THURSDAY, AUGUST 23, 1900.

A MUSEUM CATALOGUE.

Descriptive and Illustrated Catalogue of the Physiological Series of Comparative Anatomy contained in the Museum of the Royal College of Surgeons of England. Vol. i. Second edition. Pp. xlix + 160. (London: Taylor and Francis, 1900.)

OF the catalogues of the Royal College of Surgeons, rendered famous by the labours of Hunter, Owen and Flower, but one in osteology, by Dr. R. Bowdler Sharpe, and one in teratology, by Mr. B. Thompson Lowne, have appeared during the present conservator's term of office. Prof. Charles Stewart, unlike his predecessor in this office, who concentrated his attention upon one special department, has since his appointment greatly developed all sides of the collection, and with the aid of his competent assistants has added year by year specimens of surpassing value and interest, which have become the admiration of all beholders. The period of his conservatorship has been one of unparalleled activity in all branches of zoology, and in the labour of keeping pace with this he has not been found wanting, as, for example, when, on the discovery of the calcified teeth of the Monotreme, he produced from his rich store of material the famous specimen which has since adorned his shelves, and shows more than all others yet described. With this museum, as with others in our own land, the Englishmen's colonising instinct has come forcibly into play, in the accumulation of objects, not merely of local interest, such as are generally to be found in the museums of other countries, but general and universal, wherefore the present catalogue is of necessity based upon a matchless material.

It is explained in the preface that the specimens registered have been dealt with on the original Hunterian lines, the intention being to bring together examples of such structures in plants and animals as perform the same function; and comment is further made upon the necessity for a large number of preparations "to supply the places of those that have become worthless, and to serve as illustrations of new discoveries, and phases of thought." At the outset, necessity, begotten of progress and advancement, is met by the propounding of a scheme, clearly explained in the text, under which it is proposed to distinguish, firstly, between "Structures concerned in the preservation of the individual or to its advantage" and those "concerned in the preservation of the race." Under the former of these departments, sixteen headings are included, under the latter eight; and since the present catalogue deals with but the first three of the former series, those alone need be further remarked upon. They read "Endoskeleton"; "Flexible Bonds of Union and Support"; "Muscular and Allied Systems"; but before passing on to consider them more fully, it may be said that they and the twenty-one headings of sections to follow are, in the preliminary portion of this, the first volume of a series, individually set out in greater detail, each with a concise statement of the order of arrangement to be adopted, and a definition, when necessary, of what is implied in the heading it bears.

The arrangement of each section is so framed as to include both plants and animals, whenever possible, the former being considered first, and each in ascending order. Turning now to the three sections to be specifically considered, we find specimens illustrating, under the first, the chemical composition, structure, and mode of formation of the various endoskeletal systems; under the second, the various forms of ligaments and joints; and under the third, the forms, structure and texture of muscles. To particularise in these columns concerning the details of either of these is impossible; but it may be said that no leading type of tissue or arrangement of parts is unrepresented, and that preparations like that of the cartilages of the cuttle, the elastic honeycomb of the flexor carpi muscle of the elephant, or of the leaves of the sensitive plant fixed in the diurnal position, must be seen to be appreciated.

The most attractive portions of the work are those dealing with the marvellous array of processes occurring in the growth of the coral skeleton, and (as pertaining to the study of joints and jointing) with the question of adaptation in these to the conditions of existence. The study of the general question of origin, detailed constitution and relationship, of the coral skeleton, has for years engrossed the attention of Prof. Stewart; and to our knowledge of this subject and the allied one of the structural variations of the bony tissues of the osseous fishes, he has in the long run added more than most other investigators since von Kölliker. Beyond laying this magnificent result of his labours before audiences which annually assemble on the occasion of lectures delivered in his official capacity, and occasional "exhibits" before the Linnean and Zoological Societies, he, with a modesty so marked as to be well-nigh depreciative of his talents, has published nothing concerning them; and the present volume therefore comes rather as a memoir from his hands than as a mere official catalogue, and it is worth all the scattered papers he could have published in the time. It teems with interest and records of beautiful objects, and is illustrated by fourteen magnificent plates, mostly in colour, done from his own drawings by the facile hand of Green, than whom no better English lithographer in zoology exists. Of these plates no praise can be too high, and we expect for them an unprecedented popularity in the future. They must be seen to be appreciated, and, with the exquisite preparations they illustrate, constitute a possession of which even the Royal College of Surgeons may well be proud. Each of the entries in the catalogue bears a registration number, and where desirable a short bibliographic reference, as an aid to the student.

The success of this volume augurs well for the future of the museum and its collections, and knowing the unparalleled excellence of the numerous additions which during the last decade have been made to the series of which it treats, thanks to the curatorial genius of Prof. Stewart and the unrivalled skill of his lieutenant, Mr. R. H. Burne, we can safely predict even a better result for the volumes yet to come. The collection of zootomical preparations arising under their hands is far and away the best in existence, and together with the governors of the college they have ensured a debt of gratitude which it will take generations to repay.

In the pages of this volume the student will find records of structures and relationships undreamt of in the text-books, unrecorded in the best monographs; and it is a pity that he is not informed of this. The work is a positive storehouse of new facts and intensely interesting details, and will be of inestimable value to zoologists at large.

A TEXT-BOOK OF MAMMALS.

Text-book of Zoology, treated from a Biological Standpoint. Part I., Mammals. By O. Schmeil. Translated by R. Rosenstock, and edited by J. T. Cunningham. 8vo. Pp. vii + 138, illustrated. (London: A. and C. Black, 1900.)

AS stated in the first title-page, this book is intended for the use of schools or colleges, forming, in fact, a portion of the series of School Text-books now in course of issue by the publishers. It is, therefore, essential that it should be written in a popular and attractive style, and also that it should be absolutely accurate and up-to-date, both as regards the facts recorded, and, so far as possible, in nomenclature. So far as this first item is concerned, the present fasciculus appears to fulfil the required conditions fairly well, the anatomical details being treated in a manner which renders them of easy apprehension by the student, while the descriptions of the animals themselves are, if anything, written in a too popular style. The plan adopted is to take a more or less typical member of a group for special treatment, and then to refer to the kindred forms in a more general manner. Illustrations are numerous; and while many of them are excellent, others, especially the cut of a family of orangs on p. 19, can only be described as hideous caricatures. In a book written primarily for German students, it must be inevitable that the animals of the Fatherland come in for a fuller share of notice than would have been the case had it been the product of an English author, but this is a fault of no special importance.

When, however, we come to the second essential feature of an elementary text-book—accuracy as regards facts, classification and nomenclature—we are bound to confess that the fasciculus before us fails lamentably. Indeed, its appearance is almost a calamity for zoological science in England, since the student who intends to pursue the subject seriously will have much to unlearn; and even for those who only desire a smattering of the subject, it is most important that they should become acquainted with animals by their proper titles, and that what they are taught as facts should really be such. In his preface the editor tells us that he has practically restricted his task to comparing the translation with the original, correcting the proofs, making here and there emendations in detail where a statement seemed open to doubt, or where differences between the faunas of Britain and Germany had to be indicated. For the sake of his own reputation it is a pity that he did not compare the work in detail with a standard English treatise on mammals, when he could scarcely have failed to detect some of the shortcomings of the original text, despite the fact that all the English treatises on the subject are now more or less out of date.

As regards the general classification of the group, although this differs to a certain extent from the one generally adopted in this country, we have no comment to make, except that for some unaccountable reason the order Sirenia is totally omitted, while there appears to be no mention of the animals by which it is represented anywhere in the text!

Turning to some of the ordinal groups, we find the orang taken as a typical representative of the apes, and rightly named *Simia satyrus*. Naturalists will, however, be considerably surprised to see the chimpanzee (p. 22) assigned to the same genus (*Simia*), whereas the gorilla is made the type of a genus by itself; since if there is one well-established zoological fact, it is the intimate relationship existing between the chimpanzee and the gorilla, and the wide gulf separating both from the orang. Again, under the heading of the Platyrrhine apes, there is no reference to the marmosets, and we quite fail to find a reason for the statement (p. 22) that the howling monkeys are the best known members of that group. In treating of the Lemuroids, the author departs from his rule of selecting one species for special notice, and the space allotted to the group is ludicrously inadequate.

As an instance of careless writing we may refer to the notice of the tiger (p. 33), when, after stating that this animal is found in Amurland and Central Asia, the anthor proceeds to say that its "favourite haunts are swampy districts of the tropical zone, thickly overgrown with bamboo and similar bushes." Again, on p. 84 we find *Cricetus frumentarius* alluded to as "the marmot or hamster," although the true marmots are noticed in an earlier page. Passing on to p. 105, we meet with the statement that the Indian buffalo is *said* to exist in a wild state in the "East Indies"; while the European bison is stated to be extinct, although on an earlier page (98) its existence in Lithuania and the Caucasus is alluded to! Although we do not propose to notice in detail the hopelessly obsolete generic and specific nomenclature adopted, the statement on p. 106 that "the best-known African antelope is the gazelle (*Antilope dorcas*)" is, however, too ludicrously absurd and incorrect to be passed over. And as a second instance of incorrect nomenclature we may refer to the inclusion of the roe (p. 108) in the same genus as the red deer, from which the fallow deer is excluded. And in this connection it may be mentioned that the editor, who has been recently writing on deer antlers, should have been aware that the brow-tine is not developed in those of the roe.

Before leaving the Placentals, it may be mentioned that the practice of reckoning the carnassial teeth of the land Carnivora as distinct alike from the molar and premolar series is not calculated to give the student an idea of the homology of the cheek-teeth throughout the class. And we also venture to think that the statement on page 37, that "in its dentition the wolf very nearly resembles the cat," in spite of the subsequent qualification that the number of teeth is greater, scarcely accords with the facts.

In the definition of the Marsupials, exception must be taken to the statement that the young are *always* nourished in a pouch; and when mentioning the occurrence of the group in America no reference is made to *Caenolestes*. Indeed, the account of the whole group is entirely inadequate; and when the author speaks of the value of American opossum fur, we strongly suspect he had in his mind the product of the so-called opossums of Australia.

Finally, when treating of the Monotremes, the author states that the spiny anteaters are represented solely by *Echidna hystrix* and *E. setosa*. As a matter of fact, these two forms are but local races of a single species whose name is *E. aculeata*; and the author appears to be totally unacquainted with the very distinct genus commonly known as *Proechidna!*

As already said, we do not intend to criticise in detail the nomenclature employed; but in the retention of names now discarded by those who have made a special study of the class the author has done his best to put his work out of touch with the present state of science.

In making this statement, we are aware that the author lays stress on the circumstance that he is treating his subject from a biological standpoint. This, however, in our opinion, is no excuse for neglect of the details o classification and nomenclature.

