

THURSDAY, SEPTEMBER 28, 1876

THE INTRA-MERCURIAL PLANET OR PLANETS

THE question of the existence of one or more planetary bodies revolving within the orbit of Mercury is again revived by Weber's observation of a round black spot just within the sun's eastern limb, on the afternoon of April 4 in the present year which had not been visible on the same morning, and early on the following day had disappeared. The position at $2\frac{3}{4}$ ' only from this limb is one, where an ordinary spot would not be expected to exhibit a circular outline; and a round black disk, in such a position more especially, must instantly attract the attention of a practised observer. On April 4 clouds unfortunately prevented lengthened observation, and in Weber's notice there is no reference to any perceptible motion during the short time the spot could be watched.

This observation resembles others already upon record, made by persons equally worthy of credit, which it is hardly possible to explain except on the hypothesis that one or more planetary bodies exist with mean distance less than Mercury, the rate of motion where motion has been detected by the most reliable observers, not being consistent with greater distance from the sun. While it is certain that comets with perihelia within the earth's orbit have transited the solar disk, and notwithstanding such transits may have been more frequent than is generally supposed, the appearance of the spots now in question seems, at least in several of the best authenticated cases, to negative any idea of their being due to the passage of comets across the sun, near their nodes. At the same time there are several instances where the form of the spots would perhaps accord better with the assumption of a cometary transit, unless we can admit that the deviation from circular contour is attributable to an optical cause.

It may be remembered that the attention of astronomers was first seriously directed to the possible existence of a planet or planets interior to the orbit of Mercury, by M. Leverrier's announcement that the motion of the perihelion of this planet was not explained by known causes of perturbation, but that an excess of 38 seconds in the century must be admitted beyond the value derived from theory, to produce an agreement between calculation and observation in the discussion of the long series of observed transits across the sun's disk. The unexplained motion of the line of apsides might, as M. Leverrier remarked, be due to the existence of a single interior planet of a mass which would depend upon its mean distance. With a distance of 0.17 (period of revolution, 25.6 days) the mass would be precisely equal to that of Mercury, and it would vary inversely with the distance. Or it might be due to a group of small planets circulating within the orbit of Mercury.

Having before us the whole of the recorded observations of the presence of suspicious spots upon the sun's disk, we shall soon discover that they hardly admit of explanation on the hypothesis of a single planet, even if we assume a small inclination of the orbit of this planet to the ecliptic, a condition which, while it would greatly extend the transit-limits, must at the same time render

the transits so frequent that it is in a high degree improbable the planet could have so long escaped certain detection. Some few of the observations, as just remarked, we may perhaps refer to comets in transit; it remains to endeavour to ascertain from observations not thus explained what period or periods will best represent them, with the view to being warned of the probable times of future transits.

This subject has engaged the attention of M. Leverrier during the last few weeks, or since he became cognisant of Weber's observation last April, the notification of which was long delayed. It appears that the observations of Stark and Steinheibel, 1820, February 12, Lescarbault, 1859, March 26, and that of Weber, may refer to the same planetary body if the revolution be supposed 280.077 days; this being the sidereal revolution with respect to the node, the synodical period would be 30.33 days; the corresponding mean distance from the sun is 0.18, and the maximum elongation $10\frac{1}{2}$ degrees. Such a planet would again be in conjunction with the sun on October 2nd or 3rd of the present year; and if Lescarbault's observation affords any approximation to the position of the line of nodes would pass across the sun's disk, and for this reason M. Leverrier has directed attention to the importance of a close watch upon the same, during these days, such watch, if possible, to extend to distant meridians, so as to insure pretty continuous observation through the forty-eight hours, Paris time. He has already advised American observatories through Prof. Henry, Secretary of the Smithsonian Institution, and it is to be hoped the chance of making an important discovery at this time, may be made known to observers in the East. It will be seen that the aid of the telegraph is indispensable, in order to secure complete evidence of the existence or non-existence of the hypothetical planet this autumn.

Other observations may be reconciled with a period of similar length, but the planet to which they may be supposed to refer cannot be identical with the above. Thus if Mr. Lummis's sketch of the path of the small round black spot, which he remarked upon the sun at Manchester on the morning of March 20, 1862, is reliable in the hurried and otherwise disadvantageous circumstances under which it was made, the ascending node was almost diametrically opposite to that of Lescarbault's planet, elements which have been attributed to MM. Valz and Rádau, and exhibiting a near agreement in the position of the line of nodes, being certainly erroneous. Again, one of the most interesting observations bearing upon the existence of an intra-mercurial planet is that made about the end of June or beginning of July 1847 in this country, which can hardly be supposed to refer to either of the objects seen by Lescarbault and Lummis respectively. The exact date of this observation is unfortunately lost beyond recovery.

Mr. B. Scott, the City Chamberlain, observing the sun's disk near London, a short time before sunset late in June or on one of the first days in July, remarked upon it a perfectly circular black disk, and was so confident of the unusual character of the spot that he was on the point of making known his observation through one of the London daily journals on the evening of the same day, when unfortunately an astronomical friend, under the impression that an ordinary spot had been observed, dissuaded Mr. Scott

from so doing. It thus happened that the matter dropped until the announcement in 1860 of Lescarbault's observation on March 26 in the preceding year, when Mr. Scott, in a communication addressed to the *Times*, drew attention to his experience in the summer of 1847. It was then discovered that he had not been the only observer of the strange object. Mr. Wray, the well-known optician, then resident at Whitby, had remarked a small circular black spot upon the sun late one afternoon at the end of June or early in July, though he also had, in 1860, lost the exact date. Both these gentlemen have furnished the writer with every other particular of their observations. That they refer to the same object can hardly be doubted. Mr. Wray had it under observation for forty minutes, when the sun sank into a bank of cloud and was not again visible that day. In this interval the spot appeared to have moved about five minutes of arc, and when last perceived was so near the western limb of the sun that Mr. Wray believes if the cloud had not interfered, in about ten minutes he would have witnessed the egress. This circular spot, the diameter of which he judged to be about six seconds of arc, was not visible early on the following morning, though other spots of ordinary form which were present on the disk remained nearly unchanged. Mr. Scott was observing with a refractor of about $4\frac{1}{2}$ inches aperture, Mr. Wray with a fine 6-foot Newtonian reflector of equal aperture, which he was employing at the time in a study of the varying aspect of the solar spots. Notwithstanding the unfortunate loss of the date of these observations, such particulars as are available are still of value as certifying the existence of such objects in transit; there is no observation of the kind resting upon more excellent authority.

A letter from Prof. Heis, of Münster, the author of the "Atlas Cælestis Nova," received while closing these remarks, gives full details respecting Weber's observation. The spot was intensely black, perfectly round, and smaller than the planet Mercury in transit. Prof. Heis expresses the utmost confidence in this observation by his friend, who has long been accustomed to examine the solar disk.

J. R. HIND

UNIVERSITY COLLEGE, BRISTOL

WE have been able to keep our readers informed of the various steps which have been taken to bring to fruition the movement which was commenced about three years ago to establish in Bristol an institution for University education. This movement, we are glad to say, has been so far successful that a beginning is to be made on Tuesday week, October 10; on that day commences the first term of the first session of what will be henceforth known as University College, Bristol. From the first it was sought to make the proposed institution something more than a mere "technical" college. All along it has been kept in view that the only really liberal training is one in which all the faculties of man are drawn out harmoniously and equally, in which a broad basis for future special work is laid, by education in all the great departments of human knowledge. The Bristol institution is not to be a mere special college, it is to be a University. Prof. Jowett, at the meeting held in June, 1874, struck the right note when he said: "The distinction he would draw between liberal education and merely

technical education was this—the one comprehended the other; it was the other, with something added to it, carried on in a higher spirit; it was the one pursued not merely for the sake of getting on in the profession, or making a man an engineer, or a miner, or a doctor, but for the sake of the improvement of the mind. No man will be a first-rate physician or engineer who is not something more than either." The first programme of the classes of this new college is certainly a modest one so far as extent is concerned, but it comprehends all the elements of a liberal education—literature, science, and art. In science there will be instruction in chemistry, physics, zoology, botany, geology, mathematics and applied mechanics, and political economy; in literature, [classes for] modern history and literature; and in art (for evening classes at least) geometrical and mechanical drawing. In all these branches professors or lecturers have already been appointed, but the programme contains other subjects—classical languages and literature, modern languages, and textile industries—to which no appointments have yet been made, but which will no doubt be filled up as soon as circumstances permit. Affiliated to the Bristol College, moreover, is the old-established Bristol Medical School, for which new buildings will be erected, and on which, we believe, the new institution will have a stimulating and liberalising effect. The principal work of the college will of course be carried on during the day by means of lectures and laboratory work, but we are also glad to see that the directors have resolved to follow from the first the excellent example of Owens College, Manchester, by establishing evening classes for those who are unable to take advantage of the day classes.

Altogether the originators of this movement and the Council of the College are to be congratulated on the fair start they have made, and if they continue as they have begun, we cannot doubt that in no long time University College, Bristol, will become as great and as firmly-established a centre of culture as the Owens College, Manchester. But in the meantime the great want of the new institution is money. Owens College, Manchester, has received many liberal donations since John Owens left his 100,000*l.* for the endowment of professorships, and by these gifts it has been enabled to develop wonderfully. But even Owens College feels itself hampered from want of sufficient funds, and now with justice advances its claims to government endowment. The originators of the movement which has just reached a successful culmination in Bristol calculated that they could not make a beginning without a capital sum of 25,000*l.*, and an annual subscription of 3,000*l.* for five years. They have received many liberal donations and subscriptions, and have obtained so nearly all that they thought was required, that they have felt authorised in making a beginning. From the first Balliol and New Colleges promised 300*l.* a year each for five years. A good many donors, individuals, firms, and companies have given 1,000*l.* each, and many subscriptions of smaller sums have poured in. The Clothworkers Company have offered a handsome subscription, on condition that means are taken to promote technical education in the West of England clothmaking districts, and as we have said, "Textile Industries" is put down as one of the lectureships to be filled up. We hope, however,

that the Worshipful Company will not put too narrow a construction on the conditions of their subscription, but that they will have the shrewdness to see that the best possible preparation for a special knowledge of textile industries is a thorough grounding in the sciences on which these are founded.

Still, notwithstanding all that has been done and promised, the Bristol College must stand still, and therefore fail of its purpose, unless subscriptions and endowments continue to pour in handsomely until it be enabled to offer an education not inferior to that offered by Owens College, Manchester, or the Universities of Edinburgh and Glasgow. We feel confident, however, that once the institution is fairly started and at work, and has had opportunity of showing the vast benefits it is able to confer on the large industrial population with which it is surrounded, and thus indirectly on the general culture and material welfare of England, that some at least of the many rich and liberal-minded men in the country, who only wait for a worthy object on which to exercise their generosity, will see that here is one that deserves and requires their help, by giving which liberally they will not only benefit their country but do lasting honour to themselves. No similar institution that has been started on a liberal and disinterested basis and has been properly brought before the public has yet proved a failure; we need only refer again to Owens College, to the Newcastle College, and to the more recent Yorkshire College of Science, which, however, has much to do before it gets beyond the stage of a merely technical school. Soon also will we have an institution in Birmingham, the Josiah Mason College, so liberally endowed by its still living founder. These institutions have all sprung up to supply what was felt as a great want; and no district in the country has more need of a centre of liberal culture than the south-west of England, the seat of so many and so varied industries. We venture to think that all that has been yet obtained is nothing to what the extensive district, containing so many rich landed proprietors, manufacturers, and merchants, can afford. Now that they see the institution actually at work in their midst, and perceive how impossible it is for it to do efficient work on its present basis, we cannot doubt that they will extend their liberality, and, aided by others throughout the country who are able and always ready to help in a noble and deserving cause, establish University College, Bristol, on as solid a pecuniary foundation as any similar institution in the country.

We need not insist here again, as we have often done already, on the fact that we are in danger of losing our lead among the nations so far as industry is concerned, from the inefficient training of those who have the conduct of our commerce and manufactures in their hands. It is a fact which is being ever and anon reiterated on the platform and by the general press. Along with a sound and comprehensive system of elementary education, it is only by establishing all over the country, in the great centres of industry, institutions where a comprehensive education can be obtained as the only satisfactory basis on which a special training can be built, that we shall be able to hold our own on the Continent and with America. We have five such centres in England either established or about to be, some of them, however, greatly deficient

in comprehensiveness. Bristol, we have no doubt, will one day become one of the most efficient in the country. Everything has gone smoothly hitherto. Even the Clifton Association for the Higher Education of Women intend to have no courses of lectures this winter, to see how far women in and around Bristol will avail themselves of the College; for the lectures will be given to both sexes at once, though the class instruction will be separate. We only hope that other important centres will follow the examples already set, and that ere many years no man in England will have to go without a liberal education because it is not within his reach. If Scotland with her four millions of people finds it difficult to meet her wants in this direction with four universities, how much has yet to be done in England with her twenty-four millions ere she is on the footing of even her poor relation of the north.

FIELD GEOLOGY

Field Geology. By W. Henry Penning, F.G.S., Geologist, H.M. Geological Survey of England and Wales. With a Section on Palæontology, by A. J. Jukes-Browne, B.A. F.G.S., H.M. Geological Survey. (London: Baillière, Tyndall, and Cox, 1876.)

IN the modestly-written preface to this little volume, the author naturally refers to the difficulty which he experienced in determining what subjects ought properly to be treated under the title of "Field Geology." It would have been defensible to have included in such a work as the present useful, if somewhat desultory, suggestions upon almost every branch of geological inquiry, and thus to have expanded the convenient manual into a ponderous treatise; we believe, however, that the author has exercised a very wise discretion in restricting the work within its present limits, and making it of as practical a character as possible; for everything calculated to increase the size, weight, and price of the book must, perforce, have tended to prevent it from occupying that place for which it is primarily designed—the portmanteau of the working geologist.

The Geological Survey of the British Islands, the foundation of which was laid by the labours of De la Beche and Logan nearly half a century ago, and which is now approaching completion, differs in some important respects from most of the official geological surveys of European and American states. While the latter usually aim at little if anything more than defining the boundaries of the areas occupied by each of the geological formations, the former sets before itself a much more lofty ideal—no less in fact than such a delineation of all the lines of outcrop of the strata, with indications of their flexures and dislocations, as will enable any competent person using the maps and sections to realise the actual geological configuration of the rock-masses to a considerable depth below the surface.

Of course the execution of such a design as this must necessarily be very unequal. Not to mention differences of individual ability and scientific culture in the members of the staff of the survey—differences, the consequences of which not even the most perfect organisation or rigid supervision can altogether neutralise—we must remember that the data on which the geological surveyor has to rely in drawing his lines in different areas, are so varied as greatly to affect the value of the results attained. In one sheet of the Geological Survey map, which happens to

represent a district free from superficial accumulations, and in which numerous alternations of hard and soft beds afford the greatest facilities in detecting every deviation of the strata from their normal position, the minute structure of the country will be found delineated in the most exquisite detail; while in an adjoining sheet broad spreads of colour separated by dotted lines constitute a confession that the surveyor was here engaged in an almost hopeless task. It may, indeed, be questioned whether on a map of so large a scale as that of the Geological Survey, any useful purpose is answered by attempts to define the boundaries of formations buried under several hundreds of feet of boulder-clay or gravel. Another serious obstacle to the perfecting of our geological maps is found in the circumstance that rocks of identical mineralogical composition, but of very different geological age, are found occasionally in direct apposition; and in such cases (except in the rare instances of numerous sections affording fossils characteristic of either formation presenting themselves) the field-geologist finds himself hopelessly at fault. For example, in those parts of England in which the limestones and grits of the Coralline Oolite are found intervening between the Oxford and Kimmeridge clays the work of the surveyor is an easy one; but where, as is frequently the case, the first-mentioned formation is absent and the one series of argillaceous strata lies directly upon the other, the result attained by him is necessarily of the most vague and uncertain character. On the other hand, many of the hard and well marked rock-masses, which the geological surveyor naturally seizes upon in drawing his lines of boundary, are too frequently, alas, shown by the palæontologist to be altogether destitute of any important significance.

In spite, however, of these unavoidable inequalities and imperfections in its execution, the map of the Geological Survey is a splendid work, and one of which the country may justly be proud; it has already largely prevented the wasteful expenditure of the resources of the empire in futile undertakings, while it has brought to light many unsuspected sources of mineral wealth; and it is hard to say whether in the future the aid which it will render to those engaged in scientific research will not outrival that which it now affords to industrial enterprise.

The methods pursued by the Geological Survey of this country, in seeking to realise that ideal to which we have adverted, have been gradually *developed* in the hands of the numerous able observers and sagacious thinkers, who have since its foundation been members of its staff. Hitherto, however, these methods have been handed down by tradition only, and no work has existed to which an outsider or foreigner could refer for an exposition and illustration of them. Hence we gladly hail the appearance of the present work, as satisfying a want which has long been felt and frequently expressed.

In the execution of his task we consider that the author has been on the whole very successful, especially when we remember that the experiment is the first of its kind. His explanations are strikingly clear, simple, and full; indeed, we may perhaps suggest that some of the minute details into which he enters are unnecessary for the class of persons to whom alone the book is likely to be of service—those, namely, who have mastered the elementary principles of geological science. For ex-

ample, we think that the author might fairly have given his readers credit for sufficient knowledge of plane trigonometry to have enabled them to make use of a very simple formula; and he should therefore, it seems to us, have substituted such a formula, with a table of tangents, for the rule-of-thumb and not very accurate methods for calculating true from apparent dips, given in pp. 42-46. His very minute directions, too, concerning the method of running levels for the purpose of preparing geological sections are, we think, a little out of place here, as they differ in no respect from those in ordinary use among engineers and surveyors, and may be found described in any treatise on land-surveying. On the other hand, his suggestions as to the use of two aneroids, one to be examined every half hour at a fixed station, though correct enough in theory, with other less exact methods applied to running lines of level, are certainly likely to be of little actual value to the geologist; while there is an omission of any reference to the really practicable applications of a single aneroid, when used with Airy's tables, either for calculating approximately the difference of level between two points (*e.g.*, the height of a bed of gravel above the level of the present stream), or in supplementing the data found on a contoured map; neither does our author refer to the use of Abney's level and several other simple contrivances which will be found very useful for the same purpose.

The sections on "Lithology" and "Palæontology" are treated with less diffuseness than those on the preparation of geological maps and sections. In a work of reference like the present we cannot but regard the reduction of the information to a tabular form, wherever this is practicable, as a great convenience; and we commend the adoption of the method in this part of the work. Mr. Jukes-Browne's remarks on the collection, preservation, and determination of fossils are, if not exhaustive, at least very useful and practical; but we can only consider the index of characteristic fossils, as unnecessarily increasing the bulk of the book, for no geologist who is able to determine the species of a fossil is likely to be at any loss as to the geological horizon to which it belongs.

Geological surveying is an art which for its successful performance requires some natural aptitude, a considerable knowledge of the principles and results of geological science, careful training, and much practice. The perusal of Mr. Penning's valuable hand-book will not make a man a geological surveyor, but it may enable him to appreciate some of the methods employed in the work—at least under its simplest conditions—as carried out by our national survey. And he who has mastered these first principles as here set forth will be the better prepared to encounter the more difficult problems which are presented by areas of more complicated geological structure and provided with less perfect topographical maps than our own.

J. W. J.

THE BATS OF ASIA

Monograph of the Asiatic Chiroptera. By G. E. Dobson, M.A., M.B. (Printed by order of the Trustees of the Indian Museum, 1876.)

BIOLOGISTS have, during the last few years, learnt with interest many of the valuable facts brought forward by Mr. Dobson, of Netley, with reference to the

anatomy and classification of the bats of Asia. The author tells us in the work under notice, which is the summary of the results of his investigation, that he was led to the special study of the Chiroptera from a desire to write a descriptive catalogue of the species of bats preserved in the Indian Museum at Calcutta. Finding, however, that but few species were not therein contained, the author, much to the advantage of his fellow-zoologists, determined to incorporate an account of all the Asiatic forms, the result being that he has presented us with a complete Monograph of the Asiatic Chiroptera.

Further, there being but four species of bats found in Europe which are not also Asiatic, these are also described in footnotes, which still further increases the value of the volume, making it, in fact, a monograph of the Asiatic and European Chiroptera.

There are a hundred woodcuts, mostly original illustrating the configuration of the head and nasal appendages of the most characteristic of the 122 species described; and the work in its letterpress and size corresponds with the valuable catalogues of the zoological collections in the British Museum.

Mr. Dobson divides the order primarily into the Megachiroptera and Microchiroptera, these sub-orders corresponding to the Frugivorous and Insectivorous Bats as usually described. The former of them are arranged in two groups—the Pteropi, with the tongue short and the molar teeth well developed; and the Macroglossi, with lengthy tongues and molars scarcely elevated above the gums.

With reference to the Microchiroptera two branches are assumed to have diverged from the ancestral forms (Palæochiroptera) of the order; one of these, the Vespertilionine Alliance, includes the Vespertilionidæ, Nycteriidæ, and Rhinolophidæ; the other, the Emballonurine Alliance, the Emballonuridæ and Phyllostomidæ. This important division is shown to be based upon several well-marked anatomical characters, the members of the Vespertilionine Alliance having the tail always contained within the interfemoral membrane, which it never perforates; the first phalanx of the middle finger extended, during repose, in a line with the metacarpal bone; the premaxillary bones rudimentary, and consequently the incisors small; and the hair scales imbricated, the tips of the scales being arranged in an oblique line, not terminating in acute projections. In the members of the Emballonurine Alliance, on the other hand, the tail, if present, generally perforates the interfemoral membrane; the first phalanx of the middle finger is more or less completely folded forwards, during repose, upon the superior or inferior surface of the metacarpal bone; the premaxillæ with the incisor teeth are large; and the hair-scales are arranged in a transverse series, the tips of the scales nearly always terminating in acute projections.

The character of the hair-scales is one which Mr. Dobson has investigated with special care, and he has submitted his specimens—from more than forty genera—to the inspection of Dr. J. D. Macdonald, who has confirmed his generalisation, except with reference to *Miniopterus* and *Mystacina*, the one otherwise recognisable as an intermediate form, and the other quite peculiar as far as its hair is concerned.

Although the Fruit-bats are included in a separate sub-

order, in other words, though they are assumed to have developed “from a group of Palæochiroptera distinct from that from which the Vespertilionine and Emballonurine alliances have sprung,” nevertheless, Mr. Dobson considers that they have affinities with that section of the latter group from which the Emballonuridæ are derived. This we cannot quite understand. May not the retention of a second index phalanx in *Rhinopoma*, and of well-developed incisors in the Phyllostomidæ be but a want of divergence from the Palæochiropterous type in the branch on which they are placed? a similar absence of modification in the independently-developed Pteropinæ being followed by a similar result as far as structure is concerned. This would, however, have no effect upon the independence of the pedigree-lines of the two groups, and would not make them blend in any parts of their course.

Mr. Dobson lays stress, in his definition of the sub-family Phyllorhininæ on the union of the ilio-pectineal spine with the antero-inferior surface of the ilium, forming a large preacetabular foramen. This unique arrangement, discovered by Mr. Dobson himself, is one which has scarcely attracted the attention of osteologists to the extent which it deserves.

The descriptions of the species are detailed and extremely precise; the synonymy is full, at the same time that the tables of measurements as well as those of specific distinctions will be found invaluable. The work, as a whole, is one of the most important recent additions to zoological literature.

LETTERS TO THE EDITOR

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

Sun-Spots suspected to be Identical with an Inter-Mercurial Planet

- 1875, Feb. 22^d 0^h.—One very minute spot was seen near the first limb. Not seen afterwards (on Feb. 23^d 0^h), others seen.
- 1875, Aug. 30^d 1^h.—A circular spot of intense blackness was seen near the second limb.
- 1875, Aug. 30^d 23^h.—Not seen, perhaps from clouds; other spots seen.
- 1876, March 7^d 0^h 30^m.—After a careful search only one very small spot was seen. This was without penumbra, but surrounded by bright faculæ (the drawing represents it as circular).

Transits of the Spot and the Limbs of the Sun.

	h.	m.	s.
☉ 1 2	23	29	47.7
Spot	29	59	0
☉ 2 2	31	57.8	Cloudy afterwards.

Not visible March 9^d 0^h 30^m, though another spot appeared in quite another part of the sun.

Observations of the spots on the disk of the sun are made regularly every day (excepting Sundays) when the sky is clear by Mr. F. Bellamy, and the above are notes which were made by him at the times of observation. ROBERT MAIN
Radcliffe Observatory, Oxford

Erratum in Mr. Wallace's Address

PLEASE allow me to point out an error in my address as given in your issue of September 7 (vol. xiv. p. 407). Instead of “*Pelargonium* of Kerguelen's Land” read “*Pelargonium* of Tristan d'Acunha.” This oversight was pointed out to me by Dr. Hooker in time to be corrected in the “Address” as published by the Association. ALFRED R. WALLACE

THERE is a curious error in Mr. Wallace's address which seems to deserve notice, as otherwise it will be often quoted without suspicion. He remarks (*NATURE*, vol. xiv. p. 407):—

"It must also be remembered, as Mr. Moseley has suggested to me, that a flower which had acquired a brilliant colour to attract insects might, on transference to another country, and becoming so modified as to be capable of self-fertilisation, retain the coloured petals for an indefinite period. Such is probably the explanation of the *Pelargonium* of Kerguelen's Land, which forms masses of bright colour near the shore during the flowering season, while most of the other plants of the island have colourless flowers in accordance with the almost total absence of winged insects."

Now the difficulty is that there is no such *Pelargonium*¹ in Kerguelen's Land, though it is true that the insects are apterous. The flora of Kerguelen's Land is enumerated in the *Journal* of the Linnean Society, xiv. pp. 389, 390. Of such a persistence as that alluded to by Mr. Wallace there is an instance in the case of *Pringlea*, of which Mr. Eaton detected some specimens with petals, though the coloration was, I believe very faint.

W. THISELTON DYER

Zittel's Palæontology

IN the review of Prof. Zittel's "Handbook of Palæontology," which appeared in *NATURE*, vol. xiv. p. 445, it should have been stated, in connection with the occurrence of Radiolaria in pre-Tertiary beds, that Mr. W. J. Sollas, of Cambridge, met with specimens in coprolites from the Upper Greensand, some three or four years ago, *vide Geol. Mag.*, 1873, vol. x. p. 272. Prof. Zittel duly records this fact in his paper on "Radiolaria from the North German Chalk," and mention of it was accidentally omitted in the concluding paragraphs of the review.

Newcastle-on-Tyne, September 25 HENRY B. BRADY

Visual Phenomena

IT is evident A. Mallock (vol. xiv. p. 351) and H. Airy (p. 392) describe two different, though allied, phenomena; the latter describes the appearance of rays of light, which, after entering the eye, meet at a focus, and diverge before reaching the retina; and the former, that of rays which reach the retina before meeting at a focus. For convenience, I shall call these respectively the "over-refracted radiance" and the "under-refracted." They differ in the following particulars in the case of my own eyes:—

<i>The Over-Reflected Radiance</i>	<i>The Under-Reflected Radiance</i>
(1) is diminished	is increased
by concave spectacles.	
(2) is increased	is diminished
by convex spectacles.	
(3) any given part may be cut off by advancing an opaque body in front of the pupil from the same side as the given part of the radiance.	opposite side to the radiance.
(4) is green outside (or blue, if sunlight is used).	is red outside.
(5) consists chiefly of a more or less perfect ring surrounding indefinite rays. When the radiance is very large the rays disappear in the general brightness.	consists chiefly (when small, entirely) of well-defined rays, mostly forked.
(6) is not materially increased by increasing the size of the pupil; unless the radiance is very large, and even then it is increased much less than in proportion to the pupil.	is increased proportionately, or more than proportionately, to the size of the pupil.

