dromedarius*, ♂) from Mogador, presented by Mr. F. G. Aflalo; a Stone Curlew (*Oedicnemus scolopax*), European, presented by Mr. S. M. Sargant; a Common Raccoon (*Procyon lotor*) from Barbados, deposited.

ERRATA.—Lord Kelvin asks us to notify the following errata in the MS. of his letter on the "Blue Ray of Sunrise over Mont Blanc," published last week (p. 411):—Line 1, for 5 o'clock read 4 o'clock; line 7, after "light" insert "of sunrise."

OUR ASTRONOMICAL COLUMN.

HOLMES' COMET 1899 *d* (1892 III.).—

Ephemeris for 12h. Greenwich Mean Time.

1899.	R.A. h. m. s.	Decl. ° ′ ″		Br
Sept. 7	2 6 50·14	+41 41 40·0		
8	7 15·57	41 55 35·8		
9	7 39·02	42 9 25·3	0·1814	0·05435
10	8 0·45	42 23 8·0		
11	8 19·84	42 36 43·8		
12	8 37·16	42 50 12·4		
13	8 52·40	43 3 33·5	0·1795	0·05538
14	2 9 5·51	+43 16 46·9		

During the week the comet passes through the north-west of Andromeda, being a few degrees west of γ Andromedæ on the 11th. It is in a good position for observation, but is reported as extremely faint.

In *Popular Astronomy* (vol. vii. pp. 340–342) Prof. C. D. Perrine describes the circumstances of his rediscovery of this comet on June 11 of the present year. The observation was made in the early morning with the 36-inch Lick refractor, the atmospheric conditions being very good. The comet appeared as a round nebulous mass about 30″ in diameter, very faint and with but little central condensation. The orbit is more nearly circular than that of any other known comet, lying wholly between the orbits of Mars and Jupiter, thus suggesting a possible, but as yet unproved, connection with the asteroids also occupying that position.

THE NEW ALGOL VARIABLE IN CYGNUS.—The following are the predicted minima of this newly-discovered variable, which will admit of observation during September :—

	d. h. m.
1899, September ...	12 11 58 G.M.T.
	21 15 27

Mr. J. A. Parkhurst gives (*Popular Astronomy*, August 1899, vol. vii. p. 380) two charts of the stars in the neighbourhood, which will greatly facilitate the detection of the variable. Observations may be satisfactorily made with telescopes of 3 inches aperture. The position is about 1° south preceding the 5th mag. star o^1 Cygni.

HARVARD COLLEGE OBSERVATORY.—Prof. Pickering has recently issued the second part of vol. xxiv. of *Annals of Harvard College Observatory*, containing an exhaustive discus-

sion of the observations made with the meridian photometer during the period 1882–88. The magnitudes, as given in the "Harvard Photometry," are compared with both the "Uranometria Argentina" and the *Bonn Durchmusterung*.

For the greater part there is close agreement, but the magnitudes in the *Bonn Durchmusterung* are found to have a systematic variation according to the right ascension, the stars grouped at about R.A. 7h., in the Milky Way near Monoceros, being more affected than others also in the Milky Way, but at R.A. 18–19h., in Aquila.

Part of the differences between the "Harvard Photometry" values and those of the "Uranometria Argentina" are ascribed to the difference in position of the two stations, as the zenith distances of the stars would be different, and therefore, presumably, the atmospheric absorption; no correction being applied for this, the southern stars at Cordoba would be estimated too bright.

An attempt to revise the scale of the *Durchmusterung* decided that it was practically impossible to reduce it to the photometric scale by any simple rule, and for purposes of comparison the necessary corrections are given to convert one scale into the other from magnitudes 1·0 to 9·2.

Pages 185–233 are devoted to a discussion of the relation between the magnitudes in the *Harvard Photometry* and those determined by Sir William Herschel. Of the six catalogues of Herschel's observations, the third is considered more accurate, and the fifth less so, than the others. In all he published observations of 3000 stars, and the *average* difference from the photometric catalogues of the present day is only ±0·16 magnitude, this including both the possible change during the century which has elapsed and the errors of both determinations. Prof. Pickering is surprised that these observations should not have been repeated at intervals of ten or twenty years, so that deviations of individual stars might be detected. With this idea he gives a special table including all stars in which the difference between Herschel's magnitudes and the photometric ones equals or exceeds half a magnitude.

The remainder of the volume, pp. 234–245, deals with investigations in regard to the relative performance of the large and small meridian photometers which have been employed in the production of the *Harvard Photometry* itself. No difference exceeding the hundredth of a magnitude was detected. Tables are given showing that the values of the *Harvard Photometry* are not sensibly affected by variations of magnitude, right ascension, declination, or proximity to the Milky Way.

TORSION-STRUCTURE IN THE ALPS.[1]

ONE of the most brilliant and suggestive chapters in Suess' monumental work "Das Antlitz der Erde" is that in which he deals with the remarkable whirl-shaped arrangement of the leading lines of the Alpine system (vol. i. chap. 2).

Prof. Suess describes how the "leading line" sweeps round the north in one great curve convex to the north, the Apennines describe a curve convex towards the east, whereas the Dalmatian mountains form opposite it a curve convex to the west; and the curve of the Apennines is continued westward along the Algerian ranges of North Africa, whereas the Dalmatian curve is continued eastward towards Asia Minor. Prof. Suess points out that movements of crust-folding have always taken place towards the convex or outer side of these curves, and have in most cases caused an actual transgression of the curves above the regions in front of them. He further states that it is not fully understood why the mountain-systems should follow curved lines, or why the curves of the Alpine upheaval should in many areas repeat those of former mountain-systems.

Let me, before going further, remind the reader of a lecture given by one of the greatest of stratigraphers, Prof. Lapworth, at a meeting of the Royal Geographical Society five years ago, and reported in these pages ("The Face of the Earth," NATURE, April 26, 1894). This lecture set forth the conception of crust-torsion, demonstrating that "like the present surface of a typical geological formation . . . the surface of the earth-crust at the present day is most simply regarded as the surface of a continuous sheet which has been warped up by the two sets of undulations crossing each other at right angles. But in the case of the earth-surface, the one set of undulations ranges parallel with the equator, and the other ranges from pole to pole."

Prof. Lössen's explanation of the involved stratigraphy of the Harz mountains lays the foundation of our knowledge of torsion phenomena in the field, and, although other explanations have been given of the special difficulties in the Harz mountains, Prof. Lössen's is now generally accepted.

When working out the detailed stratigraphy of a part of the Dolomites, I experienced the same difficulties which Prof. Suess had indicated in connection with the "whirled lines" of the Alpine system generally. My results were laid before the Geological Society in December 1898, and are now published in the August issue of the *Quart. Journ. Geol. Soc.*, along with a stratigraphical map of the district examined. In that paper I have tried to show that the possible solution of some of the difficulties lies in the association of torsional movements in conflicting directions through the crust, with movements of crust-folding taking place across a pre-existing set of crust-folds. The *change in the direction of the resultant earth-thrust* is the cause to which I have ascribed the torsional phenomena observed in the crust-folds.

The following notes will indicate as briefly as possible wherein the characteristic features of Sella and Enneberg in the Dolomites are analogous with characteristic features of the Alpine system, and how far the elucidation I have offered for that area on the lines of torsion may be capable of a wider application.

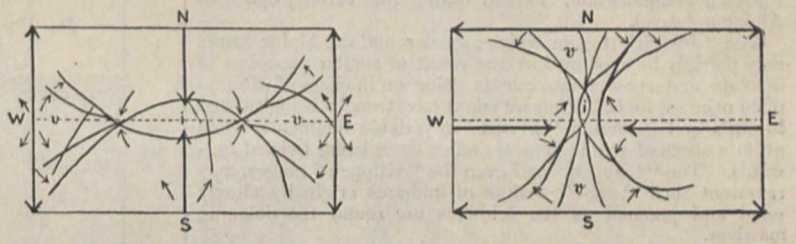

FIG. 1.—Formation of fold-arcs under the influence of torsion-forces. *i*, areas of interference; *v*, areas of virgation.

The stratigraphy of Sella and Enneberg is characterised by twisted strikes, twisted cleavages, twisted arches, twisted troughs, twisted faults, twisted dykes and sills—in fact, the rocks have been twisted and sheared to such a degree that thick deposits have been twined into the form of rock-whorls and large masses of limestone for the greater part changed to dolomite. The various combinations of twisted strikes produce the effect of "whirled" stratigraphical lines round individual centres of the region examined. Sigmoid curves in one direction are correlated with sigmoid curves in another, and arcs which are convex towards north and south are connected by virgating lines with arcs which are convex towards east and west.

Thus we may say that the curves round the north, east, and south of the Sella mountain resemble the "whirl-shaped leading lines" of the Riviera Alps, Apennines and Algerian mountains round the western basin of the Mediterranean Sea; while the curves round the north, west, and south of the Prälongia and Sett Sass area resemble the whirl-shaped lines of the Dalmatian and Pindus mountains and the curvature through the eastern basin of the Mediterranean Sea. The latter curvature resembles that of the mountains around the Roumanian plain, or of the Alps round the plain of Piedmont.

Examples might be multiplied interminably, and on great and small scale, *the reason being that the essential structure of the Alpine system is based upon spirally twisted folds, and not upon linear anticlines and synclines.*

The formation of fold-arcs is illustrated in the accompanying diagrams (Fig. 1), which show that the action of one torsion-couple must be compensated by the reverse action of a correlated torsion-couple, and a fold-arc convex towards one compass direction must be coordinated with a fold-arc convex towards the opposite compass direction. When the convexities approach one another during torsional movements the result is that oppositely-curved fold-arcs intertwine in an area which may be

[1] Condensed from the concluding chapter, "Application to the Alps," in a paper presented at the Roy. Geol. Soc., December 1898.

termed an area of "interference," to distinguish it from the areas of "virgation" where fold-arcs curve away from one another.

A fold-arc is not a homogeneous fold, but is made up of a series of unit-folds, each of which is the segmental portion of a curve. Any one fold, as it were, dies out in its particular direction and horizon, but is replaced by a fold in the next part of the curve passing through slightly different horizons of the crust. Thus the arc round which a series of unit-folds is arranged comes under the category of curves that change their plane.

In Enneberg, series of fold-arcs with their convexities towards different compass directions have been overcast, and the overcast folds have been penetrated by reverse and normal fault-planes, reverse movement having taken place in the subjacent slices of the overcast folds. But, combined with reverse movements in virtue of vertical components, there have been converse movements in virtue of torsional components, so that the actual resultant movement has been spiral—*e.g.* while the middle or "arch" slice of an overcast fold moved in clockwise direction and outward, the upper and under slices of the same fold moved in counter-clockwise direction and inward.

The problem resolves itself into involute and evolute movements of crust-slices with reference to central areas, the evolute slices tending ever to spread, the involute slices ever to narrow.

Shear-breccias and fragmentary portions of folds fill up the inwardly-tilted troughs. The fault-rocks in certain of the sheared and twisted troughs of Enneberg had been formerly treated as independent zones of rock, and termed "Buchenstein Agglomerate"; but in my paper they are shown to be practically a "Flysch conglomerate," formed during the Tertiary epoch of Alpine upheaval.

The "Flysch" troughs which appear round the Alpine curves may possibly be explained as the result of similar processes of involute and evolute movements going on in slices of closely-piled overcast folds. Thus we might have troughs being twisted inwards and gathering "Flysch" in variable fragments, while evolute slices of the reciprocal arches were being twisted outwards. The "Klippen," and even the "Klippen" ranges, may represent such "arch" wedges of fold-arcs originally closely piled and jammed as the fold-arcs are round the dolomite massives.