When treating of the adaptation of animals to their environment, the author is always interesting; and the paragraphs devoted to this part of the subject are, to our mind, the best in the whole fasciculus. R. L.

GOOD AND BAD AIR.

The Carbonic Anhydride of the Atmosphere. By Prof. E. A. Letts, D.Sc., Ph.D., and R. F. Blake, F.I.C., F.C.S., "Scientific Proceedings of the Royal Dublin Society," vol. ix. (N. S.), Part ii. No. 15. Pp. 270. (Dublin: 1900.)

The Air of Rooms. By Francis Jones, F.R.S.E., F.C.S. Pp. 59. (Manchester: Taylor, Garnett, Evans and Co., 1900.)

THE first of these pamphlets would amply justify its publication, if it only served to emphasise the necessity of further investigation into the methods of estimating carbon dioxide in the atmosphere. It is partly experimental, partly bibliographical in character. The authors, finding themselves called upon to make a series of observations on the carbon dioxide of the air, have made a careful study of Pettenkofer's method, and have introduced some necessary corrections, without detracting very much from its simplicity. They take the precaution, suggested by other observers, of preventing the action of the baryta solution on the glass by coating the vessel with a layer of paraffin wax. It may be pointed out that a solution in benzene is more suitable than the melted wax. The thinner film obtained with the solution is less liable to crack. The baryta solution is manipulated very ingeniously out of contact with air. Yet with all these precautions the results show that perfection is far from being attained.

In the set of analyses on page 132 there is a discrepancy between the highest and lowest figures of 10 per cent., in another set on the same page the difference amounts to 17 per cent., and on the next page to 20 per cent.

It seems superfluous to introduce the third decimal into the result when the experimental error affects the first decimal place, and equally unnecessary to make a correction for aqueous vapour, which only amounts to about one and a half per cent. on the volume of carbon dioxide, as against 10 per cent. or thereabouts from experimental error.

The authors omit to mention how long the baryta remains in contact with the sample of air. This is an important factor which should not be neglected, for there can be no doubt that the absorption of carbon dioxide by the baryta proceeds at a rapidly decreasing rate and that the final traces of the gas may take many hours to disappear.

The book is full of useful information, drawn from a variety of sources, the collection of which must have cost the authors no little trouble. At the same time, one is inclined to think that the value of the information would have been enhanced if they had gone another step and made a critical selection from the mass of analyses which they reproduce, for the figures cannot all be equally trustworthy, and many of them must be entirely illusive.

The second pamphlet relates to domestic hygiene. It treats of the effects produced on the air of rooms by the use of gas, coal and electric light for heating and lighting purposes. The effect is determined by estimating the amount of carbon dioxide by Pettenkofer's method, and by exposing a layer of permanganate solution to the air and finding the quantity of the salt reduced.

Mr. Jones, unlike the authors of the previous pamphlet, is not troubled by misgivings about Pettenkofer's method, except in the matter of the baryta attacking the glass. He therefore substitutes lime-water as the absorbent, apparently unaware of the fact that its effect on glass is precisely of the same character, which may be easily observed by placing very dilute lime-water coloured with phenolphthalein in any glass vessel; in a day or two the colour will be completely discharged. As the results here are only required for comparison, great accuracy is not requisite, and the ordinary method may be relied on. The results of the permanganate method will scarcely serve to recommend it. We find, for example, that in two experiments made on July 21 two-thirds the quantity of permanganate was reduced in the one case in double the time. As Mr. Jones points out, the quantity of dust may affect the rapidity of reduction. If this is the case, effective ventilation will produce disturbance of the air and movement of dust as well as local currents from gas-jets, and it will be difficult to differentiate the two. The results which Mr. Jones obtains are precisely what might be anticipated if we take into account the fact that a coal fire produces an enormous air current through a room near the floor level, whereas a gas fire usually only serves to carry away its own products of combustion. Mr. Jones finds that the purest atmosphere is maintained with a coal fire and electric light; then follow gas fire with electric light, coal fire and gas light, and gas fire and gas light. The worst effect is produced by an open gas cooking stove without flue. The author shows, moreover, that gas light is more deleterious than a gas fire. That the worst atmosphere exists at the top of a room where the heated products of combustion accumulate is only natural. That is the reason, it is to be presumed, why the topsy-turvy method of ventilating at the floor level with a coal fire is the one most generally in use.

J. B. C.

OUR BOOK SHELF.

Lamarckiens et Darwiniens; Discussion de quelques Théories sur la Formation des Espèces. Par Félix Le Dantec, Chargé du Cours d'Embryologie générale à la Sorbonne. Pp. 191. (Paris: Félix Alcan, 1899.)

THIS is a well-intended, but scarcely adequate, endeavour to reconcile the Darwinian with the Lamarckian conception of evolution. While admitting the principle of natural selection as an important factor in organic development, the author seeks to explain the origin of species mainly on a Lamarckian basis. It may be doubted whether his suggested compromise will commend itself to either party. We are of opinion, *pace* M. Le Dantec, that Darwin's estimate of Lamarck was perfectly just; and that if Lamarckian views are to prevail, it must be by dint of facts and arguments other than those adduced by Lamarck himself. The present volume contains nothing approaching a demonstration of the inheritance of acquired characters; and until this is forthcoming, the Lamarckian fabric must be held to lack foundation. It is curious that the author, who has undoubtedly grasped the principle of natural selection, should not see how groundless is his hesitation in applying it. A reason for this failure is doubtless to be found in his tendency to deal with cases of adaptation as if they were ready made; he has apparently not taken into account the evidence of gradual approximation to the completely adapted condition. How, he asks, can chance have produced the aspect of *Kallima*? A study of allied forms might have shown him that his question was wide of the mark. On the crucial subject of mimicry and protective resemblance, this strange reluctance to carry an admitted principle to its legitimate end produces especially unfortunate results. M. Le Dantec is constrained, not only to suppose that the white of Arctic animals may be a direct result of the colour of their surroundings "as in Poulton's experiments on caterpillars," but to assume the conscious adoption of appropriate habits on the part of protected organisms. It would seem that not much is here gained by the abandonment of the Darwinian standpoint. In the last few chapters of the book the author expounds his "biochemical" theory of heredity, but without throwing any new light on the familiar difficulties of the subject. It is open to any one to proclaim his faith in the essentially chemical character of all kinds of protoplasmic activity, but the fact remains that among these phenomena there is a residuum which does not easily relate itself with what is known of the properties of other kinds of matter. This is where the problem was found by M. Le Dantec, and this is where he has left it. F. A. D.

Helen Keller: Souvenir. Pp. 65. (Washington: Volta Bureau, 1899.)

THE achievements of Miss Helen Keller bear striking testimony to what it is possible to accomplish in the education of the deaf. Though totally blind, as well as deaf, from infancy, she succeeded in passing the examination for admission into Radcliffe College, Harvard University, a year ago. In honour of this remarkable result, the Volta Bureau, which exists for the increase and diffusion of knowledge relating to the deaf, has published this souvenir, containing an account of her career, contributed by Dr. A. Graham Bell, Miss A. M. Sullivan and other instructors, and herself.

Dr. Bell considers that the lesson taught by Miss Keller's case is that books should be used at the earliest stages of a deaf or blind child's education. "I would have a deaf child read books in order to learn the language," he remarks, "instead of learning the language in order to read books." Miss Sullivan describes how she gave Miss Keller books printed in raised letters long before her pupil could read them. Words of particular shapes were associated with particular objects and actions, and in a comparatively short time Miss Keller thus acquired an exceptional knowledge of the English language.

Miss Sullivan employed the manual alphabet exclusively as a means of communication at the commencement of the child's education. She adopted the method of talking to Miss Keller just as she would to a seeing and hearing child, spelling into her hands the words and sentences she would have spoken to her if she could have heard, in spite of the fact that at first much of the language was unintelligible to the child. Three years after beginning to communicate by means of the manual alphabet, Miss Keller began to try to imitate sounds. Some deaf children are taught to speak by imitating the movements of the lips of the teacher. Miss Keller could not see these movements, but she could feel them by touching her teacher's lips, and she was soon able to reproduce the same sounds and articulate words.

How Miss Keller was prepared for admission into Radcliffe College, the entrance examination of which is exactly the same as that of Harvard College, is described by her instructors, and Miss Keller gives a simple chronological account of her studies. The whole statement is a remarkable narrative, and will be of the deepest interest to teachers of the deaf and students of psychological development.

The Psychology of Reasoning. By Alfred Binet. Translated by Adam Gowans Whyte. Pp. 191. (Chicago: Open Court Publishing Company. London: Kegan Paul and Co., Ltd., 1899.)

THIS is a translation of the second edition of M. Binet's well-known book, the main object of which is to show the essential similarity of perception and reasoning, and to illustrate the nature of the latter by our more complete knowledge of the former, especially with reference to the part played in perception by mental images. When the "Psychologie du raisonnement" first appeared, the more or less novel facts about mental imagery discovered by Galton and Charcot were described in a clear and interesting manner, and this feature remains without alteration. In fact no appreciable alterations have been made in the present edition, even when called for; thus Parinaud's evidence in favour of the central seat of after-images is repeated, although generally acknowledged to rest on a misconception of the relations between the two eyes. The book is full of interesting psychological facts; but unfortunately most of these are drawn from hypnotic experiments, and Binet does not yet appear to have recognised that, owing to the influence of unconscious suggestion, it is very dangerous to found psychological theories on such a basis.

Nevertheless M. Binet's work should be very welcome in an English form, and this the more so that the translation has been very well done.

Electric Batteries: How to Make and Use Them. Edited by P. Marshall. Pp. 63. (London: Dawbarn and Ward.)

THE principal forms of primary electric batteries are described in this little book, and some serviceable details are given concerning their working and use. The book will be particularly helpful by amateur electricians; and students of electricity will find in it some information not usually given in the text-books.

LETTERS TO THE EDITOR.

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

Snow-drifts on Ingleborough in July.

ON July 4 last I was on Ingleborough with a party of geologists examining the swallow-holes which mark where the water, running off the impervious drift and shale above Newby Moss on the southern shoulder of the hill, first reaches the Mountain Limestone. Some of these swallow-holes are what we may call obsolete—that is to say, when new openings have been formed and enlarged as time went on, some of the chasms which obviously at one time carried off the flood-water from a large gathering-ground, now receive only what oozes in from the peat and drift immediately round it, or the rain which falls directly on it: and rain seldom falls vertically up there. Some of them seem to have been developed without any large body of water having ever invaded them. They run to great depths, the open shaft being from 30 to 360 feet deep, below which the cavernous rock carries the water on through caves and crevices and open joints far down to the valley below.