The first four of these must evidently, from the theory of the mode of production of the two kinds of radiance, be constant for all eyes; but not knowing the cause of the last two, I cannot say whether they are so, or whether they are peculiar to some eyes. From H. Airy's notes, 4 and 6, it would appear that the latter is the case; for he is evidently short-sighted.

Though contracting my pupil to its smallest size has little effect

¹ There is a *Pelargonium* in Tristan d'Acunha (see Moseley, in *Yourn. Linn. Soc.* xiv. p. 383.)

in reducing the over-refracted radiance, yet by placing still smaller diaphragms in front, I can reduce it almost to a point.

With my naked eye I see both kinds of radiance; No. 3 of the above differences supplying an easy means of separating them, viz., by covering half the pupil. All then that lies on the same side as the exposed part of the pupil, belongs to the over-refracted radiance; and that on the other side to the under-refracted. But as I am rather short-sighted, the over-refracted radiance (which makes a nearly octagonal corona) greatly preponderates; so that I was not previously aware that the other existed with my naked eye. The application of the weakest spectacles (convex or concave) completely abolishes one or the other.

A. Mallock is hardly correct in calling his "phenomenon A" a limiting form; he is probably what is often called "long-sighted"—I do not know whether there is any proper name for this peculiarity of vision—for the limiting form of both kinds of radiance is a point. This is what I. W. Ward sees (see p. 423), his eyes being neither too refractive nor too little refractive, but just right (he uses the word "long-sighted" in a different sense from what I have). As most people are either long or short-sighted, they see one or the other kind of radiance with the naked eye; but it also appears from my own case that a person may see both kinds together, and such cannot see a point of light free from radiance, no matter what spectacles he uses, unless he uses a diaphragm. I should be interested in hearing whether, when I. W. Ward looks through spectacles, the rays appear.

The brightness of the point scarcely affects the size of either kind of radiance; but a red glass between the eye and it cuts off the outer edge of the over-refracted radiance.

Unlike H. Airy, I have failed to discern any relation between the positions of the beams of the over-refracted and under-refracted radiances, except that I suspect that the beams in the one may in some degree correspond to the opposite gaps in the other.

It would appear from the foregoing description of the phenomena in question more probable that the "wedge-shaped" portions of the crystalline lens, alluded to by H. Airy, instead of having the least refractive power, as he suggests, really have the greatest. The question arises, do different eyes differ in this respect?

THOS. WM. BACKHOUSE

Sunderland, September 19

Antedated Books

THE writers who have called attention to this point in *NATURE* will have rendered an important service to students if they obtain an amendment in the lax system often followed in this country. But it is to be hoped that the reform may be made complete. It is perfectly easy to have the exact date of issue and the number and letters of the sheets contained in the part stamped on the wrapper or cover, and then by binding these wrappers with the parts, an exact reference to the date is always at hand. This is done with praiseworthy exactness by some of the foreign societies. For instance, I receive this morning a part of the *Annales de la Soc. Entomologique Belgique*, the wrapper of which bears "Tome dix-neuvième, fasc. i., signatures 1 à 13 et a à f. Paru le 16 Septembre, 1876." This it will be seen leaves nothing to be desired. Even in Spain, which we flatter ourselves is so far behind us, they manage this point accurately. The *Annales de la Soc. Esp. de Hist. Nat.* bears on the wrapper of each cuaderno the exact day of publication. Now that attention has been called to this point, let us hope that each society will instruct its secretary or editor, to see that the exact day of publication, and a summary of its contents, be stamped on the wrapper of every part issued.

Thornhill, September 19

D. SHARP

OUR ASTRONOMICAL COLUMN

THE BINARY STAR ξ BOOTIS.—A satisfactory orbit for this fine star is still wanting. Elements founded upon measures to 1833 were calculated by Sir John Herschel (period 117 years); and an orbit, on an extension of measures to 1854, was given by Hind (period 169 years), but later observation has shown them to be inadmissible. The great difficulty encountered in this case undoubtedly arises from the errors which must exist in one or more of Sir W. Herschel's angles, as was pointed out by his son in the *Memoirs of the Royal Astronomical Society*, vol. vi.,

and in any further investigation the first step must be to ascertain under what interpretation these can best be reconciled with subsequent measures, it being evident that all cannot be accepted as registered. Sir John Herschel considered the angles of 1792 and 1795, especially the former, must be affected with considerable error. These angles are respectively $355^{\circ}74$ and $354^{\circ}9$, or by a mean $355^{\circ}3$ for 1793.76, but if we suppose that they should have been registered in the north-following quadrant, instead of the north-preceding one, the mean would become $4^{\circ}7$, an angle in much better accordance with the progression shown by the observations of 1782, 1802, 1804, and those of Struve, Herschel, and South about 1821. It might be worth while to determine how far this alteration would lead to a more admissible orbit. At the same time we have to bear in mind Sir W. Herschel's remarks with respect to his observation, 1792, April 20, in *Phil. Trans.*, 1804, p. 367. And equally are we to take into consideration for our guidance the same observer's estimations of distance in 1782 and 1804.

DIAMETER OF VESTA.—At the opposition of 1855, this brightest of the minor-planet group, which during the last spring, as in previous favourable oppositions, was discernible with the naked eye, was observed by Prof. Secchi to present a diameter but little inferior to that of the first satellite of Jupiter, “ma molto più debole di luce, e di colore ranciato carico,” and he estimated it at $0''8$; this we find corresponds to a true diameter of 450 miles. The least distance of the planet from the earth in 1855 was 1.26.

PIGOTT'S COMET OF 1783.—On the night of November 19, 1783, a comet was discovered by our countryman, Pigott, at York, well known as having also detected the remarkable variable stars, R Coronæ Borealis, R Scuti, and η Aquilæ. Pigott notified his discovery to Mechain, who observed the comet at Paris on the 26th of the same month, and, in conjunction with Messier, determined its positions until December 21. It was at no time visible without the telescope. Elements were calculated by Mechain and Saron, though without satisfactory results on a parabolic hypothesis. Subsequently Burckhardt investigated the orbit without this assumption, and finally arrived at elliptical elements, with a period of revolution of 5.61 years. But the most precise determination of the orbit from the Paris observations has been made by Prof. Peters, of Clinton, U.S., who reduced the observations anew, and introducing Hansen's Tables for the earth's positions, found elements which “represent the whole series to satisfaction.” These elements are published in the “Astronomical Notices” issued by Prof. Brünnow, while in direction of the Observatory at Ann Arbor, Michigan (No. 19), but as this periodical is comparatively little known in this country, having been continued for a short time only, we transcribe the orbit here:—

Perihelion Passage, 1783, Nov. 19.93685 M.T. at Paris.

Longitude of perihelion	$50^{\circ} 17' 25''.4$	} M.Eq. 1783 0
" ascending node	$55^{\circ} 40' 30''.5$	
Inclination	$45^{\circ} 6' 53''.8$	
Angle of eccentricity	$33^{\circ} 32' 8''.4$	
Log. semi-axis major	0.5133056	
Period of revolution	5.888 years.	

In this orbit the perihelion distance is 1.4593, and the aphelion distance 5.062.

The comet has not been found since 1783. As remarked by Prof. Peters, a major-axis differing but little from the above would have sufficed to bring the comet into close proximity to the planet Jupiter, at one or other of the subsequent aphelion passages, whence it is possible great perturbations may have resulted, even of magnitude sufficient to effect an entire change of orbit. Indeed with the above elements we find the distance of the comet

when in aphelion, from the orbit of Jupiter, is only 0.42 . Independently of this, there is another cause which might have long operated to prevent the re-discovery of the comet: in 1783 it appeared under nearly the most favourable circumstances possible for observation, yet as before stated it was at no time visible to the naked eye, and while approaching pretty near the earth, did not exceed $8'$ in diameter, presenting throughout the appearance of the great majority of telescopic comets.

The orbit of Pigott's comet passes very near to that of the planet Mars: in heliocentric longitude $55^{\circ}2$, we find the distance is only 0.032 , and it is to be remarked that this close approach takes place in one of the regions where the orbit of the lost comet of De Vico also comes into such near proximity to that of the planet; still after M. Leverrier's statement with reference to past perturbation of De Vico's comet by Mars, we are not to suppose that the bodies can be probably identical.

With respect to the introduction of Pigott's comet into our system, small variation in the major-axis assigned by Prof. Peters would have caused a very close approach of the comet to Jupiter at the aphelion passage immediately preceding the comet's appearance, or early in 1781.

THE SELF-FERTILISATION OF PLANTS

MR. THOMAS MEEHAN, one of the most acute and thoughtful of American botanists, has several times during the present year brought before the Philadelphia Academy of Natural Sciences the subject of the fertilisation of plants. He has observed that there are plants with conspicuous and attractive flowers, which are as much adapted to secure self-fertilisation as other flowers are for cross-fertilisation. One of his examples is the green-house annual, *Browallia elata*, belonging to the order Scrophulariaceæ, having an attractive blue flower. Not only does it produce abundance of perfect seeds without insect aid, but also the entrance of an insect would ensure self-fertilisation. The style is nearly as long as the corolla-tube, and the slightly longer stamens are arranged closely around it. Two of the anthers are inverted over the stigma, and their connective is densely bearded, appearing like petaloid processes, completely closing the tube of the corolla. No insect can thrust its proboscis into the tube except through this mass; and if it has foreign pollen adherent to it, it will be cleaned off by the beard. Furthermore, the very act of penetration will thrust the anthers forward on to the pistil, and aid in rupturing the pollen sacs, and securing self-fertilisation.

Another phenomenon, the “sleep” of plants, or closing of the flowers at nightfall, has been found by Mr. Meehan to have reference to self-fertilisation in *Claytonia virginica* (order Portulacacæ) and some buttercups, which seed abundantly, without being visited by insects. In *Claytonia*, the stamens, on expanding, fall back on the petals expanded during daylight. At night, when the flower closes, the petals carry the anthers into close contact with the stigmas, and actual fertilisation only occurs in this way. In many cases, the stamens recurve so much as to be considerably doubled up by the nocturnal motion of the petals; thus the anthers are not brought into contact with the stigmas, and the flowers are barren.

In *Ranunculus bulbosus*, in the evening following the first day's expansion of the flower, Mr. Meehan has found the immature anthers and the young stigmas covered with pollen-grains. This would naturally be supposed to be the consequence of insect visits; but no insect visits had taken place in the cases examined. However, on carefully studying the flower it was found that coincidentally with its' expansion, a single outer series of stamens shed their pollen into the petals, from which it easily fell to the immature anthers and the stigmas when the flower closed for the night. Another equally remark-

able instance of self-fertilisation occurs in *R. abortivus*, whose petals do not close at night. It seeds profusely, yet is wholly neglected by insects, notwithstanding that it possesses large nectariferous glands. Instead of the flower closing, the slender pedicels droop at night, inverting the flower, and thus allowing the pollen to fall from the petals, on which it is shed, upon the stigmas. Mr. Meehan concludes that some deeper purpose than has yet been conceived governs the fertilisation of plants. In view of these examples, nature cannot "abhor" in-and-in-breeding, and it can hardly be that colour, fragrance, and honeyed secretion in flowers have been developed solely to secure cross-fertilisation. Evolutionists will await with interest further researches by Mr. Meehan, and confirmatory evidences from other inquirers.

THE BRITISH ASSOCIATION REPORTS

Mr. Chrystal read the following summary of a Report upon a Comparison of the B.A. Units of Electrical Resistance that had been performed by himself and Mr. S. A. Saunder.—The experiments, of which I have here an account, were undertaken for the purpose of comparing the British Association Standards of Resistance now deposited in the Cavendish Laboratory at Cambridge. In the account of the work Mr. Saunder and myself have endeavoured as much as possible to enable anyone who consults it to judge by internal evidence of the accuracy of the comparison.

The experiments were so arranged as to give a check on their own accuracy.

In work of this kind the limit of accuracy is much sooner reached in the temperature than in the electrical measurements. It is to them therefore to which the greatest attention has to be given.

We took advantage of an extremely convenient source of nearly constant temperature in the tap-water of our experimenting room, which we found by careful observation to remain constant within the tenth of a degree centigrade for a sufficiently long time. By means of this we could find the differences between the resistances of the several coils at temperatures all near 10° C. The method used for obtaining these differences was a very convenient one, described by Prof. Carey Foster in the October number of the *Journal of the Society of Telegraph Engineers* for 1874.

To obtain the co-efficients of resistance-temperature variations it was necessary to make resistance measurements at a higher temperature. The temperature chosen was 16° C.

The coils were brought to this temperature by careful nursing for an hour or more.

The results of these experiments combined with those at the lower temperature gave the variation co-efficients. The differences at any given temperature could then be calculated.

Lastly, a series of direct comparisons were made, and the result compared with calculation in order to get an idea of the accuracy of our work.

There is a difficulty in giving a comparison between our results and those of the last measurements given in the British Association Report on Electrical Standards. This arises from a want of definite information about these last measurements.

Unfortunately on most of the coils the brass labels have never been completed as was intended, and although we think we managed to identify the coils described in the report with one exception, yet still more definite information is desirable. It is because we have felt this want that we have made our own report more minute than might otherwise have seemed necessary.

We hope that no ambiguity will exist when the coils are compared again either now to check our results or some ten years hereafter to find whether the standards have varied relatively to each other.

With this caution I give a series of temperatures at which the standards are equal to each other according to our measurements side by side with one of the temperatures given in the report.

		Last found.	B.A. Report.
1/2 Pt. Ir.	2	16.1	16.0
"	3	15.8	15.8
Au. Ag.	58	15.3	15.3
Pt.	35	16.0	15.7
"	36	15.8	15.7
Pt. Ag.	29	18.2	15.2

We have laid these measurements before the British Association in the hope that they will be found useful and be made accessible to those interested in such matters.

Report of the Committee for effecting the Determination of the Mechanical Equivalent of Heat.—Progress has been made with the experiments undertaken by Dr. Joule on behalf of the Committee. Friction of water is the method employed, and the average result of upwards of sixty experiments is 772.2 in British gravitation units at Manchester. The greatest deviation from the above average is $\frac{1}{10}$.

Experiments have yet to be made on the capacity for heat of the brass of which the calorimeter is constructed, which has provisionally been calculated from the results of Regnault for this alloy. The greatest possible error which may have arisen in this way is believed to be $\frac{1}{100}$ th. Dr. Joule also proposes to compare his mercurial thermometers with the air thermometer with a view to obtain accurate boiling points, and thus correct values of the thermometric scale. The greatest correction which it may be found needful to apply on this account amounts to about $\frac{1}{100}$ th. These maximum corrections, if taken in the same direction, would necessitate the addition or subtraction of 4.5 from the equivalent above named. The experiments made by Hirn on the friction of water have led him to the number 786; but the average of his results, derived from the friction, boring, and crushing of metals, gives 774.

Assuming that the above experiments and those made by Dr. Joule for the Committee on Standards of Electrical Resistance are to be relied on, the unit issued by it would appear to have a resistance one-fortieth too small. Inasmuch as the locality in which the experiments for that unit were conducted was open to objection, it appears desirable that they should be conducted under more favourable circumstances.

Report of the Committee on the Distribution of Erratic Boulders. Read by the Rev. H. W. Crosskey, M.A.—One hundred and sixty-five additional erratics have been catalogued west and south-west of Birmingham, of which 105 have travelled from considerable distances. West and south-west of the midland table-land a large proportion of the blocks are portions of highly indurated ash-beds. To the north and west granite is much more abundant.

Between the 400 and 500 ft. contour lines at Bothel (North Cumberland) is a large block which has been transported from the north-west portion of Dumfriesshire, about forty miles from N.N.W. to S.S.E. Fragments of Shap Fell Granite occur near Dufton (Westmoreland), 800 feet above the sea-level. The east and north-east boundary of the Arenig dispersion may be roughly defined as extending from Chirk by Cefn, Ruabon, Wrexham, Caergwile, Mold, and the east side of Halkin Mountain to Holywell, and thence in a westerly direction to the Vale of Clwyd. This line nearly coincides with the boundary of the great Northern Granite drift. The Welsh and the northern drifts have to a slight extent crossed the average boundary, and a few Arenig boulders have crossed the estuary of the Dee into the peninsula of Wirral, where they become mixed with the very abundant northern drift from the Lake district and the south of Scotland. The felspathic blocks from the Arenig range have radiated to great distances over an area extending from N.N.E. to E., and to short distances from east to south-east; and have found their way across valleys and over watersheds and high mountains. The direction of the glacial striae on rock surfaces in the eastern part of North Wales as well as in the Arenig mountains, agrees in general with the course taken by the boulders.

The Committee invoke the assistance of geologists in carrying on their investigation. Schedules indicating the particulars required, may be had from the secretary. The rate at which the boulders are disappearing, owing to agricultural and building operations, makes it desirable to register their occurrence without delay.

The report of the Close Time Committee gave an account of the steps which led to the passing of Mr. Chaplin's Bill for the Preservation of Wild Fowl last Session, and included a circular extensively distributed by the Committee to further that object. Lord Walsingham, Mr. Chaplin, and Mr. Rodwell were warmly thanked for their exertions in the matter. The Committee thought it possible that something further might be done to regulate the proceedings of bird-catchers; but the difficulties in the way appear so serious, that immediate success is not expected. The Sea Birds Preservation Act continues to work satis-

factorily on the whole, though there is reason to fear that its provisions have been disregarded in certain places. A few prosecutions in the coming year may be useful. The East Riding Justices have, with the assent of the Home Secretary extended the close time on the Yorkshire coast from August 1 to August 15.

Dr. M'Kendrick read the report of the committee *On Intestinal Secretion and Movement*. The conclusions to which the Committee had come were—First, that the application of various soda and potash salts to the intestinal mucous membrane produced a more or less profuse secretion, that caused by sulphate of magnesia, acetate of potash, sulphate of soda, and tartrate of potash and soda being most abundant; second, that the presence in the intestines or in the blood of atropia, morphia, chloral, &c., did not prevent the abstraction of sulphate of magnesia; third, that the splanchnic nerves were, as usually admitted, the vaso-motor nerves of the intestines, but either had no centrifugal fibres to their muscular coats or affected them only indirectly by diminishing their supply; fourth, the secretory nerves of the intestines had the small ganglia of the solar and superior mesenteric plexuses for their centres, and this secretion was unaffected by the splanchnics, the vagi, or the dorso-lumbar parts of the cord; fifth, destruction of the lumbar part of the cord after extirpation of the solar plexus produced hæmorrhage, or hyperæmia of the intestinal mucous membrane, which was absent after the division of the splanchnics, destruction of the semilunar ganglia and solar plexus, or division of the mesenteric nerves themselves; and sixth, the splanchnics were the afferent nerves for peristalsis of the intestines, the efferent stimulus probably reaching its intraparietal ganglia through the lumbar cord and the abdominal sympathetic, the effect of the former being inhibitory and the latter stimulating to these ganglia.

Mr. Heywood read the *Report of the Committee on the Metric System*. It pointed out that while the House of Commons had legalised the metric system for contracts and general purposes, no provision was made for the verification of the standards by the authorities, the consequence of which was that they could not be used in this country as they were liable to be seized. The Committee recommended that steps should be taken to have the weights and measures verified in the same manner as those of the imperial system. They regretted the striking out of the Education Code of the clause introduced by Mr. Forster referring to the metric system, and hoped it would be re-introduced. The report also entered into the question of the decimalisation of coinage.

The Report of the Committee on the Use of Steel for Structural Purposes, stated that after repeated correspondence with the Board of Trade, with the view of getting them to settle the conditions under which steel may be used, Colonel Yolland, R.E., Sir John Hawkshaw, F.R.S., and Mr. W. H. Barlow, F.R.S., had been appointed by the Board of Trade to endeavour to arrange these conditions.

Mr. Symons, secretary of the Rainfall Committee, read their Report to Section G. for the past year, from which it appeared that the rainfall of 1874 was slightly below the average, owing to a rather dry spring and exceedingly dry summer. The most remarkable feature of the year was the heavy fall of rain on October 6, when the average fall over England and Wales was slightly above 1 inch in the 24 hours, and the fall at most stations in North Wales and the Lake District was upwards of 5 inches. So heavy a fall over so large an area was rare. The rainfall of 1875 was greatly above the average in England (especially in the Midland Counties), and irregular in Scotland and Ireland. A very heavy rainfall occurred in Wales and Southern England on July 14, the fall in 24 hours exceeding 1 inch at 252 stations, 2 inches at 109, 3 inches at 39, 4 inches at 7, and 5 inches at 3 stations. The Committee reported last year the success of their efforts to improve the geographical distribution of rainfall stations in Ireland, showed that the gauges started at the cost of the Association had been supplemented by many others established at the cost of private individuals, and gave a map showing the present complete distribution of stations. Almost all the observers have proved good ones, and the returns had been forwarded with regularity. The period was too short to yield precise results, but a good system had been inaugurated and was in full operation. The Committee felt they had done service to rainfall work. When they commenced their labours, the weakest part of rainfall observations was the defective geographical distribution of the stations. This defect had now been

materially lessened. By the grants of the Association nearly 250 gauges had been erected in districts hitherto without observations.

Mr. Bramwell asked what the Committee meant to do in the future.

Mr. Symons said he understood the Association wished to discontinue its grant to the Committee, and that the connection between the two should now cease. This he very much regretted, because if anything happened to himself he did not see how the work of the past could be maintained. Mr. Symons added that we had now in this country a system of observations which was the admiration of all countries. America and other countries were copying us. The system now embraced something like 2,000 stations, so scattered that it was scarcely possible to drop a man down in any place where he would be more than four or five miles from a rain-gauge. The consequence was, that when hydraulic and waterworks questions turned up, data were almost always available which did not exist ten years ago for ascertaining the quantity of water which could be collected from any given gathering ground. With reference to the future maintenance of the system it simply rested with himself.

It was ultimately stated by the President that the Sectional Committee considered the time had now arrived when this work should be taken up in a larger public spirit, and consequently that the grant hitherto made should now cease. This recommendation was made in the confident expectation that those who had hitherto so greatly benefited by the laborious and successful work carried on by Mr. J. S. Symons for the Association, would come forward and make the work of the Rainfall Committee their own. The Committee had also to record its most hearty thanks to that gentleman for his valuable services, which had proved so important to many branches of science, and had redounded to the credit of the British Association.

Mr. J. W. L. Glaisher gave the report of the *Committee on Mathematical Tables*. He stated that the whole of the theta function tables 0° to 89° (360 pp.) were now completed, and a copy taken from the stereotype plates was exhibited to the Section. He also gave an interim report of the Committee on mathematical notation and printing. The Committee had met and agreed to several suggestions with regard to notation, but it seemed desirable to postpone the report till next year, when the report on printing would be ready.

Mr. R. B. Hayward read the report of the Committee upon the improvement of geometrical teaching; it stated that the Committee approved generally of the syllabus issued by the Association for the improvement of geometrical teaching, although they criticised some few portions of it.

SECTION A.—MATHEMATICAL AND PHYSICAL.

One of the papers which excited the most attention was by Prof. Osborne Reynolds, *On the Resistance encountered by Vortex Rings, and the Relation between the Vortex Rings and the Streamlines of a Disc*. It was illustrated by many most interesting experiments relating to the motion of vortex rings in a large trough of water. The following is an abstract of the paper:—

The comparatively small success which has attended nearly all attempts to refer the various movements of fluids to fundamental laws may, I think, be attributed principally to our being in ignorance of many most important circumstances of motion attending the phenomena with which we wish to deal.

We can see the way in which the surface of a fluid moves, but of the internal motions observation affords us no idea, we having no sense by which to perceive them. Accordingly, such steps as have been made towards success for the most part relate to surface phenomena, or to movements which have been rendered apparent by accident. My object, on the present occasion, is to describe certain results which have been obtained by colouring portions of the water within a tank so as to render them visible. These results are somewhat striking, and I venture to think that they are in some respects in advance of what has been hitherto taught; but they are now brought forward rather as illustrations of the importance of the method of study than on account of their own value.

The Cause of the Resistance to the Motion of Solids through Water not known.—The development of the theory of stream lines with which the name of the late Prof. Rankine is so intimately connected, has been a great advance, from the theoretical side, in the study of fluid motion. This theory, however, only applies

strictly to hypothetical fluids without viscosity, and its results as applied to water fall very far short of experimental verification.

Thus, as was so beautifully illustrated before this Section last year by Mr. Froude, a solid should move through a frictionless liquid in a rigid inclosure without resistance; the liquid moving out of its way, from front to rear, in filaments or streams which, closing together behind, cause pressure which exactly balances the pressure in front. In fact, however, water opposes very great resistance to the rapid motion of solids through it. If a ball which will just float be allowed to fall from a great height into water it will only descend a very short distance; and when we come to speeds like the speed of a shot, a foot of water opposes nearly as much resistance as an inch of iron. Opposite as these facts are to what the stream line theory might lead us to expect, they do not disprove the truth of this theory because it does not take into account the viscosity of water. But it is clear that before we can make much practical use of this theory in dealing with the only fluids with which we have to deal, we must ascertain in what way it is that viscosity affects the behaviour of these fluids, so that it may be taken into account in applying the theory. This is a point on which I think some light is thrown by rendering the motion of the water visible.

The Stream Line Theory applies to the Vortex Ring.—The idea of colouring the water to render its motion apparent was doubtless suggested by the effect of smoke and the beautiful phenomena of the smoke-ring. In the smoke-ring we have an instance of a most important form of fluid motion accidentally rendered invisible, of which we should otherwise have most certainly been in ignorance; as it is, however, it has caught the attention of mathematicians, and in the hands of Sir William Thomson, Prof. Tait, Helmholtz, and others, has led to most important researches.

That which is most striking in the smoke-ring is the regularity and extreme beauty of its internal structure. Our familiarity with objects moving rapidly through the air tends to diminish our surprise at the ease with which these rings move. But when we see these rings in water, this rapid motion and the small disturbance which they cause, although only a few inches below the surface, are, I think, the most striking points of the phenomenon.

Vortex rings in water were exhibited at Edinburgh, in 1871, by Mr. H. Deacon, but only on a very small scale, being formed of a single drop. About three years ago I tried a method of forming them, very similar to that used by Prof. Tait for smoke-rings. This method succeeded perfectly. From an orifice $\frac{1}{4}$ " in diameter, I could send rings the full length of my trough (20 feet), and with velocities so great, that during the first part of their course the eye could not follow them. It would appear, from the absence of all disturbance either behind the ring or at the surface, that these rings must move without resistance; and yet this appears at first sight to be inconsistent with the way in which the speed of the rings diminishes as they proceed, either in water or air. There is, however, a cause for this diminution of speed, which cannot properly be called resistance. The rings grow in size as they proceed, and consequently they are continually adding to their bulk water taken up from that which surrounds them, and with which this forward momentum has to be shared. A loss of velocity must result from this growth in size, and the only question with regard to resistance is whether the one of these is sufficient to account for the other, whether, notwithstanding the loss of velocity, the momentum of the moving mass remains constant.