There is abundant evidence in Enneberg that the molten layers immediately below the crust have shared in the movements of torsional-folding. They have filled the body of the virgating fold-arcs produced by these movements, and have there been incorporated in the local crust-whirl of torsion-movements, finding inlet into the planes of fold-shearing, and being dragged and twisted along with adjacent fault-blocks. An inrush during earlier phases of torsion has been in its turn invaded by the next inrush, and so on, in accordance with the gradual progress of torsion; the latest invasions occur along transverse and oblique faults, belonging to a system of faults which has affected Oligocene strata in the Judicarian area; hence such injected rock is not older than Middle Tertiary.

The fundamental feature of torsional folding may be said to be centralisation; whether it be involution of certain horizons in covered troughs, or evolution of other horizons in overcast arches, the movements have reference to the centres of torsion-basins and torsion-buckles.

The principles thus demonstrated in Enneberg will be seen to involve the "fan-shaped structure" of central massives. They could not fail to do so, since they have been deduced from the stratigraphy of Sella massive in Enneberg, which presents a wonderfully symmetrical, although obliquely elongated example of "fan-structure."

I have shown in my paper on Enneberg that the transverse faults define a later or Tertiary series of arches and troughs, through whose septal portions they chiefly pass. The faults are shearing-planes, and are the result of oppositely-directed movements of twisting and thrusting which have taken place from opposite arches upon common reciprocals, the intermediate troughs. These movements have produced the virgating groups of north and south fold-arcs which meet the east and west fold-arcs, and the sigmoidal combinations of torsional fold-arcs and fault-curves represented in Fig. 2.

The continuance of the faulting during a protracted period of crust-adjustment has caused displacement of the arcs on the opposite sides.

There are several well-known lines of tranverse and oblique shearing through the Alps which repeat these phenomena on a larger scale, and at the same time no detail is wanting in the comparison. Some of these may be indicated: (1) The Judicarian-fault; (2) Iseo-Ortler; (3) Como-Sonthofen; (4) Maggiore-Sargans; (5) Tarentaise-Thun; (6) Savoy-Freyburg—all these represent directions of inthrow and faulting along the "septum" or "middle limb" between great transverse arches and troughs which form part of major Alpine torsion-curves.

With regard to the eastern Alps, there are also well-marked N.N.E.–S.S.W. directions of faulting and displacement. The pre-eminent example is the remarkable series of down-throws at the eastern limit of the Alps, with which is associated the displacement of the northern curve of the Alps towards the Carpathian curve. At the same time, the influence of the co-ordinated torsional movements round the Hungarian basin is evidenced in the eastern Alps by N.N.W.–S.S.E. directions of transverse-shearing.

All the transverse directions of tectonic disturbance in the Alps have in common with the parallel Enneberg lines (*a*) the

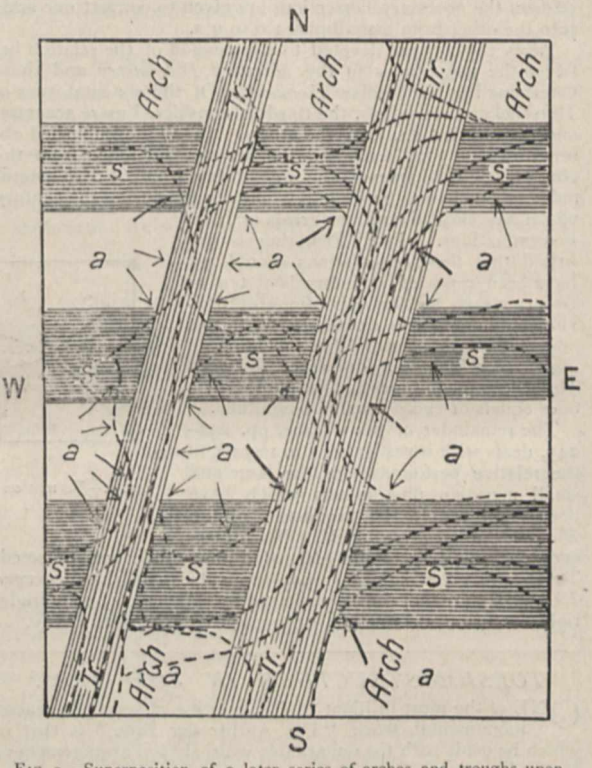

FIG. 2.—Superposition of a later series of arches and troughs upon an east and west series (a, s); chief result, overcasting and overthrusting of old and new arches over synclinal troughs. - - - - fold-curves and faults formed by the twisted shearing.

virgation from them of an eastern and western series of torsion-curves, representing fold-arcs; (*b*) the injection of igneous rock along the main direction of "septal" shearing, associated with the presence of larger masses in the areas of fold-expansion; (*c*) the fact that they have continued to act as lines of crust-adjustment subsequently to the period of acute torsional upheaval. In the Alps the repeated displacement of the main chain to the north would simply indicate that the arch on the east of any transverse depression had been originally less elevated than the arch on the west of the same depression. The extent to which eastern curves have been twisted away from western curves originally belonging to the same "bundle" of virgating folds, may give us some idea of the tremendous shearing that has taken place, and the great compression that the Alpine regions have sustained from east and west in virtue of this oblique, sigmoidal movement of opposite arches over intervening synclines (Fig. 3).

The law which I deduced from my observations at Sella was that the southwardly-convex torsion curves are marked by over-

thrust of the folds towards the south, the northwardly-convex curves by overthrust of the folds towards the north. This law agrees with the movements which Prof. Suess has described along the curved lines of Alpine upheaval, and finds further confirmation in the curious effects of reversal of thrust-movements which are so highly characteristic of all the great transverse Alpine arches. To cite one example, compare the great overthrusts round the south curves of the western Alps with the northwardly-directed overthrusts in the Bernese Oberland.

The drawing (Fig. 3) shows that the eastern and western fold-arcs associated with any transverse direction of faulting provide the same fundamental conditions of peripheral overthrusts with reference to definite centres which I demonstrated in Enneberg. And as the centres are comprised in the very highest transverse Alpine arches which were determined during the later epoch of Alpine upheaval, it is here that, according to torsional laws, the highest individual massives should be present.

The essential structure is the same, whether it be exemplified in the variously-shaped dolomite massives or in the variously-shaped central massives—elliptical, lenticular, or elongated, clearly or less clearly defined from one another—all may be regarded as an inevitable result of crust-torsion.

Even when considerable subsequent faulting and lateral displacement might seem to have obliterated the original relationship of opposite torsion-curves, there are long streaks or interrupted appearances of igneous injections along the main fault-line, which afford evidence of a probable original connection between eastern and western fold-arcs now fairly remote from one another.

The more or less sickle-shaped form of some Alpine curves represents a north and south fold-arc on the same side of a transverse direction of shearing. The Enneberg curve (Langs-da-Für, Campolungo, Cherz Hill) is an example on a small scale, the Banat curve round the Roumanian Plain is an example on a grand scale.

The chief line of fault there is the "Banat" line, which in its tectonic relations bears a strong resemblance to the Judicarian line. It runs north and south and separates a western area of mica schists from an eastern depressed area of Jurassic and Cretaceous strata, eruptive rocks occurring at intervals along the fault. In describing the Banat fault, Prof. Suess never doubts the Tertiary age of the folds and of the eruptive rocks associated both with the folds and with the fault. He notes the twisting character of the strike, and expressly states that the eruptive rocks "*must have been Tertiary notwithstanding the resemblance almost amounting to identity which they present with those of the Judicarian and Predazzo areas*" ("Antlitz," i. p. 623 and pp. 210–213; the italics are mine). Further, he quotes Dr. Posepny's opinion "that these eruptive masses are not masses exerting pressure, but themselves pressed. The subsidence of a neighbouring district induces such eruptions, but the eruptive masses themselves are pressed into the dykes by the pressure of the sinking masses" (*l. c.* p. 210). Similar reasoning was followed by Dr. Salomon in his paper on the Peri-Adriatic eruptive masses, wherein he advocated the theory that the Peri-Adriatic masses originated in consequence of the Peri-Adriatic subsidence, and were of the age of the subsidence.

I would be inclined to class both the Judicarian and Banat faults as phenomena of torsional eruptivity which may, upon the evidence of the sedimentary strata involved in the folds, be referred to the Mid-Tertiary epoch of Alpine upheaval.

Two great internal torsion-basins within the Alpine systems of southern Europe are the Hungarian and the west Mediterranean. The arrangement of the Carpathian mountains round the Hungarian basin presents all the characteristic features of torsion. Mountain fold-arcs have formed peripherally, and broken arches have been thrust outwards and upwards from the basin, while fold-slices produced by normal faulting have had an involute movement inward and downward. Eruptivity has been particularly active in the main septal zone between the oppositely moving portions of the fold-arcs. The Dalmatian mountains represent a series of peripheral folds whose arches have moved towards the south-west, while the eastern Alps betray the influence of this movement of folding, and also a co-ordinated movement to north-west.

The centrifugal movements round the periphery of the western part of the Mediterranean basin have caused the up-folding of the Apennines towards the north-east, and again an igneous zone runs irregularly between the area of peripheral out-thrust and inward down-throw. It is still further within the igneous zone that we must look for the buckling-up of new rock-folds, but the new folds can never be absolutely parallel with the predecessors, *since crust-torsion is going on all the time.* Hence the virgation of successively formed ranges in great mountain-systems would appear to rest upon much the same principle as the virgation of fold-arcs illustrated at Gröden Pass in Enneberg (*Q.J.G.S.*, August 1899, *l.c.*, Plate I.).

While torsion-basins tend by reason of repeated buckling to narrow within themselves, the tendency of the regions outside the outermost peripheral fold-arcs is to subside towards the torsional sag. To such return involute movements we may probably attribute the present subsidence going on in the Adriatic areas, as also the tendency for lakes and plains to form on the outer skirts of torsional mountain-systems.

The Caucasus mountains afford an example of the occurrence of an internal area of down-throw in various parts of which

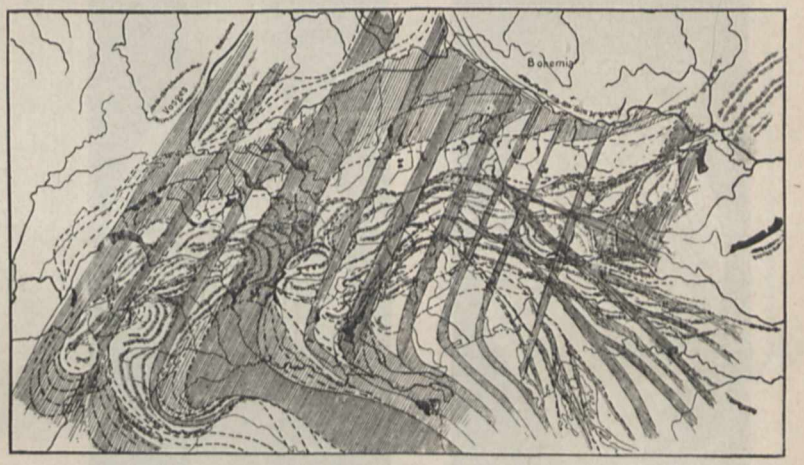

FIG. 3.—The leading oblique arches and troughs of the Tertiary upheaval of the Alps. (The troughs are shaded, the arches are between the troughs, and the chief fold-arcs of the mountain masses are indicated within the arches by shading and broken lines.)

vulcanicity has been active, and of outer areas along which overcast folds of immense size have been gradually involuted. The Alps show at the present day an advanced phase in their torsional history. Earlier outer folds have been broken down owing to dynamic as well as aërial causes of denudation, and have disappeared along interrupted outer shear-zones which I would identify as those occupied by "Flysch" rocks of whatever age. These rocks represent the necessary deformation of older and less twisted folds by the process of involution during the gradual evolution of later and more twisted folds.