In one of the vertical caves, which lies east of Long Kin and runs down into the limestone rock to a depth of some 70 feet, there were masses of snow to 4 feet in thickness. It was speckled brown on the surface, from the particles of peat which had been blown on to it, but was pure and white within. Obviously the chasm had been filled by drifted snow during the winter, and the summer's sun could not reach that depth to melt it, while the earth temperature was lost on the moist pinnacled rock on which it rested. No flood ever filled this particular chasm with a swirling torrent, such as at times fills Weathercote Cave, Hunt Pot, or even Gaping Ghyll up to the brim, and causes them to overflow.

Here, therefore, we had an accidental combination of conditions favourable for the preservation of snow, long past midsummer, at a height of not more than 1500 feet above the sea, on the flank of a Yorkshire mountain.

This is an interesting fact to bear in mind when speculating upon the causes of glacial conditions having so recently prevailed over the British Isles. We see here that half-way up Ingleborough, in an exceptionally hot summer, the air temperature alone could not remove last winter's snow.

T. MCKENNY HUGHES.

The Total Eclipse of the Sun of May 17-18, 1901.

IN the *Nautical Almanac Circular* (No. 18) local particulars of this eclipse are given for four places in the eastern portion of the shadow track, three of which, Padang, Pontianak and Amboyna, are situated in Netherlands India. In the explanations it is mentioned that, from inquiries which have been made, it appears that the positions selected are the most accessible, and that it would probably be impossible for observers to occupy any neighbouring station for which the astronomical conditions might be more favourable.

Surely these inquiries have not been made on the spot, where the information could best have been obtained. Other localities in the Government of Sumatra's west coast are as easily accessible as Padang, from which a railway leads to the interior; and other localities on the banks of the Kapuas as easily as Pontianak. Moreover, many other places may give opportunity to eclipse parties for observation, viz. on the islands of Lingga and Singkep, on the banks of the Barito and the eastern coast of Borneo, in the Gulf of Tomini (Celebes) and in the Moluccas. The conditions, however, will be most favourable in the western part of the Archipelago, both on account of the longer duration of totality and also for local resources. Through the Koninhlijhe Natuurkundige Veruniging at Batavia, data have been gathered referring to the conditions of weather and cloudiness at a number of stations most suitable for the observation of the eclipse, and the data will be published in due time. The general impression is, however, that the chance for fine weather is nowhere very great. The Society will be pleased to procure full information as to the choice of stations, and observers may be sure to receive every available assistance from the local authorities and officials in the Dutch colonies.

J. J. A. MULLER.

(President of the Kon. Natuurk. Ver., Batavia, July 17).

The Reform of Mathematical Teaching.

MANY schoolmasters tell us that boy-nature is so depraved that his time must be fully occupied, and that a "regular hard grind" is the only way to keep him out of mischief. They give him things to grind that do not interest him; it may be that he does not understand them, or that they have no human interest. And yet every boy has interests, and teaching directed towards those interests would enthral him. The first aim should be to attract the boy's attention, and a subject which no excellence of teaching will make interesting to a particular boy is no fit subject of study for him.

The case of mathematics is bad. The reasoning is too abstract for a boy's mind, but it has worse faults. Long strings of reasoning are employed to deduce fairly obvious conclusions from premises no more obvious, *e.g.* in the theory of parallels. On the other hand, incorrect proofs are given because the boy cannot grasp correct proofs, *e.g.* for the binomial theorem. Geometry is in worse case than algebra. Euclid's interest was logical rather than geometrical; he wished rather to put together a consistent series of syllogisms than to give the best solution of his problems: witness his bisection of a straight line. In consequence, the natural order of development is lost sight of. A boy ought to be at home with ruler and compasses before he reasons about the constructions possible with them, and yet most schoolboys have never handled compasses. A few weeks ago I asked some hundred boys in a well-taught school (as present teaching goes) to give a certain construction of Euclid's, and also to carry out the construction with ruler and compasses on a given line. Hardly one failed to write out the construction and proof, but only one of the hundred carried out the practical construction. Clearly our present Euclidian teaching has little to do with geometry.

To lay before a boy a proof he does not understand is useless, to prove the obvious is confusing, to give an incorrect proof is immoral. Prof. Perry's plan to abolish proofs in the early stages is a great step in advance of present teaching. For the boy of mathematical ability it would perhaps be well to run theory alongside, at the rate of five or six propositions for Euclid's entire first book. This would, however, interfere with class teaching, and the mathematical boy would lose little by going through a good deal of the practical course before touching theory; if with a hint here and there he could be got to evolve the theory for himself, he would gain much.

Possibly a theoretical training leads one to look with too favourable an eye on early theory. In any case, that in the hands of a good teacher theory and practice could well go hand in hand for boys even of average ability is shown by two able papers by Mr. Branford, in the *Journal of Education*, on the first teaching of geometry. We may finally reach this stage, but till we have these good teachers practice should precede theory.

DAVID MAIR.

Functions of an Organ of the Larva of the Puss Moth.

THIS season I am breeding, with the object of observing their gradual growth and development, a number of the larvæ of *Cerura* (or *Dicranura*) *vinula*, the Puss Moth; but I have sought in vain for the function performed by the slender red filaments, ejected, at the insect's will, from its twin tails. They appear to shoot from their sheathes, just on the same principle as do a cat's claws; but to what purpose?

Surely such a beautiful, delicate organism could not have been appointed to no purpose! Is this merely an instance of entomic mimicry, simulating, for its own protection, the sting of some venomous insect; or does this strange organ perform some practical, active function?

I shall be very greatly obliged if you can tell me whether this point has been already decided or not, and, in the latter case, perhaps some of your correspondents will kindly communicate their views upon it. ARTHUR S. THORN.

4 Malcolm Road, Penge, August 10.

Dark Images of Photographed Lightning Discharges.

A VERY clear illustration of the reason why some of the lightning discharges in a photographed thunderstorm appear dark was afforded me at Wednesfield, Staffordshire, about mid-day on Thursday, July 19. There were a number of double flashes, that is, two discharges occurring rapidly in the same apparent region, but following different courses, and separated in time by from one-eighth to one-half of a second. But one flash, quite near to where I stood (one second and a half between flash and sound), gave a repetition following absolutely the same path as the first flash and practically as bright. The only difference was that two faint branches of the first flash were not repeated in the second discharge. The second flash followed so quickly (about an eighth of a second, I estimate), that the impression on the retina of the first discharge had not died out when the second exactly covered it, so that I could appreciate the absolute coincidence. A few kinematographic records of thunderstorms would show whether or not such repetitions are common, and whether they are the cause of dark flashes on the photographic plate.

Cave Castle, Dumbartonshire, N.B. J. B. HANNAY.

THE LAVOISIER MONUMENT.

THE monument erected by international subscription in honour of Lavoisier was unveiled on July 27, in the presence of M. Leygues, French Minister of Public Instruction, and many eminent men of science, including most of the members of the International Congress of Chemistry. The committee entrusted with the raising of the fund for the statue succeeded in obtaining a sum of 100,000 francs, which was subscribed by admirers of Lavoisier in most parts of the civilised world. M. Moissan was the secretary of the committee, and he acknowledged at the unveiling ceremony that there had not been the slightest difficulty in obtaining the means to erect the monument—many subscribers, indeed, were astonished to learn that Paris, where monuments abound, did not possess a statue of the eminent chemist whose investigations helped to lay the foundations of modern chemistry. It is true that appreciation of the great chemist has been shown by the publication of his complete works, but these are only known to a limited number of students, and the people who live in the present are likely to forget how much they owe to the past unless they are reminded of their indebtedness by some striking monument in bronze or stone. For this reason, it is well that a permanent memorial of Lavoisier's greatness has now been erected.

FIG. 1.—The statue of Lavoisier.

The statue, which is represented in the accompanying illustration from *La Nature*, is erected in the open space behind the Madeleine Church, close to the house where Lavoisier lived for some years. It is of bronze, and stands upon a granite pedestal ornamented with bas-reliefs. The statue is by M. Barrais, and the pedestal is due to M. Gerhardt. Upon the front of the statue the following inscription appears, in French, "Antoine Laurent Lavoisier, 1743-1794, founder of modern chemistry. Erected by public subscription, under the patronage of the Academy of Sciences. M. Berthelot, Permanent Secretary of Physical Sciences, 1900." One of the bas-reliefs represents Lavoisier explaining his discovery of the composition of air to his colleagues of the Academy of Sciences, of which he was president, the characters introduced into the scene being Monge, Lagrange, Condorcet, Berthollet, Vicq d'Azyr, Laplace, Lamarc and Guyton de Morveau. On the other bas-relief Lavoisier is shown in his laboratory dictating notes to his wife. The statue appears to be a real work of art, worthy of the sculptor and of the subject.

M. Berthelot was to have presided at the ceremony of the unveiling, but illness prevented him from being present, and his address was read by M. Darboux.

Reference was made to the fact that the inauguration took place under the auspices of the Institute of France, the City of Paris and the French Government, and stress was laid upon the truly international character of the homage to the genius of Lavoisier, as testified by the subscriptions. The following is a free translation of parts of the address:—

> The names of Galileo, Newton, Leibnitz and Lavoisier show that science has no nationality, a monopoly of pure or applied science being the property of no one nation. The erection of a statue in a public place is an honour usually reserved for statesmen and warriors, men who have spattered the earth with blood, too often without lasting profit to the nation devoted to them. To-day the famous savant, thinker, artist, is put in the first rank by enlightened people, and posterity will doubtless continue to show an increasing respect for the memory of those men who have served the human race, and to relegate to obscurity the men of blood and intrigue who have enslaved it.

> The work of Lavoisier is epoch-making from two points of view, from that of philosophy, because he established the fundamental law which governs the chemical transformations of matter, and from the practical point of view, because this law has become the base of innumerable industries founded on these transformations, and the origin of the rules of hygiene and therapeutics which follow from it. The fundamental discovery of Lavoisier was the distinction between matter and the imponderable agents, such as heat, light, electricity, which

influence it. The discovery of this distinction overthrew all the old ideas dating from antiquity, and which continued up to the end of the last century. According to the ideas which were current when Lavoisier started his work, there were four elements —earth, air, fire and water—from which all substances existing in nature were said to be built up. By associating these elements in different proportions and by different methods, it ought to be possible to produce all bodies and transform any one into any other. As a matter of fact, the prolonged researches of serious workers had never succeeded in establishing this transformation, nor has this been accomplished since. But preformed ideas are tenacious, especially when supported by mysticism.