To determine this I measured (by the best means I could devise) the momentum of a series of equal rings at different distances from this origin; the result was that (within the limits of accuracy of the experiments) there was the same momentum in the rings after they had travelled 15 feet, and were not moving more than 3 inches per second, as when at 2 feet from the origin, and moving more than 5 feet per second. I conclude, therefore, that these rings do move without any appreciable resistance.

When this freedom from resistance is considered along with the internal motion of the fluid in and around these rings, it shows that in them we have an instance, and I believe it is the only one, in which the stream line theory applies accurately to motion in a viscous fluid. The form of the mass of fluid moving forward is not nearly that of the ring, but is an oblate spheroid a good deal longer than this ring which it encloses.

This spheroid, like the ring, is continually growing, but at any instant it has a definite shape, and the motion of the water which surrounds it, is at that instant exactly the same as it would be according to the stream line theory if the spheroid were solid and the water were frictionless.

The spheroidal form of the bounding surface, which, of course

being fluid, is perfectly flexible, is maintained against the unequally distributed pressure of the surrounding water by the motion of the water within it, the motion being such that at each point in the bounding surface it causes the same pressure as that arising from the motion of the external water.

The Effect of Viscosity on the Motion of the Ring.—The motion of the internal water, besides maintaining the shape of the bounding surface, is such that at each point of this surface the motion of the water in contact with it on the inside is identical with the motion of that in contact with the outside. So that not only is the bounding surface at each instant definite in form, but every point of the surface is in motion in exactly the same manner as the water in contact with it.

The action of the viscosity of the water in causing the gradual growth of the ring and its attendant mass is not confined to the bounding surface, but extends throughout the moving water both internal and external to this surface. There is a gradual diminution in the velocity with which the water moves along the stream lines from the centres outwards in all directions as far as the motion extends into the surrounding water, and, as is well known, when the velocity of a stream varies from point to point in a section across the direction of motion, the effect of viscosity is towards equalising the velocity. Hence, in the case of the vortex ring, the effect of viscosity will be to diffuse the motion outwards, to diminish the whirling velocity at the centre where greatest, and to extend the space through which the water is in motion, that is, to diminish the velocity and extend the size of each element of the ring.

And it appears that this effect to diminish the velocity and extend the size of the ring is the only effect of viscosity. That is to say, if the water at any instant were to lose its viscosity, then the ring would proceed onwards in exactly the same manner as it was proceeding at that instant, for the internal motion would be just as necessary to balance the external pressures and preserve the form of the bounding surface in frictionless fluid, as in water, and hence the same law must hold between the internal and external motions.

The author then supposes that at a certain instant one of the vortex rings is converted into ice, and further, that there is no friction between the surface of the ice and the surrounding water. He proceeds:—

Whatever might be the resistance of the ideal smooth ice, it is clear that its actual resistance must exceed this by what ship-builders call its skin resistance, the drag of the water moving past the surface. This may be estimated from the resistance of a plane surface of equal extent when moving edgewise through the water, and this is not much.

This, however, is only one of the effects of the surface friction. Whatever drag the friction may cause on the surface, there is an equal drag on the water moving past it, and thus the surface friction aids the diffusion in diminishing the velocity with which the water would otherwise move in the stream lines near the surface, and so tends to increase the disturbance of the stream lines in the rear of the solid.

Actually we find that the resistance resulting from the disturbance of the stream lines is ten times as great as the mere skin resistance, and so far is a solid, of the shape of the bounding surface of the vortex ring, from moving freely, that if it be set in motion, it stops at once and is altogether dead in the water.

That the disturbance of the stream lines as described above really takes place, is shown when we colour the water. When we first start the solid we see a somewhat irregular vortex ring behind it, which grows rapidly and then breaks up; after this the water behind is all confused, and follows the solid. Whereas with the vortex ring, if there are streaks of colour in the water through which the ring passes, it leaves them so nearly as they were before, that there is scarcely a trace of its path. Thus this disturbance of the stream line appears to be the cause of the resistance encountered by a solid over and above the skin-resistance.

The magnitude of the effect depends on the curvature of the streams, and hence we see why a body having a fine after-part like a fish encounters so much less resistance than a full body like a spheroid. Whereas if the stream lines were complete according to the theory, the extra surface of the fish should cause it much greater resistance.

Relation between the Vortex Ring and the Stream Lines of a Disc.—Another matter on which I have been able to throw some light by colouring the water, relates to the form of the stream lines of a thin surface, such as a disc. It is, I believe, generally assumed that the theory of stream lines shows all bodies would

move without resistance in a perfect fluid; and that thin surfaces are no exception to the general rule, but I am not aware that any satisfactory figure for the stream line of such bodies has as yet been given.

Now what I have to show raises two very important points in connection with the stream-line theory, even as applied to a perfect fluid. In the first place it will appear that a thin open surface has no stream lines of its own (so to speak) except such as it can claim as forming part of a closed surface. And in the second place it will appear that the closed surface may assume the form of a cylinder of indefinite length continually passing away from the thin surface, in which case the surface or vane does not move freely even through a frictionless fluid. If we place a disc in front of one of these rings, the ring comes on until the disc is against the bounding surface, and then carries the disc on with it. It is certainly surprising to see a flat disc moving freely through the water. I doubt not that the general impression is that a thin flat disc is about the worst form of body to move through water. And so it is, except when it has a vortex ring behind it.

Owing to the growth of the ring, if the speed of the disc be maintained, the ring will gradually fall behind the disc, and the disturbance caused in front will break it up. But if the disc be allowed to move with the ring it will move freely as far as the ring goes. A disc when first started forms its own ring. Thus if a disc be floated on a light bar of wood, when the wood is drawn forward at first, the disc offers considerable resistance to its motion, but this resistance soon dies away, and if then the bar be released, the disc will proceed steadily onward with a gradually diminishing velocity.

A little colour in the water shows how the ring is formed and how it moves onward behind the disc.

The Resistance of an Inclined Vane.—The fact that the disc will start its own ring, will close its own surface, is due to its being symmetrical with respect to this surface. Half a disc will not do it, much less any portion of the spheroid which was inclined to the front.

When we draw a disc or flat surface edgewise through the water it causes a continuous vortex cylinder which, forming at the forward point of the vane passes away behind. The gyratory motion of the water is somewhat disturbed by the friction of the vane sliding past it, but by letting a little air down with the vane the central lines of the vortices may be shown for several feet in length.

Having to form the vortices the forward edge encounters the greatest resistance, and the whole resistance is steady and continuous.

If my reasoning is right these facts are somewhat at variance with the general notion as regards the results of the stream-line theory, and at all events they furnish definite ideas of the results we have to explain. It is, however, with the utmost diffidence that I venture to bring forward my own explanation before such an authoritative body as Section A. in the University of Glasgow, and my chief object has been to illustrate the method of studying fluid motion by observations on the motion of partly coloured water.

On the Protection of Buildings from Lightning, by Prof. J. Clerk Maxwell.—Most of those who have given directions for the construction of lightning-conductors have paid great attention to the upper and lower extremities of the conductor. They recommend that the upper extremity of the conductor should extend somewhat above the highest part of the building to be protected, and that it should terminate in a sharp point, and that the lower extremity should be carried as far as possible into the conducting strata of the ground, so as to "make" what telegraph engineers call "a good earth."

The electrical effect of such an arrangement is to *tap*, as it were, the gathering charge by facilitating a quiet discharge between the atmospheric accumulation and the earth. The erection of the conductor will cause a somewhat greater number of discharges to occur at the place than would have occurred if it had not been erected; but each of these discharges will be smaller than those which would have occurred without the conductor. It is probable, also, that fewer discharges will occur in the region surrounding the conductor.

It appears to me that these arrangements are calculated rather for the benefit of the surrounding country and for the relief of clouds labouring under an accumulation of electricity, than for the protection of the building on which the conductor is erected.

What we really wish is to prevent the possibility of an electric discharge taking place within a certain region, say in the inside

of a gunpowder manufactory. If this is clearly laid down as our object, the method of securing it is equally clear.

An electric discharge cannot occur between two bodies, unless the difference of their potentials is sufficiently great, compared with the distance between them. If, therefore, we can keep the potentials of all bodies within a certain region equal, or nearly equal, no discharge will take place between them. We may secure this by connecting all these bodies by means of good conductors, such as copper wire ropes, but it is not necessary to do so, for it may be shown by experiment that if every part of the surface surrounding a certain region is at the same potential, every point within that region must be at the same potential, provided no charged body is placed within the region.

It would, therefore, be sufficient to surround our powder-mill with a conducting material, to sheathe its roof, walls, and ground-floor with thick sheet copper, and then no electrical effect could occur within it on account of any thunderstorm outside. There would be no need of any earth connection. We might even place a layer of asphalt between the copper floor and the ground, so as to insulate the building. If the mill were then struck with lightning, it would remain charged for some time, and a person standing on the ground outside and touching the wall might receive a shock, but no electrical effect would be perceived inside, even on the most delicate electrometer. The potential of everything inside with respect to the earth would be suddenly raised or lowered as the case might be, but electric potential is not a physical condition, but only a mathematical conception, so that no physical effect would be perceived.

It is therefore not necessary to connect large masses of metal such as engines, tanks, &c., to the walls, if they are entirely within the building. If, however, any conductor, such as a telegraph wire or a metallic supply-pipe for water or gas comes into the building from without, the potential of this conductor may be different from that of the building, unless it is connected with the conducting-shell of the building. Hence the water or gas supply pipes, if any enter the building, must be connected to the system of lightning conductors, and since to connect a telegraph wire with the conductor would render the telegraph useless, no telegraph from without should be allowed to enter a powder-mill, though there may be electric bells and other telegraphic apparatus entirely within the building.

I have supposed the powder-mill to be entirely sheathed in thick sheet copper. This, however, is by no means necessary in order to prevent any sensible electrical effect taking place within it, supposing it struck by lightning. It is quite sufficient to inclose the building with a network of a good conducting substance. For instance, if a copper wire, say No. 4, B.W.G. (0.238 inches diameter), were carried round the foundation of the house, up each of the corners and gables and along the ridges, this would probably be a sufficient protection for an ordinary building against any thunderstorm in this climate. The copper wire may be built into the wall to prevent theft, but should be connected to any outside metal such as lead or zinc on the roof, and to metal rain-water pipes. In the case of a powder-mill it might be advisable to make the network closer by carrying one or two additional wires over the roof and down the walls to the wire at the foundation. If there are water or gas-pipes which enter the building from without, these must be connected with the system of conducting-wires, but if there are no such metallic connections with distant points, it is not necessary to take any pains to facilitate the escape of the electricity into the earth.

Still less is it advisable to erect a tall conductor with a sharp point in order to relieve the thunder-clouds of their charge.

It is hardly necessary to add, that it is not advisable, during a thunderstorm, to stand on the roof of a house so protected, or to stand on the ground outside and lean against the wall.

On a Cyclone Periodicity, in connection with Sun-spot Periodicity, by C. Meldrum.—This paper is a continuation of the one published in the report for 1874; it contains a discussion of the cyclones that occurred in the Indian Ocean, from the equator to 32° S. and 0° to 120° E., in the years 1868-75. From 1868 to 1872 the cyclonic area increased, and since 1872 it has been decreasing. In 1868 it was two millions of square miles, in 1872 between four and five millions, and in 1875 nearly two millions. The rainfall over the globe generally seems to have had a similar march, the rainiest year being 1872.

Mr. O. J. Lodge exhibited diagrams of a model to illustrate mechanically the passage of electricity through metals, electrolytes, and dielectrics, according to Maxwell's theory. The model consisted of an endless cord passing with friction through buttons supported on elastic strings, and by altering the relation

between the friction and the elasticity of different parts it could be made to exhibit very completely the phenomena observed when an electromotive force is made to act (1) between the ends of a metal wire; (2) through an electrolytic liquid; (3) in an accumulator with perfectly insulating dielectric; (4) across a dielectric which is homogeneous, but has a slight conducting power; (5) across a non-homogeneous or stratified dielectric, in which a "residual charge" is possible. To illustrate in a simple manner the phenomena observed in a submarine cable, the cord might be made elastic.

Capt. A. W. Baird, R.E., contributed a paper *On Tidal Operations in the Gulf of Cutch*.—The primary object of the operations was to determine whether secular changes in the level of the land at the head of the gulf, *i.e.*, the "Runn of Cutch," are taking place. Col. Walker at first intended to restrict the observations to a few weeks' duration, but he found that by extending them to a period of a little over a year, scientific results of the highest value would be obtained, and also that this course would be necessary in order to obtain data sufficient to detect minute changes in the relative level of land and sea. The author described the difficulties that had been experienced; but stated that the whole of the tidal and meteorological observations were in progress of reduction, and when completed were likely to afford results of importance. It was hoped that the effect of the wind and barometer upon the tide might be determined more accurately than had yet been done. Tracings of the actual diagrams were exhibited, and the tidal curves were seen to be very regular and continuous.

The number of important experiments shown before Section A at this meeting was very remarkable. Besides the experiments of Prof. Osborne Reynolds already referred to, there were several others relating to liquids. Prof. James Thomson illustrated experimentally the origin of the windings of rivers in alluvial plains, as explained in a recent paper in the Royal Society's *Proceedings*, and Sir William Thomson showed many experiments upon the precessional motion of a spheroidal top filled with liquid. Sir William Thomson also exhibited a new form of astronomical clock with a free pendulum actuated by an independent governor to give approximately correct uniform motion to the escapement-wheel. Mr. H. W. Bosanquet illustrated experimentally his paper on the conditions of the transformation of pendulum vibrations, and Mr. Colin Brown exhibited his voice harmonium in connection with his paper on just intonation. Sir William Thomson explained a method of taking deep-sea soundings in a ship moving at high speed by means of pianoforte-wire and an apparatus which was exhibited and explained. The Rev. J. Ker described an experiment proving rotation of the plane of polarisation of light reflected from a magnetic pole. The attendance throughout was excellent, and the room was generally crowded; in fact in recent times there has been no meeting at which so much interest has been taken in the proceedings of this Section. Owing to the number of papers the Section was divided into two on the Monday and Tuesday, and the Section met to finish the work on Wednesday. Saturday was, as usual, devoted to mathematics. There were altogether twenty-three mathematical papers, among which may be mentioned those by Prof. Cremona on systems of spheres and systems of lines, by Prof. Tait on two general theorems relating to closed curves, by Mr. J. W. L. Glaisher, giving determinants expressing the number of partitions of a number, and the sum of the divisors of a number; by Prof. Jung, of Milan, on the inverse problems of the moments of inertia and the moments of resistance of a plane figure, and on a construction for the central nucleus (*Central kern*, of Culmann) of a body, and by Mr. G. H. Darwin on graphical interpolation and integration. Two new committees were appointed at the recommendation of Section A, one for commencing experiments upon the elasticity of wires, and the other upon the lunar disturbance of gravity.

SECTION C.—GEOLOGY.

On the Upper Limit of the essentially Marine Beds of the Carboniferous System in the British Isles, and the necessity for the establishment of a Middle Carboniferous Group, by Prof. E. Hull, M.A., F.R.A.S.—Prof. Hull distinguished seven stages in the Carboniferous Rocks, each stage being capable of identification by its fossils over large areas in Great Britain and Ireland. These stages are:—

- (1) Upper Coal Measures.
- (2) Middle Coal Measures.
- (3) Lower Coal Measures or Gannister Beds.
- (4) Millstone Grit.
- (5) Yoredale Rocks.
- (6) Carboniferous Limestone.
- (7) Lower Limestone Shale.

He argued that as thirty-three out of fifty-three species of marine shells pass from the Carboniferous Limestone upwards into the Gannister Beds, while only five passed up into the Middle Coal Measures, a palæontological break was indicated of such magnitude as to warrant a more distinct separation of the Gannister Beds from the Middle Coal Measures; especially as the shells of the former are marine, while many palæontologists regard those of the latter as of fresh-water origin. He therefore proposed to include all from the Gannister Beds to the Yoredale Rocks as Middle Carboniferous; the term Lower Carboniferous to include as at present the Carboniferous Limestone and Lower Limestone Shale.

Note on Sections exhibiting Variation of thickness in the Middle Coal Measures of West Lancashire, by C.E. de Rance, F.G.S.—The sections described by the author lie between Prescott and Barnsley, where the Middle Coal Measures, containing several thick coal-seams, and the Gannister Beds, containing few important seams, are represented. Having made many sections of the Middle Coal Measures of the district, Mr. de Rance was satisfied that the amount of the subsidence from south to north for a distance of ten miles, increased at the rate of about 60 feet per mile, and that the deposition of the Coal Measure strata kept pace with the subsidence.

On the Changes affecting the Southern Extension of the Lowest Carboniferous Rocks, by G. A. Lebour, F.G.S.—The author contended for the division of the Carboniferous Rocks into Upper and Lower, drawing the line between the Millstone Grit and Yoredale Series. Dealing with the lower division, it was pointed out that the Upper Old Red Sandstone was in part identical with, or passed upward into Macharen's "Califerous Sandstone," known in the North of England as "Tuedian," and in Ireland as "Valentian." In England the upper limit of the "Tuedian" is equally indefinite, as the series dovetails into the lower members of the "Bernician" Group, in which term the author includes the "Yoredale Series and Calcareous Group in part, Scar Limestone Series and Calcareous Group in part, plus Carbonaceous Group in part."

On the Mountain Limestone on the West Coast of Sumatra, by Prof. Ferdinand Roemer, of Breslau.—Dr. Verbeck, Director of the Dutch Geological Survey of Sumatra, sent to the author a large collection of fossils from the west coast, for determination and description. The result of this investigation is that the Mountain Limestone is developed on the western coast of Sumatra, with mineralogical and palæontological characters, very similar to those of the same formation in Europe. The same genera of shells, and partly the same species occur in these widely remote localities. Hitherto the Mountain Limestone has only been known to occur in the Malay Archipelago in Timor, a small but characteristic Mountain Limestone fauna from that island having been described by Prof. Beyrich. It may be expected that by-and-by the Mountain Limestone may be ascertained to have a wider range in the Malay Archipelago than is at present known. From the geographical position of Java, between Sumatra and Timor, it is probable that a zone of Mountain Limestone, and perhaps of other palæozoic rocks, may be found to exist there, although partly hidden by volcanic and tertiary deposits.

On some New Minerals and on Doubly-refracting Garnets, by Prof. A. von Lasaulx, of Breslau.—Prof. von Lasaulx exhibited specimens of a new mineral from Girgenti, Sicily, where it occurs in small cubes on crystals of sulphur and celestine. Its chemical composition is,—silica 86 per cent., water 3 per cent., iron and strontium small quantities, and sulphuric acid, or some acid of the thionic series not yet determined, 7 per cent. From the behaviour of the mineral before the blowpipe, the author named it melanophlogite. The author also described a series of garnets exhibiting the phenomena of double refraction.

On the Raised Beaches of the Cumberland Coast between Whitehaven and Boness, by A. Russell, C.E., F.G.S., and T. V. Holmes, F.G.S.—The authors exhibited a map showing several fragments traced by themselves, of raised beaches sloping inland from 25 ft. above the present sea-level to an upper limit of 40 ft. The terraces are covered by low gravel ridges parallel to the old

cliff line, similar to the little mounds left by the sea at the present day half-way between the mark of the highest spring tides and that of the lowest neap tides. With regard to the date of the elevation of the beaches, the authors observed that all the evidence available tended to place it before the time of the Roman occupation.

Tidal Retardation—Argument for the Age of the Earth.—The Secretary read a paper by James Croll, LL.D., F.R.S., of the Geological Survey of Scotland, *On the Tidal Retardation Argument for the Age of the Earth*. Many years ago Sir William Thomson demonstrated from physical considerations that the views which then prevailed in regard to geological time and the age of our globe were perfectly erroneous. His two main arguments were—first, that based on the sun's possible age; and secondly, that based on the secular cooling of the earth. More recently he has advanced a third argument (*Trans. Geol. Soc. of Glasgow*, vol. iii., p. 1), based on tidal retardation. It is well known that owing to tidal retardation the rate of the earth's rotation is slowly diminishing, and it is therefore evident that if we go back for many millions of years we reach a period when the earth must have been rotating much faster than now. Sir William's argument is, that had the earth solidified several hundred millions of years ago the flattening at the poles and the bulging at the equator would have been much greater than we find them to be. Therefore, because the earth is so little flattened it must have been rotating, when it became solid, at very nearly the same rate as at present. And as the rate of rotation is becoming slower and slower, it cannot be so many millions of years back since solidification took place. A few years ago I ventured to point out (*NATURE*, August 21, 1871; "Climate and Time," p. 335) what appeared to be a very obvious objection to the argument, and as the validity of the objection, as far as I am aware, has never been questioned, I have been induced to believe that the argument referred to had been abandoned. But I find that Prof. Tait in his work on "Recent Advances in Physical Science," restates the argument as perfectly conclusive, and makes no reference whatever to my objection. As the subject is one of very considerable importance, I may be permitted to direct attention to the objection in question, which briefly is as follows:—

It has been proved by a method pointed out a few years ago (*Philosophical Magazine*, May, 1868, pp. 378–384, February, 1867, p. 130, "Climate and Time," Chap. xx. *Transactions of Geological Society of Glasgow*, vol. iii., p. 153), and which is now generally admitted to be reliable, that the rocky surface of our globe is being lowered, on an average, by subaerial denudation at the rate of about 1 foot in 6,000 years. It follows as a consequence from the loss of centrifugal force resulting from the retardation of the earth's rotation, occasioned by the friction of the tidal wave, that the sea-level must be slowly sinking at the equator and rising at the poles. This, of course, tends to protect the polar regions, and expose equatorial regions to subaerial denudation. Now it is perfectly obvious that unless the sea-level at the equator has, in consequence of tidal retardation, been sinking during past ages at a greater rate than 1 foot in 6,000 years, it is physically impossible the form of our globe could have been very much different from what it is at present, whatever may have been its form when it consolidated, because subaerial denudation would have lowered the equator as rapidly as the sea sank. But in equatorial regions the rate of denudation is, no doubt, much greater than in the temperate regions. It has been shown in the papers above referred to, that the rate at which a country is being lowered by subaerial denudation is mainly determined not so much by the character of its rocks as by the sedimentary carrying power of its river systems. Consequently, other things being equal, the greater the rain-fall the greater will be the rate of denudation. We know that the basin of the Ganges, for example, is being lowered by denudation at the rate of about 1 foot in 2,300 years, and this is probably not very far from the average rate at which the equatorial regions are being denuded. It is therefore evident that sub-aerial denudation is lowering the equator as rapidly as the sea-level is sinking from loss of rotation, and that consequently we cannot infer from the present form of our globe what was its form when it solidified. In as far as tidal retardation can show to the contrary, its form may have been as oblate as that of the planet Jupiter when solidification took place.

There is another circumstance which must be taken into account. The lowering of the equator by the transference of materials from the equator to the higher latitudes must tend to

increase the rate of rotation, or, more properly, it must tend to lessen the rate of tidal retardation.

On Siliceous Sponges from Carboniferous Limestone near Glasgow, by John Young, F.G.S.—Mr. Young observed that siliceous sponges had not hitherto been obtained from deposits of Carboniferous Limestone age in Britain. Recently, however, Mr. John Smith had discovered large numbers of them in fissures in a limestone at Cunningham Baidland, near Dalry, Ayrshire. The limestone bed in which they occur is 40 feet thick, and belongs to the upper division of the Carboniferous Limestone series. It contains, at different horizons, producta and spirifers, corals, crinoids, and polyzoa. Prof. Young and the author proposed to name them *Acanthospongia Smithii*.

On the Granite of Strath-Erick, Loch Ness, by James Bryce, LL.D.—Having ascertained that the gold of Sutherland occurred not in quartz veins, but in the granite itself, Dr. Bryce tested the granite of Strath-Erick, and was rewarded by finding gold there also, although in small quantities. Proceeding to examine more carefully than had previously been done the relations of this granitic mass to the surrounding rocks, he found that, although at one locality it clearly overlaid the Lower Old Red Sandstone, in another place it alternated with slate, as if the slate had been brought up by the granite. It was remarkable that, although the slates are cut up by veins of the granite, none pass into the Old Red strata. The author considered that the evidence in favour of the intrusive character of the granite was incontrovertible.

On the Upper Silurian Rocks of Lesmahagow, by Dr. Robert Slimon.—Dr. Slimon gave an interesting historical account of the mapping of the Upper Silurian rocks of Lesmahagow, and of the discovery and determination of their remarkable crustacean fauna.

On the Age, Fauna, and Mode of Occurrence of the Phosphorite Deposits of the South of France, by J. E. Taylor, F.G.S.—The author visited the phosphorite caverns within the last two months, and gave an account of what he saw.

On a Deep Boring for Coal at Scarle, Lincolnshire, by Prof. E. Hull, M.A., F.R.S.—The boring, after penetrating the Lower Lias, New Red, and Permian, entered the Carboniferous formation at the depth of 1,900 feet. The Carboniferous Rocks bored through were grey sandstones, with plants and shales with anthracosia, &c., 55 feet; calcareous shales and earthy limestone, 65 feet; fine breccia, 4 feet; chocolate-coloured clay, 6 feet. This succession was very puzzling. The beds above the breccia were pronounced by Prof. Ramsay and the author, without any consultation, to be Yoredale Rocks, but since the breccia has been reached, Prof. Hull inclines to regard it as belonging to the uppermost beds of the Coal Measures. As the boring is still going on, it is hoped that something more definite may be discovered.

A feeder of water was tapped in the Keuper Sandstone at the depth of 917 feet, and a still more powerful one in the Bunter Sandstone, at 1,250 feet, sent a jet of clear water 4 feet above the ground. The water must percolate from the outcrop of these beds ten or twelve miles to the west, being prevented from rising by the presence of the overlying impervious Lias Clay.

On Tertiary Basaltic Dykes in Scotland, by R. L. Jack, F.G.S., of the Geological Survey of Scotland.—Mr. Jack exhibited a map showing the courses of all the dykes of this age traceable for any distance which have hitherto been mapped by the Geological Survey, and described their peculiarities, referring specially to their avoidance of faults and other obvious lines of weakness. One dyke crosses Scotland from Helensburgh to Grangemouth, while two others maintain a parallel course from the heads of the River Irvine to the head of the Tweed, a distance of nearly forty miles. It was pointed out that a number of the larger dykes tend to converge towards the peninsula between Lochs Riddun and Striven, where, however, no evidence of volcanic activity, either in the shape of lava-flows or plugged-up vents, is known to exist.

On certain Pre-Carboniferous and Metamorphosed Trap-Dykes and Associated Rocks in North Mayo, by W. A. Traill, M.R.I.A., of the Geological Survey of Ireland.—In the district between Ballycastle and Belmullet the rocks belong either to the Carboniferous age or are older and metamorphosed. The author distinguishes at least two sets of dykes, both being basaltic. Those of the newer set run in straight lines, traverse both metamorphic and Carboniferous strata, and appear to fill vertical fissures or to come up along lines of fault. The older dykes

disturb only the metamorphic rocks, occur chiefly in sheets, and are often crumpled and contorted, while fragments of them occur in a conglomerate at the base of the Lower Carboniferous. This set must therefore be pre-Carboniferous, while the upper set is post-Carboniferous, and possibly Miocene.

SECTION D.—BIOLOGY.

Department of Anatomy and Physiology.

ADDRESS BY JOHN G. MCKENDRICK, M.D., F.R.S.E., VICE-PRESIDENT.

The Future of Physiological Research.