Such an explanation of the relation of the Flysch to the present Alps would agree with the observed fact that fragments of granitoid and metamorphic rocks contained in the Flysch show less metamorphic change than those in the central massives of the Alps, since it would relate the Flysch to lost earlier folds which had undergone a smaller degree of torsion than the succeeding folds.

The widely-extended subsidence during Jurassic and the greater part of Cretaceous time in Europe seems to have been the turning-point in the history of Alpine upheaval, since previously, in Alpine regions, the resultant forces had acted more strongly from north and south than from east and west, and afterwards the movements came almost transversely. Hence the long continuation of the great Mesozoic epoch of deposition and subsidence, in inducing the strong action of east and west crust-strains over a region where previously the action of north and south crust-

strains had been pre-eminent, has probably been the initiative cause of an acute epoch of crust-torsion and folding along oblique and transverse lines.

The new movements affected all European areas, dovetailing new folds into the midst of, and across, old folds, and determining new centres of virgation. In the Alps new arches and troughs were formed obliquely and transversely across the older series; the first-formed basins in the new movement were themselves over-arched or blocked up as the fan-shaped mountain-massives gradually became more and more compactly pressed together, and the great torsion-basins of southern Europe became confirmed in their new shape and position acquired in accordance with the altered conditions of crust equilibrium.

As might be expected, there is frequent indication that eruptive activity in Tertiary time broke out afresh in the same areas where eruptive activity had marked the Upper Carboniferous and Permo-Triassic period of movements. But the chief groups of eruptive rock round the inner *curves* of the Alps, Apennines and Carpathians, as well as the injections along oblique directions of shearing, may be clearly identified with the Tertiary torsion movements, for the most part, with the acute Mid-Tertiary epoch of torsion. The larger masses of igneous rocks in the central massives may belong in part to the ancient Palæozoic or Permo-Carboniferous epochs of upheaval, in part to the late-Mesozoic and Tertiary epochs.

A general conclusion may be made from the above that there are serpentines, diorites, granites, felsites, basalts in Alpine folds and faults which can be identified more especially with the "evolute" phenomena of Tertiary torsional movements. And these intrusions, injections, and eruptions involved in the last acute epoch of upheaval in Southern Europe are clearly correlated with similar eruptive phenomena throughout the same period in other parts of Europe, *e.g.* Auvergne, Scotland, Iceland.

MARIA M. OGILVIE.

THE MOTION OF A PERFECT LIQUID.[1]

IF we look across the surface of a river, we cannot fail to observe the difference of the movement at various points. Near one bank the velocity may be much less than near the other, and generally, though not always, it is greater in the middle than near either bank. If we could look beneath the surface and see what was going on there, we should find that the velocity was not so great near the bottom as at the top, and was scarcely the same at any two points of the depth. The more we study the matter, the more complex the motion appears to be; small floating bodies are not only carried down at different speeds and across each other's paths, but are whirled round and round in small whirlpools, sometimes even disappearing for a time beneath the surface. By watching floating bodies we can sometimes realise these complex movements, but they may take place without giving the slightest evidence of their existence.

You are now looking at water flowing through a channel of varying cross section, but there is very little evidence of any disturbance taking place. By admitting colour, although its effect is at once visible on the water, it does not help us much to understand the character of the flow. If, however, fine bubbles of air are admitted, we at once perceive (Fig. 1) the tumultuous conditions under which the water is moving and that there is a strong whirlpool action. This may be intensified by closing in two sides (Fig. 2), so as to imitate the action of a sluice gate, through the narrow opening of which the water has all to pass,

FIG. 1.

FIG. 2.

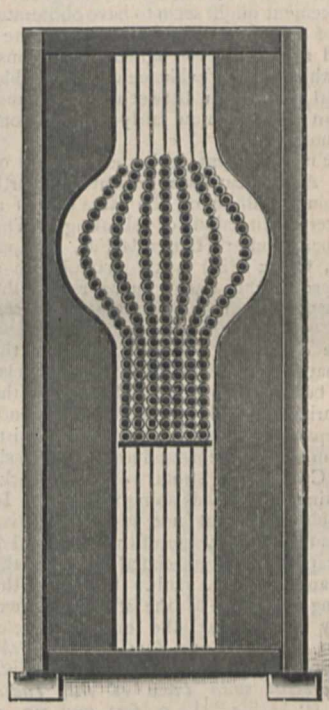

FIG. 3.

the presence of air making the disturbed behaviour of the water very evident.

Now you will readily admit that it is hopeless to begin to study the flow of the water under such conditions, and we naturally ask, are there not cases in which the action is more simple? Such would be the case if the water flowed very slowly in a perfectly smooth and parallel river bed, when the particles would follow one another in lines called "stream-lines," and the flow would be like the march of a disciplined army, instead of like the movement of a disorderly crowd, in which free fights taking place at various points may be supposed to resemble the local disturbances of whirlpools or vortices.

The model (Fig. 3) represents on a large scale a section of the channel already shown, in which groups of particles of the water are indicated by round balls, lines in the direction of flow of these groups (which for convenience we may call particles) being coloured alternately. When I move these so that the lines are maintained, we imitate "stream-line" motion, and when, at any given point of the pipe, the succeeding particles always move at exactly the same velocity, we have what is understood as "steady motion."

[1] A discourse delivered at the Royal Institution on Friday, February 10, by Prof. H. S. Hele-Shaw.

As long as all the particles move in the straight portion of the channel, their behaviour is easy enough to understand. But as the channel widens out, it is clear that this model does not give us the proper distribution. In the model the wider portions are not filled up, as they would be with the natural fluid; for it must be clearly understood that the stream-lines do not flow on as the balls along these wires, passing through a mass of dead water, but redistribute themselves so that every particle of water takes part in the flow. Perhaps you may think that if these wires were removed, and the wooden balls allowed to find their own positions, they would group themselves as with an actual liquid. This is not the case; and, for reasons that you will see presently, no model of this kind would give us the real conditions of actual flow. By means of a model, however, we may be able to understand why it is so absolutely essential we should realise the correct nature of the grouping which occurs.

First look at the two diagrams (Figs. 4 and 5), which you will see represent channels of similar form to the experimental one. The same number of particles enter and leave in each under apparently the same conditions, so that the idea may naturally arise in your minds, that if the particles ultimately flow with the same speed whatever their grouping in the larger portion of the channel, it cannot much matter in what particular kind of formation they actually pass through that wider portion. To understand that is really very important. Let us consider a

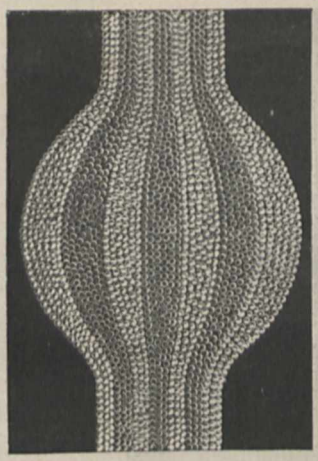

FIG. 4.

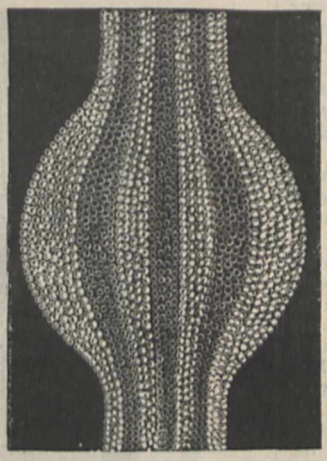

FIG. 5.

model (Fig. 6) specially made for the purpose. You will see that we have two lines of particles which we may consider stream-lines, those on the left coloured white, and those on the right coloured red. The first and last are now exactly 18 inches apart, there being eighteen balls of 1 inch diameter in the row. If I move the red ones upward, I cause them to enter a wider portion of the channel, where they will have to arrange themselves so as to be three abreast (Fig. 7). It is quite clear to you, that as I do this their speed in the wider portion of the channel is only one-third of that in the narrow portion, as you will see from the relative positions of the marked particles. Now, directly the first particle entered the wider channel, it commenced to move at a reduced speed, with the result that the particles immediately behind it must have run up against it, exactly in the same way that you have often heard the trucks in a goods train run in succession upon the ones in front, when the speed of the engine is reduced; and you will doubtless have noticed that it was not necessary for the engine actually to stop in order that this might take place. Moreover, the force of the impact depended largely upon the suddenness with which the speed of those in front was reduced. Applying this illustration to the model, you will see that the impact of these particles in the wider portion would necessarily involve a greater pressure in that part. Turning next to the white balls, I imitate, by means of the left-hand portion, the flow which will occur in a channel six times as large as the original one, and you now see (Fig. 7) that as the particles have placed themselves six abreast, and the first and last row are 3 inches apart instead of 18 inches, the speed in the wider portion of the channel must have been one-sixth of that in the narrow portion. Evidently, therefore, the velocity of the particles has been reduced more rapidly than in the previous case, and the pressure must consequently be correspondingly greater.

We may now take it as perfectly clear and evident, that the pressure is greater in the wider portion and less in the narrower portion of the channel. Turning now to the two diagrams, we see that the pressure is in each case greater in every row of particles as in the wider portions of the channel, but that instead of being suddenly increased, as in the model, it is gradually increased. The width of the coloured bands, that is, rows of particles, or width apart of stream-lines, is a measure of the increased pressure. Thus you will now regard the width of the bands, or what is the same thing, the distance apart of the stream-lines, as a direct indication of pressure, and the narrowness or closeness of the stream-lines as a direct indication of velocity.

Next notice the great difference between the two diagrams. In one diagram (Fig. 4) the change of width is uniform across the entire section. In diagram (Fig. 5), however, this is not the case. In the narrowest portion of the channel in each diagram there are seven colour bands of little balls each containing three abreast, but we find that in one diagram (Fig. 4) they are equally spaced in the wider part six abreast throughout. In the other diagram (Fig. 5) the outer row is spaced eight abreast, the second row rather more than six, and the inner rows rather more than four abreast, and the middle row less than four abreast, making in all forty-two in a row, as in the previous case. One diagram (Fig. 5) therefore will represent an entirely different condition to the state represented by the other diagram (Fig. 4), the pressure in the wide part of the latter varying from a maximum at the outside to a minimum in the middle, while the corresponding velocity is greatest in the middle and least at the outside or borders.

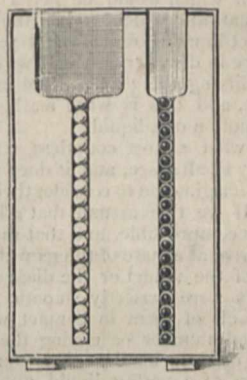

FIG. 6.

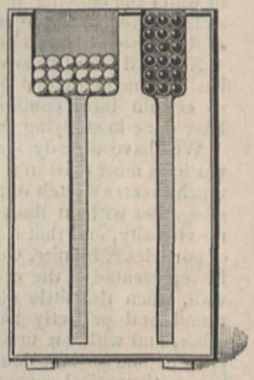

FIG. 7.

Now, when we know the pressure at every point of a liquid, and also the direction in which the particles are moving, together with their velocity at every point, we really know all about its motion, and you will see how important the question of grouping is, and that, in fact, it really constitutes the whole point of my lecture to-night. How then shall we ascertain which of the two groupings (Fig. 4 or 5) is correct, or whether possibly some grouping totally different from either does not represent the real conditions of flow?

Now, the model does not help us very far, because there seems to be no means of making the grouping follow any regular law which might agree with fluid motion. In whatever way we improve such a model, we can scarcely hope to imitate by merely mechanical means the motion of an actual liquid, for reasons which I will now try to explain.