An equally grave mistake was committed in supposing that bodies submitted to the influence of heat alone could vary in weight, a variation apparently proved by innumerable observations with the balance in chemistry. It is, in fact, a most singular error, although one frequently held, that the use of the balance in chemistry dates only from the end of the last century. In reality its use is sixteen centuries old. The balance was used both in chemistry and in trade; it may be seen represented on the monuments of ancient Egypt. Bodies such as coal, oils and organic substances under the action of heat were known to lose their weight, hence was drawn the conclusion that matter may be transformed into heat and disappear; whilst heat, on the other hand, under inverse conditions, could be fixed, becoming visible ponderable matter. These opinions gave way to the views of Stahl, according to whom combustible bodies were rich in phlogiston, or fixed heat. Such was the state of science about 1772, when Lavoisier appeared on the scene. Ten years were sufficient for him to effect a complete transformation. He established, by the most precise experiments, a fundamental distinction, previously unknown, between the nature of bodies which we know, and heat and other agents capable of modifying these; it is the distinction between ponderable bodies and the imponderable heat, light, electricity, the intervention of which causes no change of weight in ponderable matter.

It could hardly have been expected that one man alone should make all the researches establishing the properties of gases, the composition of air and of hot water, and in this respect there can be no doubt that Lavoisier profited by the partial work of his predecessors and of contemporary workers; but to him alone belongs the merit of demonstrating the connecting links, and of giving the facts their true interpretation.

Two fundamental problems were first attacked by Lavoisier, the gain in weight of metals on calcination, and the apparent loss of weight of carbon, sulphur and oils on combustion. His first discovery was to put these phenomena upon a proper experimental basis. He demonstrated that in all such cases a weighable substance contained in the air takes part in the change, the addition of which explains the increase of weight of the calcined metals, an increase equal to the loss of weight sustained by the air. The same ponderable element in the air was shown to take part in the burning of carbon, sulphur and oils, forming gaseous compounds, the weight of which was also determined by Lavoisier. It was thus established, what had never been done before, that the materials of bodies possessing weight kept this weight constant throughout a series of chemical changes, heat and other agents of the same order having no effect either in increasing or decreasing the weight of the original bodies. This fundamental distinction between ponderable matter and imponderable agents is one of the greatest discoveries that has ever been made; it lies at the base of physical, chemical and mechanical science. Lavoisier, however, went farther than this, and attempted to penetrate the constitution of ponderable matter itself. He recognised that in all known experiments it presents itself as constituted by a certain number of undecomposable elements or simple bodies, which, combining amongst themselves, form all known compounds.

The two fundamental laws of nature once established—the distinction between matter and imponderable agents, and the invariability of the nature and weight of the simple bodies—Lavoisier went on to draw important conclusions on the composition of the acids and metallic oxides, the composition of air, water and organic substances, on the *rôle* of heat in chemistry, on animal heat and on the nature of respiration in physiology.

What share ought to be now attributed to Lavoisier in the classical discovery of the compound nature of air and water, a discovery in which he competed with Priestley and Cavendish? The matter would take too long to give here in detail. Suffice it to say that he alone swept from the composition of air and water the erroneous notion of phlogiston maintained by his contemporaries.

All these discoveries, accumulated in the course of only a dozen years, and carried out with wonderful ardour and energy, were not simple proofs of isolated facts; on the contrary, they were the consequences logically deduced and experimentally demonstrated of the two fundamental laws due to the genius of Lavoisier. The physiological questions relating to respiration were also answered completely and successfully; given a correct knowledge of the elementary composition of carbonic acid, of food materials and of air, respiration was then obviously a slow combustion of food by the oxygen of the air, a combustion producing carbonic acid and developing at the same time sufficient heat to maintain the human body at a nearly constant temperature.

A complete account of the after effect of Lavoisier's work would require almost a history of physical science during the nineteenth century; but an attempt will be made to recapitulate the more immediate consequences upon existing knowledge. The notion of the invariability of the weights of the simple bodies dominates the whole of chemistry at the present time; it is the scientific basis of all our chemical equations of composition and constitution, the origin of the new and singular algebra which, from its origin in the works of Lavoisier, so struck the mathematicians of his time. It is also the solid foundation of all our analyses, and is a certain starting-point in industries the most diverse, the manufacture of acids, alkalis, colouring matters, scents, drugs, in metallurgy and in agriculture.

And here a necessary reflection occurs. It cannot be pretended that Lavoisier was the direct and personal author of the vast array of discoveries here enumerated; but it is certain that it is he who has established the solid base upon which the modern chemical edifice is constructed, and without which these discoveries could not have been made; it is he who has raised the flaming torch of truth which we daily invoke, and for that reason it is just and equitable to give to him a part of the glory of the inventions of science and modern industry.

NILE FLOODS AND MONSOON RAINS.

THE practice or science of weather forecasting will evidently proceed on two very different lines—according to the relative importance of local or seasonal changes in the general meteorological conditions, and whether the prediction has reference to a long or a short period. The machinery employed in cases where the forecast aims at great minuteness over a small area consists mainly of the synoptical chart, based on information supplied by rapid telegraphic communication, and in the hands of experts this means probably proves sufficient, and furnishes a fair percentage of accurate predictions. But in the more difficult, as certainly the more important, problem of predicting the weather some time in advance, and over a considerable area, a problem which regularly recurs in the monsoon forecast for India, one must evidently depend on the more general physical conditions that are produced by the motions of the earth and the distribution of land and water on its surface. These causes, it is true, are always operative, and to a certain extent meteorological phenomena, broadly considered, must be periodic in their main features. The causes of deviation from periodicity, and the extent of the area affected by such abnormal conditions, are problems which the professional meteorologist has to encounter, and it is to be feared with insufficient means. But it seems not unlikely that, in proportion as the problem becomes more general, by bringing wider areas within the scope of the discussion, the prospects of greater success will become more assured; and it cannot but be considered a most significant feature that indications are not wanting that in the two considerable areas, India and Egypt, the respective climates betray peculiarities which may either react upon each other, or the origin of which must be sought in a common source.

From two independent investigations come attempts to trace a connection between the amount of the Nile floods and the abundance or deficiency of the south-west monsoon rainfall in India. Mr. Willcocks broached this subject in a paper read before the meteorological congress at the World's Exposition in Chicago, and there suggested that famine years in India are generally years of low flood in Egypt, and that when the summer supply of the Nile had been deficient and late, a high flood might well follow, since the drought in the valley of the White Nile must create a powerful draught from the Indian Ocean or the Arabian Sea, a district in which is to be sought the origin of the massive current of the south-west monsoon. Unfortunately, any exact data to establish this interesting connection are not forthcoming, and can hardly be expected, since the Nile is supplied from two distinct sources, and it is impossible to separate and trace the effect of either contribution. Of the great lakes of Central East Africa which constitute a reservoir for the Nile waters, little is known as to the variation in their relative height due to the rainfall in their vicinity, which lasts from March to December. At Port Alice, on the Victoria Nyanza, and at some other stations, observations, more or less regular, are made of the variation in the heights of the water, but in the absence of any common datum level these heights are referred to that of the mean lake. Much surveying work and long-continued observations will have to be made before these scanty statistics can be turned to full account. Of the second source of supply to the Nile, viz. the flood waters in the Atbara, the Blue Nile and other rivers, fed during the rainy season from June to November, we know practically nothing as to their amount. But it is this seasonal supply which is probably the greatest factor in causing variations in the Nile floods, and where a connection with the causes of the Indian rains is closest. Whatever influences the flow of the monsoon current from the equator northwards over the Indian seas towards the heated regions of India and the Malay Peninsula must have a proportional effect on East Africa and South Arabia. With heavy monsoon rains, therefore, it is not unlikely that the contributing rivers add materially to the volume of the Nile waters, but it is not altogether a trustworthy guide to gauge the amount of water that enters the Nile by measuring the quantity that passes a particular station. Much water enters the Nile that never contributes to the irrigation of Egyptian lands. Of the amount lost by evaporation no account can be taken, but a source of greater error arises from the peculiar flatness of the ground about Shambé, which forms the apex of the swamp delta. Here the Nile can spread its waters over a large area, and practically lose itself as a river among the beds of reeds and rushes, which form a veritable swamp. Engineering works, already projected or actually begun, aim at clearing some or other of the feeding streams, such as the Bahr el Gebel or the Bahr el Zarab, and the effect must be, when completed, to break the continuity of such observations as have been made. Other sources of error are to be found in the varying quantity and character of the "sudd" which may interrupt the flow or diminish the amount of evaporation; but without insisting on too much accuracy, there exists a certain amount of evidence that the two great agricultural countries of Egypt and India are likely to be prosperous together or to suffer in common.

Mr. Eliot, the meteorological reporter to the Government of India, in his recent forecast of the probable character of the south-west monsoon rains of 1900, not only fully endorses Mr. Willcocks' statement, but adds some statistics which render a connection highly probable. Omitting a few local particulars from Mr. Eliot's statistical summary, the broad features are shown below.

Year.	Variation of mean rainfall of year from normal.	Character of Nile flood.
	Inches	
1876	− 4·49	Good high flood.
1877	− 4·28	Poor flood.
1891	− 3·54	Late flood.
1896	− 4·83	Low Nile.
1899	− 11·14	Very low flood: lowest of century.

The years of excess of Indian rainfall tell a similar tale, even more distinctly.

Year.	Rainfall variation in inches.	Character of Nile flood.
1878	+ 6·34	Very severe flood: banks of river carried away in October.
1886	+ 3·02	High flood.
1892	+ 5·09	Very high and late flood.
1893	+ 9·07	High flood.
1894	+ 6·47	High flood.

Having mentioned some of the causes which prevent a rigorous comparison between the Nile floods and the Indian rainfall, one is not unprepared to find some discrepancies; but Mr. Eliot certainly does not overstate his case when he contends that these tables indicate that in at least four out of five seasons in which there was a partial failure of the rains in India there was a low Nile, and that generally the two countries are similarly affected by the meteorological conditions and the variations of those conditions. The causes of these variations are obscure, and at present very imperfectly recognised, for a complete solution, as Mr. Eliot points out, demands a much more intimate knowledge of the atmospheric conditions that prevail over a large area. The meteorology of Australia and the Indian Ocean, and perhaps also of the Antarctic Ocean, must be linked on to that of the Indian monsoon area "before it will be possible to ascertain the missing factors necessary to complete the explanations of the relations between the chief features of the monsoon currents and rainfall of India and the antecedent and concurrent conditions in the Indian area and the regions to the south." To trace and anticipate the effect of weather conditions over the area that embraces both India and Egypt, in which our interests are so largely involved, should stimulate further inquiry, with the result of placing at the command of science additional means for dealing with so grave a problem.

THE FORTHCOMING MEETING OF THE BRITISH ASSOCIATION AT BRADFORD.