BEARING in mind the fact that one of the objects of the British Association is to interest the public in the advancement of scientific truth, it has been the practice of the presidents of the various sections to make some remarks of a general character, or to give a *résumé* of the recent progress of science in their particular department. I shall follow so far the examples of my predecessors. I shall not attempt to enumerate, far less to describe, the contributions made to anatomical and physiological science during the past year, because that would entail a long and wearisome report regarding investigations with which most of us are already acquainted by the perusal of those excellent summaries that appear from time to time in our scientific and medical periodicals. With the view of limiting the scope of this address, I propose to offer a few observations bearing generally upon some of the scientific and social relations of anatomy and physiology, with the view of interesting the public in what we have been doing, and what we hope yet to do.

These sciences present different views of the same great system of truth. Each can be conceived as existing independently, while at the same time the one science is the complement of the other. Anatomy is the science of organic form, while physiology is that of organic function. The anatomist investigates structure, its form, general arrangements, and laws, and he may include in his survey the purposes or functions which the structure fulfils. Recently an opinion has been prevalent, and has cropped up in various quarters, that anatomy is but a preparatory science for physiology. This opinion has probably arisen in consequence of the rapid growth of physiological science during the last twenty or thirty years. But there can be no doubt that anatomy has a *role* of her own by no means inferior to that of physiology. She has to educe the formal laws which determine the structure of organised bodies and their parts, and thus she establishes the basis for scientific classification and arrangement. Anatomy is the beginning, of course, of all medical education, and the ground work on which the practical arts of medicine and surgery are reared; but in a broader sense, the science has to do with the structure of every animal, from the simplest to the most complex, and from the facts obtained in the investigation of the structure of any animal, we are able to recognise the relationships it has with other animals, or, in other words, its position in the zoological scale.

Dr. McKendrick then proceeded to speak of the methods of anatomy, histology, the methods of physiology, the vivisection question, the importance of teaching biology, the practical aspects of anatomy and physiology, the importance of investigations on the physiological action of active substances, the relation of physiology to medicine; after which, with reference to the relation of physiology to psychology, he remarked that as physiology is intimately connected with psychology, or the science of mind, and as this department of physiological work has lately been his chief study, he may be allowed to refer to it a little more in detail.

Psychology may be divided into two parts: first, all those phenomena which we may include under the term mind properly so-called, such as feeling, volition, and intellectual processes; and second, the phenomena which are associated with, and which indicate the alliance between, mind and matter. Every mental act may be regarded in the present state of knowledge as having a double aspect—on the one side it is known to our consciousness, and on the other side it is the result of a number of physical processes occurring in the brain.

The Methods of Psychology.

In the investigation of mental phenomenon, two modes of inquiry have been followed: first, that of introspection and reflection, in which the investigator looks within himself for the facts of his experience; and second, that of the examination of physiological processes which coincide with sensorial or mental changes.

It is evident that the first of these methods, usually called the subjective, is open to the objection that by it a mind attempts to observe its own operations, and that the proceeding is somewhat analogous to asking a machine to investigate its own mechanism. This objection urged in other words by Comte, Maudsley, and others, may be answered by replying that the subjective method does not attempt to explain the physiological phenomena concomitant with mental states, but the laws which regulate these mental states themselves. Suppose a complicated machine possessed consciousness, I can readily understand that by the exercise of this consciousness it might be unable to discover the relation and mechanism of its own parts, because in attempting to do so the machinery would be so interfered with as to prevent normal action; but it might still be able to study the products of its operations. I do not, therefore, decry this old method of psychological research as it is so much the fashion to do in these days. Apart altogether from the philosophical speculations and systems of philosophy founded upon them, I think many data accumulated by such men as Locke, Berkeley, David Hume, Thomas Reid, Dugald Stewart, Thomas Brown, Sir William Hamilton, and James Mill, have as good a right to be considered correct as many of the quasi-metaphysical conceptions of physical science. Subjective inquiry carried on by such men cannot be given up as a mode of psychological research. It may not carry us much further than it has done, but it has rendered good service already, and may possibly do more.

But, on the other hand, the objective method appears to me to be the one which, in future, will be principally cultivated, and it is for this reason that, as a physiologist, I wish especially to refer to it.

It is the business of physiology to supply psychology with information regarding physical processes occurring in the nervous system; and it is one of the special features of the physiology of the present day to direct attention to the physical side of mental phenomena. No doubt Aristotle, Hobbes, and Hartley incorporated into their psychological theories much that was purely physiological; but in their days the physiology of the nervous system was in a crude state, and, consequently, did not lead to great results. In comparatively recent times, a new inductive and experimental department of science has arisen, the nature of which is indicated by the term physiological psychology, and which is being diligently cultivated by numerous workers, both at home and abroad. In our own country the writings and researches of Herbert Spencer, Alexander Bain, Dr. Laycock, George Henry Lewes, Dr. Maudsley, Dr. Carpenter, Alfred Barratt, and James Sully, and on the continent those of Fechner, Helmholtz, Wundt, Hermann Lotze, Taine, Donders, Plateau, and Dalboef, have excited much interest, and have led to the formation of a new school of thought.

I think it right to mention here specially the name of Prof. Laycock, who has done more, in my opinion, in this field of inquiry than any other member of the medical profession of this country in our time. His teaching has largely contributed to our present humane methods of treating the insane; he has attracted year by year some of the best students of the University of Edinburgh to this important department of medical practice; and his earlier writings incontestably show that, many years ago, and prior to most of the writings of those great men whose names I have just enumerated, he not only recognised the value of physiological research with regard to mental phenomena, but made important contributions himself.

Physiology has thus encroached on psychology, and is attempting to supply from the objective side an explanation of at least the simpler mental phenomena. As a proof of awakened interest in this department, one of the features of the past year has been the appearance of *Mind*, a quarterly journal of psychology, edited by my able friend Prof. Croom Robertson of University College. In the prospectus of this journal, it is stated that "psychology, while drawing its fundamental data from subjective consciousness, will be understood in the widest sense, as covering all related lines of objective inquiry. Due prominence will be given to the physiological investigation of nerve-structure." This quotation indicates the view which the editor takes of the relation of the two sciences, and already valuable papers have appeared on subjects connected with physiological psychology, from the pens of Sully, Lewes, Wundt, and others.

Now a certain class of thinkers are alarmed by work of this kind. They are afraid of the tendency "to represent the mental fact as a physical fact," and they are inclined to shut their eyes to the physical facts connected, undoubtedly, with psychological processes, and to be contented with the study of subjective

phenomena. But as most admit that there are two aspects in which mental phenomena may be viewed, why should not both be looked at carefully? If it be also admitted, that it is impossible to connect any physical process (supposing we knew it) occurring in brain cells with an act of consciousness, what is the use of taking a one-sided view of the phenomena in question? Why not study both sides of the problem, and give up the attempt at reconciliation, which is entirely beyond the pale of our faculties? This mystery of mind and matter has puzzled thoughtful men from the earliest times. Some have attempted a reconciliation. They have reasoned in a circle, so that most people, after perusing their works, are no nearer an ultimate solution than they were at the beginning. We always come back to this view of the case, namely, that every fact of mind has two aspects, a physiological and a psychological. That is one way of looking at the problem, and it is the one which, in the present state of knowledge, personally I prefer. But there is another. Thus, as has been well argued by Mr. George Henry Lewis in his recent work, "Problems of Life and Mind," two very different descriptions may be given of one and the same mental activity. The one may be expressed in the language of psychology, which is the language we commonly use to describe our feelings; the other may be stated in the language of physiology, a language intelligible only to those acquainted with the present state of physiological research. He says: "All that we have to guard against, is the tendency to mistake difference of aspect for difference of process, and to suppose that changes in feeling can exist independently of changes in the organism, or that any change in the organism can be effected otherwise than by some previous change." This way of stating the question may be more satisfactory to some minds. At all events, it is a fair attempt to solve the puzzle of our present state of existence, in which we are constantly brought face to face with the antithesis of object and subject.

Abandoning these speculations which are fruitless in practical effects, let me now endeavour very briefly to indicate the lines of inquiry in the domain of physiology, along which progress has been and may be made in the attempt to solve psychological phenomena; and I wish it to be understood that I do not take these in any logical order, but merely adduce them by way of illustration. It will also be my aim not so much to describe what has been done in the past, as to indicate what remains to be done in the future.

Research in Physiological Psychology.

First of all, then, it is quite evident that all researches on the general physiology of the great nerve centres are of paramount importance. Such researches as those of Hitzig, Fritsch, and Ferrier on the excitability of the cerebral hemispheres, supplying new ideas regarding the mechanism of the brain as a compound organ; of Wundt on central innervation and consciousness, in which he discusses in a manner never before attempted, the phenomena of reflex excitation; of William Stirling on the summation of excitations in reflex mechanisms; of various French physiologists on the mode of action of ganglia in insectæ; and of many others, are all recent important contributions to this department of science. Here, however, we have to confess that we have little accurate information regarding the minute structure of the parts involved, and consequently no anatomical basis on which to found our views. We have a general idea of strands of nerve-fibres and groups of nerve-cells of various forms, but we have no precise knowledge of the relative quantity of these, or of the relations of one group of nerve-cells to another group. We are unacquainted with any peculiarity in structure, for example, by which even an accomplished histologist could identify three microscopical sections as respectively portions of the brain of a man, of a monkey, and of a sheep. All this has still to be worked out. Every little area of brain-matter has to be surveyed and carefully described. Supposing this were done in the case of the human brain, and of the brains of the higher animals, the same must be attempted with the brains of animals lower in the scale. I can then conceive a grand collection of facts which may throw light on the intricate working of different kinds of brains, and, perhaps, afford a rational explanation of certain psychological characters.

Suggested Investigation.

What I mean may perhaps be better understood by a research, which I would suggest by way of experiment. No one who has kept an aviary of small birds—say a collection of our native and foreign finches—can have failed to observe marked

differences of character and habits among different members of the same genus, and even among different members of the same species. One manifests cunning, another combativeness, a third kindness to smaller brethren, a fourth bullies all about him, a fifth may usually be quiet and peaceable, but occasionally gives way to uncontrollable rage, and so on. The question arises, then, Have these psychological peculiarities any organic basis, any explanation in the structure of the brain? or, are we to rest satisfied by asserting that these peculiarities are due to the action of some kind of psychological principle regarding which we know nothing? I have little doubt most will agree that these psychical characteristics of birds depend on peculiarities of brain structure the result of hereditary transmission through many generations. If so, here we have an opportunity of examining the microscopical structure of small brains, relatively simple, and easy of manipulation, with the view of ascertaining whether or not there are any structural differences which will account for these differences in psychical character. This is a line of inquiry likely, in my opinion, to establish an organic basis for a comparative psychology.

After referring to recent researches on the chemistry of the brain, Dr. McKendrick proceeded to refer to those on the physiology of the senses, which afford another series of data for the psychologist. These researches may be said to be of three kinds—(1) inquiries into the anatomical and physiological mechanism of the sense organ itself, such as, in the case of vision, the general structure of the eye as an optical instrument, and its movements by the action of muscles, so as to secure the conditions of monocular or binocular vision; (2) inquiries into the nature of the specific action of the external stimulus upon the terminal organ of sense, and the transmission of the effect to the brain; as, for example, the action of light on the retina, and transmission along the optic nerve; and (3) experiments in which various stimuli are permitted to act under certain conditions on the terminal apparatus, and the result is observed and recorded by the consciousness of the experimentalist himself, as in researches on colour, duration of impressions on the retina, positive and negative after-images, &c. By these three modes of inquiry a large number of facts relating chiefly to the senses of hearing and vision have been collected; and most of these facts, inasmuch as they assist him in understanding the conditions of sensory impressions and sensational effects, are of importance to the psychologist.

Measurement of Time in Sensory Impressions.

The next step of importance made by physiology into the domains of psychology is the measurement of time or duration in sensational effects.¹ This has been carefully measured by objective methods. Speaking generally, the time occupied from the commencement of the action of the stimulus to the termination of a sensation, may be divided into four portions, each of which has a certain psychological interest:—First, an interval of time is occupied by the primary physical change produced by the stimulus. During this interval, called the period of latent stimulation, no effect is observed. Thus, when a motor nerve distributed to a muscle is stimulated by a short electrical shock, about 1-60th of a second passes before the muscle contracts. Second, when the change in the nerve or terminal organ has begun, a second interval of time is occupied in the transmission of the impression to the nerve centre, which is succeeded by a third interval, during which changes occur in the nerve centre, and the result of which is a sensation. The time occupied in transmission, or the rate of conductivity in nerve, is tolerably well known, being at the rate of about 200 feet per second in the nerves of man; but the time occupied in the production of the sensation in the centre has not yet been clearly ascertained, owing to the difficulty of supposing such a sensory nerve centre to be, previous to the stimulus, in a state of absolute inaction. Lastly, it has been found that when a nervous action of any kind has been initiated by a stimulus, it goes on for some time after the stimulus has ceased to act. This prolongation of the sensation may be well studied in the case of impressions on the eye, where the time of the duration of the impression has been measured by Helmholtz, Plateau, and others. These distinguished observers also found that the length of time occupied by the after effect varied according to the intensity of the light. Thus, after a weak light, the unchanged impression lasts longer than with a strong light. A strong illumination is followed by an after impression fading sooner than with a feeble

¹ In the following observations I am much indebted to the essays of Mr. James Sully, contained in his volume, "Sensation and Intuition," (London.)

stimulus; the result being that, so far as the retina is concerned, it comes to the same thing whether an intense light acts for a brief time, or a faint light for a longer time.

Exhaustion of Nerve or Sensory Organ.

This line of research has also made it possible to measure the time required for exhausting a nerve or sensory organ. When, for instance, a limited area of the retina has been stimulated for a certain time, and the stimulus has been removed, the after positive effect, due to increased excitation of the parts, disappears, and is followed by a negative effect, due to temporary diminution of the sensibility of the parts, in the form of what is called the negative after-image. Suppose, for example, an area of the retina be acted upon for a period of from five to ten seconds, and the stimulus be then removed, the so-called positive after-image vanishes quickly, and the negative after-image, frequently of a complementary colour to that of the exciting cause, appears, and lasts for a short time, gradually fading away as the nervous parts recover from the effects of the stimulus. Similar phenomena may be observed in studying the durations of sensations of tone, which I have frequently perceived in experiments made by myself; but it is more difficult to identify, by description and designation, the after effects in the case of audition than in the case of vision. Probably it may be found still more difficult to notice these after sensations in the other senses, although in all there is often the experience of a lingering feeling after the cause has been removed, which no doubt has its place in those transient sensations which assist in filling up the spaces, as it were, in our conscious life.

In experiments upon a sensory organ, such as the retina, a little consideration will show that it is almost impossible to ascertain the effect of a stimulus upon a retina which has never before been affected. This difficulty has been felt by all experimenters. Molecular action in such a structure has been in operation from the very beginning, and such action, if of sufficient intensity, must produce a certain effect on the conducting tract, and on the recipient centre. This effect, although of too weak intensity to produce those changes which result in consciousness, must be taken into account in the measurement of the intensity and duration of sensory impressions. Thus the eye has a light of its own due to changes in the retina, although this may never be conscious to us as a luminous impression. This conception of the state of matters in a terminal organ such as the retina, when applied to actions going on in the brain, at once indicates that similar actions, or rather that similar states of unrest, of change, variation, and modification, are going on in these deeper parts which may never result in consciousness, *per se*, but which altogether may have an influence on our mental existence comparable to that of the feeble impressions constantly transmitted to the cerebrum from the viscera, sometimes termed the internal senses.

Relation between Strength of Sensation and Magnitude of Stimulus.

Having shown that sensory impressions are distinctly related to time, the next advance made by physiologists was to prove that there was a relation between the strength of the sensation and the magnitude of the stimulus. Here there are difficulties in explaining what is meant, because language fails. We have no words to discriminate ideas which hitherto have related to two distinct fields of knowledge—the objective and the subjective. To speak of the strength or magnitude of a sensation seems to be using terms applicable only in another region, and quite inapplicable to psychological phenomena, although no one has any doubt in distinguishing the intensity or magnitude of one pain from that of another. There is no difficulty in understanding the phrase-magnitude of the stimulus. A weight of ten pounds is greater than that of one pound, light from ten candles of equal size is more than that given out by one, and the tones of a violin of equal pitch and quality, may vary in intensity according to the pressure of the bow on the string. It is difficult, however, to obtain an absolute measurement of variations in sensation, which is, of course, a subjective phenomenon. This can only be done by varying the objective cause, by observing a large number of instances, and by expressing variations in the subjective phenomenon in terms applied to variations in the objective cause. If the average result obtained from a large number of instances indicate any ratio between the magnitude of the stimulus and the subjective phenomenon, then we may conclude that there is a relation between the two.

This mode of inquiry, first originated by Prof. E. H. Weber in his celebrated experiments on tactile impressions (and which

were first introduced to notice in this country by Prof. Allen Thomson), was afterwards carried out by his colleague Prof. Fechner, and has been subsequently elaborated by Prof. Wundt. It has led to various remarkable results, the chief of which are—(1) That in the case of each sense there is an upper and a lower limit, beyond which the amount of stimulus produces no appreciable difference of effect; and (2) that within this range there is a definite ratio between the stimulus and the amount of the sensation. The upper limit beyond which an increase of external stimulation is not followed by any observable increase in sensational effect, was first observed by Prof. Wundt. The lower limit has been noted by many observers, and it is indicated in almost every physiological text book. Now it does not matter much to us in taking a general view of things, what the limits are, provided we are sure that such limits exist, inasmuch as it indicates another element of proof that psychological phenomena, so far as sensation is concerned, occur within certain physical limits.

Fechner's Investigations.

The next step naturally was to establish the ratio between the magnitude of the stimulus and the magnitude of the sensation. To do this directly is impossible, as any estimation of the amount of sensational effect following a given stimulus would probably be erroneous, because our perceptions are usually qualitative and only rarely, and never absolutely, quantitative. Fechner recognised this fact, and he employed for the solution of the problem various methods by which he measured not sensations themselves, but the amount of discriminative sensibility between two sensations produced by stimuli of unequal magnitudes, and he studied the ratio between the difference of weight and the absolute quantity of the stimulation. By varying the amount of the stimulus in every possible way, he eliminated the chances of error, and arrived at definite results. These results he formulated into a general "psycho-physical law," which may be expressed in various ways. Mathematically it may be put, that "sensation increases in proportion to the logarithm of the stimulus." Now "logarithms increase in equal degrees when the numbers so increase that the increment has always the same ratio to the magnitude of the number." It may be put in another way by saying that "the more intense a sensation the greater must be the added or diminished force of stimulation in order that this sensation undergo an appreciable change of intensity." The mode of arriving at some of Fechner's results may be better understood by an experiment which any one can repeat. In the case of muscular sensation, suppose two weights A and B: we wish to ascertain the least difference between these perceptible by the muscular sense, say when we lift them in the hand. Let it be so arranged that both weights are composed of different pieces, so that the one may be made less or more than the other at pleasure. If A and B be nearly equal in absolute weight, the person on whom the experiment is made will judge them to be of equal weight. Let weights be now added to B until the difference between A and B becomes perceptible, and as a test, let the weights be again removed from B until, in sensational effect, A becomes again equal to B; let the same experiment be repeated with weights of different absolute amount, and it will be found that there is a distinct ratio between the absolute weight and the weight that had to be added to it or taken from it to produce the least perceptible difference of impression of whatever these weights may be, up to the limit, of course, which I have already noticed. It will always be found that the additional or subtracted weight is one-third that of the absolute weight—a fraction which indicates the degree of intensity of the stimulus required to produce the least perceptible feeling of difference of sensation, and may be termed the *constant proportional* of that kind of sensation. This fraction, in the case of sensibility to temperature, Fechner found to be one-third; Renz, Wolf, and Volkmann arrived at the same fraction with regard to auditory impressions; and various observers have found that in visual impressions it is one-hundredth.

Now the intensity of sensation depends on two conditions: (1) the intensity of the excitation; and (2) the degree of excitability of the sensory organ at the moment of excitation. But suppose the excitability of the organ equal on two occasions, the intensity of the sensation does not increase proportionately to the increase of the excitation. That is to say, suppose we bring into a dark chamber a luminous body such as a candle—it produces a certain luminous sensation; then introduce a second, third, and fourth—the excitation is double, triple, or quadruple; but experiment shows that the increase in the amount of the sensation is much less; in other words, let the stimulus increase from 10 to 100 times, and from 100 to 1,000 times, the sensation will be

only one, two, and three times stronger. The importance of the discovery of this remarkable law is, that it shows a distinct mathematical relationship between stimulation and sensation. Possibly it may be found to have applications to other psychological phenomena. May it not vary in different animals, and even in different individuals?

Criticism of Fechner's Method.

It is quite noticeable, however, that in the case of each sense, the law did not hold good throughout the whole range of variations in intensity of stimulus; and it is not surprising, when we consider the complexity of the conditions, that such should be the case. All of these experiments were made in the case of visual impressions, for example, on the living eye, connected by the optic nerve with the brain; and it is manifestly impossible, as has been remarked by Hermann, "to localise this relationship between sensational effect and variation in amount of stimulus, which has been called the psycho-physical law of Fechner." Between the sensational effect and the first contact of the stimulus, there are a series of complicated processes occurring in retina, nerve, and brain, processes undergoing incessant modification by the interchanges between these tissues and the warm circulating blood. In which of these does this relation between stimulus and conscious state occur—in retina, in optic nerve, or in brain? The only method of answering this question, so far as I know, is to examine the effects of stimulation upon these parts separately. It is manifestly next to impossible to do this in the case of the optic nerve and the brain; but by the method pursued by Holmgren, in Sweden, and by Prof. Dewar and myself in this country, it can be done, so far as the retina is concerned. In carrying out this method, Prof. Dewar and I found that light produced a change in the electrical condition of the retina in an eye removed from the head or kept in normal conditions, and we ascertained that the general phenomena of this change corresponded with our sensational experiences of luminous impressions. We were, therefore, entitled to assume that the change in the electrical conditions of the retina, produced by the action of light, might be regarded as a phenomenon intimately related to those changes in the brain which result in consciousness of a luminous impression. Consequently we had an opportunity of ascertaining whether or not Fechner's law agreed with the effects of a stimulus of light in altering the electrical condition of the retina, and we found that it did so. The inference, therefore, is that the relation between degree or variation in stimulus and the corresponding sensation of a luminous impression, is a function of the sense organ or retina.

Mode of Investigating the Sensory Organ Itself.

I may here remark that this mode of inquiring into sensory impressions has by no means been exhausted. The subjective method of observing sensational effect under the stimulus of light from revolving discs, by the contrasting of colours, by comparison of auditory sensations produced by tones of different intensity, pitch, and quality, is always open to the charge that the results may not be due to specific histological structure of the sense organ, as is almost invariably assumed, but to structure of the recipient of impressions from the sense organ, namely, the brain. The only way of proving that the effects are due to structural peculiarities of the sense organ is to examine the effects of stimuli applied to the sense organ separated from the brain by some method the same or analogous to ours. If in these circumstances the sense organ give results similar to those observed in the phenomena of consciousness, then we may assume that these results are due to specific peculiarities of the sense organ, and not to the brain. If, on the other hand, the results do not agree, then we must look in the brain for the mechanism by which these different results are produced. Thus I have always held, that as there is little or no histological evidence of complexity of structure in the retina capable of accounting for the theory of Thomas Young regarding the perception of colours, or of the facts of colour-blindness, or of the sensibility of different zones of the retina to lights of different colours, we may have to look to the complex structure of the corpora quadrigemina, cerebellum, or some portion of the cerebral hemispheres for an explanation of these facts. It may be objected that such scepticism simply removes the difficulty a little further back, but I think it is better to search for facts than to be contented with an hypothesis.

Conclusion.

Time will not permit me to discuss other researches in this field of inquiry, nor the interesting speculations which have

sprung from them, but I think I have said enough to show the line of advance in this direction.

True it is that apparently the physiological causation of many mental phenomena may be, in its precise nature, inaccessible to direct proof, but it is our duty as physiologists to push legitimate research as far as it will go. I would remark also that such researches are not incompatible with those spiritual ideas, matters of faith and not of science, which are the basis of our most cherished hopes. They demand, however, caution in the scrutiny of facts, and judgment in drawing conclusions from them. More than in any other kind of scientific labour, perhaps, it is of the utmost importance here to keep the mind unbiassed, a task by no means easy. To maintain a calm unprejudiced attitude to inquiries which seem to demand a change of opinion regarding what was supposed to be final, requires an effort which varies in different persons. Some find it comparatively easy to do so, while others succeed only after a severe struggle. Still it is the state of mind which a man true to science ought to aspire to, so that while he will not be blown about by every wind of doctrine, he may be ready to accept what is apparently true when he has had it clearly put before him.

In conclusion, let me observe that it would save not a little heart-burning, and might possibly remove acrimony from various scientific and social controversies, could we only remember that it is not very probable that we, in this nineteenth century, have arrived at the final solution of many problems which have puzzled wise men from the earliest times. Probably we have got nearer the truth, but it is presumptuous to suppose that we have reached the ultimate truth. Many hypotheses much in favour at present may turn out to be inadequate. Still if they serve as stepping stones to something better, and to more rational conceptions of the mysterious phenomena about us, they will have done good service. In the meantime it is our duty vigorously to prosecute research, in all departments, pushing ahead fearlessly, and with that enthusiasm which is the prime mover in all great deeds, so that we may be able to transmit our department of knowledge to posterity not only less burdened with error, but with many additions of truth.

Prof. Turner, of Edinburgh, gave an account of his researches into the structure of the placenta in mammals, and showed how forms originally supposed to be distinct and unconnected by gradations, were really but modifications of one fundamental type. Thus the obstacle to the reception of the theory of evolution, which had been supposed to be constituted by the various placental structures, did not exist. But it was difficult to see in many respects what causes had determined the evolution. In some cases it appeared that the great dilatation of blood capillaries in the uterus might be of advantage, because less force would be required for the propulsion of the blood. Again, in the upward ascent, there was complication of the placental structures with restriction of area; and he supposed that with this restriction there would be a diminished danger of hæmorrhage after parturition, and consequently greater safety.

Mr. F. M. Balfour, Fellow of Trinity College, Cambridge, read a paper *On the Development of the Protovertebra and Muscle-plates in Elasmobranch Fishes*. The most important points on which he laid stress were the origin of the notochord from the hypoblast, the splitting of the mesoblast from the hypoblast as two distinct lateral halves, the consequent appearance of the body cavity at first as two cavities, the extension of the body cavity on each side up to the summit of the muscle-plates, and the derivation of a large portion of the voluntary muscular system from the splanchnic or visceral layer of the mesoblast. He compared these embryological facts with many occurring in the Invertebrates, especially in Sagitta, in Brachiopods, and in Echinoderms, showing how it was possible to unify them by adopting Haeckel's gastræa theory, and by no other method. Dr. Allen Thomson warmly commended Mr. Balfour's researches, saying that it was quite a new thing for such a continuous series of embryological papers of great importance to proceed from a British investigator.

Mr. G. J. Romanes, M.A., gave an account of his further researches on the physiological functions of the Medusæ this summer. To this we shall return.