In the first place, apart from the particles having no distinguishing characteristics, either when the liquid is opaque or transparent, they are so small and their number is so great as to be almost beyond our powers of comprehension. Let me try, by means of a simple illustration, to give some idea of their number, as arrived at by perfectly well recognised methods of physical computation. Lord Kelvin has used the illustration that, supposing a drop of water were magnified to the size of the

earth, the ultimate particles would appear to us between the size of cricket-balls and foot-balls. I venture to put the same fact in another way, that may perhaps strike you more forcibly. This tumbler contains half a pint of water. I now close the top. Suppose that, by means of a fine hole, I allow one and a half billion particles to flow out per second—that is to say, an exodus equal to about one thousand times the population of the world in each second,—the time required to empty the glass would be *between* (for of course we can only give certain limits) seven million and forty-seven million years.

In the next place, we have the particles interfering with each other's movements by what we call "viscosity."

Of course, the general idea of what is meant by a "viscous" fluid is familiar to everybody, as that quality which treacle and tar possess in a marked degree, glycerine to a less extent, water to a less extent than glycerine, and alcohol and spirits least of all. In liquids, the property of viscosity resembles a certain positive "stickiness" of the particles to themselves and to other bodies; and would be well represented in our model by coating over the various balls with some viscous material, or by the clinging together, which might take place by the individuals of a crowd, as contrasted with the absence of this in the case of no viscosity as represented by the evolutions of a body of soldiers. It may be accounted for, to a certain extent, by supposing the particles to possess an irregular shape, or to constantly move across each other's paths, causing groups of particles to be whirled round together.

Whatever the real nature of viscosity is, it results in producing in water the eddying motion which would be perfectly impossible if viscosity were absent, and which makes the problem of the motion of an imperfect liquid so difficult and perplexing.

Now, all scientific advance in discovering the laws of nature has been made by first simplifying the problem and reducing it to certain ideal conditions, and this is what mathematicians have done in studying the motion of a liquid.

We have already seen what almost countless millions of particles must exist in a very small space, and it does require a much greater stretch of the imagination to consider their number altogether without limit. If we then assume that a liquid has no viscosity, and that it is incompressible, and that the number of particles is infinite, we arrive at a state of things which would be represented in the case of the model or the diagram on the wall, when the little globes were perfectly smooth, perfectly round and perfectly hard, all of them in contact with each other, and with an unlimited number occupying the smallest part of one of the coloured or clear bands. This agrees with the mathematical conception of a perfect liquid, although the mathematician has in his mind the idea of something of the nature of a jelly consisting of such small particles, rather than of the separate particles themselves. The solution of the problem of the grouping of the little particles, upon which so much depends, and which may have at first seemed so simple a matter, really represents, though as yet applied to only a few simple cases, one of the most remarkable instances of the power of higher mathematics, and one of the greatest achievements of mathematical genius.

You will be as glad as I am that it is not my business to-night to explain the mathematical processes by which the behaviour of a perfect liquid has been to a certain extent investigated. You will also understand why such models as we could actually make, or any analogy with the things with which we are familiar, would not help us very much in obtaining a mental picture of the behaviour of a perfect liquid. If, for instance, we try to make use of the idea of drilled soldiers, and move the lines with that object in view, we see that instead of the ordinary methods of drill, the middle rank soon gains on the others, and enters again the parallel portion of the channel in a very different relative position to the opposite lines, although the stream-lines would all have the same actual velocity when once again in the parallel portion. Since, then, we cannot use models or any simple analogy with familiar things, or follow—at any rate this evening—the mathematical methods of dealing with the problem, what way of understanding the subject is left to us?

If we take two sheets of glass, and bring them nearly close together, leaving only a space the thickness of a thin card or piece of paper, and then by suitable means cause liquid to flow under pressure between them, the very property of viscosity, which, as before noted, is the cause of the eddying motion in large bodies of water, in the present case greatly limits the freedom of motion of the fluid between the two sheets of glass, and thus prevents, not only eddying or whirling motion, but also counteracts the effect of inertia. Every particle is then compelled by the pressure behind and around it to move onwards without whirling motion, following the path which corresponds exactly with the stream-lines in a perfect liquid.

If we now, by a suitable means, allow distinguishing bands of coloured liquid to take part in the general flow, we are able to imitate exactly the conditions we are seeking to understand.

[Prof. Hele-Shaw here gave demonstrations of the stream-lines in liquids flowing under the conditions of a gradually enlarging and contracting channel. He proved that the condition of flow corresponded closely with that shown in Fig. 5 and *not* with that given in Fig. 4. The method of the experiments has already been described in NATURE (vol. lviii. p. 34), though by using glycerine instead of water much more perfect results were obtained than in those then described.]

But at this stage you may reasonably inquire how it is that we are able to state, with so much certainty, that the artificial conditions of flow with a viscous liquid are really giving us the stream-line motion of a perfect one; and this brings me to the results which mathematicians have obtained.

The view now shown represents a body of circular cross-section, past which a fluid of infinite extent is moving, and the lines are plotted from mathematical investigation and represents the flow of particles. This particular case gives us the means

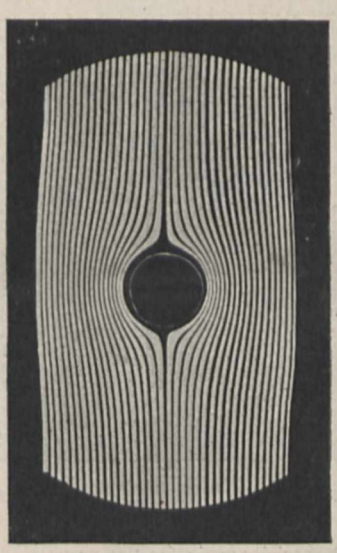

FIG. 8.

of most elaborate comparison; although we cannot employ a fluid of infinite extent, we can prepare the border of the channel to correspond with any one of the particular stream-lines, and measure the exact positions of the lines inside.

By means of a second lantern, the real flow of a viscous liquid for this case is shown upon the second screen, and you will see that it agrees with the calculated flow round a similar obstacle of a perfect liquid. The diagram shown on the wall is the actual figure employed for comparison, and upon which the experimental case was projected. By this means, it was proved that the two were in absolute agreement. If we start the impulses, as before, in a row, we at once see how the middle particles lag behind the outer ones, as indicated by the width of the bands, showing that it is not necessarily the side stream-lines that move more slowly. It may be more interesting to you to see, in addition to the foregoing case—in which for convenience, and as quite sufficient for measurement only, a semi-cylinder was employed—the case of a complete cylinder (Fig. 8). In this case two different colours are used in alternate bands, and these bands are sent in, not steadily, but impulsively, in order to illustrate what I have just pointed out. You will see how the greater width of the colour bands before and behind the cylinder indicates an increase of pressure in those regions. This in a ship-shape form accounts for the standing bow and stern

waves, whereas the narrowing of the bands at the sides indicates an increase of velocity and reduction of pressure, and accounts for the depression of water level, with which you are doubtless familiar, at the corresponding part of a ship.

I will now take a more striking case. If, instead of a circular body, we had a flat plate, the turbulent nature of the flow is evidently very great, as you will see from the view (Fig. 9), which is a photograph of the actual flow under these conditions, made visible by very fine air bubbles, and showing water at rest in the clear space behind the obstacle.

We can, however, take steps to reduce this turbulence, and you now see on the second screen the flow by means of apparatus which time does not permit me to describe, but which gives a slow and steady motion that it would be impossible to improve upon in actual conditions of practice, or even, I am inclined to think, by any experimental method. Instead of using air to

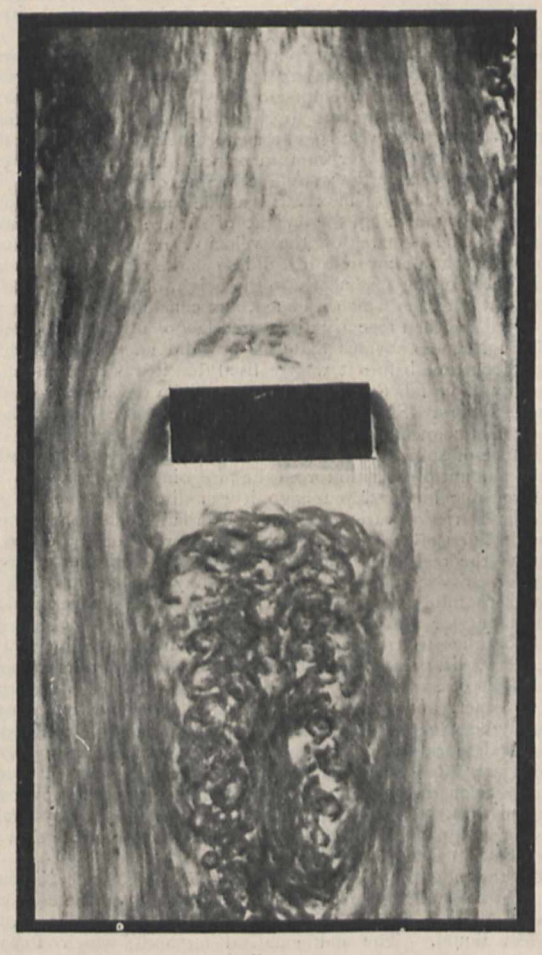

FIG. 9.

make this flow clear, we now allow colour to stream behind the plate, and you will see that the water still refuses to flow round to the back, and spreads on either side. We have so slow a velocity as not to induce vortex motion, but the inertia of the particles which strike the flat plate causes them to be deflected to either side, exactly as tennis-balls in striking against a wall obliquely. The sheet of water is so thick, that is to say, the parallel glass plates are so far apart, that they do not enable the viscosity of the water to act as a sufficient drag to prevent this taking place.

Mathematicians, however, predicted with absolute certainty that with stream-line motion, the water should flow round and meet at the back, a state of things that, however slow we make the motion in the present case, does not occur owing to the effect of inertia. They have drawn with equal confidence the lines along which this should take place. We could either effect this result with the experiment you have just seen, by using a much more viscous liquid, such as treacle, or, what comes to the same thing, bringing the two sheets of glass nearly close together; and the flow which you are now witnessing (Fig. 10) shows the result of doing this. The colour bands in front of the plate no longer mix at all with the general body of flow, or are unsteady, as was the case in the last experiment, but flow round the plate, and flow so steadily, that unless we jerk the flow of the colour bands, it is impossible to tell in which direction they are actually moving. It is interesting to note that where the divided central colour band re-unites is clearly shown in the illustration.

Whilst I have been dealing with the stream-lines of a perfect liquid, your minds will doubtless have turned to the lines along which magnetic and electrical forces appear to act. We are possibly further from realising the actual nature of these forces, than from a correct conception of the real nature of a liquid. We have long agreed to abandon the old ideas of the electrical and magnetic fluids flowing along these lines, and to substitute instead the idea that these lines represent merely the directions in which the forces act. Now we can easily see that this conception is quite a reasonable one, for in the case of the model it is not necessary to have the row of balls actually moving in order that the effect may be transmitted along the different lines they occupy. If I attempt to raise the plate upon which they rest, the pressure is instantly transmitted through

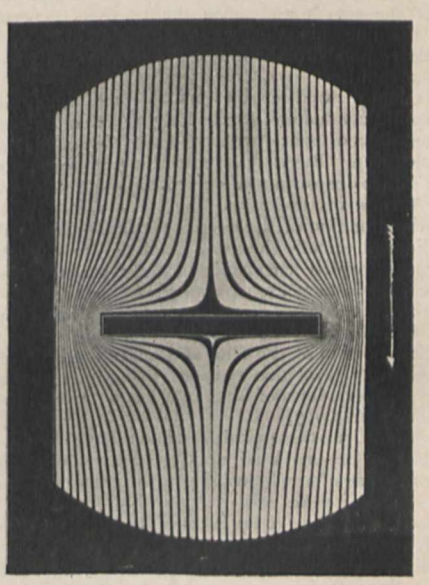

FIG. 10.

the whole row to the top ball along each line, whatever curve the line may take. In the same way, you will remember that it was not necessary to have the colour bands actually in motion, for, though apparently free to move in any direction, they retain their form for a considerable time, and the path along which they would influence each other as soon as the tap is opened would be along those lines in which the liquid was flowing before it was brought to rest. Hence it is possible, with some suitable means, to cause a viscous liquid to reproduce exactly the lines of magnetic and electrical induction. In the case of magnetism and electricity, it is of course possible, by means of a small magnetic needle or a galvanometer, by exploring the whole surface through which magnetic induction or electrical flow is acting, to plot the lines of force for innumerable cases, where we can work in air or on the surface of the solid conductor.