IN the last article on the subject of the forthcoming meeting of the British Association an account was given of the handbook that is to be published in connection with the visit, and some information was furnished in regard to hotel and lodging accommodation. In the present article it is proposed to give a description of the excursions arranged by the local committee.

Following the custom of former years, it has been arranged that half-day excursions only shall take place on the Saturday, and that the whole-day excursions shall be reserved until the Thursday, when the serious work of the Association will be completed. The only exceptions to this are that the Mayor and Corporation are inviting a small party of engineers to visit their waterworks at Gowthwaite, in the Nidd Valley, and that a party exclusively for geologists will travel to Pateley Bridge by the same train in order to visit the Brimham Rocks. These two excursions will occupy the whole of Saturday. The excursions, then, arranged for Saturday, September 8, are as follows :—

BOLTON PRIORY.—The party will leave the Bradford (Midland) Station at 1.32. Drive from Bolton to the Priory, where they will be received by the vicar, the Rev. A. P. Howes, who will give a brief description of its history and architecture. They will then drive forward along the banks of the Wharfe to the Wooden Bridge, where tea will be provided : an opportunity will be given for a visit to be made to the Strid (celebrated by Wordsworth), and then the party will be driven back to the station in order to reach Bradford in time for dinner. Mr. Geoffry Fison will be the leader of the excursion.

FARNLEY HALL.—The residence of the Fawkes family, which contains a wonderful collection of Turner's pictures. The Hall is of great historical interest, as it was the residence of Lord Fairfax in the time of the Civil War, and many relics of the period are shown. The party will leave the Midland Station at 1.15 for Otley, where they will be met by Major Mitchell, of Cayley Hall, the leader ; they will then be driven to Farnley, and as much time as possible will be spent in inspecting the Turner pictures and the beautiful old Hall. Major Mitchell will afterwards entertain the party to tea in his grounds, and they will then be driven back to the station.

ILKLEY.—The excursion will start from the Midland Station at 1.32, under the leadership of Mr. Mortimer Wheeler ; at Ilkley the party will be divided into several smaller bodies, who will in turn visit the Roman camp and fortifications, some curious Saxon crosses that are to be seen in the churchyard, and some remarkable instances of cup and ring marks, which are to be seen on Rombald's Moor above the village. At 4.30 the different parties will reassemble in the beautiful grounds of the Wells House Hydro, where they will be entertained to tea by the invitation of the directors. They will arrive back in Bradford about 7 o'clock.

HAWORTH.—The train will leave the Midland Station at 1.20, and the party will be met at Haworth by Mr. F. Greenwood, the president of the Brontë Society, who will escort them to the church and the Brontë Museum, and show them many places which will be familiar, from description, to the readers of "Shirley." The leader of the party will be Mr. J. A. Clapham.

KNARESBOROUGH.—Major H. D. Sichel will conduct a party to Knaresborough, the train leaving at 1.15. On arrival, the visitors will be divided into two parties, and, under the leadership of Major Sichel and Mr. Arthur Harris respectively, they will be taken by opposite routes to visit the Castle, the petrifying Dropping Well, and Eugene Aram's Cave. Afterwards they will be driven to Plumpton Rocks, where tea will be provided, and they will return by a train reaching Bradford about 7.30.

KIRKSTALL ABBEY AND ADEL.—The train will leave the Midland Station at 1.25, and Kirkstall Abbey will be described by Mr. E. Kitson Clarke, the leader. The visitors will then be driven to Adel Church, which is almost a unique instance of Saxon architecture, and which will be described by the vicar. They will then drive back to the Yorkshire College, Leeds, where they will be entertained to tea by the principal, Dr. Bodington, one of the vice-presidents of the Association.

PATELEY BRIDGE.—As indicated above, this is the only excursion extending over the whole day. Two parties will leave by a special train at 1.15, the one conducted by the Mayor (Mr. Wm. C. Lupton, J.P.), for a small party of engineers, who will be driven from Pateley to the Nidd Valley Waterworks ; the other, exclusively for geologists, who, under the leadership of Mr. J. Lower Carter, will walk to the Brimham Rocks, and visit other places of geological interest.

For the week-end (September 8–10), the Yorkshire Naturalists' Union are organising a specially interesting excursion. The district which has been selected is the neighbourhood of Grassington, in Upper Wharfedale, which is not merely a romantically picturesque region, but a remarkably good district for nearly all branches of natural history and geology. The excursion is intended, as far as possible, to be one strictly for practical working naturalists, and as accommodation is very limited, it will be needful to give preference to such members of the British Association as are likely to investigate in their own particular department. The arrangements will be under the direction of leading Yorkshire naturalists, who hope to introduce their comrades from other parts of the country to a remarkably interesting district. There will be the usual fully descriptive circular prepared, which will be sent to any one who may apply for it to the hon. secs. of the Yorkshire Naturalists' Union, Leeds.

THURSDAY, SEPTEMBER 15.—The whole-day excursions arranged for the concluding day of the meeting are as follows :—

THE ACKTON COLLIERY.—This excursion, which will be under the leadership of Mr. C. J. Cutcliffe-Hyne, is intended for a limited number of botanists, geologists and engineers, in order that some opportunity may be given them of examining the Yorkshire coal-measures. The party will be divided into two on arriving at Featherstone : the one will be taken down the pit, while the other will examine the machinery and various interesting material on the bank. They will then meet at one o'clock and will be entertained to lunch by Lord Masham, the owner of the mine, after which the proceedings will be reversed, and the respective parties will be taken round the bank and down the pit ; they will then reunite, and after partaking of afternoon tea will return to Bradford.

BOLTON PRIORY.—This is an amplification of the previous half-day visit, again under the leadership of Mr. Geoffry Fison. Fuller opportunities will be furnished of seeing the Priory and the Strid, and lunch will be provided at the Wooden Bridge. In the afternoon a visit will be made to Barden Tower, the ancient keep of the Lord Clifford, of the Wars of the Roses fame, and of his son, the Shepherd Lord.

RIPON AND FOUNTAINS ABBEY.—Mr. Mortimer Wheeler will conduct a party to Ripon ; after a special musical service at the Cathedral, they will be driven to Fountains Abbey, and lunch will be provided in the Refectory. They will then be taken over the ruins by the Dean of Ripon, after which the Marquis of Ripon will entertain the party to tea. On returning to Ripon, if time permits, they will be conducted to the crypt and the more interesting parts of the Cathedral by the Dean before leaving for Bradford.

SWINTON PARK.—By the invitation of Lord Masham and under the leadership of the Mayor, Mr. William Lupton, a party will visit Masham. On arrival, they will be driven to Jervaulx Abbey, the ruins of which, of course, possess great historical interest, and will then return to Lord Masham's residence, Swinton Hall, the drive each way being of extraordinary beauty. At the Hall they will be entertained to lunch by Lord Masham, after which the afternoon will be spent in inspecting the very fine collection of Old Masters and modern pictures, and the party will drive to the station to join the special train, which will convey also the party from Ripon.

MALHAM.—A party, under the leadership of Mr. Cecil Slingsby, will leave at an early hour for Bell Busk ; thence they will drive across country to Malham, and after lunch they will visit Gordale Scar, and, if time permits, at the invitation of Mr. Walter Morrison, M.P., they will go on to Malham Tarn and Malham Cove. They will leave Malham about 5 p.m. and drive to Skipton, visiting Skipton Castle on the way, and thence by train back to Bradford.

SETTLE AND CLAPHAM.—By the same train which conducts the party to Malham, another party will leave for Settle under the guidance of Mr. J. J. Brigg. After visiting the Victoria Caves, they will drive to Ingleton and lunch. From there they will walk through the beautiful grounds of Mr. J. A. Farrer and explore the Clapham Caves, in which most extraordinary specimens of stalactites and stalagmites are to be seen.

The two last excursions are specially intended for geologists.

YORK.—It is, of course, essential that York, where the first meeting of the British Association was held seventy years ago, should be visited. The party will arrive in York about 11 o'clock, under the leadership of Mr. J. A. Clapham. The visitors will immediately proceed to see the walls, the museum, and St. Mary's Abbey. Then, after lunch at the Station Hotel, they will visit the Minster, where most of the afternoon will be spent. By the invitation of the Lord Mayor, they will afterwards be entertained to tea at the Guildhall before leaving for the station.

For all the half-day excursions a uniform charge will be made, and similarly for Thursday's excursions there will also be a uniform charge. Visitors applying for excursions will be required to hand in this fee, together with the application form; and tickets, as nearly as possible in accordance with their preferences, will be allotted to them. By making all the excursions of equal cost, it is expected that the work of allotment will be simplified.

The next article will deal with the mayoral and civic functions that have been arranged, and some account will be given of the large garden-party which the municipality will hold on Monday, September 10, and of the various private garden-parties to be given on September 12.

RAMSDEN BACCHUS.

NOTES.

WE regret to announce the death of Dr. John Anderson, F.R.S., the distinguished zoologist.

DR. D. MORRIS, C.M.G., the Imperial Commissioner of Agriculture for the West Indies, has just arrived in this country.

PROF. G. CAREY FOSTER, F.R.S., has been appointed Principal of University College. Prof. Foster is a Fellow of the College, and was formerly professor of Experimental Physics and Quain Professor of Physics; he is also a Fellow of the University of London, in which University he acted as examiner previous to his election to the Senate.

THE International Geological Congress is now in session at Paris. Among the items included in the programme are discussions on international co-operation in geology, fundamental researches for the establishment of a definitive classification, scheme for an international lexicon of petrology, and the photography of types of fossil species.

REUTER reports that Major Gibbons, the African traveller, reached Omdurman on August 20. The line of route traversed by the expedition represents a distance of 13,000 miles. Among the objects attained were the mapping of Barotseland; the accomplishment of the first steam navigation of the Middle Zambesi; and the tracing of the whole course of the river, the discovery of its source, and the determination of its watershed. Thence the route of the expedition was eastward, and by way of the Great Lakes and the Nile.

THE annual meeting of the English Arboricultural Society was held at Manchester last week. Prof. Somerville was appointed president for the ensuing year. Reports were read from the judges upon essays on "Foreign *versus* Native Timber," "Agricultural and Woodland Drainage," and "Thinning." The silver medal for the first essay was awarded to Mr. George Cadell, late of the Indian Forest Department, and bronze medals for the other essays were given to Mr. D. A. Glen, of Kirby, near Liverpool, and Mr. A. Dean, of Egham.