Prof. Haeckel described two of the simplest forms of animals with two layers in their body-wall—Haliphysema and Gastrophysema. They were Cœlenterata of the simplest type; the first form had one body cavity; in the second it was partly divided into two cavities, whereof one was specially appropriated to the formation of ova, the other to nutrition. If there had

been pores in the body-wall he should have referred both forms to sponges. Their development showed that they arose strictly in conformity with the *Gastræa* type. He then gave some account of the mode of development of the chief animal stocks, as explained in the "History of Creation." Dr. Allen Thomson said that Prof. Haeckel had been regarded in many quarters with somewhat of the same suspicion that had greeted the first promulgation of Mr. Darwin's theories, and he was considered one of the most rash and daring speculators of the day. Those who had listened to his exposition would probably take a different view, and see how much of sound observation went to the establishment of his theories. In so extensive a field as that over which Prof. Haeckel's views carried him, he might be sometimes led into error, and might possibly be widely wrong, but at the same time they could not but admire the manner in which observation of fact was always placed as the basis of his theory.

Dr. D. J. Cunningham read a paper *On the Spinal Nervous System of the Cetacea*. He found that while great similarity prevailed between their cervical and dorsal nerves and those of other mammalia, the nerves of the lumbar and caudal regions differed widely. The superior and inferior divisions of those nerves in cetacea were of nearly equal size. Two great longitudinal cords or trunks are formed by their union on each side of the vertebral column, and these become situated on either side of the spines of the vertebrae, and on either side of the bodies below the transverse processes. These great cords supply the four great muscular masses which act upon the tail.

Prof. Burdon-Sanderson gave an account of his further researches *On the Electrical Phenomena exhibited by *Dionæa muscipula* (the Fly-trap)*. He had accurately investigated the phenomena by means of the electrometer. He found that normally the whole leaf with the petiole was somewhat negative, but that when excited by a stimulus, an electrical change took place throughout, making every part more negative; the greatest change was on the external surface of the leaf immediately opposite to the three sensitive hairs. There was no relation between the pre-existing currents and the electrical disturbance consequent on stimulation. The period of latent stimulation was about one-sixth of a second; the period during which the disturbance lasted was one second, more or less. As the leaf becomes fatigued, the period of latency gradually increases to one second and three-quarters, and then most likely the next stimulation would produce no effect. The change appears to be a function of the protoplasm of the parenchyma of the region out of which the sensitive hairs arise. Certain of the characters of the change are similar to those presented by muscle and nerve. Why the variation should be a negative one, Prof. Sanderson had no idea.

Prof. Struthers described the finger-muscles of several whales. He concluded that such muscles existed in the whalebone whales, but in ordinary toothed whales they were merely represented by fibrous tissue. These muscles existing in the true bottle-nosed whale had a special interest, as the teeth in that whale were rudimentary and functionless. He had found these muscles in the forearms of whales largely mixed with fibrous tissue, so the transition was easy. He also gave an account of dissections of the rudimentary hind-limb of the Greenland right-whale. Prof. Macalister, of Dublin, expressed his opinion that the whales were not of very ancient origin, for he thought the existence of the rudimentary limbs tended to show that a sufficient length of time had not elapsed since the use of the limb was essential to the earlier animal, to produce its complete obliteration.

Mr. C. T. Kingzett read a paper *On the Action of Alcohol on the Brain*. He said the question of what became of alcohol taken into the system had been extensively studied. Thudichum was the first to determine quantitatively the amount of alcohol eliminated by the kidneys from a given quantity administered, and the result he obtained was sufficient to disprove the elimination theory then widely prevailing. Dupre and many others continued these researches from which, according to Dupre, they might fairly draw three conclusions: (1) that the amount of alcohol eliminated per day did not increase with the continuance of the alcoholic diet, therefore all the alcohol consumed daily must of necessity be disposed of daily, and as it was certainly not eliminated within that time it must be destroyed in the system; (2) that the elimination of alcohol following the taking of a dose was completed twenty-four hours after the dose was taken; and (3) that the amount eliminated in both breath and urine was a minute fraction only of the amount of alcohol taken. In 1839 Dr. Percy published a research on the presence of

alcohol in the ventricles of the brain, and, indeed, he concluded "that a kind of affinity existed between the alcohol and the cerebral matter." He further stated that he was able to procure a much larger proportion of alcohol from the brain than from a greater quantity of blood than could possibly be present within the cranium of the animal upon which he operated. Dr. Marcet, in a paper read before the British Association in 1859, detailed physiological experiments which he considered to substantiate the conclusions of Dr. Percy; inasmuch as they demonstrated that the alcohol acted by means of absorption on the nervous centres. Lallemand, Perrin, and Duroy had, moreover, succeeded previously in extracting alcohol from brain-matter in cases of alcoholic poisoning. But all these researches left them entirely in the dark as regarded the true action, if any, of alcohol on cerebral matter, and no method of investigation was possible until the chemical constitution of the brain was known. Thudichum's researches in this direction, together with some more recent and published investigations by Thudichum and the author, had placed within reach new methods of inquiry regarding the action of alcohol on the brain. In his research he (Mr. Kingzett) had attempted this inquiry by maintaining the brains of oxen at the temperature of the blood, in water, or in water containing known amounts of alcohol. The extracts thus obtained had been studied in various ways, and submitted to quantitative analysis, while the influences exerted by the various fluids on the brain had been also studied. These influences extended in certain cases to hardening and to an alteration in the specific gravity of the brain-matter. Water itself had a strong action on brain matter (after death) for it was capable of dissolving certain principles from the brain. It was notable that water, however, dissolved no kephaline from the brain. Alcohol seemed to have no more chemical effect on the brain than water itself, so long as its proportion to the total volume of fluid did not exceed a given extent. The limit would appear to exist somewhere near a fluid containing 35 per cent. of alcohol. But if the percentage of alcohol exceeded this amount, then not only a larger quantity of matter was dissolved from the brain, but that matter included kephaline. Such alcoholic solutions also decreased to about the same extent as water the specific gravity of brain substance, but not from the same cause; that was to say, not merely by the loss of substance and swelling, but by the fixation of water. Many difficulties surrounded the attempt to follow these ideas into life, and to comprehend in what way these modes of action of water and alcohol on the brain might be influenced by the other matters present in blood. On the other hand, it was difficult to see how any of the matters known to exist in the blood could prevent alcohol, if present in sufficient amount, from either hardening the brain (as it did after death) or dissolving traces of its peculiar principles to be carried away in the circulation; that was to say, should physiological research confirm the stated fact that the brain in life absorbed alcohol and retained it, it would almost follow of necessity that the alcohol would act as he had indicated and produce disease, perhaps *delirium tremens*. Dr. McKendrick said Mr. Kingzett's researches into the chemistry of the brain and the action of various agents upon it were a valuable step in the right direction. This was essential if the mode of working of the brain were ever to be understood; but it would be a long way from the knowledge of the dead tissue to the comprehension of its vital action. No doubt alcohol had a marked effect upon the convective-tissue elements in the brain. He suggested as a useful method of research the submitting a certain class of animals for a length of time to the action of a definite amount of alcohol, and then examining their brains to discover what effect was produced. The investigation was of very great importance as regarded the treatment of drunkards; no doubt in many cases where it was thought that they had to do with merely moral evil, there was a fundamental change in physical organisation. Prof. Burdon-Sanderson said the question was one that ought certainly to be taken up by Government, and the best men in the country should be engaged upon the inquiry. It had a most important bearing upon the welfare of the community and the diminution of human suffering.

Surgeon-Major Johnston, in a paper *On the Diet of the Natives of India*, came to the conclusion that the natives require much more nitrogen and carbon than Europeans, and also took much more salt, owing to the comparative absence of salt from the substances which form a large part of their food. The natives took more dry food than the Europeans, and those who lived on food from the tables of the Europeans enjoyed a considerably greater immunity from cholera than others.

Mr. Wanklyn read a paper *On the Effects of the Mineral Sub-*

stances in *Drinking-Water on the Health of the Community*. One of the questions which has often been asked is, Whether is it better to drink hard water or soft water? The reply which has been given is that at present we cannot tell, but that apparently the system can accommodate itself to either, and that a soft-water drinker is sometimes disordered when he begins to drink hard water. He wished to call attention to the opportunity that physicians had at present of discovering the effects of hard waters by reason of the great use that was being made of a very hard water, the Tannus water. Ordinary hard water might contain from 13 to 20 grains of carbonate of lime per gallon; but the Tannus water contained, roughly speaking, 100 grains of carbonate of lime and 200 grains of common salt per gallon, besides considerable quantities of carbonate of magnesia, chloride of potassium, and sulphate of soda. In the course of the discussion which followed the reading of Mr. Wauklyn's paper, Dr. Carr stated that in Kent, where the water was hard, he believed the amount of salts of lime was exceedingly beneficial to children, and the Kentish children were singularly well supplied with straight legs and good bones. Mr. Wauklyn stated that Kent water was one of the purest he had ever seen; average drinking-water contained ten times as much organic matter as the Kent water. The real objection to the latter was that it contained a large proportion of sulphate of lime; whenever it was met with in any volume it had something of the odour of rotten eggs, due to the presence of sulphuretted hydrogen and sulphate of lime. He questioned whether hard water, however useful for children, was altogether desirable at a later period of life.

Dr. Paton's able paper *On the Action and Sounds of the Heart* gave an account of excellent experimental researches, by which he claimed to have proved that the ventricle in coming to complete contraction itself exerts a strain on the base of the distended aorta that produces the simultaneous reaction of the aorta, closing the valves and completing the wave. This is contrary to the usual view which considers that the aorta reacts after the conclusion of the ventricular contraction. The influence of this new conception on the comprehension of the sounds of the heart is important; for if a sound be produced in closing the semilunar valves, it must terminate the first sound of the heart, and cannot be the second sound. The latter arises after the first pulse-wave has terminated, and is synchronous with the diastole of the ventricle. In a series of experiments on the action of the denuded heart of the terrapene during the highest temperature of the season, when the action of the heart was strong and vigorous, Dr. Paton distinctly identified the first sound with the contraction of the ventricle and the reaction of the aorta, the sound being produced by the rushing of the blood through the orifice and terminated by the recoil of the aorta. The second sound, short, sharp, and acute, was produced by the contraction of the auricles sending the blood through the auriculo-ventricular orifices. The effect of these facts upon pathological sound is well followed out.

Among other contributions to this department may be mentioned Prof. Dewar's continuation of his important researches *On the Physiological Action of Light*, Dr. Urban Pritchard's paper *On the Termination of the Nerves in the Vestibule and Semicircular Canals of the Ear of Mammals*, and the same author's *Demonstration of a New Microscope adapted for showing the Circulation in Man*.

The five days' session of this department was fruitful in important memoirs on physiology, anatomy, embryology, and histology, showing that a considerable amount of good work is going on in this country. The discussions were of more than usual value, as many eminent anatomists and physiologists were present and took part in them.

Department of Zoology and Botany.

Among the botanical contributions was an interesting one by Dr. I. B. Balfour, entitled *Notes on Mascarene Species of Pandanus*. He said that no portion of the flora of the Mascarene Islands was more peculiar than the various species of the genus *Pandanus*, or screw Pines. There were many species endemic to the islands, but many species were found all over India, and they also extended into China and other places in the Malay Archipelago, and a few species were to be found in Australia. Of the twenty-two species which occurred in the Mascarene Islands, twenty were endemic to the islands; their generic characters were exceedingly well marked, and the definition of species was a very difficult matter. An investigation of the whole genus was very much wanted, but this had hitherto been rendered difficult by the want of knowledge of the Mascarene

species. The descriptions of the first author who wrote anything about these Mascarene species were exceedingly short, and just now the confusion in regard to the whole genus was something extraordinary. There were nine species at least endemic to the Mauritius, and in the Bourbon they had record of four distinct species, three of which were peculiar to the island. He had examined the fruits and leaves of these plants, but the leaves afforded very few characters. They were dioecious plants, and the male flowers would furnish them with very good characters for distinction. Three species had been grouped together by their carpels never or at least very rarely being united. Two of these were endemic to Mauritius, and one to Bourbon.

Prof. W. C. Williamson gave an address on his recent researches on the structure of the coal plants, especially *Calamites*, *Lepidodendron*, and *Sigillaria*. He considered that the accurate determination of the true nature of each of the coal plants was of the utmost importance to the theory of evolution. He combated the view which would divide the genus *Calamites* into two, *Calamites* and *Calamodendron*. He described some new forms of *lepidostrophi* or cone-fruits of fossil lycopods, and concluded by showing the remarkable tendency of many of these coal plants to develop into a very uniform type, making it almost impossible to identify small fragments either of their wood or of their bark. Hence it was absurd to attempt to establish genera and species upon such unrecognisable fragments.

Mr. C. W. Peach read a paper *On Circinate Vernation of Sphenopteris affinis, and on the Discovery of Staphylopteris in British Rocks*. Mr. Peach has found *Sphenopteris affinis* in the black shale at West Calder, near Edinburgh, in a series of specimens showing its vernation from the earliest stage till the complete development of the plant; he believed that other observers had described several species of *Sphenopteris* from this one form in its various stages. The genus *Staphylopteris*, which he had also found at West Calder, was well known as occurring in the carboniferous rocks of Illinois and Arkansas. Prof. McNab gave an account of the structure of the leaves in several species of *Abies* (larches), which will be fully illustrated in the *Proceedings* of the Royal Irish Academy.

Prof. Leith Adams described the fossil remains of the Maltese caves, with especial reference to the gigantic land-tortoises, similar to those of the Galapagos and Mascarene Islands, but much larger still. Nevertheless they were very much alike in osteology, so that there had been great difficulty in determining that the species were distinct. Another notable animal was a dormouse as large as a guinea-pig, so numerous that five or six specimens could be obtained out of one spadeful of mould. Among the fossil-birds was a swan one-third larger than any modern one. Altogether 150 terrestrial vertebrates had been found in Malta, and it was impossible that they could have lived in that locality unless Malta was part of a continent.

Mr. Spence Bate, in continuing his report on the structure of the Crustacea, dealt especially with the eyes, pointing out that these organs were in some cases covered by, and received support from, the carapace, and in others they were supported by a jointed peduncle. The chief modifications of the appendages of the head were examined, and they led Mr. Spence Bate to the conclusion that the seven sections of which the head was composed should be regarded as completely different from the other parts of the body.

Dr. W. B. Carpenter reported the result of further researches *On the Nervous System of Antedon (Comatula) rosaceus*, and also read a paper by his son, Mr. P. H. Carpenter, *On the Anatomy of the Arms of Crinoids*. He maintained that the tract of tissue in the axis of the arms, by which motor impulses were conveyed to the arm-muscles, was equivalent to a nerve, although it did not present the microscopic structure of nerve-fibres.

Dr. D. J. Cunningham read a paper *On a Specimen of Delphinus albirostris* which he had procured this spring. Prof. Cohn, of Breslau, made a number of beautiful experiments to show the artificial formation of silica shells.

Prof. Young gave a description of the novel arrangements adopted by him in the new Hunterian Museum. The cases were arranged so that visitors could walk around them on the outside while curators or students were at work upon them on the inside. The cases were to contain skins, skeletons, soft parts, and fossil remains in close proximity, so that the whole of what was known about one series of forms might be brought together, instead of being scattered as usual. The fittings had been made with great skill by Messrs. D. and T. Robertson of Glasgow.

A discussion on spontaneous generation arose on a paper by Dr. Carmichael, of Glasgow, entitled *Spontaneous Evolution*

and the *Germ Theory*. Dr. Carmichael had made a considerable series of experiments, the results of which were generally confirmatory of those of Dallinger and Drysdale, and of Tyndall. Prof. G. S. Boulger read a paper *On Sex in Plants*, giving a comprehensive view of recently-acquired knowledge on the subject.

This department certainly did not produce papers ranging over more than a small portion of the field allotted to it. Some contributions of high merit were made, but in many departments of natural history no sign was made that any work was going on in the British Isles.

Department of Anthropology.

Mr. James Shaw read a paper *On Righthandedness*, expressing the opinion that there was a constitutional reason for the greater use of the right hand. Lefthandedness seemed very mysterious physiologically; it must be far more common than transposition of the viscera which had been supposed to account for it. In several cases of transposition of the viscera, the persons affected had been found to be right-handed. Another paper by Mr. Shaw was *On the Mental Progress of Animals during the Human Period*. In the discussion which followed Dr. Grierson mentioned an instance of intelligence which had come under his own notice. Five years ago a barrel was put up in his garden at the top of a high pole. The barrel was perforated with holes and divided in the centre. In the course of two days two starlings visited the barrel, and returned on the following day, and in about a week afterwards two pairs of starlings came and occupied it, and brought up their young. They were very wild starlings, and readily took flight when any person went near the barrel. In the second year four pairs of starlings occupied the barrel, and they were much tamer than the previous ones, and this last year there were a number of pairs of starlings so tame that they would almost allow him to take hold of them. They had now changed their mode of speaking, for the starlings in his garden frequently articulated words.

Mr. Hyde Clarke read a paper *On the Prehistoric Names of Men, Monkeys, and Lizards*, tending to prove that in early times and by some savage races at the present day, every word which was used as distinctive of man was likewise applied to other animals, but only to those which used their fore feet as hands, or in a distinctive manner. A paper contributed by Herr von Humboldt von der Horck was read by Mr. Hyde Clarke. The author was in charge of an expedition to the polar seas, and sent an account of the Laplanders and people of the north of Europe. He divided the Lapps into the nomadic or mountaineers, and the sea or fish Lapps. The nomads were stronger, healthier, and better developed, and rarely intermarried with the Finns or the Norwegian settlers.

Mr. Hyde Clarke's researches *On the Relations between the Hittite, Canaanite, and Etruscan Peoples and the Early Peruvians and Mexicans* were laid before the department. He believes that they really belong to one family, representing an early culture which became arrested. They had little community with the Semitic or Aryan types. Mr. J. Park Harrison dealt with the origin and meaning of the "Picture Writing" of Easter Island. He said that many of the tablets were gradually getting destroyed, and he called attention to the desirability of acquiring as many of them as possible, and of instituting a careful ethnographical exploration of Easter Island.

Mr. Bertram F. Hartshorne, late of H.M. Ceylon Civil Service, read a paper entitled *The Rodiyas of Ceylon*. The people treated of in his paper were a numerically small race, living in various isolated communities in the hill country of Ceylon. Their caste is the very lowest, and they have from time immemorial been regarded by the Singhalese people with disgust and abhorrence, their very name implying the notion of filth. The popular belief has commonly considered them to be either in some way connected with the Weddas, an aboriginal race of the highest caste, or else to be outcast Singhalese or ostracised Kandyans. There appears, however, to be no real ground whatever for either of these theories—the features of the Rodiyas, as well as their general *physique* and their craniology, marking them out as a separate and distinct race, no less than their customs and language. Their customs are distinguished by peculiar funeral ceremonies, and by sacrifices offered to two sorts of devils in cases of serious sickness; and their language, which is now in one of the last stages of decay, is of unknown origin and development, and can neither be classified as Aryan nor Dravidian. In all probability it represents the remnants of a more complete and extremely ancient language, although it pos-

esses no separate alphabet, nor any literature. The earliest historical mention of the Rodiyas apparently occurs in the year 437 B.C., and they are expressly referred to by name in the year 204 B.C., and again in the year 589 A.D. in the ancient Singhalese chronicles. The condition of the people, however, has at all times been degraded, notwithstanding the fact that the males are invariably possessed of a fine *physique*, and the females are considered to be handsome. The peculiar social disabilities which have been imposed upon the Rodiyas by the uses of ages are now rapidly disappearing with the advance of civilisation, whilst at the same time the idiosyncrasy of the people themselves as well as their customs and their language, is gradually becoming merged in the more modern type of their Singhalese surroundings. The president (Mr. Wallace), in moving a vote of thanks to Mr. Hartshorne, said the Rodiyas were a race of people who, though in a degraded condition, yet possessed physical characters which seemed to show they were intellectually superior to the races who treated them in this manner. This might be another of those examples to which he alluded in his address, of a remnant very fast dying out—a remnant of one of those early higher races which had been overrun and overcome by a lower race intellectually, but more energetic, and had been reduced to an extremely degraded position. It was also a valuable example proving that degradation long continued did not alter to any great extent the physical features of the race. Though they had been for ages in this degraded condition they retained a fine type of face, almost equal to many European forms.

Mr. William Harper contributed a paper *On the Natives of British Guiana*, who were generally said to belong to five tribes, namely, the Arawacks, the Caribs, the Accawoi, the Macuri, and the Warans. Representatives of several other tribes were, however, frequently met with on British soil. These people were merely remnants of a few barbarous tribes found, for the most part, between the Amazon and the Orinoco. It was extremely difficult to obtain any information as to the origin of these tribes; and the general result of the author's investigations was that, though it did not now admit of proof, it was very probable that all the Brasilio-Guarani tribes came from the north, though not at the same time. Of the tribes in British Guiana, the Warans and Macuri had probably been longer in the country than the Caribs, Accawoi, and Arawacks. These tribes differed a good deal from one another in their language, characteristics, and habits, but not in their outward appearance or mode of living. The author suggested that light might be thrown on the origin of these tribes by collecting fac-similes of the rock-writing to be found among them, and comparing them with similar writing to be found in other parts of America, especially in the valley of the Mississippi.

Mr. Kerry Nichols read a paper *On the New Hebrides, Banks, and Santa Cruz Islands*. The natives inhabiting these islands seemed to owe their origin to the same stock from which the western and southern portion of New Guinea and the islands lying immediately to the southward of that country appear to have been peopled. The stock was evidently Papuan, and had, by its numerous and wide-spreading branches, not only extended itself over the islands of the coral sea, but as far east as the Fijis, in which latter country, however, the race had evidently received a great infusion of Malay blood. Whatever opinion might be formed on the identity of the present race, the striking resemblance in person, feature, language, and customs which prevailed throughout, justified the conclusion that the original population issued from the same source, and that the peculiarities and characteristics which distinguish the tribes or communities on different islands had been mainly brought about by long separation, local circumstances, and the intercourse of foreign traders and settlers. Physically considered, these people were a well-built, athletic race of savages, who appeared to inherit, in a very marked degree, all the characteristics of the Papuan race. The men average about 5 feet 6 inches in height, are erect in figure, with broad chests and massive limbs, which in many instances display great muscular development. The colour of the skin was usually of a dark reddish brown, but sometimes it was quite black, and was often covered with a short, curly hair, especially about the breast, back, and shoulders. He saw several instances in the Island of Tanna where the body was almost completely covered in this way. They had well-formed heads, the cranium in the majority of instances betokening a fair degree of mental development. The hair, which formed one of the most remarkable features of this race, was distributed thickly over the head in the form of small spiral curls, and when allowed to grow in its natural way,

had a woolly appearance, and resembled at first glance that of the African negro, but it was in reality much finer and softer. The beard was worn short, and usually trimmed, with a tuft beneath the chin. They shave with the teeth of the shark, an oyster shell, or a piece of bottle glass, and perform the operation with the skill of accomplished barbers. In the northern islands the men went completely naked; but in the southern islands, where the climate was slightly cooler, they affected a scant covering, after the fashion of the primitive fig-leaf. They were fond of decorating the head with flowers and feathers, and of tattooing the face with red and blue pigments, which imparted to them a savage and ferocious look. All things considered, the physical condition of the islanders did not appear to manifest any sign of degeneration. A very complete account of the social and intellectual condition of these islanders was given. The slight idea of religion possessed by the islanders might be described as the most primitive form of Paganism. On some of the islands they worshipped rude idols of wood, while in others they seemed to put implicit faith in imaginary gods who were supposed to inhabit the highest mountain tops. The dread of evil spirits and demons was universal among them. The natives of each island had a distinctive dialect of their own, and even the various tribes inhabiting each island had also distinct and separate dialects.

Mr. W. Pengelly, F.R.S., gave an account of the contents of an urn which had been found in a field near Chudleigh in Devonshire. The urn contained a large number of pieces of pottery supposed to be Roman, and a number of calcined bones which were the bones of goats or sheep. This was the only occasion, as far as he knew, in which the bones of animals had been found in such urns.

Dr. Knox read a paper *On Bosjes Skulls*. One of his specimens had a capacity of only sixty-four cubic inches; the longest measured seventy-four cubic inches. The skulls belonged to the long-headed type, though not of the longest. The skeleton to which one of the skulls belonged, was remarkable for the wedge-like shape of the pelvic bone, which was also very deep.

Dr. Allen Thomson exhibited and described two skulls from the Andaman Isles; and referred to the custom the natives had of preserving portions of their friends' skeletons and wearing them as ornaments. The skulls of their husbands were actually worn upon the shoulders of widows.—Prof. Cleland described the skull of a Sooloo Islander.—Dr. McCann, in a paper *On the Origin of Instinct*, brought forward well-known objections to Mr. Darwin's explanations, referring to the descent of bees, the first birds hatching eggs, &c.

Nearly the whole of one day was occupied by the reading of a paper by Prof. Barrett, of Dublin, *On some Phenomena Associated with Abnormal Conditions of Mind*, on which an excited discussion arose. Many phenomena of mesmerism, clairvoyance, and spiritualism were alleged, and Mr. Crookes, Mr. Wallace, Lord Rayleigh, and Dr. Carpenter expressed opinions which are well known, based on facts witnessed by themselves.

The work done in this department does not compare well with the result at Bristol last year. Scarcely anything of importance was brought forward in prehistoric anthropology. Some good accounts of savage tribes of the present day were given; but otherwise the scientific value of the department is this year comparatively small. The concluding portion of Mr. Wallace's presidential address is perhaps the most noteworthy feature in anthropology, as exhibited at Glasgow.

SECTION E.—GEOGRAPHY.

There were an unusual number of papers of general interest and importance in this as well as in Sections F. and G., and we therefore regret that our space does not permit of reporting them at length.

Mr. Octavius Stone read a paper *On his Recent Journeys in New Guinea*. The island, he said, extended in a south-easterly direction for a distance of over 1,400 miles, having a maximum width of 450 miles and a minimum of only 20. The neighbourhood of the Baxter River and the entire shores to the west of the Papuan Gulf for an average of 100 miles inland were low and more or less swampy, being intersected by water-courses and covered with forests of mangrove trees. This part of the country was thinly populated by the Dandé Papuans, who in consequence were subjected to periodical raids from the adjoining islands of Borgu, Saibai, and Daun, the invaders generally returning victorious with the heads or jawbones of their slaughtered

victims. The only trace of cultivation he saw was 80 miles up the river, where a space of six acres had been neatly fenced round, and planted with yams, taros, sugar-cane, and tobacco. Outside the inclosure were two or three uninhabited bark huts, which appeared to afford shelter to these roving people, in which they prolonged their stay, as game was more or less plentiful. Traces of wild boar and kangaroo were observed in the Upper Baxter. No other large animal was known to exist. They were hunted with the bow and barbed arrow, while the war arrows were poisoned by steeping in the putrid carcase of a victim until sufficiently saturated. The district of the Baxter River contrasted strikingly with the Fly River discovered by Capt. Evans, whose banks for sixty miles swarmed with human beings. Mr. Stone's impression of the western coast was that it would prove a grave to such Europeans as should choose to reside there. This part of the country was inhabited by the Papuan race, a dark race of people, though not so dark as the Australian negro, and one of cannibal propensities. The Eastern Peninsula, on the other hand, was inhabited by the Malay race. Of this race Mr. Stone thought they had come to New Guinea from islands farther east, some of them making the change at a comparatively recent date. This race was far above the savage, both in intellectual and moral attributes. They were cultivators of the soil—each having his own plantation—and strongly opposed to the cannibalism and polygamy which obtained among their western neighbours, the Papuans. The women, too, of the Malay race were not debased as among the dark race, but mixed with the men, with whom they shared the management of public affairs. The Owen Stanley mountains ran through the centre of the country, from south to north, and the east country was on the whole favourable to cultivation, and probably possessed great mineral wealth. It accordingly offered sufficient inducement for colonisation, but colonisation, if attempted, would require to be set about with much previous consideration, owing to the peculiar situation of the peninsula and the circumstances of the people.