But in this building it seems natural to take as an example the case first used by the great man to whom the conception of lines of magnetic force is due, for the first reference I have been able to find to such lines is in one of Faraday's earliest papers on the indication of electric currents ("Experimental Researches in Electricity," vol. i. p. 32), in which he says, "By magnetic curves I mean the lines of magnetic forces, however modified by the juxtaposition of poles, which would be depicted by iron

filings, or those to which a very small magnetic needle would form a tangent."

You are all familiar with the way in which iron filings set themselves when shaken over the north and south poles of a magnet. The magnetic lines are then nearly, but not quite, circular curves between the two poles. Now, the mathematics of the subject tells us that if the poles could be regarded as points, the lines of force between them would be perfect circles.

You are now looking at the colour bands, the edges—or indeed any portion—of which represent lines obtained by admitting coloured liquid from a series of small holes round a central small orifice, which admits clear liquid, and allows them to escape through another small orifice (called respectively in hydromechanics a *source* and *sink*), and I leave it to you to judge how far these curves deviate from the ideal form.

My assistant is now allowing the colour to flow, first steadily, and then in a series of impulses, and the latter gives us the conception of waves or impulses of magnetic force, though of course the magnetic transmission force would be instantaneous. Regarded as a liquid, it is here again clear how absolutely the truth of our views concerning the slower movement in the wider portion is verified by this experiment.

A last experiment shows the streams admitted, not from a source, but from a row of orifices in what corresponds to the slowest moving portion of the flow. The result is that the colour bands are much narrower, and although the circular forms of the curves are, as in the previous experiment, preserved, the lines are so fine at the point of exit, which, as before, corresponds to the South Pole, as to really approximate to ideal stream-lines.

The same method enables us to trace the lines of force through solid conductors, for, as long as we confine ourselves to two dimensions of space we may have *flat* conductors of any shape whatever. But it does something more, for by making the film rather deeper in some places than others, more particles arrange themselves there, and the lines of flow will naturally tend in the direction of the deeper portion. This will give the stream-lines identically the same shape as the magnetic or electrical curves which encounter in their paths a body of less resistance, for instance, a para-magnetic body.

If, on the other hand, at these points the film is made rather thinner, less particles will be able to dispose of themselves in the shallow portion of the film, and hence the lines of flow will be pushed away from this portion, giving us exactly the same forms as magnetic lines of force in a magnetic field in proximity to a diamagnetic body.

Here, again, mathematical methods have enabled lines of actual flow to be predicted, and you may compare the actual flow for the case of a cylindrical para-magnetic body, which was worked out some years ago.

You will doubtless not be inclined to question the practical value of stream-lines in the subject which we have just been considering, because, unlike the flow of an actual liquid, magnetic lines of force can never be themselves seen, and because there is no doubt as to the correspondence of the directions to the lines of a perfect liquid. It was the conception of these lines in the mind of Faraday, and more particularly their being cut by a moving wire, that enabled him to realise the nature of the subject more clearly than any other man at the time, and to do much towards the rapid development of electrical science and its practical applications.

When we come to consider the relation of the study of the motion of a perfect liquid with hydromechanics and naval architecture, it must be admitted that the matter is a difficult one. Probably one of the most perplexing things in engineering science is the absence of all apparent connection between higher treatises on hydrodynamics and the vast array of works on practical hydraulics. The natural connection between the treatises of mathematicians and experimental researches of engineers would appear to be obvious, but very little, if any, such connection exists in reality, and while at every step electrical applications owe much to the theories which are common to electricity and hydromechanics, we look in vain for such applications in connection with the actual flow of water.

Now the reason for this appears to be the immense difference between the flow of an actual liquid and that of a perfect one owing to the property of viscosity. A comparison of the various experiments which you have seen to some extent indicates this.

In the first place, let us consider for a moment some of the things which would happen if water were a perfect liquid. In such a case, a ship would experience a very different amount of resistance, because, although waves would be raised, owing to the reasons which we have already seen, the chief causes of resistance, viz. skin friction and eddying motion, would be entirely absent, and of course a submarine boat at a certain depth would experience no resistance at all, since the pressures fore and aft would be equal. On the other hand, there would be no waves raised by the action of the wind, and there would be no tidal flow, but to make up for this rivers would flow with incredible velocity, since there would be no retarding forces owing to the friction of the banks. But the rivers themselves would soon cease to flow because there would be no rainfall such as exists at present, since it is due to viscosity that the rain is distributed, instead of falling upon the earth in a solid mass when condensed. In a word, it may be said that the absence of viscosity in water would result in changes which it is impossible to realise.

We may now briefly try to consider the difference between practical hydraulics and the mathematical treatment of a perfect liquid. The earliest attempts to investigate in a scientific way the flow of water appears to have been made by a Roman engineer about 1800 years ago, an effort being made to find the law for the flow of water from an orifice. For more than 1500 years, however, even the simple principle of flow according to which the velocity of efflux varies as the square of the head, or what is the same thing, the height of surface above the orifice varies as the square of the velocity, remained unknown. Torricelli, who discovered this, did so as the result of observing that a jet of water rose nearly to the height of the surface of the body of water from which it issued, and concluded therefore that it obeyed the then recently discovered law of all falling bodies.

Though it was obvious that this law did not exactly hold, it was a long time before it was realised that it was the friction or viscosity of liquids that caused so marked a deviation from the simple theory. Since then problems in practical hydraulics, whether in connection with the flow in rivers or pipes, or the resistance of ships, have largely consisted in the determination of the amount of deviation from the foregoing simple law.

About one hundred years ago it was discovered that the resistance of friction varies nearly in accordance with the simple law of Torricelli, and also—although for a totally different reason—the resistances due to a sudden contraction or enlargement of cross-section of channel or to any sudden obstructions appear to follow nearly the same law. Now it is extremely convenient for reasons which will be understood by students of hydraulics to treat all kinds of resistance as following the same law, viz. square of velocity which the variation of head or height of surface has shown to do. But this is far from being exact, and an enormous amount of labour has consequently been expended in finding for all conceivable conditions in actual work tables of coefficients or empirical expressions which are required for calculations of various practical questions. Such data are continually being accumulated in connection with the flow of water in rivers and pipes for hydraulic motors and naval architecture. This is the practical side of the question.

On the other hand, eminent mathematicians, since the days of Newton and the discovery of the method of the calculus, have been pursuing the investigation of the behaviour of a perfect liquid. The mathematical methods, which I have already alluded to as being so wonderful, have, however, scarcely been brought to bear with any apparent result upon the behaviour of a viscous fluid. Indeed, the mathematician has not been really able to adopt the method of the practical investigator, and deal with useful forms of bodies such as those of actual ships, or of liquid moving through ordinary channels of varying section, even for the case of a perfect liquid, but he has had to take those cases, and they are very few indeed, that he has been able to discover which fit in with his mathematical powers of treatment.

This brief summary may possibly serve to indicate the nature of the difficulties which I have pointed out, and will show you the vast field there yet lies open for research in connection with the subject of hydromechanics, and the great reception which awaits the discovery of a theoretical method of completely dealing with viscous liquids, instead of having recourse as at present principally to empirical formula based on the simple law already alluded to.

We may, however, console ourselves with the thought, that in the application of the laws of motion themselves to *any* terrestrial matters, the friction of bodies must always be taken into account, and renders it necessary, that we should commence by studying the ideal conditions. In this as in other matters, the naval architect and engineer must always endeavour as far as possible to base their considerations and work upon the secure foundation of scientific knowledge, making allowances for disturbing causes, which then cease to be the source of perplexity and confusion. From this point of view, the study of the behaviour of a perfect liquid, even when no such form of matter appears to exist, has an interest for the practical man in spite of the deviation of actual liquids from such ideal conditions. If the truth must be told, it is such a deviation from the simple and ideal conditions that really constitute the work of a professional man, and it is only practical experience which, based upon sound technical knowledge, enables 50,000 tons of steel to be made to span the Firth of Forth, Niagara to be harnessed to do the work of 100,000 horses, or an *Oceanic* to be slid into the sea with as little misgiving as the launch of a fishing-boat.

I have, I am afraid, brought you only to the threshold of a vast subject, and in doing so have possibly employed reasoning of too elementary a kind. After all, I may plead that I have followed the dictum of Faraday, who said, "If assumptions must be made, it is better to assume as little as possible." If I have assumed too little knowledge on your part, it is because of the difficulties I have found in the subject myself. If I have left more obscure than I have been able to make clear, it is consoling to think how many centuries were required to discover even what is known at the present time, and we may well be forgiven if we cannot grasp at once results which represent the life-work of some of the greatest men.

A PROBLEM IN AMERICAN ANTHROPOLOGY.[1]

WHILE engaged in writing the address that I am to read to you this evening, the sad news reached me of the death, on July 31, of our President of five years ago, Dr. D. G. Brinton. Although not unexpected, as his health had been failing since he was with us at the Boston meeting, where he took his always active part in the proceedings of Section H, and gave his wise advice in our general council, yet his death affects me deeply. I was writing on a subject we had often discussed in an earnest but friendly manner. He believed in an all-pervading psychological influence upon man's development, and claimed that American art and culture were autochthonous, and that all resemblances to other parts of the world were the results of corresponding stages in the development of man; while I claimed that there were too many root coincidences with variant branches to be fully accounted for without also admitting the contact of peoples. Feeling his influence while writing, I had hoped that he would be present to-night, for I am certain that no one would have more readily joined with me in urging a suspension of judgment, while giving free expression to opinions, until the facts have been worked over anew, and more knowledge attained.

Now that his eloquent tongue is silent and his gifted pen is still, I urge upon all who hear me to-night to read his two addresses before this Association—one as Vice-President of the Anthropological Section in 1887, published in our thirty-sixth volume of *Proceedings*, the other as retiring President in 1895, published in our forty-fourth volume. In these addresses he had in his usual forcible and comprehensive manner presented his views of American anthropological research and of the aims of anthropology.

Dr. Brinton was a man of great mental power and erudition. He was an extensive reader in many languages, and his retentive memory enabled him to quote readily from the works of others. He was a prolific writer, and an able critic of anthropological literature the world over. Doing little as a field archæologist himself, he kept informed of what was done by others through extensive travels and visits to museums. By his death American anthropology has suffered a serious loss, and a great scholar and earnest worker has been taken from our Association.

[1] Address delivered before the American Association for the Advancement of Science, at Columbus, Ohio, on August 21, by Prof. Frederic Ward Putnam, the retiring President of the Association.

In the year 1857 this Association met for the first time beyond the borders of the United States, thus establishing its claim to the name American in the broadest sense. Already a member of a year's standing, it was with feelings of youthful pride that I recorded my name and entered the meeting in the hospitable city of Montreal, and it was on this occasion that my mind was awakened to new interests which in after years led me from the study of animals to that of man.