THE third annual report of the Council of the Röntgen Society shows that the society is making satisfactory progress. The demonstrations at the meetings are very valuable to all workers with Röntgen rays, and the papers and abstracts published in the *Archives* enable members who are unable to attend the meetings to keep well in touch with the latest developments of radiographic work. Dr. J. B. Macintyre, one of the earliest and most prominent investigators with Röntgen rays, has consented to be nominated as the next president of the society.

SIR WILLIAM STOKES, the eminent surgeon, died suddenly at Pietermaritzburg on Saturday. He filled the post of President of the Royal College of Surgeons of Ireland in 1896; and among his other appointments was the professorship of surgery in the Royal College of Surgeons in 1872, senior surgeon of the Government Hospital of Ireland in 1868, president of the Pathological Society of Ireland, and Surgeon in Ordinary to the Queen in Ireland from 1892. He was the author of a number of addresses, and contributions to the medical press, on clinical and operative surgery.

A REUTER telegram from St. Petersburg states that news has been received there from Dr. Sven Hedin, showing that his expedition this spring to Lob Nor to settle the various questions in dispute regarding that lake and its surroundings has resulted in discoveries exceeding his expectations. He found, in fact, that the lake known to previous explorers no longer exists, having dried up, leaving its bottom strewn with shells and marine growths. Around this old basin, however, a regular system of new lakes has been formed, which Dr. Sven Hedin has explored and mapped. In connection with this announcement, it is worth remark that at the time of the visit of Prince Henry of Orleans to Lob Nor, towards the end of 1889, the lake consisted of a number of interlacing lakes and river-arms, the contraction of the former large water-area being probably due to the using up of the waters of the Tarim for irrigation by the increasing population of the river basin.

THE *Scientific American* announces that the U.S. Congress has granted funds for the inauguration of agricultural experiment stations in the islands of Hawaii and Porto Rico. Prof. S. A. Knapp has been selected to investigate the agricultural conditions and possibilities of Porto Rico. He went to the island in June, and will study the lines of experimental investigation which should be undertaken there, places suitable for stations, and the approximate expense of inaugurating and maintaining the work. Dr. W. C. Stubbs will make a preliminary survey of the conditions in the Hawaiian Islands. He sailed for Hawaii about the middle of July, and will spend the month of August in the islands. The conditions there are somewhat different from those of Porto Rico, as a station for experiments in sugar production has been maintained by private beneficence for a number of years.

THE Berlin Academy of Science has (says *Science*) made the following grants for scientific work: Prof. Adolf Schmidt, of Gotha, for the collating and publication of material on terrestrial magnetism, 750 marks; Dr. Leonhard Schultze, of Jena, for investigations on the heart of invertebrates, 2000 marks; Prof. Emil Ballowitz, of Greifswald, for investigations on the structure of the organs of smell of vertebrates, 800 marks; Dr. Theodore Boveri, of Würzburg, for experiments in cytology, 500 marks;

Prof. Maxime Braun, of Königsberg, for studies on the Trematodea, 970 marks; Dr. Paul Kuckuck, of Heligoland, for investigations on the development of Phæosporeæ, 400 marks; Dr. Wilhelm Solomon, of Heidelberg, for his geological and mineralogical investigations in the Adamello mountains, 1000 marks; Prof. Alexander Tornquist, of Strassburg, for the publication of his work on the mountains of Vicenza, 1100 marks; Prof. Alfred Voltzkow, of Strassburg, for the drawings of his work on the development of the crocodile, 1000 marks: Prof. Johannes Walther, of Jena, for the publication of his work on deserts, 1000 marks.

WE learn from the *Daily Graphic* that the Norwegian Government has built and fitted out a steam vessel for the express purpose of marine scientific research, and has placed her, as well as a trained staff of assistants, in charge of Dr. J. Hjort as leader of the Norwegian Fishery and Marine Investigations. The vessel herself, the *Michael Sars*, has been constructed in Norway on the lines of an English steam trawler—that type of boat being regarded as the most seaworthy and suitable for such an expedition—but considerably larger, being 132 feet in length, 23 feet beam, and fitted with triple expansion engines of 300 horse-power. The fishing gear includes, *inter alia*, trawls, nets, and lines of all kinds, with massive steel hawsers and powerful steam winches to work the heavy apparatus, while the numerous scientific instruments are of the very best and latest description. The expedition left Christiania in the middle of July, on what may be termed its trial trip along the Norwegian coast (accompanied for part of the time by Dr. Nansen, who was desirous of testing various instruments in which he had made improvements), and has just sailed from Tromsö on a lengthy cruise to the North Atlantic and Arctic Oceans. Dr. Hjort has already added so much to the knowledge of pelagic fishes, their life, habits, and the causes affecting their migrations, that, with the means now at his disposal, a considerable amount of valuable information will probably be gained which will prove of service to the fishing industry of all nations.

MESSRS. JOCHELSON AND BOGORAS, of the Jesup North Pacific Expedition of the American Museum, recently started for the north-eastern part of Asia, by way of San Francisco and Vladivostok, to continue the work of the expedition in Siberia. A few particulars of the investigations undertaken are given in the *American Museum Journal*. The region to be visited is situated north-east of the Amur River. The explorers will study the relations of the native tribes of that area to the inhabitants of the extreme north-western part of America, and also to the Asiatic races visited by Dr. Laufer, under the auspices of the Museum, and to those living farther west. It is expected that in this manner they will succeed in clearing up much of the racial history of these peoples, and it is hoped that the question as to the relations between the aborigines of America and Asia will be definitely settled. Thus the work proposed is part of the general plan of the Jesup North Pacific Expedition, which was organised for the investigation of the relations between the tribes of Asia and America. It is fortunate that this inquiry has been taken up at the present time, since the gold discoveries along the coast of Bering Sea are rapidly changing the conditions of native life; so that within a few years their primitive customs, and perhaps the tribes themselves, will be extinct. It is expected that the journey, which will extend over a period of two years, will result in a series of most interesting additions to the collections of the Museum, and in an important advancement of our knowledge of the peoples of the world.

IT has already been noted (vol. 61, p. 451) that Prof. A. Heilprin has brought forward evidence which throws doubt upon the permanence of the waters of Lake Nicaragua, the fountain head of the San Juan River. His conclusions have been criticised, but he gives further reasons for them in the *Bulletin* of the Geographical Society of Philadelphia (July), and shows that this new factor will have to be taken into consideration in connection with the proposed Nicaragua Canal. The full conclusions now drawn by Prof. Heilprin from data furnished by the Nicaragua Canal Commission of 1897–99, and the special reports of the chief engineer and hydrographer appended thereto, are:—(1) Lake Nicaragua has undergone marked shrinkage during the period of the last twenty-five to fifty years. (2) The shrinkage is a progressive one, and there are no known conditions by which the loss incurred can be made good. (3) The assumption is well founded that the earlier measurements of the altitude of the lake surface, made by Galisteo and Baily, indicating an abasement of the waters by 20 to 30 feet, were accurate. The relations of these conditions to canal construction become immediately apparent, and it may well be agreed that a region subject to the changes which have been indicated "would offer serious obstacles to the construction of a canal of the magnitude of the one proposed or to its permanency after construction."

MR. W. N. SHAW, F.R.S., informs us that Mr. W. Kennedy, the observer for the Meteorological Office at Roche's Point, co. Cork, notes that at 9.15 p.m. (G.M.T.) on August 13 a very large meteor shot into view eastward, going E.S.E. At about an altitude of 70° it exploded with a brilliant flash, and a noise was heard like that of a rocket fired off at some distance. The meteor left a long luminous track visible for some seconds after the explosion. The trail would have been very brilliant but that the eastern sky was lit up by the moon at the same time.

IN the afternoon of Friday, August 17, some parts of the south of London were visited by one of the sharpest thunderstorms that have occurred for some time. The weather was very close, the thermometer reaching 82°, and the distribution of barometric pressure was of a complex character. During the storm, which lasted about an hour, and was accompanied by a heavy hail squall, the amount of rainfall at the central part, near Herne Hill, was 1·2 inch. In some parts of the suburbs the roads were completely flooded, while in others comparatively little rain fell. At Westminster there was none, at Brixton 0·4 inch, and at Greenwich only five-hundredths of an inch. During the same afternoon a severe thunderstorm also occurred at Ilford, Essex.

A DISCUSSION of the thunderstorm observations recorded in 1897 at ten selected stations in India, by Mr. W. L. Dallas, is contained in Part ix. vol. vi. of the *Indian Meteorological Memoirs*. The results for the year have been divided into five-day periods. The storm-frequency varies considerably in different parts, but, generally speaking, the number of storms is unimportant during February and the early part of March; but after the middle of March the thunderstorm season commences, and continues until the middle of October, the maxima occurring towards the end of May and September. After October 23 no storms are reported. Storms are much more frequent in the afternoon than in the morning, and when a storm occurs in the forenoon it is followed, almost without exception, by another in the afternoon. There is a belief that the damage done by lightning in the tropics is slight compared with that done in temperate zones, and the fact that at ten observatories in the year in question only four instances of damage being recorded gives support to this belief.

PROF. CANCANI remarks in a recent paper (*Ital. Soc. Sismol. Boll.*, vi. pp. 37–42) that seismology stands almost alone among the sciences of observation and experiment in that so far no pattern instrument and no comparable apparatus have been introduced. He admits that the Seismological Committee of the British Association have taken a step in the right direction, but considers that the instrument used by them possesses several defects which prevent its general adoption. The conditions which should be satisfied by the type apparatus, he describes as follows: It must be astatic or possess a stationary mass, and must be equally capable of recording the very small and rapid preliminary vibrations and the subsequent undulations of long period; it must have the sanction of experience, the cost of erection and maintenance must be small, and the construction so simple that it does not easily get out of order; it must allow the continuous inspection of the traces, and its sensibility must lie within convenient limits.

ABOUT two years ago Dr. Sambon brought forward evidence that sunstroke was an infectious disease, and consequently due to microbic influences. This view has not met with general acceptance, and Mr. E. H. Freeland, who has had exceptional opportunity of observing cases of sunstroke, both ashore and afloat, shows in the *Middlesex Hospital Journal* (July) that all the phenomena of this affection can be explained on general physiological principles without reference to germs at all. He concludes his paper as follows:—"Whether sunstroke be due to external physical causes, or whether it be an infectious disease and due primarily to a micro-organism which has yet to be isolated, must be decided in the future. For the present it seems to me that there is ample evidence for believing that sunstroke is due primarily to thermic influences—the exposure of the body to a hot moisture-laden atmosphere—and secondarily to the circulation in the blood of certain toxic poisons, the result of perverted tissue metabolism; and that, until more tangible evidence is brought forward to prove that the affection is due to microbic influence, one may safely accept the older doctrine with regard to its causation as a sound working hypothesis, if nothing else."