Mr. Kerry Nicholls read a paper *On the Islands of the Coral Sea*, which embraces that portion of the Pacific Ocean extending from the south of New Guinea, westward to the coast of Australia, southward to New Caledonia, and eastward to the New Hebrides. The New Hebrides' banks and Santa Cruz Islands, he said, constitute an almost continuous chain of fertile volcanic islands, extending for a distance of 700 miles, between the parallels of 9° 45', and 20° 16' south latitude, and the meridians of 165° 40', and 170° 33' east longitude. Espiritu Santo, the largest island of the archipelago was seventy-five miles long, and forty miles broad. The geological formation of the islands was composed of volcanic and sedimentary rocks. The chain of primary volcanic upheaval might be traced running in a general course longitudinally through the islands always in their longest direction, the axis of eruption being marked by active and quiescent volcanoes. On the north end of the island of Vanu Lava there were extensive springs of boiling water, solfataras, and fumaroles. The hot springs were of two kinds—some were permanent fountains where water was in a constant state of ebullition, others were only intermittent, and the water became heated at certain intervals, when it varied from a tepid degree of heat to boiling point. The physical features of the islands were remarkably bold, and betokened at first sight their volcanic origin. The plains, table lands, and valleys of the mountain region were, many of them, of considerable extent.

Capt. V. L. Cameron, R.N., C.B., read a paper *On his Journey through Equatorial Africa*. Capt. Cameron said that soon after entering the country from the east coast he came to a large plateau, 4,000 feet in height, encircling Lake Tanganyika, and forming the water-shed between the Congo and the streams flowing into Lake Sangora. Another table-land to the south rose to the height of 3,000 feet. The water-shed between the two basins of the Lualaba and the Congo at that part is a large, nearly level country, and during the rainy season the floods cover the ground between the two rivers, and a great portion of it might easily be made navigable. One thing he noticed in Africa was this system of water-sheds, dividing the country into portions, each having its own peculiarity, and also that in each there was a difference in the habits of the natives. Within twenty days he crossed the Nsagara Mountains and came upon a level open country where a great quantity of African corn was grown, the stalks of which rose to the height of from 20 to 24 feet. In this country no animal could live except the goat, the tsetse fly being destructive to all others. The principal geological formation was sandstone. A few marches brought him to Ugogo, an extensive plain broken by two ranges of hills, composed of loose masses of

granite piled together in the wildest confusion. The soil was sandy and sterile. Coming to the country of the Ugari he found a tribe almost identical with Unyamwesi. The principal streams of this district fall into the Mulgarazi. Unyamwesi was the commencement of the basin of the Congo. He believed that the natives of Unyamwesi were of the Malay race. They had crossed a great deal with negroes, and had lost the distinctive colour and distinctive marks of the race, but their features were much the same as the dominant races in Madagascar. Ugari is a large plain nearly as flat as a billiard table. The people here were different from the Unyamwesians; they had not got the same features or the same tribal marks. After passing over the mountains of Komendi, which are an offshoot of the mountains round the south end of Tanganyika, they came to a fertile land, much of it laid waste by the ravages of a neighbouring tribe. All the mountains in that district were of granite. There was there a large quantity of salt and what was remarkable was that the rivers ran perfectly fresh through soil which, when the natives dug wells, gave water which was full of salt. At Uji the people are of a different race from those already described, as they shave their hair differently and have not the same features. Along Lake Tanganyika in some places there were enormous cliffs and hollows of rugged granite lying in loose boulders; in other places the cliffs were of red sandstone, and in others a sort of limestone and dolomite. At one place he saw exposed on the shores of the lake large masses of coal. Passing down to the south end of the lake, he found it regularly embedded in cliffs 500 to 600 feet high, with waterfalls discharging themselves down the face. Travelling along the side of the lake he came to the Lukogo, a large river more than a mile wide, but partly closed by a sort of sill on which a floating vegetation was growing, a clear passage, however, being left of about 800 yards. After proceeding some four miles up the river, Capt. Cameron's boat got jammed amongst the floating vegetation which grows to the thickness of two or three feet, and it was with difficulty the boat was extricated. The Kasongo country was next reached, the principal characteristic of which was the extraordinary trees, of which boats a fathom wide are sometimes made. Crossing the mountains of Bambara he arrived at Mamyumba. Here he found the race entirely different from anything he had yet seen. The houses were differently built, the people were differently armed, dressed their head differently, and there was no tattooing to speak of. The villages were built in long streets thirty or forty yards wide, two or three streets being alongside each other, and a space left between the houses, which were of reddish clay with sloping thatched roof—the only houses of that description he saw in the interior of the country. All the Mamyumba are cannibals. Journeying northwards, but still in Mamyumba, a district was reached where iron was very plentiful, and where large forges were at work. Many of the spears and knives which they turned out looked as if finished off by a file or polished by some means, although all done by hand-forging and patient labour. The Lualaba River was next reached, which is about 1,800 yards in breadth. The southern shore is occupied by a tribe called the Wägenga, who do the whole carrying business of the river, being the only canoe proprietors, who take for pay the products of the country to the different markets. The young women make immense quantities of pottery in the mud and back water, which they exchange for fish. After referring to a country between Nywangi and Loami, where a palm oil grows in great profusion, Capt. Cameron passed through Kilamba, and reached Lake Kigongo. This lake is covered with floating vegetation, on which the people build their houses, cut a space round about them, and so transform their habitations into floating islands, so that when desirable they change the locality from one place to another. Coming to the coast he passed through one of the most magnificent countries in the world to look at, possessing a climate in which any European might live. The Portuguese had been settled in this neighbourhood for thirty years. The whole of this country was just one vast slave field. In the country there was a vast mineral wealth and an ordinary population that with education might be rendered very industrious instead of carrying on a continual warfare against each other for the purpose of obtaining slaves.

An interesting discussion followed.

Col. R. L. Playfair, H.M.'s Consul-General in Algeria, read a paper *On Travels in Tunis in the Footsteps of Bruce*. The paper gave a narrative of the Colonel's observations made in the course of a journey in Tunis over places visited by Bruce about 1763. There had been recently put into Col. Playfair's hand for publication a large number of Bruce's sketches, of which his

Barbary sketches were, he said, the most interesting, forming about 120 sheets of drawings, completely illustrating the archaeology of North Africa. In these circumstances, the Colonel had determined to follow Bruce in his journey, and to satisfy himself as to the present condition of those interesting ruins which were almost unknown to the modern traveller.

Mr. A. Bourden read a paper, the object of which was to show that ready access could be had to the Niger and the African interior from Sierra Leone.

The Secretary, in the absence of the author, read a paper by Lieut. W. H. Chippindall, R.E., containing *Observations on the White Nile between Gondokoro and Ajuddo*. The object of the paper was to establish Lieut. Chippindall's opinion that the oft-repeated assertion that the White Nile could not be navigated higher up than Gondokoro had no warrant in fact. He was sure the White Nile was navigable all the way up to the Albert Nyanza.

A paper was read by Staff-Commander Tizzard, R.N., *On the Temperature obtained in the Atlantic Ocean, during the Cruise of H.M.S. "Challenger"*. Over a great portion of the Atlantic the bottom temperature has this peculiarity—If the depth be less than 2,000 fathoms, we find the temperature at the bottom lower than that of any intermediate depth, but when the depth exceeds 2,000 fathoms, we find that the bottom temperatures are nearly the same as they are at that depth. This holds good for three-fourths of this ocean. In the remaining fourth the temperature obtained at the bottom is much lower than in the other parts, and this fourth is not at either extreme, where there is a large current of surface cold, but occupies the whole of the western portion of the South Atlantic as far north as the Equator. The results of these temperatures may be classified thus: If an imaginary line be drawn from French Guiana to the westernmost island of the Azores and from thence north on the western side of this line, the bottom temperatures at depths exceeding 2,000 fathoms are 35 degrees—that is, taking the mean of all the temperatures obtained which differ but slightly. On the eastern side of this line the bottom temperatures are 35.3 deg., and this uniform temperature appears to extend as far south as Tristan d'Acunha, as the German frigate *Gazelle* obtained similar bottom temperatures eastward of the line joining that island with Ascension to the southward of a line joining Tristan d'Acunha with the Cape of Good Hope. The bottom temperatures are decidedly colder between the eastern coast of South America and a line joining Tristan d'Acunha and Ascension Island; and from the Equator to the southward the bottom temperatures were invariably colder than at any intermediate depth. These temperatures varied from 31 deg. to 33 deg. 5 sec., that is when the depth exceeds 2,000 fathoms, and temperatures of less than 33 deg. were found as far north as the Equator, while a few miles northward this bottom temperature was 35 deg. It therefore appears that in the western portion of the South Atlantic the highest bottom temperature is less than the lowest obtained elsewhere in this ocean, exceeding there the very low result of 29 was found by the *Porcupine* in 1869 between the Faroe Isles and the north extreme of Scotland. The question thus arises as to the causes which confine this cold water to the bottom portion of the western half of the South Atlantic. The examination of the soundings which had been taken in this ocean, combined with the results of their temperature, leads to the conclusion that there is a series of ridges dividing its bed into two basins, one of which occupies the whole of the western portion of the North Atlantic, while the other extends the whole of the length of the ocean on its eastern side, and that the cold water in the western portion of the South Atlantic is owing to there being no obstruction between the bed of this portion of the ocean and the bed of the Antarctic basin, and from the results of the serial temperatures' soundings it would appear that these ridges cannot exceed 1,950 or 2,000 fathoms in depth. To ascertain the thermal condition of the Atlantic (from the surface to the bottom), serial temperatures were obtained in the *Challenger* at 150 positions, observations having been made at each 100 fathoms to 1,500 fathoms in depth, and frequently at, say ten fathoms to 200 fathoms in depth, at each of these positions. An examination of these temperatures shows that between the parallels of 40 deg. N. and 40 deg. S. there is a much larger amount of warm water in the North than in the South Atlantic, and that in the equatorial regions the isotherm of 60 deg. is much nearer the surface than in the temperate zones, but that the isotherms below 60 deg. are at nearly as great a depth at the Equator as in any part of the South Atlantic, especially at the isotherm of 40 deg.,

and that between the parallel of 30 deg. and 40 deg. N. latitude, the isotherm of 60 deg. occupies a depth of 300 fathoms, over an area of 1,200,000 square miles, while the average depth of this isotherm between the parallels of 30 deg. and 40 deg. S. latitude is 160 fathoms; also that the isotherm of 40 deg. which is at an average depth of 800 fathoms across the North Atlantic, between the parallels of 30 deg. and 40 deg. N. latitude, occupies only half that depth in any part of the South Atlantic. This phenomenon may be explained in the following manner:—The power of the sun indirectly heating the water below the surface appears not to extend below 100 fathoms even in the tropics, and this power decreases as the higher latitudes are reached, until a position is attained where the temperature is that of the freezing-point of salt water. As salt water at its temperature of congelation is denser than at any higher temperature, its weight would cause it to sink, and it would in time, did no other cause intervene, occupy the whole of the space in the ocean not influenced by the sun's heat. But in considering the effect of the heat imparted to the surfaces we have also to consider the effect of evaporation and precipitation. In the equatorial regions evaporation is rapid, so that the surface film would become cleared through increased salinity were it not for the increased temperature and large precipitation, as well as to its being transported by the friction of the trade winds and earth's motion to the westward. This surface film, constantly moving westward in the equatorial regions, meets in the Atlantic with an obstructing point of the South American continent, which directs it to the northward, so that the greater part of the water directly heated by the sun's rays in the tropical regions is forced into the North Atlantic. As the salinity of this water is greater than that of the subjacent layers, and its increased temperature only renders it less dense, directly a portion of this temperature escapes in the colder regions of the temperate zone, the surface film sinks and imparts heat to the water beneath. Consequently, the isotherms will be found at greater depths where the heated surface films are constantly descending than when, owing to their being less dense than the subjacent layers, they remain on the surface.

Mr. J. Murray stated some results of his observations on board the *Challenger*—On the *Geological Distribution of Oceanic Deposits*. These deposits were stated to be of three classes—first, those which were found all round the continents and islands existing over the world, without any exception, but which varied according to the places where they were found; secondly, those found at from 200 to 300 miles from the land, consisting of shell and lime deposits, and covering most of the bed of the ocean; thirdly, those existing at other depths, and which were of silicious character. The observations showed that a curious relation existed between the nature of the deposits and the depth of the water. It was also pointed out that in the neighbourhood of volcanic islands, and in no other places, were found large deposits of manganese, coating the shells and other things brought up from the bottom.

Mr. Buchanan submitted a communication of observations of the *Challenger*, bearing upon *The Specific Gravity of the Surface Water of the Ocean*. He also explained the principles on which he constructed a new deep-sea thermometer with which his observations were made.

Professor Porter read a paper *On some Points of Interest in the Physical Conformation and Antiquities of the Jordan Valley*. The general geological structure of the valley was, he said, of lime, and of the same age as the basin of the Sea of Galilee, and its surface was flat. The breadth varied from three to ten miles, extending a little towards the east, and from the nature of its thick alluvial covering, it was of more recent formation than of the mountains, the valley having been at one time apparently a lake, of which the soil was the deposit. The river Jordan as it at present existed, could have had nothing to do with the formation of the valley itself. He recommended to the notice of men of science that geological remains on the site of Sodom and Gomorrah pointed to an explosion of bitumen much later than the ordinary geological formation, and probably within the historic period.

Signor G. E. Cerruti read a paper *On his Recent Explorations in N. W. New Guinea*. After several visits to the islands and part of the mainland on the north, he was in 1869 sent out by Count Menabrea for the purpose of making investigations preliminary to the formation in New Guinea of a penal settlement. He secured at the same time means for turning his expedition to profit geographically. He believed that a great part of the region from the Xulla Islands to New Guinea, and perhaps more to the

north, had been subject to very important volcanic action in an epoch not very far distant, and one could see the work now going on—the western coast showing gradual subsidence. But whatever the origin of the islands, they were now covered with a vegetation which he had not found equalled in luxuriance in any part of the world. He urged in strong terms the colonisation of New Guinea.

This Section was brought to a premature close on Tuesday the 12th from want of an audience. The meetings were held in the Queen's Rooms, at a considerable distance from the University, which no doubt to a great extent accounts for the poor attendance.

SECTION F.—ECONOMIC SCIENCE AND STATISTICS.

Dr. William Jack read a paper *On the Results of Five Years Compulsory Education*. After entering into considerable details as to the working of the system, he concluded that he had established the following points:—1. That the need of the country for compulsory education was a crying need in 1870. 2. That the success of the experiment which has now been tried in Scotland, and in nearly half of England, justifies the very modest advances that have been made by the Government in the bill of the present year. 3. That compulsion has been carried out in one great city with perfect efficiency, and with a very trifling amount of legal process. 4. That there is no agency short of compulsion which can bring Ireland on a level in popular education, with her sister countries. A very interesting discussion followed the reading of this paper.

Mr. J. Heywood, F.R.S., read a paper *On the Memorial of Eminent Scientific Gentlemen in favour of a Permanent Scientific Museum*. He advocated the placing on a permanent basis an institution similar to the Loan Scientific Institution now open at South Kensington.

The Rev. Dr. M'Cann then read a paper *On the Organisation of Original Research*, in which he advocated an exceedingly elaborate system for carrying out the object in view.

After some discussion in which Dr. Jack, Professor Hennessy, of Dublin, and others took part, Mr. Heywood submitted the following resolution—"That this Section approve of the maintenance of a scientific museum in London, containing scientific apparatus, appliances, and chemical products."

Sir George Campbell, in summing up the discussion, said he should support this motion, and he also agreed with Dr. M'Cann that there should be a national system of scientific education.

The motion was unanimously passed.

An important discussion took place in this Section *On the Civilisation of South-Eastern Africa*, caused by the reading of a paper on the subject by Mr. Stevenson.

SECTION G.—MECHANICAL SCIENCE.

This Section met under the presidency of Mr. Charles W. Merrifield, F.R.S., who in his address spoke of our shortcomings in those subjects of instruction which are the necessary preludes to mechanical science. He urged the importance of physical science as that which had given us command over the material powers of nature, and which alone could enable us to keep pace with other nations in industrial competition, and to maintain the health of crowded populations. With their populations, which had more to fear from war and famine than from want of elbow-room, political and historical knowledge in the governing class was more important than exact knowledge in the administrative class; but as the population thickened, the latter assumed more importance; and while he did not think political wisdom would ever lose its value, he thought it only a part of such wisdom to recognise that in such communities as ours the spread of natural science was of more immediate urgency than any other secondary study. One of the obstacles to the spread of science and to our national prosperity he took to be the undue preference given to literary over natural knowledge, and in particular the sacrifice of mathematics to classical study in the secondary schools. Apart from the general fault of giving too low a place to mathematical teaching, a great fault was our not paying sufficient attention and sufficiently early attention to mechanical and geometrical drawing. He concurred with a remark of Professor Fleeming Jenkin that descriptive geometry was not what was wanted. A much more important exercise of geometry, and one more immediately useful, was the geometrical representation of arith-

metic, such as was seen in diagrams of thrust, pressure, speed, and so forth. But this would take care of itself provided linear drawing were taught sufficiently early. Passing on to discuss certain points connected with the crowding of the population, he remarked that the real problem of civilization had been to render life tolerable in large aggregations, and that this problem was yet only partially solved. Among the difficulties of town life he reckoned—(1) the insufficient supply of fresh air; (2) the mere proximity of persons facilitating the spread of contagious or infectious disease; (3) the getting rid of excreta or waste products; (4) a wholesome water supply to be provided and kept pure.

Mr. Baldwin Latham read a paper *On Hydro-Geological Surveys*, in their bearing on health. He dwelt on the importance of ascertaining the sub-water course, and making certain that the well was on a higher level, so that it could not be contaminated by cesspools or other pollutions. These surveys showed the absolute necessity of sewers being made watertight.

Mr. W. J. Millar read a paper *On the Strength and Fracture of Cast-Iron*. The author described the results obtained in testing cast-iron bars 36 inches span, 2 inches deep, and 1 inch broad. The bars usually broke with straight fractures, but occasionally curved fractures were observed. The average breaking strength of 29 bars showing straight fractures was 3584 lbs., the average strength of 25 bars showing curved fractures was 3551 lbs. Some results of "set" and deflection were given, showing that for successive applications of the same load, 2800 lbs., there was a decrease of set. The principal object aimed at by the author of the paper was to show the relation existing between form and position of fracture, straight fractures taking place at or close to centre of span, and curved fractures occurring at points more or less removed from centre of span.

Sir William Thomson read a paper *On Naval Signalling*, in which he advocated the use on board ship of the fog signalling system instead of the flag system now in use. His method is simply this—to signal according to the Morse telegraphic code by means of two sounds of slightly different pitch. For the long signals he would take a grave note, and for the short signal a less grave note, or what he might call an acute and a grave note for the dot and the dash. Sir William Thomson then gave several signals to show the efficacy of the plan he proposed, and he maintained that the shortness of the time required to make flag signals was far less than could be attained by the phonetic method. Long before the signal flags could be hoisted, the order would be given and read by every ship, and repeated by the different ships in order, back to the admiral. Two sounds of different pitch made in rapid succession was all that was necessary, and to accomplish this all that was required was two steam whistles, each with a different note.

Many other papers of great value were read both in Sections F and G, but as they were mainly technical, or very special, our space prevents us referring to them in detail.

THE CHALLENGER EXPEDITION¹

THE task which I have undertaken this evening—to give a general sketch, however slight, of the work and results of the *Challenger* expedition in the space of a single lecture—is by no means an easy one, for two reasons. The various lines of inquiry bear on so many different subjects, and these dovetail into one another in such a complicated manner, that it would take many hours to explain them even in the most superficial way. The other reason is that the observations which were made during the *Challenger* expedition have only as yet been very imperfectly examined, and only half digested, owing to want of time, and the great collections in natural history which were brought home in the ship have been only glanced at, and it is therefore scarcely safe for me to use either the observations or the collections as the bases of generalisation. I must therefore this evening, in this address, only be regarded as giving a most elementary idea of the objects of the expedition and its results, and what I say must be regarded as preliminary, and subject to further reconsideration. Still, some new and remarkable facts and phenomena which have hitherto been unknown, or only vaguely guessed at, are sufficiently definite, and I will devote the short time at my disposal to the consideration of one or two of these. The superficial area of this world of ours is about 197,000,000 of square miles, and of these about

140,000,000 are covered by the blue sea at an average depth of 2,500 fathoms—about 15,000 feet. This vast region under the sea has not until comparatively recently excited much curiosity. It seemed to be practically inaccessible, and certain hasty and incorrect assumptions in regard to some of its conditions had reduced it to a barren uniformity and divested it of any interest. The laying of deep-sea cables for the purposes of ocean telegraphy, by bringing to light certain phenomena which threw a doubt upon previous conclusions, stimulated inquiry, and gave rise to new speculation; and the systematic scientific exploration of the depths of the sea by several special exploring expeditions put our knowledge upon a totally different footing. We now know that the sea covers a vast region which is to a certain degree comparable with the land—a region which has its hills, valleys, and great undulating plains; that it has its various soils—widely different materials laid down and accumulated in different places; that it has its climates, whatever the very exceptional conditions of those climates may be; and that it has its special races of inhabitants which depend, like the inhabitants of the rest of the world, upon the conditions of climate and on the nature of the soil for their distribution.

The *Challenger* expedition was despatched on a very special errand—to investigate the physical and biological conditions of the great ocean basins. And, under this general heading, certain more minute instructions indicated the particular questions, physical and biological, which were specially to engage our attention. We were instructed throughout our long course, which extended round the world and traversed the Atlantic and Pacific Oceans and the Southern Sea so far south as it was possible to go without running the risk of being entangled for a winter in the ice—a contingency for which we were not prepared—to select certain stations at convenient distances, and at each of these to determine certain points. We were to determine, in the first place, the exact position of the station; then, with the best appliances at our disposal, we were to determine the precise depth; we were to bring up by means of the sounding apparatus a certain amount of the material of the bottom for microscopical examination and for chemical analysis; we were to bring up a specimen of the water from the bottom for analysis and physical examination; we were to determine the bottom temperature with accuracy; and we were to determine the temperature of the sea at different levels from the surface to the bottom; we were to get specimens, if possible, of the sea water from various depths. Lastly, we were to endeavour, by the use of the trawl or dredge, or any other instrument which might be suitable, at each station to procure a fair sample of the creatures which inhabit the bottom, and in this way to get, if possible, a general idea of the fauna inhabiting the depths of the sea. The instructions of those in charge of the scientific departments in the *Challenger*, both naval and civilian, did not, however, by any means end here. The officers had been selected in order that they might study by the light of their own previous experience the bearings of those various data upon one another, and this was a very serious addition to the work of the expedition. It was found necessary, in order that this might be carried out to its fullest extent, that the instructions given by the Admiralty should be comparatively flexible, and that the details of the working of the ship should be left to a certain extent to the captain of the ship and to the director of the scientific staff, so as to enable them to deviate from any definite line or course when it became desirable for any purpose that they should do so. I have only to add that the equipment of the vessel was such as to leave very little to be wished for, and that the liberal arrangements of the Admiralty, which were admirably carried out by the Hydrographic Department, worked in the most satisfactory way. The *Challenger* left Sheerness on December 17, 1872. She crossed the Atlantic four times during the year 1873, and along a course of nearly 20,000 miles she established 150 observing stations, at each of which, with few exceptions, all the required observations were made. In 1874 she went southwards from the Cape of Good Hope, spending nearly a month among the southern ice, and dipping within the Antarctic Circle, as far as she could with safety, considering the lateness of the season and her unprotected condition. She then traversed the seas of Australia and New Zealand, and made some most interesting observations among the islands of the Malay Archipelago. She arrived at Hong Kong on November 10, having run a course in the year 1874 of upwards of 17,000 miles, along which sixty-six observing stations had been established. In 1875 she traversed the Pacific, with a course of about 20,000 miles and 100 stations; and in the early part of the present year she crossed the Atlantic for the fifth time, filling up here and there blanks in her former

¹ Report of Address given at the Glasgow Meeting of the British Association, September 11, by Sir C. Wyville Thomson. Revised by the Author.

observations brought out by increased experience, and reached England on May 24, 1876.

The cruise on the whole has been singularly fortunate, and it has only been in very unusual circumstances that we have been prevented by the weather from doing our work. The health of the party has been exceptionally good, and the loss by death small. Two misfortunes only befell us in any way sufficiently grave to affect the success of the expedition—one was the death of one of the most zealous and most promising of our civilian staff, Dr. von Willemoës-Suhm, which for long through a gloom over our little party; and the other was the recall of Capt. Nares to take command of the Arctic Expedition. Capt. Nares had acquired, to a remarkable degree, the esteem and confidence of all on board, and although we could not but feel that no other selection of a leader for the Arctic Expedition could have been made in any way so satisfactory, still the fact remained that by the loss of his experience we were greatly crippled. We all trust that he and his bold companions may now be in safety and nearing the goal of their hazardous enterprise; and I am sure that, with the exception of his wife and children, none so earnestly pray for his welfare as his old comrades of the *Challenger*.

Before endeavouring to sketch one or two of the general results at which we have arrived, I wish to give a few words of explanation. I shall have to bring before you various matters which to you may appear novel, but many of these are not entirely original—many have been shrewdly hinted or guessed at from time to time, and many isolated observations have furnished clues which have been eagerly seized by students and made the bases of speculations more or less touching the truth. It is only the unexampled opportunity which we have had at our command which now enables us to place them before you in a connected form, and with a completeness which in some directions at all events precludes the possibility of grave error. It would be impossible for me on this occasion to acknowledge individually the debts we owe to our predecessors, but I must do so in one or two instances. The American Coast Surveyors commenced this work about the same time that we did, and their results are of the greatest possible value. I have only lately become acquainted with a most thoughtful and suggestive paper by Prof. Williamson, which was published so far back as 1847, in the *Transactions of the Manchester Philosophical Society*, in which the origin of organic deposits—one of the most important points which we have to consider—was worked out with great care and skill. In 1869, 1870, and 1871 the observations made in the *Lightning*, *Porcupine*, and *Sheerwater* completely revolutionised our ideas of many of the questions involved. I shall not, however, consider it necessary to quote continually the speculations of my colleague, Dr. Carpenter, on the physics of the ocean, to which, however widely I may differ from his conclusions, I attach a high value; nor the investigations of Mr. Gwyn Jeffreys on the distribution of marine animal forms; for these two gentlemen must be regarded as members of the firm. Among the many points of interest which engaged our attention there were three more especially prominent, and, if possible, I will confine myself to these three, so as to bring my work within a certain limit. The first of these

the contour of the bottom and the nature of the deposits now being formed upon it; the second is the more difficult question of the distribution of deep-sea climate; and the third, which is perhaps the most interesting and curious, is the nature and distribution of the peculiar races of animals which are now found at the bottom of the sea. I shall take up these three points in succession, and endeavour very briefly to give an idea of the condition of knowledge with regard to them when the *Challenger* started, and the light which her observations have been enabled to throw upon them.