On Sunday, August 16, while strolling along the side of Mount Royal, I noticed the point of a bivalve shell protruding from roots of grass. Wondering why such a shell should be there, and reaching to pick it up, I noticed on detaching the grass roots about it that there were many other whole and broken valves in close proximity—too many, I thought, and too near together, to have been brought by birds, and too far away from water to be the remnants of a musk-rat's dinner. Scratching away the grass and poking among the shells, I found a few bones of birds and fishes and small fragments of Indian pottery. Then it dawned upon me that there had been an Indian home in ancient times, and that these odds and ends were the refuse of the people—my first shell-heap or kitchen-midden, as I was to learn later. At the time this was to me simply the evidence of Indian occupation of the place in former times, as convincing as was the palisaded town of old Hochelaga to Cartier when he stood upon this same mountain side more than three centuries before.

At that meeting of the Association several papers were read, which, had there been a section of anthropology, would have led to discussions similar to those that have occurred during our recent meetings. Forty-two years later we are still disputing the evidence, furnished by craniology, by social institutions and by language, in relation to the unity or diversity of the existing American tribes and their predecessors on this continent.

Those were the days when the theory of the unity of all American peoples, except the Eskimo, as set forth by Morton in his "Crania Americana" (1839), was discussed by naturalists. The volumes by Nott and Gliddon, "Types of Mankind" (1854) and "Indigenous Races of the Earth" (1857), which contains Meigs' learned and instructive dissertation, "The Cranial Characteristics of the Races of Men," were the works that stirred equally the minds of naturalists and of theologians regarding the unity or diversity of man—a question that could not then be discussed with the equanimity with which it is now approached. The storm caused by Darwin's "Origin of Species" had not yet come to wash away old prejudices and clear the air for the calm discussion of theories and facts now permitted to all earnest investigators. Well do I remember, when, during those stormy years, a most worthy Bishop made a fervent appeal to his people to refrain from attending a meeting of the Association then being held in his city, on account of what he claimed to be the atheistic teachings of science. Yet ten years later this same venerable Bishop stood before us, in that very city, and invoked God's blessing upon the noble work of the searchers for truth.

At the meeting of 1857 one of our early presidents, the honoured Dana, read his paper entitled "Thoughts on Species," in which he described a species as "a specific amount or condition of concentrated force defined in the act or law of creation," and, applying this principle, determined the unity of man in the following words:—

"We have therefore reason to believe, from man's fertile intermixture, that he is one in species; and that all organic species are divine appointments which cannot be obliterated unless by annihilating the individuals representing the species."

Another paper was by Daniel Wilson, recently from Scotland, where six years before he had coined that most useful word "prehistoric," using the term in the title of his volume, "Prehistoric Annals of Scotland." In his paper Prof. (afterwards Sir Daniel) Wilson controverted the statement of Morton that there was a single form of skull for all American peoples, north and south, always excepting the Eskimo. After referring to the views of Agassiz, as set forth in the volumes of Nott and Gliddon, he said, "Since the idea of the homogeneous physical characteristics of the whole aboriginal population of America, extending from Terra del Fuego to the Arctic circle, was first propounded by Dr. Morton, it has been accepted without question, and has more recently been made the basis of many widely comprehensive deductions. Philology and archæology have also been called in to sustain this doctrine of a special unity of the American race; and to prove that, notwithstanding

some partial deviations from the prevailing standard, the American Indian is essentially separate and peculiar; *a race distinct from all others*. The stronghold, however, of the argument for the essential oneness of the whole tribes and nations of the American continents is the supposed uniformity of physiological, and especially of physiognomical and cranial characteristics; an ethnical postulate which has not yet been called in question."

After a detailed discussion of a number of Indian crania from Canada and a comparison with those from other parts of America, as described by Morton, he makes the following statements:—"But, making full allowance for such external influences, it seems to me, after thus reviewing the evidence on which the assumed unity of the American race is formed, little less extravagant to affirm of Europe than of America, that the crania everywhere and at all periods have conformed, or even approximated, to one type."

"As an hypothesis, based on evidence accumulated in the "Crania Americana," the supposed homogeneity of the whole American aborigines was perhaps a justifiable one. But the evidence was totally insufficient for any such absolute and dogmatic induction as it has been made the basis of. With the exception of the ancient Peruvians, the comprehensive generalisations relative to the southern American continent strangely contrast with the narrow basis of the premises. With a greater amount of evidence in reference to the northern continent, the conclusions still go far beyond anything established by absolute proof; and the subsequent labours of Morton himself, and still more of some of his successors, seem to have been conducted on the principle of applying practically, and in all possible bearings, an established and indisputable scientific truth, instead of testing by further evidence a novel and ingenious hypothesis."

At the close of this instructive paper are the following words: "If these conclusions, deduced from an examination of Canadian crania, are borne out by the premises and confirmed by further investigation, this much at least may be affirmed: that a marked difference distinguishes the northern tribes, now or formerly occupying the Canadian area, in their cranial conformation, from that which pertains to the aborigines of Central America and the southern valley of the Mississippi; and in so far as the northern differ from the southern tribes, they approximate more or less, in the points of divergence, to the characteristics of the Esquimaux: that intermediate ethnic link between the Old and the New World, acknowledged by nearly all recent ethnologists to be physically a Mongol and Asiatic, if philologically an American."

The third paper of the meeting to which I shall refer was by another of our former presidents, the then well-known student of Indian institutions and the author of the "League of the Iroquois" (1851). In this paper, on "The Laws of Descent of the Iroquois," Morgan discusses the league as made up of five nations, each of which was subdivided into tribes, and he explains the law of marriage among the tribes, the family relationship and the descent in the female line, as essential to the maintenance of the whole system. He then says:—

"Now the institutions of all the aboriginal races of this continent have a family cast. They bear internal evidence of a common paternity, and point to a common origin, but remote, both as to time and place. That they all sprang from a common mind, and in their progressive development have still retained the impress of original elements, is abundantly verified. The Aztecs were thoroughly and essentially Indian. We have glimpses here and there at original institutions which suggest at once, by their similarity, kindred ones among the Iroquois and other Indian races of the present day. Their intellectual characteristics, and the predominant features of their social condition, are such as to leave no doubt upon this question; and we believe the results of modern research, upon this point, concur with this conclusion. Differences existed, it is true, but they were not radical. The Aztec civilisation simply exhibited a more advanced development of those primary ideas of civil and social life, which were common to the whole Indian family, and not their overthrow by the substitution of antagonistic institutions."

After calling attention to the fact that a similar condition exists among certain peoples of the Pacific Islands, he writes:—"Whether this code of descent came out of Asia or originated upon this continent is one of the questions incapable of proof; and it must rest, for its solution, upon the weight of evidence, or upon probable induction. Its existence among American races, whose languages are radically different, and without any traditional knowledge among them of its origin, indicates a very ancient introduction, and would seem to point to Asia as the birth-place of the system."

It would be interesting to follow the succeeding meetings of the Association, and note the recurring presentation of views which the quotations I have given show to have been most seriously discussed over a generation ago. An historical review of the literature of American anthropology during the present century would also be interesting in this connection. It is probable, however, that a review of this literature for the first half of the century would reveal the fact that the writers, with here and there a notable exception, were inclined to theorise upon insufficient data, and devoted little time to the accumulation of trustworthy facts. The presentation and discussion of carefully observed facts can almost be said to have begun with the second half of the century, and this is the only part of the subject that now commands serious attention.

A reference to the very latest *résumé* of this subject as presented in the "History of the New World called America," by Edward John Payne (vol. ii., Oxford, 1899), is instructive here. In this volume Mr. Payne admits the great antiquity and unity of the American tribes, which he considers came from Asia in pre-Glacial and Glacial times, when the north-western corner of America was connected with Asia, and when man "as yet was distinguished from the inferior animals only by some painful and strenuous form of articulate speech and the possession of rude stone weapons and implements, and a knowledge of the art of fire-kindling. Such, it may be supposed, were the conditions under which man inhabited both the old and the new world in the paleo-ethnic age. . . . Even when a geological change had separated them (the continents), some intercourse by sea was perhaps maintained—an intercourse which became less and less, until the American branch of humanity became practically an isolated race as America itself had become an isolated continent" (Preface).

Mr. Payne discusses the growth of the languages of America, the various social institutions and arts, and the migrations of these early savages over the continent, north and south, during the many centuries following, as one group after another grew in culture. He considers all culture of the people autochthonous. In writing upon the physical characters of the people, he says, "It may however be suggested that, as in the Old World, the earlier and the smaller tribes tend to dolichocephaly, while the better developed ones are rather brachycephalous, a conclusion indicating that the varying proportions of the skull should be taken less as original evidence of race than as evidence of physical improvement."

This volume by Mr. Payne is replete with similar statements of facts and theories, and shows how difficult it is for us to understand the complications of the subject before us. It cannot be denied that, taking into consideration the number of authors who have written on this subject, Mr. Payne is well supported in his theory of the autochthonous origin of all American languages, institutions and arts; but the question arises, Has not the old theory of Morton, the industrious and painstaking pioneer of American craniology, been the underlying cause of this, and have not the facts been misinterpreted? At the time of Morton, the accepted belief in the unity and universal brotherhood of man was about to be assailed, and it seems, as we now look back upon those times of exciting and passionate discussions, that Morton may have been influenced by the new theory that was so soon to become prominent, namely, that there were several distinct creations of species of the genus *Homo*, and that each continent or great area had its own distinct fauna and flora. Certainly Morton ventured to make a specific statement from a collection of crania which would now be regarded as too limited to furnish true results.

The anthropologist of to-day would hardly venture to do more than to make the most general statement of the characters of any race or people from the examination of a single skull, although after the study of a large number of skulls from a single tribe or special locality he would probably be able to select one that was distinctly characteristic of the special tribe or group to which it pertained.

Relatively long and narrow heads and broad and short heads occur almost everywhere in greater or less proportion. In determining the physical characters of a people, so far as this can be done from a study of crania, the index of the height of

the skull is quite as important as that of its breadth. These indices simply give us the ready means of expressing by figures the relative height and breadth of one skull in comparison with another—a small part of what the zoologist would consider in describing, for instance, the skulls of different species of the genus *Canis*. So in our craniological studies we should determine the relative position, shape and proportions of the different elements of the skull. In fact we should approach the study of human crania with the methods of the zoologist, and should use tables of figures only so far as such tables give us the means of making exact comparisons. Here again are the anthropologists at a disadvantage, inasmuch as it is only very recently that we are approaching a standard of uniformity in these expressions. It is now more than ever essential that anthropologists should agree upon a method of expressing certain observed facts in somatology, so that the conscientious labours of an investigator, who has a special opportunity for working upon one group of man, may be made available for comparison by investigators of other groups.

Probably the old method, still largely in vogue, of stating averages is responsible for many wrong deductions. If we take one hundred or more skulls of any people, we shall find that the two extremes of the series differ to a considerable extent from those which naturally fall into the centre of the series. These extremes in the hands of a zoologist would be considered the sub-varieties of the central group or variety. So in anthropology, we should take the central group of the series as furnishing the true characters of the particular variety or group of man under consideration, and should regard the extremes as those which have been modified by various causes. It may be said that this central group is defined by stating the mean of all the characters; but this is hardly the case, for by giving the mean of all we include such extraneous characters as may have been derived by admixture or from abnormal conditions.

The many differing characteristics exhibited in a large collection of crania, brought together from various portions of America, north and south, it seems to me, are reducible to several great groups which may be generally classed as the Eskimo type, the northern and central or so-called Indian type, the north-western brachycephalic type, the south-western dolichocephalic type, the Toltecan brachycephalic type, and the Antillean type, with probably the ancient Brazilian, the Fuegian and the pre-Inca types of South America. Each of these types is found in its purity in a certain limited region, while in other regions it is more or less modified by admixture. Thus the Toltecan or ancient Mexican type (which, united with the Peruvian, was separated as the Toltecan family even by Morton) occurs, more or less modified by admixture, in the ancient and modern pueblos and in the ancient earth-works of our central and southern valleys. In Peru, more in modern than in ancient times, there is an admixture of two principal types. At the north of the continent we again find certain traits that possibly indicate a mixture of the Eskimo with the early coast peoples both on the Pacific and on the Atlantic sides of the continent. The North-central Indian type seems to have extended across the Continent and to have branched in all directions, while a similar but not so extensive branching, north-east and south, seems to have been the course of the Toltecan type.