PROF. F. E. NIPHER, of Washington University, St. Louis, Missouri, has sent some further particulars with reference to the methods he uses to obtain a "zero" plate. His observations upon photographic reversal have already been noticed in these columns (pp. 62, 159), and he has pointed out the bearing of his work upon eclipse photography (p. 246). The following details of the operations he follows may enable other photographers to repeat his experiments. "The plate is placed under a punched stencil in a printing frame. It is exposed at l cm. from a 16 c.p. lamp. By a few trials one can find the time-interval of exposure, so conditioned that nothing will develop on the plate in a developer of fixed composition, strength and temperature, and at a fixed distance from the 16 c.p. lamp. This is a standard developer. With a shorter time of exposure than that giving the zero plate, a negative will result, and with a longer time, a positive. A plate to be used in taking any picture to be developed in the standard developer (as a positive) is all exposed to the 16 c.p. light at a distance l cm. for a time which experiment has shown will put the film into the zero condition when developed in the standard bath. It is then put into the plate-holder, and given a camera exposure in the usual way, after which it is developed. It is not important that the developing bath should be at any particular distance from the lamp. The plate is to be pre-exposed so that a zero plate will result in that particular bath, at any fixed distance from the lamp. I usually make this distance about eight inches."

ACCORDING to Maxwell's electromagnetic theories, a moving body charged electrically produces a magnetic field. In the *Bulletin* of the French Physical Society, M. V. Crémieu gives a brief note on certain experiments destined to test the actual existence of such a field, as well as the converse result that a moving charge placed in a variable magnetic field experiences a certain ponderomotive force. Having, at the suggestion of M. Lippmann, conducted some experiments for the purpose of investigating the latter effect, with negative results, M. Crémieu now gives an account of certain investigations made with a disc of 37 cm. in diameter, rotating at the rate of 100 to 130 revolutions per second in the centre of an annular coil connected with a highly sensitive galvanometer. If the disc is suddenly charged, the convection current thus produced should give rise to an induced current through the galvanometer, and the magnitude of the convection current being determined by the number of revolutions and the density of the charge, the amount of the expected deflection of the galvanometer could be calculated. No deviation of the predicted magnitude was obtained, and the author concluded that a moving charge does not produce a magnetic field. Such a conclusion leads logically to the rejection of existing theories of the electric current, and M. Crémieu proposes to conduct further experiments with the object of throwing more light on this difficult question. The author does not, in this note, say anything about the effects of the self-induction of the rotating disc, and further information on this point appears desirable in criticising the results.

A FEW interesting details referring to the use of wireless telegraphy in the French navy are given by a naval correspondent of the *Daily Graphic*. It is stated that half-a-dozen ships in the combined French squadron recently at Cherbourg were fitted for wireless telegraphy, and the clicking, crackling, and sparking of the big coils was heard on board all day. Messages have been taken in and sent out at distances quite twice or three times as great as anything achieved with the instruments in use in the British ships. The French do not fit the wire to a gaff as in our ships; it is suspended between the funnels to the triatic stay, and is much less conspicuous. The manœuvring of the submarine boats, *Morse* and *Narval*, is described as marvellous; they are, it is stated, much ahead of the American *Holland* boat, which is considered to be a formidable weapon.

AN interesting and detailed account of Count von Zeppelin's successful trial trip of his navigable balloon on July 2 is given in *Die Umschau* by an anonymous author, who has endeavoured to dispel the somewhat exaggerated reports which have been circulated as to the success or failure of the experiment. It is pointed out that the delay in the ascent, which some persons attributed to an accident, was really caused by the wind being too strong at the time originally proposed for the trip. The wind-velocity at the time of starting was 5·5 metres per second, and the balloon was actually driven forwards for a short distance in the face of this wind. But after a short time the path deviated till it made an angle of 30° with the wind-direction. This deviation, the writer explains, was due to several causes. In the first place the rope broke which supported the movable mass necessary for the maintenance of longitudinal balance, and to restore equilibrium it was necessary to stop or even reverse one of the machines, so that the balloon could no longer be driven full ahead. Moreover, the framework was found to have undergone a little deformation, which gave the machine a slight bias to one side, interfering with the steering. The wind causing the balloon to drift towards the shore, a descent was made in order that Count Zeppelin might land on the water (to use an Irishism), and thus have his machine towed back by steamer. The descent was very gradual, the cars gently sinking down to the water without the sudden jerk which is commonly experienced in an ordinary balloon. This result is attributed to the favourable form of the balloon, a cylinder experiencing greater resistance than a sphere. The performance of the motors and screws is described as brilliant.

WE have already referred to the great loss anthropology has sustained in the death of Mr. Frank Hamilton Cushing on April 10. In the current number of the *American Anthropologist* are memorial notices by various leading American anthropologists, from which it is evident that a peculiarly gifted and winning personality has passed away. Mr. Cushing had great manual dexterity and an acute appreciation of how things were made, and he had practised himself to do anything an American Indian could accomplish, and with the same limited resources. For five years he lived with the Zuñi Indians, living their life and familiarising himself with their ideas and modes of thought, and he rose high in the social Pueblo life, taking part in their councils and in their sacred ceremonies. An intense eagerness to learn more and more of aboriginal thought and deed was the mainspring of his life, and his kindly sympathetic nature and keen intelligence and dexterity placed him in the front rank of field investigators. We understand that Mr. Cushing left an immense amount of MS. material, which it is to be hoped will be fully published, for his published works by no means do justice to the extent and value of his researches.

OUR contemporary *Science*, for July 27, contains a summary of the "Lacey Act," recently passed by Congress for the protection of game and other birds in the United States, and for the regulation of the importation of foreign birds and mammals. The carrying out of this important Act has been confided by the Secretary of the Department of Agriculture to the Division of Biology, Dr. T. S. Palmer being the officer selected to supervise its actual administration. Dr. Palmer has lost no time in making known the principles of the new law, having already published a *Bulletin* of the Department, entitled "Protection and Importation of Birds, under Act of Congress, approved May 25, 1900." As regards the importation of wild animals and birds, an absolute veto is placed on certain injurious species; and importers must in all cases obtain special permits from the Secretary of Agriculture before a single individual can be landed. These permits should be applied for in advance. No permits are issued for shipping birds from one State to another, although in certain States the Commissioners of Fish and Game have authority to allow the shipment of a limited number for breeding purposes. No permits are necessary for domesticated birds, and the same applies to natural history specimens for museums. In the case of the larger ruminants special permits will be issued, as heretofore, in the form prescribed for domesticated mammals. The prohibited species include the European house-sparrow, the starling, fruit-bats or flying foxes, and the mongoose, or ichneumon. Special inspectors are appointed to carry out the law, and to give advice in cases of difficulty. The attention of all concerned is drawn to those sections which make it unlawful to ship from one State to another animals or birds taken in contravention of local laws, and which require all packages containing live birds and animals to be clearly marked with the name and address of the shipper, and with the nature of their contents.

WHETHER or no the inferior animals have souls, forms the subject of an article by Herr S. von Uexküll in the *Biol. Centralblatt* of August 1.

IN Part 3 of vol. xxviii. of the *Morphologisches Jahrbuch*, Dr. B. Haller publishes his third memoir on the vertebrate brain, treating specially of that of the mouse, but adding some observations in regard to Echidna. The second article in the same number is by Dr. Fürbringer, and treats of the systematic position of the Myxinoids. The author is of opinion that vertebrates should be subdivided as follows:—

I. Acrania (Amphioxina).
II. Craniota:
  (1) Distoma (Myxinoides).
  (2) Cyclostoma (Petromyzontes).
  (3) Gnathostoma:
    (*a*) Anamia (Pisces, Dipneusta, Amphibia).
    (*b*) Amniota (Reptilia et Aves, Mammalia).

WE have received the *Report* of the Manchester Museum for 1899-1900. From this we learn that the Museum has been enriched during the period in question with two collections of first-class importance, one of these being Mr. C. H. Schill's cabinet of Lepidoptera, and the other the Layard collection of weapons and implements.

IN the concluding part of his "Ornithological Notes," published in the July issue of the *Victorian Naturalist*, Mr. Robert Hall, of Melbourne, discusses the question whether a tree-building diamond-bird (Pardalotes) is the foster-parent of a cuckoo. In the case referred to the young cuckoo was actually seen to be fed by the diamond-bird, one of whose own young was brought up with it. The incident is at present quite unique.

THE Library of the Patent Office is an institution known and appreciated by many students of science, both pure and applied. A series of classified catalogues of the contents of the Library has just been started by the publication of a "Subject List of Works on Photography and the Allied Arts and Sciences." Each volume of the series will contain (1) a general alphabet of subject headings, with descriptive entries, in chronological order, of the works arranged under these headings; (2) a key or a summary of these headings shown in class order. The present list comprises 557 works (73 serials, 484 text-books, &c.) wholly or in part photographic—representing 1300 volumes. The catalogue is really a valuable little bibliography of photography as well as a guide to the contents of the Library.

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus*), a —— Monkey (*Cercopithecus*, sp. inc.) from West Africa, presented by Mr. L. J. Sparrow; a Mozambique Monkey (*Cercopithecus pygerythrus*) from East Africa, presented by Mr. C. Mackay; three Pheasants (*Phasianus colchicus*), British; a Common Peafowl (*Pavo cristatus*, ♂) from India, presented by Captain G. H. Arnot; a Long-legged Buzzard (*Buteo ferox*), a Black Kite (*Milvus migrans*), two Lesser Kestrels (*Tinnunculus cenchris*), European, two American Kestrels (*Tinnunculus sparverius*) from America, presented by Mr. J. Simonds; a Bengal Weaver Bird (*Ploceus bengalensis*), a Manyar Weaver Bird (*Ploceus manyar*), four Black-throated Weaver Bird (*Ploceus atrigula*), an Indian Roller (*Coracias indica*) from India, presented by Mr. E. W. Harper; a Spiny-tailed Iguana (*Ctenosaura acanthura*) from Central America, presented by Mr. C. Hagenbeck; a Common Lizard (*Lacerta vivipara*), British, presented by Mr. Stanley S. Flower; a Military Macaw (*Ara militaris*) from South America, a Roseate Cockatoo (*Cacatua roseicapilla*), six Blue Lizards (*Gerrhonotus coeruleus*) from Australia, three Blue-tongued Lizards (*Tiliqua scincoides*) from Western North America, a White-collared Kingfisher (*Halcyon chloris*) from India, a Saddle-backed Tortoise (*Testudo ephippium*), three Albemarle Tortoises (*Testudo vicina*), two Thin-shelled Tortoises (*Testudo microphyes*) from the Galapagos Islands, deposited; an Argali Sheep (*Ovis ammon*, ♀) from the Altai Mountains, two Black Storks (*Ciconia nigra*), European; a Ring-necked Pheasant (*Phasianus torquatus*) from China, purchased; four Indian Crows (*Corvus splendens*), a