I need scarcely go into great detail with regard to the contour of the bottom, for the question is mainly a hydrographic one, and would involve use of numbers which would be scarcely suitable for a public lecture. As I have said already, the average depth of the ocean is somewhere about 2,000, or probably 2,500 fathoms. A very large portion of the ocean has a depth somewhat less than this, and a depth of 2,000 fathoms appears to be common. Where it is 2,500 or 3,000, we would appear to be going into submarine valleys, with the exception of the North Pacific, where there is an enormous extension of water of great depth, in many cases going beyond 3,000 fathoms. In the Atlantic, a great part of the northern portion has a depth of about 2,000 fathoms, with a middle ridge which passes down from Greenland, and includes the various groups of islands and single islands to Tristan d'Acunha, and probably beyond it. In

the South Atlantic, on each side of this ridge, which is there called the "Dolphin Rise," in compliment to the American ship which first surveyed it, there is a trough which runs to a considerable depth, usually down to 3,000 fathoms, and these form marked depressions roughly parallel with the axes of the South American and African continents. I will frequently allude to the Atlantic, as I have no time to enter into detail with regard to the rest of the seas, and we had the best opportunity of working it. Now, the bottom of the sea is covered with certain deposits. The whole bottom of the sea, so far as we are aware, is gradually receiving certain accumulations, and these accumulations are giving rise to formations which we look upon as the rocks of the future. We know by our knowledge of the science of geology that the whole dry land, as we have it at present, is composed—with the exception of certain volcanic rocks, which may be, in many cases, metamorphosed sedimentary rocks—of stratified beds laid down at the bottom of the sea. We know that the material of these beds is to a certain extent derived from the gradual disintegration of the land, and we look upon the sea as the great restorer of the solid material which is to form future islands and continents, as the bottom of the sea becomes raised up at some future time above the level of the ocean. Now, the whole of the sea-bottom is receiving these deposits, and it was one of our great objects in the cruise of the *Challenger* to determine what these deposits are, under what laws they are being laid down, and what the relation of these modern deposits may be to the ancient deposits, which form the solid crust of the earth. We were well aware that there was a perpetual disintegration of the land going on by rivers and by the action of the sea round its coasts, and that the material worn off the land was being carried away by the ocean and laid down at some distance from the land, and that the material was being selected and arranged according to some definite laws. Accordingly, when we came to test this, we were not surprised to find that the *debris* of the land extended for some hundreds of miles from the land out to sea. We found clays being formed, and various deposits, differing according to the material from which they were derived, and mixed up with the *debris* of animals living in the places where these deposits were being laid down. Within a certain distance of the land we found the deposits formed to a great extent of this peculiar shore material.

Many years ago it was determined by observation, even previous to the soundings for the first Atlantic cable, that over a great part of the North Atlantic a very remarkable deposit was being laid down—a deposit now known as *Globigerina* ooze. This deposit consists of the shells of minute *Foraminifera*, principally belonging to one genus—the genus *Globigerina*. This, as we found it in these deposits, was a small chambered shell extremely minute, about a millimetre in diameter, and these shells were found in enormous quantity. When dry, the ooze was something like fine sago, with little round shells falling from one another, and showing that the deposit was formed almost entirely of such shells. Some other genera were mixed with them, but the great mass were *Globigerina*. When we took up by any means material a little below the surface of the sea-bottom we found the *Globigerina* shells were becoming broken and compacted together so as to form a close and nearly amorphous mud, in which there were very many complete *Globigerina* and many pieces of the same. The whole of this deposit was composed almost entirely of carbonate of lime, and the only rock which this could possibly form was a limestone. It therefore appeared that over a very large portion of the North Atlantic, and over many other parts of the world where these observations had been made, this limestone was being laid down. Further observations showed that the chalk was composed of very nearly the same material, and the analogy between these modern formations and the chalk became very apparent. During the voyage of the *Challenger* we had many opportunities of bringing up this modern chalk, and the question which was always before our minds was one which had been mooted before we started—Where did these creatures live—did they live upon the bottom of the sea? or did they live on the surface, their shells falling to the bottom after death? Until lately none of these animals, or very few, had been found alive upon the surface. It was our general impression that they lived on the bottom, where we found their dead shells. Mr. Murray, one of my companions in the *Challenger*, has paid particular attention to the structure of the material brought up from the bottom—its composition, and the sources from which it was derived. He worked the tow-net and the sounding apparatus together during the voyage, and came to a decided conclusion, one to which we are absolutely forced to

agree with him. The tow-net upon the surface, and particularly at a little below the surface—that is to say, to the depth of a few fathoms, or even to a hundred fathoms—takes enormous numbers of these *Foraminifera*, which make up the *Globigerina* ooze alive. The *Globigerina* themselves, in many seas, are most abundant, and they present characteristics totally different from the shells as we find them below; so that I think there cannot be the slightest doubt that these shells live on the surface, and a little below the surface, and that the whole material at the bottom composed of these shells is derived from the surface. When we find these shells at the bottom they are little globules all united together, and forming a little compound mass of globules. These are rough on the surface, and perforated with minute pores. The cavity of the shell contains a little reddish material, which, at first, we were inclined to suppose was the remains of the body of the animal. When the *Globigerina* was found on the surface, the shell was of the same form, but instead of being white and opaque, it was perfectly clear and transparent. A raised frill on the shell forms a hexagon round each minute pore and runs into six points, and from each point a long spine projects—in fact the shell bristles with long spines running out in every direction, the axes of the spines on each chamber meeting in the centre of the chamber. The shell has a little animal in the interior of it, and that animal consists of a particle of gelatinous matter like the white of egg, and when alive this matter runs out of the holes on the surface of the shell to the end of each of the spines, where it absorbs minute particles of organic matter floating in the water. The *Globigerina* seem to be of the same specific gravity as the water, their weight being reduced by large oil-globules scattered in quantity through their substance; they exist in myriads on the surface, while they are perpetually dying and sinking to the bottom. Finding them so abundant in a living condition on the surface or a little below, and finding none living at the bottom, there seems to be little room for doubt that the *Globigerina* ooze is due simply to the accumulation of the dead shells of the inhabitants of the surface and of moderate depths. We should therefore at once come to the conclusion, if this be true, that the formation which arises from their accumulation ought to be as universal as they are themselves. Singularly enough, this is not the case, and this is one of the most curious points which we have determined. When we go to a depth of about 2,000 fathoms we find that the shells at the bottom are becoming, as it were, rotten or yellow, they have not the same white clear appearance which they had in shallower water, and if we go to a depth of 2,500 fathoms or so, we find no shells whatever, but that the bottom consists of a homogeneous red mud, which, instead of consisting of carbonate of lime, is formed of the materials of ordinary clay. Now, as a very large portion of the sea is below 2,000 fathoms in depth, probably by far the greatest portion is being now covered by red clay, and not by calcareous formations. The question at once arises, How is it possible that these calcareous formations are stopped at a certain point and replaced by red clay? There is no doubt that the calcareous formation is arrested by the carbonate of lime being in some way or other removed from the shells of these creatures. When we come to a certain depth the carbonate of lime is dissolved, and we have a fine red clay instead. The cause of the removal of the carbonate of lime is as yet rather obscure. We were at first inclined to believe that it is removed by excess of carbonic acid in the water. If the water contained an excess of this acid it would dissolve these shells, and it is just possible that the excess of carbonic acid in these depths may remove the carbonate of lime. We also find a large quantity of sulphate of lime dissolved in the sea, and it is just conceivable that a considerable amount of sulphurous acid may be percolating through the crust of the earth at various places, and that it may be converted into sulphuric acid, which would dissolve the carbonate of lime. But whatever be the reason there cannot be the slightest doubt that on reaching 2,000 fathoms depth the lime is gradually removed, and we have the red clay. There is another important and curious question arising—namely, where does the red clay come from? The red clay consists of the silicate of alumina and iron. This compound does not exist in any quantity in the shells in that particular form, and there is no doubt that some complicated changes taking place in the sea at this moment are producing this silicate of alumina and peroxide of iron. There is one very remarkable thing which has been observed by Mr. Murray and Mr. Buchanan, who have been watching this matter with great care, and that is that all over the sea there is a large quantity of pumice. Volcanoes—either sub-aerial or sub-marine—either exposed to the air or under

the water—are perpetually throwing out material from the crust of the earth, and the pumice, which is the froth of the lava—lava divided minutely and containing bubbles either of steam or air—is very frequently so light as to float freely in water; and almost wherever we were, in all parts of the world, we found that particles of this pumice had been caught by the sea, and so moved about in currents slowly over the surface of the ocean. In almost all parts of the sea, the trawl or the dredge brought up bits of pumice which had been waterlogged and had fallen down to the bottom, probably after swimming or floating about for a very great length of time. This pumice was constantly in various stages of decomposition, and its decomposition like that of all felspathic minerals must result in the production of a clay. It is very certain now that these calcareous formations which are being produced by the animals floating upon the surface of the sea and falling to the bottom, and there accumulating, are by no means universal, but that besides these there are huge formations of clays which are capable of giving rise to important formations of schists being produced at the bottom of the sea at the present day. Over the whole bottom of the Pacific, or a very large part of it, we find red clay, and particularly in the North Pacific, where there is a great depth of water. The red clay has all through it nodules, which vary from the size of sago or a canary-seed to the size of a child's head or an orange, composed of nearly pure peroxide of manganese. These are found in enormous quantity. The trawl sent down to the bottom in those regions brings up masses of concretions, much resembling lumps of the mineral known as *wad*, almost all of which contain as a kernel in the interior a fish's tooth, or a little bit of sponge, or some fossil of some kind, which has formed the nucleus round which the manganese has accumulated. This is altogether a most peculiar and novel observation. In the Atlantic and all over the bottom of the sea we find manganese in minute bits, but in the North Pacific particularly these pieces are in very great quantity and attain a large size. This is a phenomenon which we are as yet unable to explain, and I do not know that there is any analogous instance in any of the older formations.

Along with the *Foraminifera* we have living in the sea a great number of extremely beautiful little organisms, which are known under the name of *Radiolarians*. Instead of these having calcareous shells, they have silicious shells—sometimes external, sometimes internal, but very generally presenting extremely beautiful forms. The *Foraminifera* appear to live mainly upon the surface, or a little below it. In regard to the *Radiolarians*, it seems to be somewhat different, for when the tow-net is dragged along the sea even at the depth of 1,000 fathoms, we find that the number of *Radiolarians* increases, and that the size of the specimens of the species which are found on the surface is rather greater; and many forms occur at those great depths which are not found on the surface at all. Therefore we are inclined to believe that the *Radiolarians* live all through the sea, and down to its greatest depths, which may be something like five miles. Now, you can easily understand that these things, living in this way, add considerably to the formations which are taking place at the bottom. We even found a formation which has been called by Mr. Murray *Radiolarian ooze*, on account of its consisting almost entirely of the remains of *Radiolaria*. The mode of formation of this ooze is peculiar; it seems that the *Foraminifera*, living only near the surface, have their shells entirely dissolved before they reach the bottom; the red clay is laid down as usual, whatever may be its source; but the shells of the *Radiolarians*, living throughout the whole of the vast depth, are so numerous as entirely to overcome and mask all the other constituents of the bottom. This formation, however, only occurs at very extreme depths, and it is therefore apparently in patches at the bottom of the sea. In the Southern Sea, where the depth is not so great as the Pacific or Atlantic, we find that the surface, instead of being covered with *Radiolarians* is covered with a set of minute plants which have silicious coverings. Those plants are living on the surface in enormous quantity, and consequently dying on the surface. And when you drag the dredge or trawl over the bottom it comes up with a white matter, which looks at first extremely like chalk, though it is formed entirely of silica. There are many other points of great interest connected with these recent deposits, but my time will not allow me to refer to them. I will, therefore, now pass on to the second question of special prominence—the climates of the sea.

The temperature at the depth to which I alluded—namely, 2,500 fathoms—is very low. Over the whole bottom of the Pacific and the Atlantic, and those portions of the Southern Sea which

we have examined, the temperature is usually a little above the freezing-point. Down in the valleys it sinks to perhaps pretty near the freezing-point in some places, and in some very few places it sinks a little below it, but it is only in one or two places in the Atlantic and Pacific we find such extremely cold water. Over the elevations the temperature is somewhat higher; but in the Atlantic and the Pacific, as a rule, the rise of temperature on the ordinary elevations of the bottom of the sea is not above two or three degrees. The temperature of the bottom of the sea is, therefore, as a rule, a little above the freezing-point. When we examine the temperature of such an ocean as the Atlantic, from the surface down to the bottom, we find that it gradually falls. On the surface its height is according to the season of the year, according to the latitude, and according to the heat of the sun at the locality observed, or at that from which the surface-water is immediately derived. The temperature often rapidly falls for a certain distance and then it more gradually falls to a depth of about 500 fathoms, when it has a temperature of something like 45° . That is a very general temperature for a depth of about 500 fathoms. From that point downwards the temperature very slowly and gradually falls, and it falls till it reaches a temperature of 37° or 34° , or, as I have said before, sometimes below the freezing-point. Now, the consequence of this is that we have a very uniform as well as a very low temperature at the bottom of the sea, and we shall see shortly the result of this on the distribution of animal life. It is a uniform temperature, but it is a temperature which varies within certain limits. The question comes—Whence does the ocean derive this peculiar temperature? and this is a question of very great difficulty, and one which I have not to-night sufficient time to go into in detail, but I shall merely give you a general idea of the impression which is on our minds after the observations of the *Challenger* with regard to the sources of temperature in the Atlantic. The surface, as I have said, is affected by the heat of the sun, and by the conditions of the latitude down to perhaps about 500 fathoms. It is also very greatly affected by currents which are moving through the sea, and which are mixing water of different temperatures, and bringing water of different temperatures from different places. There is one set of currents which is particularly marked and which tends to spread warmth over the surface of the northern and southern seas, and modify the ocean temperatures. These are the great currents which are running from east to west driven by the trade winds blowing along the equatorial region and driving before them the equatorial water. They are met by the great continents—one is met by Cape San Roque in South America, in the Atlantic, and against Cape San Roque it divides: one portion going northward and another southward. In the Pacific the current is met by the coast of Asia, and in the same way one portion runs northwards and the other southwards. Thus warm water, being driven to the north and south, becomes mixed with colder water, and the temperature is modified and ameliorated by it. It is likewise affected by other currents which are produced by various reflections against coasts and other obstacles. In this way we have water moving about on the surface and conveying temperature from one place to another, and rendering the temperature of these upper 500 fathoms extremely irregular. In the Atlantic we find that from this point—about 500 fathoms—to the bottom the temperature steadily decreases until it comes down to near the freezing-point, no matter the surface-temperature or the latitude. We have come to the conclusion that this great mass of water is moving from the Southern Sea, and there seems to me to be very little doubt—although this matter will require to be gone into carefully—that the reason why this water is moving from the Southern Sea in a body in this way is that there is a greater amount of evaporation in the North Atlantic and over the Northern Hemisphere generally, than there is of precipitation, whereas it seems almost obvious that in the Southern Hemisphere, in the huge band of low barometric pressure round the South Pole, the precipitation is in excess of the evaporation. This is an extremely simple way of accounting for this mass of cold water which it has been hitherto found impossible to account for on any reasonable theory.

There is a minor phenomenon connected with this grand system of circulation which passes partly through the atmosphere and partly through the ocean, which is extremely pretty, and of this I will endeavour to give you a single illustration; and in order to understand it fully I will ask you to imagine for a moment a terrestrial globe and the relations in volume and position which the oceans and the continents bear to one another. You remember

the vast accumulation of water round the South Pole, and in the South Pacific; and the "land hemisphere" almost in the centre of which we now stand, with the two great gulfs, the Pacific and the Atlantic running up into it, almost cut off by land and shallow water to the northern end, but opening widely to the Southern Sea. Now imagine the depth along a line joining Cape Horn with the Cape of Good Hope to be 3,000 fathoms, the bottom temperature being 30° , and the temperature at 2,500 fathoms 32° , and suppose a continuous barrier to extend between the two capes to the north of this line, rising 500 fathoms from the bottom; it is clear that if the movement of the mass of cold bottom water be constantly from the south to the north, no water colder than 32° can ever enter the Atlantic, and however deep portions of that ocean may be, water under that temperature can never be found in it to the north of the 2,500 fathom barrier.

Although this is an imaginary case, at least one which is scarcely in nature so simple as I have represented it, we find the same law acting perpetually. In various parts of the world there are little isolated seas, and circumscribed basins of the great ocean, surrounded by such barriers, and we can tell at once the height of the lowest part of the barrier by the temperature of the bottom-water of the basin, for we know that it must correspond with the depth at which the like temperature occurs in the outer ocean from which the basin derives its supply.

I have now only a few minutes left to refer to the last of the three questions selected for consideration, the distribution and nature of the deep-sea fauna. The deep-sea is by no means barren, but on the contrary a fauna very remarkably constituted and comparatively rich, is universally distributed even to the greatest depths. It was our impression that when we examined this fauna we should find it very analogous to that of the ancient chalk, for we believed, and we believe still, that the deposition of chalk has been going on continuously in various parts of the ocean, from the chalk period to the present time. In this expectation we were to a certain extent disappointed, for the species found in the modern beds are certainly in very few instances identical with those of the chalk or even with those of the older tertiary. But although the species, as we usually regard species, are not identical, the general character of the assemblage of animals is much more nearly allied to the cretaceous than to any recent fauna. You have in the Clyde district some extremely interesting little localities—one for instance in Loch Fyne near Inverary, and another at Oban—where animals are found in shallow water which are usually only found in deep water, and other animals which are chiefly confined to the Arctic Seas. Prof. Edward Forbes called these animals *Boreal outliers*, and believed that the little basins in which they occur in this country—which are always cherished dredging spots for naturalists—are spots where, owing to the configuration at the bottom and to other causes, patches of the old fauna have been entangled and retained at the close of the glacial period.

Here and there on the surface of the earth we seem to have, in like manner, what we may call *Abyssal outliers*, spots where, during some process of elevation, the abyssal fauna has been caught and kept at an accessible depth. Such spots occur off the coast of Japan, near Yokohama, at various places among the Philippine Islands, off the coast of Portugal, and off the north coast of Scotland, and from each of these strange and beautiful things were brought to us from time to time, which seemed to give us a glimpse of the edge of some unfamiliar world. Among these were the lovely and wonderful *Euplectellus*, and glass-ropes *Hyalomenas*, and bird-nest-like *Hollenas*, and many others of the hexradiate order of sponges, the representatives, and no doubt the descendants, of the *Ventriculites* of the old chalk; and the graceful sea-lilies belonging to the *Pentacrinida*, and the *Apiocrinida*, whose aspect carries us back at once to the clays of the Lias and the terraced limestones of the Jura.

The fauna of the deep sea is wonderfully uniform throughout, no one who has once seen it can fail to recognise this general uniformity, whether he examines it in the middle of the Pacific, in either trough of the Atlantic, or in the Southern Sea; and yet, although in different localities the species are evidently representatives, to a critical eye they are certainly not identical, and I believe that one of the most important lines of inquiry which have been opened up to us by these investigations is the range and amount of variation, or possibly the passage of one apparent species into another over this vast area, remoteness in space being, when we consider the conditions of migration with the accompanying change in surrounding circumstances, equivalent to lapse of time.

NOTES

THE Committee for the National Monument to Alexander von Humboldt publishes a report on the proposal to erect in front of Berlin University buildings statues to the brothers Wilhelm and Alexander von Humboldt. On the occasion of the 100th anniversary of the birth of the latter in the year 1869, a number of Berlin notabilities met for the purpose of organising a public memorial to the great scientific explorer at the expense of the German nation. A committee was chosen, whose labours were crowned with such success that a sum of nearly 100,000 marks was soon obtained. At the request of the committee to allow the statue to be erected in the University grounds, the Senate stated that they could only give their consent if at the same time a similar statue were erected to Wilhelm von Humboldt, the statesman who, as councillor to King Frederick William III., had an essential hand in the erection of the University. It was then resolved to erect the statues one on each side of the gate which separates the front garden of the University from the Opernplatz. On each side of the middle gate a niche will be made, and in these will the statues of the illustrious brothers be placed. As there was some difficulty as to the means for erecting the statue of W. von Humboldt, the Emperor was appealed to, and he has promised to endeavour to get it erected at the cost of the national purse. Thus then Berlin will soon possess two new statues in her Unter den Linden, and the German people will have paid a debt of gratitude long due to two of her noblest sons.

THE well-known physicist, Wilhelm Edward Weber, the last of what was known as the "Göttinger Sieben," celebrated on August 26, in Göttingen, his Doctor's Jubilee. Weber was born October 24, 1804, at Wittenberg, a brother of the physiologist and anatomist, Ernst H. Weber, with whom, in 1825, he laid, in the wave-theory, the basis of the new Optics and Acoustics. In 1831 he became Professor at Göttingen, and in 1837 resigned his chair; with him also protested against the abrogation of the Constitution Professors Albrecht, Dahlmann, Ewald, Gervinus, Jakob and Wilhelm Grimm, who, of course, also with him resigned their chairs, and with him went into exile. In the year 1849 Weber was restored to his chair, and has just celebrated his Doctor's Jubilee in full vigour of mind, and active as ever in scientific and literary work.

MR. J. COCKBURN, of Darn Hall, Eddleston, N.B., on the night of the 23rd, when taking a photograph of some of the stars, saw the brightest meteor that he has seen for two years. The time was 9.51 P.M.; it lasted about 1½ seconds, and left a train which was visible fully half a second after the disappearance of the meteor. The colour was a darkish green, and the train was orange. Its course was from above α Lyrae across the Galaxy towards Aquila. It disappeared before it had quite crossed the Milky Way. Dr. J. E. Taylor, of Ipswich, writes that on the night of the 24th a meteor fell there about 6.30, directly over the planet Saturn. The path described by the meteor was about one-sixth of the sky. Dr. Taylor never saw one so brilliant. The meteor seemed to burst before reaching the horizon, as if it had exploded. For nearly ten minutes the line of white cloud the meteor left behind it was visible, until at length it broke up into patches and drifted away. This same meteor was seen over a wide extent of country—at Broadstairs, West Deeping, in Lincolnshire, Ipswich, Walton-on-the-Naze, Somersetshire, between Dunkirk and Calais, and at Paris. *Galignani* says:—"A meteor of extraordinary brilliancy was seen in Paris during the twilight yesterday evening at 6.40. In the northern heavens, at an angle of 30° above the horizon, a fiery globe, about the size of a cricket ball, seemed to emerge from the clear sky descending slowly towards the earth, emitting showers of sparks and a scintillating train in its flight. It fell almost perpen-

dicularly, and grew elongated in falling. It had hardly flashed into sight when it disappeared behind the houses, where it must have burst, for the whole northern sky was illuminated with two successive blazes of fire like lightning, by which the surrounding clouds were tinged as if with gold. The effect was extremely beautiful."

THE obstruction at the entrance to New York Harbour known as Hell Gate was successfully removed by an explosion of dynamite on Sunday afternoon without any of the disasters that many people anticipated. The mass to be removed was about 70,000 cubic yards. The number of borings was 3,500; the number of galvanic batteries 200, placed in an explosion-proof chamber at a distance of 200 feet from Hell Gate. The diameter of the borings was uniformly 3 inches, and the depth varied according to circumstances, from 3 to 11 feet. Fifty thousand pounds of dynamite were used. The shock was not perceptible, not even glass being broken. A vast volume of water and smoke was driven about fifty feet into the air. All the charges were exploded, and the rock is stated to have been thoroughly removed. The explosion was heard at a distance of ten miles, and a tremor like a slight earthquake was heard in New York City and the localities contiguous to Hell Gate. The work has been in progress for seven years.

THE *Golos* of Sept. 17 gives some late information received from Omsk, as to the Thibetan Expedition of M. Prejevalsky, and as to his latest arrangements relative to the route to be followed. From Omsk, which he left July 9 with MM. Povalov-Shveikofsky and Ecklon, he was to proceed through Semipalatinsk and Sergiopol to Kooldsha; thence, crossing the Tian Shan, he would go to the Lob-nor, where he is to stay during the autumn, until December. For the winter-months the expedition will return to Kooldsha. Starting thence in the spring, they propose to go through Karatar to Hlassa in Thibet. To the exploration of different parts of that country they propose to devote two years, after which they will descend the valley of the Brahmapootra. The expedition is well provided with means, having at its disposal 25,000 roubles. Their baggage, when it arrived at Omsk, weighed not less than 2,500 kilogrammes. As on his last journey, M. Prejevalsky has provided himself with a good supply of the means for hunting and self-defence, carrying 10,000 cartridges for rifles, 65 kilogrammes of gunpowder, and 250 kilogrammes of shot. Plenty of small steel instruments (knives, scissors, razors), looking-glasses, some silver tea-sets, &c., for commerce and presents, are said to be well chosen by M. Prejevalsky to gratify the taste of the Mongols.

THE splendid orang-utang in the Berlin Aquarium died last week of consumption. Its friend and playfellow, the chimpanzee, died the next day of consumption and grief. The young gorilla, the one living specimen ever brought to Europe, which we referred to some months ago, is still alive, but ailing. Hamburg not long ago offered 100,000 marks for the gorilla; it is feared that he will soon be sold for less.

AN organ for High Schools under the title *Alma Mater*, will be published in Vienna on October 1. It will appear weekly, will be exclusively devoted to the interests of the High Schools, and will advocate reforms in all academical matters. Many eminent professors in Germany and Austria have promised to become contributors.

THE City authorities of Munich have consented that the meeting for 1877 of the German Naturalists will be held in that town, and have also declared their intention of meeting all the costs of reception. An Ultramontane majority in the town-council of Aix-la-Chapelle, declared that the naturalists should not meet in that town.

THE fifth meeting of Russian Naturalists, which takes place this year at Warsaw, was opened on the 12th inst. It was well attended, the number of members having been on the opening day nearly 250, which number increased daily afterwards.

DR. THOMAS LAYCOCK, Professor of the Practice of Physic and Clinical Medicine in the University of Edinburgh, died at Edinburgh, on Thursday last.

THE number of visitors to the Loan Collection of Scientific apparatus during the week ending September 23 was as follows:—Monday, 3,082; Tuesday, 2,622; Wednesday, 375; Thursday, 345; Friday, 296; Saturday, 3,991. Total, 10,711.

THE Congress on Silk-culture, at its Milan Session, declared that its next bi-annual meeting should take place at Paris on the occasion of the general exhibition.

THE Champ de Mars has been quite closed for the works of the 1878 exhibition. A number of deputies, senators, &c., have been appointed by a recent decree members of the Administrative Commission. All the expenses of building, &c., will be supported by the public exchequer. The great undertaking is exclusively in the hands of the public administration.

DR. PETERMANN has received a telegram, dated from Hammerfest, September 19, announcing the safe arrival at that port, from the Jenisei River, of Prof. Nordenskjöld's trading expedition which, it will be remembered, started from Tromsø as late as June 25, on its voyage through the Arctic Ocean of Siberia to the mouth of the Jenisei. The voyage out to the latter and back was performed in about five weeks only, during sixteen days of which the expedition stayed at the Jenisei. The expedition found the sea perfectly navigable and free from ice; thus the practicability of a trade route from Europe through the Arctic Ocean to Siberia seems to have again been demonstrated.

THE British Association grant for the investigation of the constitution of the double compounds of nickel and cobalt was given to Mr. John M. Thomson, not to Mr. W. N. Hartley, as stated in our last week's list.

THE French Franklin Society, established for the creation of popular libraries, received a silver-gilt medal from the Brussels Exhibition for services rendered to public instruction.

THE direction of primary instruction in Paris is preparing plans for the establishment in that city of a normal school of gymnastics.