This is not theorising upon the same facts from which Morton drew the conclusion that all these types were really one and the same. Since Morton's time we have had large collections of crania for study, and the crania have been correlated with other parts of the skeleton and with the arts and institutions of the various peoples.

Although these relations have been differently interpreted by many anthropologists who have treated the subject, yet to me they seem to indicate that the American continent has been peopled at different times and from various sources; that the great lapse of time since the different immigrants reached the continent has in many places brought about an admixture of the several stocks and modified to a greater or less extent the arts and customs of all, while natural environment has had a great influence upon the ethnic development of each group. Furthermore, contact of one group with another has done much to unify certain customs, while "survivals" have played an active part in the adoption and perpetuation of arts and customs not native to the people by whom they are preserved.

The Inca civilisation, a forcible one coming from the north, encroached upon that of the earlier people of the vicinity of Lake Titicaca, whose arts and customs were to a considerable extent adopted by the invaders. It is of interest here to note the resemblance of the older Andean art with that of the early Mediterranean, to which it seemingly has a closer resemblance than to any art on the American continent. Can it be that we have here an æsthetic survival among this early people, and could they have come across the Atlantic from that Eurafric region which has been the birthplace of many nations? Or is this simply one of those psychical coincidences, as some writers would have us believe? The customs and beliefs of the Incas point to a northern origin, and have so many resemblances to those of the ancient Mexicans, as hardly to admit of a doubt that in early times there was a close relation between these two widely separated centres of ancient American culture. But how did that pre-Inca people reach the Lake region? Is it not probable that some phase of this ancient culture may have reached the Andes from northern Africa? Let us consider this question in relation to the islands of the Atlantic. The Canary Islands, as well as the West Indies, had long been peopled when first known to history; the Caribs were on the northern coast of South America as well as on the islands; and, in the time of Columbus, native trading boats came from Yucatan to Cuba. We thus have evidence of the early navigation of both sides of the Atlantic, and certainly the ocean between could easily have been crossed.

One of the most interesting as well as most puzzling of the many phases of American archæology is the remarkable development of the art of the brachycephalic peoples, extending from northern Mexico, north-eastward to the Mississippi and Ohio valleys, then disappearing gradually as we approach the Alleghenies and, further south, the Atlantic coast, also spreading southward from Mexico to Honduras, and changing and vanishing in South America. Unquestionably of very great antiquity, this art, developed in the neolithic period of culture, reached to the age of metals, and had already begun to decline at the time of the Spanish conquest. How this remarkable development came to exist amid its different environments, we cannot yet fully understand; but the question arises, Was it of autochthonous origin, and due to the particular period in man's development, or was it a previously existing phase modified by new environment? For the present this question should be held in abeyance. To declare that the resemblance of this art to both Asiatic and Egyptian art is simply a proof of the psychical unity of man is assuming too much, and is cutting off all further consideration of the subject.

The active field and museum archæologist or ethnologist who knows and maintains the associations of specimens as found, and who arranges them in their geographical sequence, becomes intimately in touch with man's work under different phases of existence.

Fully realising that the natural working of the human mind under similar conditions will to a certain extent give uniform results, he has before him so many instances of the transmission of arts, symbolic expressions, customs, beliefs, myths and languages, that he is forced to consider the lines of contact and migration of peoples as well as their psychical resemblances.

It must be admitted that there are important considerations, both physical and mental, that seem to prove a close affinity between the brown type of Eastern Asia and the ancient Mexicans. Admitting this affinity, the question arises, Can there have been a migration eastward across the Pacific in neolithic times, or should we look for this brown type as originating in the Eurafric region and passing on to Asia from America? This latter theory cannot be considered as a baseless suggestion when the views of several distinguished anthropologists are given the consideration due to them. On the other hand, the theory of an early migration from Asia to America may also be applied to neolithic time.

However this may have been, what interests us more at this moment, and in this part of America, is the so-called "mound-builder" of the Ohio valley. Let us first clear away the mist which has so long prevented an understanding of this subject by discarding the term "mound-builder." Many peoples in America as well as on other continents have built mounds over their dead, or to mark important sites and great events. It is thus evident that a term so generally applied is of no value as a scientific designation. In North America the term has been applied even to refuse piles; the kitchen-middens or shell-heaps which are so numerous along our coasts and rivers have been classed as the work of the "mound-builder." Many of these shell-heaps are

of great antiquity, and we know that they are formed of the refuse gathered on the sites of the early peoples. From the time of these very early deposits to the present such refuse piles have been made, and many of the sites were reoccupied, sometimes even by a different people. These shell-heaps therefore cannot be regarded as the work of one people. The same may be said in regard to the mounds of earth and of stone so widely distributed over the country. Many of these are of great antiquity, while others were made within the historic period and even during the first half of the present century. Some mounds cover large collections of human bones, others are monuments over the graves of noted chiefs; others are in the form of effigies of animals and of man; and, in the south, mounds were in use in early historic times as the sites of ceremonial or other important buildings. Thus it will be seen that the earth-mounds, like the shell-mounds, were made by many peoples and at various times.

There are, however, many groups of earth-works which, although usually classed as mounds, are of an entirely different order of structure and must be considered by themselves. To this class belong the great embankments, often in the form of squares, octagons, ovals and circles, and the fortifications and singular structures on hills and plateaus which are in marked contrast to the ordinary conical mounds. Such are the Newark, Liberty, Highbank and Marietta groups of earth-works, the Turner group, the Clark or Hopewell group, and many others in Ohio and in the regions generally south and west of these great central settlements; also the Cahokia Mound opposite St Louis, the Serpent Mound of Adams County, the great embankments known as Fort Ancient, which you are to visit within a few days, the truly wonderful work of stone known as Fort Hill in Highland County, and the strange and puzzling walls of stone and cinder near Foster's station.

So far as these older earth-works have been carefully investigated they have proved to be of very considerable antiquity. This is shown by the formation of a foot or more of vegetable humus upon their steep sides; by the forest growth upon them, which is often of primeval character; and by the probability that many of these works, covering hundreds of acres, were planned and built upon the river terraces before the growth of the virgin forest.

If all mounds of shell, earth or stone, fortifications on hills, or places of religious and ceremonial rites, are classed, irrespective of their structure, contents, or time of formation, as the work of one people, and that people is designated "the American Indian" or the "American Race," and considered to be the only people ever inhabiting America, North and South, we are simply repeating what was done by Morton in relation to the crania of America—not giving fair consideration to differences while over-estimating resemblances. The effort to affirm that all the various peoples of America are of one race has this very year come up anew in the proposition to provide "a name which shall be brief and expressive," and at the same time shall fasten upon us the theory of unity—notwithstanding the facts show diversity—of race.

Let us now return to the builders of the older earth-works, and consider the possibility of their having been an offshoot of the ancient Mexicans. Of the crania from the most ancient earth-works we as yet know so little that we can only say that their affinities are with the Toltecan type; but of the character of the art, and particularly the symbolism expressing the religious thought of the people, we can find the meaning only by turning to ancient Mexico. What northern or eastern Indian ever made or can understand the meaning of such sculptures or such incised designs as have been found in several of the ancient ceremonial mounds connected with the great earth-works? What Indian tribe has ever made similar carved designs on human and other bones, or such singular figures, cut out of copper and mica, as were found in the Turner and Hopewell-groups? or such symbolic animal forms, elaborately carved in stone, and such perfect terra-cotta figures of men and women as were found on the sacrificial altars of the Turner group? What meaning can be given to the Cincinnati Tablet, or to the designs on copper plates and shell discs from some of the southern and western burial and ceremonial mounds? I think we shall search in vain for the meaning of these many objects in the north or east, or for much that resembles them in the burial-places of those regions. On the other hand, most of these become intelligible when we compare the designs and symbols with those of the ancient Mexican and Central American peoples. The Cincinnati Tablet, which has been under discussion for over half a century, can be interpreted and its dual serpent characters understood by comparing it with the great double image known in Mexico as the Goddess of Death and the God of War; the elaborately complicated designs on copper plates, on shell discs, on human bones, and on the wing bones of the eagle, can in many instances be interpreted by comparison with Mexican carvings and with Mexican modes of symbolic expression of sacred objects and religious ideas. The symbolic animals carved on bone or in stone, and the perfection of the terra-cotta figures, point to the same source for the origin of the art.

In connection with the art of the builders, let us consider the earth structures themselves. The great mound at Cahokia, with its several platforms, is only a reduction of its prototype at Chalula. The fortified hills have their counterparts in Mexico. The serpent effigy is the symbolic serpent of Mexico and Central America. The practice of cremation and the existence of altars for ceremonial sacrifices strongly suggest ancient Mexican rites. We must also recall that we have a connecting link in the ancient pueblos of our own south-west, and that there is some evidence that in our Southern States, in comparatively recent times, there were a few remnants of this old people. It seems to me, therefore, that we must regard the culture of the builders of the ancient earth-works as one and the same with that of ancient Mexico, although modified by environment.

Our northern and eastern tribes came in contact with this people when they pushed their way southward and westward, and many arts and customs were doubtless adopted by the invaders as shown by customs still lingering among some of our Indian tribes. It is this absorption and admixture of the peoples that has in the course of thousands of years brought all our American peoples into a certain conformity. This does not, however, prove a unity of race.

It is convenient to group the living tribes by their languages. The existence of more than a hundred and fifty different languages in America, however, does not prove a common origin, but rather a diversity of origin as well as a great antiquity of man in America.

That man was on the American continent in quaternary times, and possibly still earlier, seems to me as certain as that he was on the Old World during the same period. The Calaveras skull, that bone of contention, is not the only evidence of his early occupation of the Pacific coast. On the Atlantic side, the recent extensive explorations of the glacial and immediately following deposits at Trenton, are confirmatory of the occupation of the Delaware valley during the closing centuries of the glacial period, and possibly also of the inter-glacial time. The discoveries in Ohio, in Florida, and in various parts of Central and South America, all go to prove man's antiquity in America. Admitting the great antiquity of one or more of the early groups of man on the continent, and that he spread widely over it while in the palæolithic and early neolithic stages of culture, I cannot see any reason for doubting that there were also later accessions during neolithic times, and even when social institutions were well advanced. While these culture epochs mark certain phases in the development of a people, they cannot be considered as marking special periods of time. In America we certainly do not find that correlation with the Old World periods which we are so wont to take for granted.

We have now reached the epoch of careful and thorough exploration and of conscientious arrangement of collections in our scientific museums. It is no longer considered sacrilegious to exhibit skulls, skeletons and mummies in connection with the works of the same peoples. Museums devoted primarily to the education of the public in the æsthetic arts are clearing their cases of heterogeneous collections of ethnological and archæological objects. Museums of natural history are being arranged to show the history and distribution of animal and vegetable life and the structure of the earth itself. Anthropological museums should be similarly arranged, and, with certain gaps which every curator hopes to fill, they should show the life and history of man. To this end, the conscientious curator will avoid the expression of special theories, and will endeavour to present the true status of each tribe or group of man in the past and in the present, so far as the material at his command permits. A strictly geographical arrangement is therefore the primary principle which should govern the exhibition of anthropological collections. A special exhibit may be made in order to illustrate certain methods by which man in different regions has attained similar results, either by contact

or by natural means. Another exhibit may be for the purpose of showing the distribution of corresponding implements over different geographical areas. These and similar special exhibits are instructive, and under proper restrictions should be made; but unless the design of each exhibit is clearly explained, the average visitor to a museum will be confused and misled, for such objects so grouped convey a different impression than when exhibited with their associated objects in proper geographical sequence.