M. TISSERAND, Inspector-General of Agriculture in France, has been appointed director of the Agronomical Institute. The lectures will be given at the Conservatoire des Arts et Métiers, and the authority of General Morin will be paramount over the new institution. A notice to the public has been published in the official paper reminding them that the course of lectures will be opened on November 15. Pupils are obliged to present a diploma of *Baccalauréat-ès-Sciences*, or to pass an examination to prove that they are conversant with the subjects of the said examination. Tuition fees are 300 francs a year, but free pupils are admitted at a reduced fee of 25 francs. Foreigners are admitted without any limitation.

THE University of Heidelberg as well as medical science and practice, has recently sustained a great loss in the death of Dr. Simon, for fifty-three years a professor of surgery therein and a skilful operator.

THE ordinary professor of mathematics in Vienna University, Dr. Ludwig Boltzmann, has been appointed professor of physics and director of the Physical Institute in the University of Graz.

DR. TÖPLER has been appointed Professor of Experimental Physics in the Polytechnic School of Dresden.

MR. WILLIAM MATHEWS, jun., M.A., F.G.S., of Birmingham, has, in consequence of ill health, resigned the office of local Secretary to the Ray and Palæontographical Societies, which he has held for upwards of twenty years, and Mr. W. R. Hughes, the Treasurer of the Borough, succeeds him.

SESSION 1876-7 of the Birmingham and Midland Institute will be opened on Oct. 5 by an address by Mr. Joshua Morley. Among the lectures to be given during the Session are the following:—Oct. 13, Recent Explorations in Africa, by Lieut. Cameron, D.C.L.; Oct. 16, Antarctic Discovery, and its Connection with the Transit of Venus, 1882, by Capt. Davis, R.N.; Oct. 23, The Early Forms of Animal Life, by Prof. W. C. Williamson, F.R.S.; Oct. 30, The Early Forms of Vegetable Life, by Prof. W. C. Williamson, F.R.S.; Nov. 20, Spectrum Analysis applied to the Heavenly Bodies, by Wm. Huggins, F.R.S.; Dec. 11, The Ancient Inhabitants of the Caves of Derbyshire, by Prof. Boyd Dawkins, F.R.S.; Jan. 22 and 29, 1877, Rots and Ferments, our Unseen Enemies, by E. Ray Lankester, F.R.S.; March 12, The General Results of the *Challenger* Expedition, by Prof. Sir C. Wyville Thomson, F.R.S.; March 19 and 26, Radiation and Radiometers, by Prof. W. F. Barrett, F.R.S.E.

EARTHQUAKES were felt on the night of September 12-13, at Salonica, and in South Italy, at Reggio. Two motions were observed in the last city, the first one being the most notable, both having taken place on the 13th, between 12 and 1 o'clock, local time. Another earthquake was felt at Salonica, on the 14th, at 5 o'clock in the morning. The Reggio commotions were propagated to Messina and vicinity. They produced quite a sensation, although not destructive.

In the *Bulletin Mensuel* of the Observatory at Montsouris for July is given an interesting comparison between the amount of atmospheric ozone observed by Schönbein's test-papers and that ascertained by the more exact method employed for some time at the Observatory, with the result that, setting aside all anomalies due to excessive moisture and excessive drought, and to the velocity of the wind, there is a pretty fair agreement between the amounts obtained by the two methods. It must, however, be added that while this result is in a sense gratifying, the observation of this important element by the ordinary method of test-papers is far from being satisfactory.

As the U.S. Congress has made the necessary appropriations to meet the expense of various Government geological and geographical surveys of the Territories, the parties have taken the field, and hope to accomplish a good deal, although the delay on the part of Congress in supplying the means will lessen the period of active work materially. Dr. Hayden's expedition will be divided into four parties. The first will be in charge of Mr. A. D. Wilson, with Dr. Endlich as geologist and Mr. Atkinson as topographer, and will complete the exploration of the small portion of Colorado lying near the Utah line, and then move northward on the west side of the Rocky Mountains. Mr. Henry Gannett will have charge of the second division, with Dr. Peale as geologist, and James Stevenson as executive officer. This division will revisit the region in which a portion of Prof. Hayden's party had an encounter with the Indians and was driven off, last year, with the loss of their implements. Mr. G. R. Bechler will be in charge of the third division, with the necessary assistants. He will pass westward through the Middle Park, working along the north-western part of Colorado. The fourth division will be in charge of Dr. Elliott Coues, with an assistant, and will be especially devoted to zoological work, visiting such portions of Dr. Hayden's region of investigation as have not been examined in previous years. Dr. Hayden himself will visit all the parties in the course of the summer and autumn, and co-ordinate their work.

SOME curious experiments on the expansion of liquids to lamellae, have recently been described by M. Cintolesi in the *Rendiconti Reale Istituto Lombardo*. He considers that the phenomenon is always accompanied with a development of gaseous masses; further, that the spreading out of liquids on each other is caused by the vapours of the substances, whose molecules moving in every direction force the liquid molecules out from each other horizontally, and, where the resistance of the liquid is not strong enough, rupture the film.

IN his thermo-chemical researches on gold and its compounds, M. Julius Thomsen has observed that gold separated out from different solutions and by dissimilar reducing agents presents allotropic differences, three of which he has studied:—1. Reduced from chloride solution with sulphurous acid, gold forms a balled mass. 2. Reduced similarly from the bromide solution, it forms a very fine dark powder, which retains its powder form even after drying. 3. Reduced from the chloruret, bromuret, or ioduret, with sulphurous acid or hydrogen acid, it forms a very fine powder with metallic brilliancy and yellow colour. These modifications are also distinguished by unequal heat-energy in the several reactions.

FROM careful measurements during 1871 and 1872, it appeared that the quantity of water annually flowing past in the Elbe, at the boundary between Saxony and Bohemia, was about 6,179 million cubic metres. M. Breitenlohner, considering the quantity along with analyses he made of Elbe water in 1866, has calculated the amount of solid matter carried away by the Elbe out of Bohemia every year. His estimate is, for suspended matters carried off, 547·14 million kilogrammes, dissolved matters, 622·68 million kilogrammes (of which 977·7 million were fixed, and 191·12 million volatile), giving a total of 1169·82 million kilogrammes of solid substances carried off. The numbers are also interesting which indicate the proportions of substances important to agriculture that are thus removed from Bohemia. In the 6 milliards of cubic metres of Elbe water, there are partly, suspended, partly dissolved, 140·38 million kilogrammes lime, 28·13 million kilogrammes magnesia, 54·52 million kilogrammes potash, 39·6 million kilogrammes soda, 25·32 million kilogrammes chloride of sodium, 45·69 million kilogrammes sulphuric acid, and 1·5 million kilogrammes phosphoric acid. The Elbe has a basin of about 880 square miles in Bohemia.

AN essay on the Wines and Wine Industry of Australia, by Rev. Dr. J. I. Bleasdale (Melbourne: Baillière), contains a great deal of information on a subject of much industrial and economic interest.

PART I. of Vol. III. of the *Transactions* of the Connecticut Academy of Arts and Sciences is a thick one, and is profusely illustrated with well-executed plates. The papers are:—"Reports on the Dredgings in the Region of St. George's Banks in 1872," by Messrs. L. J. Smith and O. Harger; "Descriptions of New and Rare Species of Hydroids from the New England Coast," by Mr. S. F. Clark; "On the Chondrodite from the Tilly-Foster Iron-Mine, Brewster, N.Y.," by Prof. E. S. Dana; "On the Transcendental Curves $\sin y \sin my = a \sin x \sin nx + 6$," by Professors H. A. Newton and A. W. Phillips; "On the Equilibrium of Heterogeneous Substances," by Prof. J. Willard Gibbs.

WE have received Part 4 of the *Transactions* of the Glasgow Society of Field Naturalists, containing an account of the proceedings for 1875-6. The part contains many valuable papers in natural history, the results of original observations, and we regret that want of space prevents us referring to them in detail.

THERE are several papers of considerable value in the last-issued part of the *Transactions* (vol. iii. No. 2) of the Academy of Science of St. Louis, and we regret that our space will admit

of our giving only the titles:—"Iron Manufacture in Missouri; a General Review of the Metallurgical Districts and their Resources," by Dr. A. Schmidt; "Remarks on Canker-worms, and Description of a New Genus of Phalenidæ," by Prof. C. V. Riley, who also contributes "Notes on the Natural History of the Grape Phylloxera (*P. vastatrix*)," and "Notes on the Yucca Borer (*Megathymus yuccæ*, Walk.);" "On a New Form of Lecture Galvanometer," by Prof. Nipher; Dr. G. Engelmann contributes "Notes on Agave (with photographic illustrations)," and "About the Oaks of the United States;" "The Rocky Mountain Locusts and the Season of 1875," by Mr. G. C. Broadhead, who also contributes papers on "The Meteor of Dec. 27, 1875," and on the "Age of our Porphyries;" Mr. A. J. Conant has a paper on the "Archæology of Missouri." The latter part of the number is occupied with the Journal of Proceedings.

IN the *Penn Monthly*, a Philadelphia publication, for May and June are two interesting articles by Mr. C. E. Dutton containing "Critical Observations on Theories of the Earth's Physical Revolution."

THE additions to the Zoological Society's Gardens during the past week include two Bonnet Monkeys (*Macacus radiatus*) from India, presented by Mr. Chas. E. Green and Mr. R. K. Meaden; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Capt. J. C. A. Lewis; a Striped Hyæna (*Hyæna striata*) from Algeria, presented by Mr. Thos. Barber; an Arabian Gazelle (*Gazella arabica*) from Arabia, presented by Mr. F. de Havilland Hall; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. Geo. J. Hendry; a Common Boa (*Boa constrictor*) from South America, presented by Mr. F. B. Bloxham; a Red and Yellow Maccaw (*Ara chloroptera*) from South America, deposited; a Hog Deer (*Cervus porcinus*), born in the Gardens.

SCIENTIFIC SERIALS

American Journal of Science and Arts, September.—In a second paper on the gases contained in meteorites, Mr. Wright first describes those of the Kold Bokkeveld stony meteorite, one of a distinct class containing a good deal of amorphous carbon, a bituminous substance, and very little metallic iron. The volume of the gases obtained was much greater, but the gaseous mixture was like that of ordinary stony meteorites, except in the very small quantity of hydrogen present. A comparative table is given of the gases of seven iron and six stony meteorites. From experiments on the manner of occurrence of carbon dioxide, the author infers that while some of the gas may be condensed on the fine particles of the iron, a large portion of it and of the water, carbonic oxide, and other gases, is mechanically imprisoned in the stony substance of the meteorite. The idea is favoured of comets consisting of meteoric masses with the gases expanding under action of solar rays. Every cubic mile of a substance like the Kold Bokkeveld meteorite would give thirty cubic miles of gas at the pressure of our atmosphere, and in space this would expand enormously before it would cease to transmit electric discharges or be visible by reflected sunlight. These views are confirmed by spectroscopic observations of meteoric gases.—Mr. Storer, questioning Carius' statement that Schoenbein's iodo-starch test for nitrates used with zinc as reducing agent, is not a specially delicate one, finds that the fatal defect of the test, as hitherto applied, lies in the fact that mere water containing no nitrates or nitrites, on being treated with zinc or cadmium, as if to test for a nitrate, will react on iodo-starch just as if a trace of some nitrate were present. This coloration is due to peroxide of hydrogen formed in the water by action of the metal. Mr. Storer also finds that no peroxide of hydrogen is formed when water slightly acidulated with sulphuric acid is boiled on metallic cadmium; and as the reduction of nitrates and nitrites occurs readily in such solutions, the iodo-starch test can be thus applied for detection of nitrates with great certainty.—Mr. J. Lawrence Smith gives an account of a new meteoric stone which fell in 1865 in Wisconsin, and which is identical with the Meno-meteorite which fell in 1861.—Mr. Brooks gives a classified list of

rocks in the Huronian series south of Lake Superior, with remarks on their abundance, transitions, and geographical distribution; and Mr. Burnham furnishes a seventh catalogue of new double stars.

Poggendorff's Annalen der Physik und Chemie, No. 7, 1876.—We have here the second portion of Dr. Root's inaugural dissertation on dielectrical polarisation. He finds (1) that there is a dielectrical polarisation which takes less than 0.0000821 sec. to be perfectly developed; (2) that all solid dielectric bodies (sulphur not excepted) show, with continuous discharge or slow commutation, a dielectric reaction which, e.g., in aragonite is perceptible within 0.0208 sec., but no longer so beyond 0.007 sec.; (3) that in direction and relative size the principal axes of elasticity of Fresnel agree with Maxwell's principal axes of electro-elasticity; and (4) that only with the aid of Faraday's supposition that a perfect conduction everywhere accompanies polarisation, can the equation $K = n^2$ (i.e., the dielectric permeability = the square of the index of refraction) be brought into harmony with experience.—A third paper from M. Kohlrausch describes experimental researches on elastic reaction in torsion, expansion, and bending. It relates chiefly to stretching and bending of caoutchouc. The various phenomena are shown to agree with a formula previously given; and a remarkable result from his study of reaction generally is, that after successive deformations of opposite sign, movements of reaction may remain in an electric body, which may pass from one direction into the opposite.—Two methods of determining the indices of refraction of liquids and glass plates are described by M. Wiedemann.—Dr. Vogel communicates observations on the spectra of the planets. The light which all of them send us is, he considers, reflected sunlight; the well-established fact that there is aqueous vapour in the atmospheres of Jupiter and Saturn makes it improbable that they have (as has been supposed) so high a temperature as to be self-luminous. The further a planet is from the sun the more marked is the influence of the gaseous envelopes in production of spectroscopic dark bands.—M. von Rath, of Bonn, describes a number of mineralogical specimens, and M. Berthold makes a contribution to the history of the radiometer, to which we shall refer in a separate note.

SOCIETIES AND ACADEMIES

LONDON

Entomological Society, Sept. 6.—Mr. J. Jenner Weir, F.L.S., in the chair. Mr. Edward Boscher was elected a member.—Mr. Edward Saunders exhibited some recently-captured specimens of *Hymenoptera* and *Hemiptera*, many of them rare in this country, and made some remarks respecting the bug of the house-martin, of which he had taken eighteen specimens in the window-sills of a house.—Mr. Weir mentioned that on a recent visit to the South Downs he had suffered much annoyance from the attacks of harvest bugs, as many as eighty pustules appearing on each foot. Several remedies were suggested, especially rubbing the affected parts with brandy and water; but Mr. Smith stated that on one occasion when he was in the Isle of Wight and exposed to their attacks, he was effectually relieved from all annoyance by a dose of milk of sulphur.—Prof. Westwood communicated a note with reference to some shoots of horse-chestnut which he had exhibited at the July meeting, having been destroyed, apparently, by some Lepidopterous larvæ or wood-boring beetles; but he had since received from Mr. Stainton some shoots that had been forwarded to him by Sir Thos. Moncrieffe, which had been destroyed by squirrels in precisely the same manner. Sir Thomas had himself seen the squirrels at work splitting the shoots with their teeth and extracting the pith. The Professor also stated that he had received from a correspondent in Oxfordshire specimens of the two small species of grasshoppers with long antennæ *Meconema varium*, Fab., and *Xiphidium clypeatum*, Panzer, which he had taken on a pear tree in his garden, where they had been regularly observed for the last five or six years. Mr. McLachlan said that the former insect was frequently observed by Lepidopterists when sugaring for moths.—Mr. Smith communicated the descriptions of three additional species of *Formicidæ*, from New Zealand, which had been sent to him by Mr. David Sharp since his description of Mr. Wakefield's collection was in the press. Two of the species belonged to genera not previously ascertained, to inhabit New Zealand, namely *Amblyopone* and *Pouera*.—The following memoirs were read:—"Monograph of the dipterous genus

Systropus, with notes on the economy of a new species of that genus;" and "Descriptions of new genera and species of Acroceridæ." Both were communicated by the President, Prof. Westwood.

BOSTON

Natural History Society.—During the Session, 1875-6, Prof. N. S. Shaler has contributed several papers on physical geology, in one of which he attempted to account for the phenomena of several areas of glacial erosion. He is persuaded that the melting caused by pressure would put a limit to the accumulation of ice at a depth probably not exceeding two miles. This melting would give the ice-sheet a chance to move freely in the direction of least resistance. The flow of the melted water would account for stratification of moraine matter, and for the rounding of pebbles.—Mr. Osten Sacken has revised the North American species of the Dipteran genus *Syrphus*.—Dr. W. K. Brooks has made a contribution to the embryology of *Salpa*, which is starting to naturalists, and will be of great importance if confirmed. He says that in tracing back the history of the zooids composing a chain, the egg is present at all periods of growth, of exactly the same size and appearance as at the time of its impregnation. He concludes that the animal, which has no existence, cannot be the parent of the egg which is already fully formed. Thus the explanation is that the solitary salpa is the female, which produces a chain of males by budding, and discharges an egg into the body of each before birth. These eggs are impregnated while the zooids of the chain are very small and sexually immature, and develop into females which give rise to other males in the same way. After the foetus has been discharged from the body of the male, the latter attains its full size, becomes sexually mature, and discharges its spermatic fluid into the water, to gain access to the eggs of other immature chains. This arrangement is compared with other cases, as in cirripeds, arachnids, argonaut, in which the male is to some extent parasitic on, or supplemental to, the female.—Mr. T. T. Bouvé has further developed his views of the origin of porphyries from metamorphosed conglomerates.—Dr. Brooks's paper on the affinity of the mollusca and molluscoida is worthy of note. He concludes that Brachiopods are derived from Vermes; and Polyzoa from some primordial Brachiopod. The polyzoan stem gave off the molluscan veliger, from which the true mollusca have originated by several offshoots. The scaphopods appear to be the least specialised. The Lamelliobranchs may be derived from one of these offshoots: they probably diverged early from the ancestral form, becoming degraded in certain respects and specialised in others. The president, Mr. T. T. Bouvé, gave a very interesting address on March 15, describing the origin and early proceedings of the society, its struggles with difficulties and ignorance, and the stages by which it has reached its present successful position. He stated that the society's museum, as now arranged, constituted a series of lessons in the structure of the earth and its constituent parts, and in the organisation of the plants and animals on its surface. Special lectures have been given to teachers, and other efforts have recently been made by the society for the spread of science.

ROME

R. Accademia dei Lincei, February—April.—The following, among other papers, were read:—On the common origin of the Marian and Vatican hills, by M. Ponzi. They were formed by a great seismic oscillation which laid bare their entire stratigraphical arrangement. The marls at the base of the Vatican hill have yielded many organic remains representing the old sub-Apennine fauna of the upper miocene.—On alkaloids of viscera that have putrefied at a low temperature, by M. Selmi.—On the presence of organs of taste in the tongue of Saurians, by M. Todaro. Having indurated several tongues of *L. agilis* and *L. viridis*, made sections, and coloured with picrocarmine of ammonia, he found a large number of gustative organs about the papillæ on the lateral margin of the tongue. They are similar in form and arrangement to those in mammals.—On a constant inductor, by M. Volpicelli (appendix to memoir). He had described one of the nature of a Leyden jar. Another consists of a dry pile, having 10,640 pasteboard discs, each covered with sheet-tin on one side and with peroxide of manganese on the other. One pole is coated with a good insulating varnish. The other communicates with the earth. The dry pile serves usefully in verifying the laws of electric action.—On artificial increase in the tenacity of cotton, by M. Manzoni.—On the inundations of the Tiber at Rome,

by M. Brioschi.—M. Moriggia presented the famous tattooed man, Konstantinos, a native of Albania, who was long a prisoner of war in Chinese Tartary. He was then tattooed from head to foot, with figures of men, tigers, crocodiles, apes, &c. The work was continued for four months. The tactile sensibility of the skin is diminished; sensibility to thermal stimuli is good, and to electrical perhaps increased; muscular force low; a difficulty of breathing and lassitude; sense of strain and smart in the skin, greatest in the feet and seat; considerable insomnia, vision and hearing affected, frequent dysentery and abdominal pains, blood rich in leucocytes, urine with traces of albumen, free perspiration still, intelligence not much affected, *morale* depressed, &c.—Historico-critical note on the theory of the electrophorus, by M. Cantoni. His results closely agree with those of Neyreneuf.—M. Ponzi presented the second part of a catalogue of fossils found in the lower marls of the Vatican Hill (141 animal species).—Observations on the solar diameter at the Royal Observatory of Campidoglio in 1875, by M. Respighi. These confirm former conclusions.—New researches on the fine structure of the electrical plates in the torpedo, by M. Boll.—Anatomical and physiological researches on the arms of cephalopods, by M. Colosanti.—On some recent palæontological discoveries in the territory of Massa Marittima, by M. Lotti.—On Zoppi's method of cementation of cupriferos solutions in Agordo, by M. Pellati.—M. Volpicelli criticised a recent experiment of Govi's (*Journal de Physique*). An inductor is brought under two light pendula suspended from a metallic ring on insulating support. There is sudden divergence, and this increases if the induced bodies be connected an instant with the ground. If the inductor be brought sufficiently near the ends of the pendula, the previous divergence is diminished; on then suppressing the induction, the divergence increases. Govi infers the induced electricity causing the divergence to be of the first species (heteronymous to that of the inductor); M. Volpicelli says if he will examine it, he will find it to be homonymous.—M. Gastaldi presented the first part of a memoir entitled "Fragments of Italian Palæoethnology."—On the Vatican fauna (continued), by M. Ponzi.—On the non-periodical movement of a system of material points, by M. Valentino.—On strata with *Aspidocerra acanthicum*, Opp., of Sicily, and their cephalopoda.—On the porphyroid quartziferous diorite of Cossato in Biellese, by M. Cossa.—On some products of putrefied cerebral substance, by M. Selmi. He finds among these the volatile alkaloid trimethylamine.

May 7.—M. Capellini presented some fragments of *Baleonotus*, found along with flint implements in the valley of Fiore, in a marl of the Lower Pliocene. In a memoir he discusses the distribution of land and water at that epoch, and offers some new views on the origin of fauna and flora of the Miocene and Pliocene formation in Italy (which origin he places in the north-west). He shows that many fossil plants found in northern regions appear in Italy in more recent formations.—On the scintillation of stars, by M. Respighi. He affirms (in opposition to Montigny) that this in its essence is independent of the quality of the light of the star. Montigny's observations regard merely the modality of the phenomenon, and the question carried into that field belongs rather to physiology than to physics. The spectroscope shows that, rigorously speaking, the variations of colour, especially in low stars, are innumerable, even in a second, and it is only by the limited power of our senses, the persistence of sensations, &c., that we succeed in perceiving distinctly a limited number of the variations, which naturally must depend on the greater or less brightness of the star, the varied proportion of rays composing its light, the means used to diminish the influence of persistence of the images, and other causes which render the eye less apt to perceive variations of colour.—On the latitude of the Royal Observatory of Campidoglio, by M. Respighi. This is $41^{\circ} 50' 33''$. M. Respighi stated that the great work of revision of the declination of stars of the first to the sixth magnitude, in the zone 21° to 62° N., was well advanced, both as regards observations and reductions.—M. Volpicelli presented a second note on the machines invented by M. Belli, and called *Duplicators*.—On palæontological discoveries in the Vatican marl, which geologist refer to the Tertiary period, by M. Ponzi. He describes carbonised trunks of *Pinus sylvestris*, eaten into by an insect, which he names *Hyllobium tortonianum*, resembling the *H. tini* of the present.

PARIS

Academy of Sciences, Sept. 11.—Vice-Admiral Paris in the chair. The following papers were read:—On preventive

trepanation in fractures with displacement of splinters of the internal or vitreous table of the cranium, by M. Sedillot.—Note on intra-mercurial planets, by M. Leverrier.—On the recent trombe of Coinces in the Loiret, by M. Faye. This was very violent, damaging a large number of houses, and lifting and throwing many people down.—Process for detecting wines coloured artificially, by M. Lamattina. The simplest way is to mix 100 grammes of wine with 15 grammes of peroxide of manganese roughly pulverised, stirring the mixture twelve or fifteen minutes, and filtering through a double filter. If the wine is pure it passes colourless, if it retains its colour it has been coloured artificially. If the peroxide is not pure, but ferruginous, the iron is dissolved; the fuchsine, if present, forms an insoluble combination, which remains in the filter, and the filtered liquid has a slightly yellow colour. The residual peroxide is treated with alcohol, acetic acid, and ammonia.—On the orbit of the planet 127, by M. Renan.—Note on a lunar rainbow observed at Roche, commune of Saint Just (Haute-Vienne), by M. Martin de Brettes. This was at 9:50 P.M. on September 2; the day had been showery, and a mist rose over the river. The centre of the rainbow was north; mean horizontal diameter about 25° , apparent width of bow 2° , colour green yellow; on close attention it was seen to be red exteriorly and violet in the interior. The bow was slightly elliptical, the vertical semi-diameter longer than the horizontal; this was likely due to the obliquity (45°) of direction of the river. The bow seemed very near, a few hundred metres off. It was enveloped by a second, 5° off.—Observation of the partial eclipse of the moon, September 3, 1876, at the Observatory of Toulouse, by M. Perrofin.—Note on the radiometer, by Mr. Crookes. He says that most of the experiments recently described to the Academy are a repetition of those he himself has made, and he has also discussed fully the various theories offered; but his researches had not become known, owing to memoirs to the Royal Society not being published in the *Philosophical Transactions* till twelve or eighteen months after presentation.—Researches on some Calamodendrea, and on their probable botanical affinities, by M. Renault.—On a block of millstone found in the eruptive sand of the environs of Beynes, by M. Meunier. This confirms the opinion that the eruptive sand is artesian, and constitutes a vertical alluvium.—On the distinctness with which one can see the bottom of the sea from a balloon situated at a great height, by M. Moret. In an ascent from Cherbourg with M. Durouf, they observed, at a height of 1,700 metres, the bottom of the channel most clearly, though the depth there must be 60 or 80 metres. The submarine rocks and currents were distinctly revealed. This method might be utilised for purposes of navigation.

CONTENTS

	PAGE
THE INTRA-MERCURIAL PLANET OR PLANETS. By J. R. HIND, F.R.S.	469
UNIVERSITY COLLEGE, BRISTOL.	470
FIELD GEOLOGY	471
THE BATS OF ASIA	472
LETTERS TO THE EDITOR:—	
Sun-spots suspected to be identical with an Inter-Mercurial Planet.—Rev. ROBERT MAIN, F.R.S.	473
Erratum in Mr. Wallace's Address.—ALFRED R. WALLACE; Prof. W. THISELTON DYER, F.L.S.	473
Zittel's Palæontology.—HENRY B. BRADY, F.R.S.	474
Visual Phenomena.—THOS. WM. BACKHOUSE	474
Antedated Books.—D. SHARP	474
OUR ASTRONOMICAL COLUMN:—	
The Binary Star ϵ Bootis	474
Diameter of Vesta	475
Pigott's Comet of 1783	475
THE SELF-FERTILISATION OF PLANTS	475
THE BRITISH ASSOCIATION:—	
Reports	476
Section A.—Mathematical and Physical	477
Section C.—Geology	480
Section D.—Biology.—Department of Anatomy and Physiology.—Address by John G. McKendrick, M.D., F.R.S.E., Vice-President	482
Section E.—Geography	489
Section F.—Economic Science and Statistics	491
Section G.—Mechanical Science	491
THE "CHALLENGER" EXPEDITION. By Sir WYVILLE THOMSON, F.R.S.	492
NOTES	496
SCIENTIFIC SERIALS	498
SOCIETIES AND ACADEMIES	499