The anthropology of America is now being investigated, and the results are being made known through museums and publications as never before.

The thoroughly equipped Jesup North Pacific Expedition, with well-trained anthropologists in charge, was organised for the purpose of obtaining material, both ethnological and archæological, for a comparative study of the peoples of the northern parts of America and Asia. Although only in the third year of its active field work, it has already furnished most important results and provided a mass of invaluable authentic material.

The Hyde Expedition, planned for long-continued research in the archæology and ethnology of the south-west—a successor in regard to its objects to the important Hemenway Expedition—is annually adding chapters to the story of the peoples of the ancient pueblos.

The results of the extensive explorations by Moore of the mounds of the southern Atlantic coast are being published in a series of important monographs.

The Pepper-Hurst Expedition to the Florida Keys has given information of remarkable interest and importance from a rich archæological field before unknown.

The United States Government, through the Bureau of Ethnology of the Smithsonian Institution, has given official and liberal support to archæological and ethnological investigations in America.

The constantly increasing patronage, by wealthy men and women, of archæological research at home, as well as in foreign lands, is most encouraging.

The explorations in Mexico and in Central and South America, the publication in facsimile of the ancient Mexican and Maya codices, the reproduction by casts of the important American sculptures and hieroglyphic tablets, all have been made possible by earnest students and generous patrons of American research.

The numerous expeditions, explorations and publications of the Smithsonian Institution and of the museums of Washington, Chicago, Philadelphia, New York and Cambridge, are providing the student of to-day with a vast amount of authentic material for research in American and comparative anthropology.

The Archæological Institute of America, the American Folk Lore Society, and the archæological and anthropological societies and clubs, in active operation in various parts of the country, together with the several journals devoted to different branches of anthropology, give evidence of widespread interest.

Universities are establishing special courses in anthropology, and teachers and investigators are being trained. Officers of anthropological museums are preparing men to be field workers and museum assistants.

The public need no longer be deceived by accounts of giants and other wonderful discoveries. The wares of the mercenary collector are at a discount, since unauthentic material is considered worthless. Anthropology is now a well-established science. It is required of those who follow any of its branches to do so in seriousness and with scientific methods.

With all this wealth of materials and opportunities there can be no doubt that anthropologists will in time be able to solve that problem which for the past half-century has been discussed in this Association—the problem of the unity or diversity of prehistoric man in America.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

A LARGE amount of information referring to examining authorities and educational institutions in this country appears in the students' numbers issued by several of our contemporaries. The *British Medical Journal* of August 26 and the *Lancet* of September 1 are almost entirely devoted to descriptions of the various methods by which a student may become a fully qualified practitioner. The *Chemical News* of September 1 gives a list of British universities and the chief colleges, technical schools, and institutes. The *Chemist and Druggist* of September 2 contains particulars of the educational requirements for qualification in pharmacy, medicine, dentistry, and veterinary surgery.

SOME time ago a departmental committee was formed to report as to the buildings and site of a proposed new Royal College of Science for Ireland. It is now announced by the *Times* that the Government have arranged to acquire the whole of the house property and ground occupying the site recommended by the departmental committee in addition to the buildings mentioned in the report. The total area thus acquired amounts to over 50,000 superficial feet, and the new buildings will have a frontage both on Kildare Place and Upper Merrion Street, and will, as the report recommends, be in immediate connection with the Museum of Science and Art.

THE West Ham Municipal Technical Institute is one of the newest of the London Polytechnics, and the first session of full work will commence towards the end of the present month. The Institute has been built by the Council of the County Borough of West Ham at a cost of 45,000*l.*, and a further sum of 15,000*l.* has been spent upon the equipment and fittings. Under the direction of the principal, Mr. Albert E. Briscoe, an admirable programme of classes has been prepared; and a glance through it shows that provision has been made for theoretical and practical instruction in most branches of pure and applied science and art. Every effort appears to be made to encourage students to use wisely the educational facilities which the Institute affords. As an instance of the excellent policy which is being pursued, the following extract from the "Program" just published is noteworthy:—"Trade students are urged not to make the mistake of joining trade classes only. If any thorough knowledge of the principles of their trade is to be gained, they must possess an acquaintance with elementary science, and have some knowledge of arithmetic, mensuration, and elementary mathematics. For example, very little progress can be made in building or engineering drawing without some knowledge of elementary geometry; plumbing and engineering students will not obtain a clear grasp of their work unless they have some knowledge of elementary physics, of arithmetic, and elementary mathematics. They are further advised to pursue thoroughly the study of one or two subjects, and not waste their energies by attempting to cover the whole ground of science, and so obtain only a smattering of knowledge. The advantage of systematic study in science lies not so much in the number of facts learnt as in the training in habits of accuracy of work and thought, that enables men to attack new problems as they present themselves in a manner likely to ensure their successful solution." Much may be hoped from a Polytechnic in which such sound educational principles are impressed upon the students.

SCIENTIFIC SERIALS.

American Journal of Science, August.—Rotatory polarisation of light in media subjected to torsion, by A. W. Ewell. The difficulties encountered in the choice of a proper material for experiment are very great, as already pointed out by Verdet and Wertheim. The author found a satisfactory combination in jelly supported in rubber tubes, and the observations with jelly, corroborated by a few observations with glass, demonstrate that torsion of a cylinder produces the rotatory polarisation of a ray proceeding in a direction parallel to the axis of the cylinder, the rotation of the plane of polarisation being opposite to the twist, and a function of the twist of degree higher than the first.—Studies in the Cyperaceæ xi., by T. Holm. This article deals with the abnormal development of some specimens of *Carex stipata*, Muhl., caused by *Livia vernalis*, Fitch.—The constitution of tourmaline, by F. W. Clarke. The author discusses the respective merits of Penfield and Foote's formula for tourmalines, regarded as salts of the alumino-borosilicic acid, $H_{11}Al_3B_2Si_4O_{21}$, and his own derivation from the similar acid, $H_{14}Al_5B_3Si_6O_{31}$, with all of the hydrogen atoms replaceable by bases. He retains the general form of his own formula, but suggests that certain irreducible differences of constitution may be due to the fact that there exists a series of borosilicic acids.—Determination of tellurous acid in the presence of haloid salts, by F. A. Gooch and C. A. Peters. In the estimation of tellurous acid by oxidation with excess of potassium permanganate, no correction

is necessary when the tellurous oxide is dissolved originally in an alkaline hydroxide and the solution made acid only to a limited degree with sulphuric acid.—An iodometric method for the estimation of boric acid, by L. C. Jones. The method is based upon the employment of mannite and of a mixture of potassium iodide and iodate.—A method for the detection and separation of dextro- and lævo-rotatory crystals, by D. Albert Kreider. The method is based upon the use of a kind of polarimeter with a wide field of vision of a uniform colour, adjusted in such a manner that any small crystal instantly reveals the sense of its rotation by its colour when brought into the field.—New meteoric iron found near the Tombigbee River, Choctaw and Sumter Counties, Alabama, by W. M. Foote. The fall consisted of a series of pieces found in almost a straight line north and south along a nine-mile stretch of the public road.—Orthoclase crystals from Shinano, Japan, by C. Iwasaki. Describes four different classes of orthoclase, mostly twinned after the Baveno type.

Wiedemann's Annalen der Physik und Chemie, No. 7.—Smallest thickness of liquid films, by K. T. Fischer. The author brings the widely varying results of various observers into approximate harmony by supposing that when a drop of oil is placed on a surface of pure water, a "precursory film" spreads over the water first, and is followed at a slower rate by the film studied by Sohncke and others. For his own experiments the author used a pure mercury surface, which he found to possess various advantages.—Wehnelt's electrolytic interrupter, by A. Voller and B. Walter. The hydrogen lines are very pronounced in the spectrum of the Wehnelt spark. Intense and pure metallic spectra may be obtained by choosing the metal in question as the substance of the anode. The hydrogen lines do not interfere, but serve as lines of reference.—Kathode rays, by A. Wehnelt. The author distinguishes between two classes of kathode ray shadows. One class is produced by rays emerging from the kathode normal to its surface and crossing each other at a focus in the case of a curved kathode. The other class consists always of upright shadows, thrown by rays travelling in a direction parallel to the axis of the tube.—Cause of the change in the conductivity of a metallic powder, by T. Sundorph. The author proves the formation of connecting chains of particles in a coherer by sparking across the gap between two metallic blocks on a glass plate, with a layer of nickel or iron filings between them.—A new vacuum discharge phenomenon, by L. Fomm. A vacuum tube is surrounded by wire rings at its two ends. At a certain exhaustion blue rings appear, concentric with the wire rings, and enclosing patches of positive light, which gradually disappear until the blue light fills the whole cross section. As the pressure diminishes still further the blue light detaches itself from the glass walls, and expands longitudinally, forming a greenish-blue beam which proceeds in the direction of the other ring, and shows all the properties of kathode rays.—Some experiments with Wehnelt's interrupter, by E. Lecher. The author describes some beautiful phenomena obtained by bringing the secondary spark discharge of a Wehnelt interrupter into a magnetic field. The secondary current is unidirectional, and is therefore deflected in a constant direction. When the discharge takes place between a circle of wire and a disc mounted in its plane, and concentric with it, the discharge consists of curved spokes of light which rotate rapidly, in a sense governed by the polarity of the magnet. The discharges form a kind of fire-wheel as displayed in pyrotechnics.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 28.—M. Michel Lévy in the chair.—On the general form of the equations of dynamics, by M. P. Appell. The advantage of the form of equation described is that it allows the use of parameters which are not true coordinates, but are connected with the coordinates by non-integrable differential relations. The method is of especial service in problems of rolling.—On the velocity of detonation of acetylene, by MM. Berthelot and Le Chatelier. Acetylene was detonated in a glass tube by caps of mercury fulminate or chlorate powder, care being taken to have the detonator as small as possible, as it was found that irregularities were introduced if the detonator used was too violent. The acetylene was contained in horizontal glass tubes, under pressures varying from 5 to 36 atmospheres, and the velocity was registered photographically upon a falling sensitised plate, the light emitted by the explosion itself being sufficient for the purpose. As the flame moved further from the source of explosion the velocity continually increased, the tube generally breaking before the speed became uniform. Even when the conditions of inflammation and pressure are apparently precisely identical in two succeeding experiments, discordant results were sometimes obtained for the increase of velocity of the flame along the tube. The results show that the propagation of the detonation of acetylene is produced with a velocity which increases with the pressure from 1000 metres per second at 5 atmospheres pressure to 1600 metres per second at 30 atmospheres.—M. Henri Moissan communicated to the Academy a letter he had received from Prof. Dewar, of London, in which details were given of the solidification of hydrogen. Solid hydrogen melts at 16° absolute (−257°), and at this temperature helium is liquefied under a pressure of 8 atmospheres.—Discovery of a new planet at the Observatory of Paris, by M. Jean Mascart.—The Perseids in 1899, by M. C. Flammarion. The tabulated list is accompanied by a chart showing the point of origin and direction of motion of the Perseids in 1899.—On beats given by vibrating strings, by M. C. Maltézos. The ordinary equation for a thin elastic string giving the relation between the number of vibrations, tension, section, and length of string gives no explanation of the phenomenon of beats. If the rigidity of the string be taken into account, the author theoretically deduces an expression for the number of vibrations from which can be deduced that the number of beats is proportional to the square root of the area of section, inversely proportional to the cube of the length, and inversely proportional to the square root of the tension. The last conclusion has been verified experimentally, but instead of the number of beats being inversely proportional to the cube of the length of the strength, it is nearly inversely as the length simply. Hence it is impossible from rigidity alone to explain all the phenomena of beats in vibrating strings.

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