Little Cormorant (*Phalacrocorax javanicus*), a Green-winged Dove (*Chalcophaps indica*) from India, received in exchange; a Japanese Deer (*Cervus sika*), five Rosy-billed Ducks (*Metopiana peposaca*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

VELOCITIES OF METEORS.—At the second annual meeting of the Astronomical and Astrophysical Society of America, recently held at Columbia University, New York, Dr. W. L. Elkin described the apparatus and results of photographs obtained at the Yale Observatory for the determination of the velocity of meteors (*Science*, vol. xii. pp. 125-6). The idea of using photography for this purpose appears to have been first suggested by J. H. Lane in 1860, but it was not until 1885 that Zenker made the next practical attempt in Berlin, and attention has again been recently called to the matter by Prof. Fitzgerald. The Yale apparatus consists of a bicycle wheel fitted with twelve radial opaque screens, fixed so that, while rotating, the screens are brought intermittently in front of the cameras. The wheel as at present worked makes about 50-60 revolutions per minute, but it would be better to increase this speed in future apparatus. A check on the velocity is afforded by records made each revolution on a chronograph. The length of interruption of the meteor trail and the consequent velocity are then determinable if a second observation of the meteor from a distant station has been obtained. In November and December 1899, five such duplicate trails were secured. The apparent velocities of these are given as 50·4, 12·2, 50·3, 20·2, 36·5 kilometres per sec.; their altitudes varying from 45 to 100 kilometres. Correcting the apparent velocities for the attraction of the earth and the diurnal rotation by Schiaparelli's formulæ, the true velocities with respect to the sun are 34·4, 32·0, 32·4, 39·8, 34·0 kilometres per sec.

Comparing these velocities with those calculated on assumption of parabolic or elliptic orbits, the real velocities are in all cases smaller, indicating that the atmospheric retardation has amounted to 8-15 kilometres per sec. The elements deduced for one meteor, an Andromedid, agree remarkably closely with those of Biela's comet, showing the method to be capable of considerable accuracy.

STANDARDS FOR FAINT STELLAR MAGNITUDES.—Prof. E. C. Pickering announced at the above-mentioned conference that a grant of 500 dollars had been made from the Romford Fund for the purpose of carrying out an investigation on the brightness of faint stars by the co-operation of several observatories possessing large telescopes. The point immediately desirable is the accurate measurement of a few stars which shall serve as standards for future work on a larger scale. Five photometers have been constructed, each having a photographic wedge which may be interposed between the eye and the star as seen by the telescope. Thirty-six regions have been carefully selected in different parts of the sky, and twenty stars (five of each of magnitudes 12, 15, 16, 17) are to be chosen in each region, the faintest to be selected and measured with the Lick and Yerkes telescopes. The stars of the 16th magnitude will be measured with the 26-inch of the University of Virginia, and perhaps also with the 23-inch Princeton refractor; those of the 15th magnitude will be measured by the 15-inch Harvard telescope. All of these are to be then compared with stars of the 12th magnitude, whose *absolute* magnitudes will finally be determined with the 12-inch Harvard meridian photometer. After the work is properly got in hand, it is hoped that it may be reduced to a simple routine without sacrificing the quality of the results.

THE TOTAL SOLAR ECLIPSE, MAY 28, 1900.—As more detailed reports of the results obtained by the American observers during the recent total eclipse come to hand, it is interesting to note the increased use which has been made of large diffraction gratings, both concave and plane. In *Science* (vol. xii. pp. 174-184), Mr. L. E. Jewell describes the work at Pinehurst, N.C., and Griffin, Georgia, of the two parties organised by the physical department of the Johns Hopkins University. At each station there were installed two spectroscopes, one having a plane diffraction grating, surface 3 × 5 inches, 15,000 lines to the inch, used in conjunction with a quartz lens to photograph the spectrum of the first order; the other having a concave grating of 10 feet radius and 15,000 lines to the inch, mounted in the usual Rowland form, with a large quartz lens to throw an image of the sun on the slit-plate from a heliostat. The photographs were very successful, and show the spectrum from wave-lengths 3000 to 6000, even the exposures of only one second giving good negatives.

In the same number of *Science* Profs. E. B. Frost and E. E. Barnard describe the apparatus they successfully used during the same eclipse at Wadesboro, N.C.

REPORT OF THE CAPE OBSERVATORY.—In his report for the year 1899 Sir David Gill, Her Majesty's Astronomer at the Cape of Good Hope Observatory, makes special mention of the completion of the new record room, providing storage for manuscripts, the safe preservation and orderly arrangement of the precious astrographic plates, and also serving as the place where the measurements of these plates are undertaken.

The pier and foundations for the new transit circle are completed, but the delay in obtaining the sheet steel dome has kept the work at a standstill. The observations with the transit instrument have been mainly those of the standard stars for the reduction of the Catalogue Astrographic plates. When the new transit circle arrives it will be entirely devoted to the systematic meridian observations of the sun, Mercury, Venus and fundamental stars. With the heliometer, observations of all the oppositions of major planets have been continued.

The 24-inch object glass of the McClean equatorial was returned to Sir Howard Grubb for refiguring, and this instrument has hitherto only been used with a slit spectroscope for stellar spectra. Since the photographic objective was dismounted the 18-inch visual lens has been used for measurements of twenty-one close double stars. The 7-inch equatorial has been used in the revision of the Cape Photographic Durchmusterung, in the observation of suspected variable stars, and in the detection of double stars.

The 6-inch instrument with a Zollner photometer has been used for determining the visual magnitudes of stars in selected areas of different galactic latitudes, the photographic magnitudes of which are already determined. A comparison between the visual and photographic magnitudes will subsequently be made. With the astrographic equatorial 152 *chart* plates and 184 *revision catalogue* plates have been passed. 103 plates, containing 38,785 stars, have been measured during the year, all observations showing an error of 0″·6 being repeated.

Seventy-eight photographs of *Iris* were taken during the period July 11–December 31, with *six* exposures on each plate. In conjunction with meridian observations of comparison stars, it is intended to use the results of the measurements of these plates for determining the mass of the moon.

The geodetic survey of South Africa and Rhodesia has been considerably advanced, but was interrupted by the outbreak of the Transvaal war. The Anglo-German boundary survey has been hindered by the waterless character of the Kalihari Desert, but the work is now completed as far as Arahoab, from which an offset chain will be carried to the 20th meridian.

ROUSDON OBSERVATORY (DEVON).—Sir C. E. Peek sends a pamphlet of sixteen pages containing the sixth contribution of systematic observations of variable stars made at his observatory at Rousdon, Lyme Regis, Devonshire. The present report furnishes the details of the variability of T Cassiopeiæ for the ten years 1889–1898, and of R Cassiopeiæ for the twelve years 1887–1898. The light curves of both stars are also plotted at the end of the pamphlet.

INDEPENDENT DAY NUMBERS FOR 1902.—A small pamphlet has been issued from the Cape Observatory giving the independent day numbers for correcting the places of stars given in the *Nautical Almanac* for 1902. The values of the constants of precession, aberration and nutation employed in these tables are those recommended by the Paris International Conference of 1896.

THE AUGUST PERSEIDS OF 1900.

OBSERVATIONS of this well-known annual display were much hindered by moonlight, though the weather was generally clear at about the time of the maximum. Our satellite was full on the evening of August 10, and obscured all the smaller meteors. Apart, however, from this interference, the shower of 1900 seems to have been a somewhat scanty one. It furnished a considerable number of large meteors it is true, and of these it is hoped the real paths may be computed; but on the nights of August 10 and 11 observers were somewhat disappointed with the character of their results. The effect of the full moon's influence in practically obliterating a meteoric shower may not, however, have been given sufficient weight. The best night appears to have been August 12, when shooting stars were tolerably frequent considering the circumstances.

But if moonlight presented an obstacle to success in the second week of August, there was no such impediment early in the month and during the last fortnight of July. The earlier stages of the shower were therefore well observed. In fact, it is questionable whether the Perseids have ever been more successfully observed in the month of July. Among those who participated in the observations were Prof. A. S. Herschel, Messrs. J. R. Bridger, W. E. Besley, A. King, and many others. At Cambridge a large number of meteors were recorded. The results show that the first Perseids were noticed on about July 16, and gradually increased in numbers until the date of maximum. The radiant showed the usual E.N.E. motion in a most decided manner.

At Bristol, between July 15–30, in 17¾ hours of observation, 177 meteors were seen, including about 24 Aquarids (radiant $338° - 10°$) and 20 Perseids. But the only night on which a sufficient number of Perseids were registered to indicate a good radiant was July 30, when the position was at $31° + 54°$ from 10 paths. Several of the most prominent of the minor showers of the epoch were observed, and their radiants accurately determined as follows:—

$24° + 43°$, 7 meteors	$305° - 12°$, 8 meteors
$291° + 59°$, 6 ,,	$315° + 47°$, 9 ,,
$292° + 52°$, 7 ,,	$335° + 73°$, 5 ,,

Other showers were indicated at $53° + 63°$, $245° + 72°$, $333° + 28°$.

Mr. W. E. Besley, at Clapham Common, London, registered the paths of 110 meteors between July 14 and 24, and the great majority of these were seen on the 23rd (30 meteors) and 24th (51 meteors). His results are important, for on the former date he found the radiant point of the Perseids at $23° + 51°$, and on the latter date at $25° + 52\frac{1}{2}°$, from 5 and 7 meteors respectively.

Prof. A. S. Herschel, at Slough, during a series of short watches between July 17 and August 1, recorded 53 meteors, including some very interesting early Perseids and several Aquarids. The position of the latter radiant was placed at $339° - 12°$, from about 7 paths.

Some of the meteors seen by Prof. Herschel were also noted by the writer at Bristol. The earliest Perseid of which duple observations were secured appeared on July 19, at 11h. 49m., and it was a fine object, estimated to equal Jupiter by the Bristol observer. The radiant from the combined paths was at $17° + 50°$, and the height of the meteor varied during its descent from 81 to 54 miles. Another Perseid was seen at Slough and Bristol on July 23, 12h. 12m., of 1st magnitude. Its radiant was at $24° + 52°$, and height 84 to 55 miles. These radiants, together with those determined by Mr. Besley on July 23 and 24, and that by the writer at Bristol on July 30, agree very satisfactorily with the ephemeris place of the radiant given by the writer in *Ast. Nach.*, 3546, and *Memoirs* R.A.S., vol. liii. p. 210.

Fairly bright Aquarids were recorded at Slough and Bristol on July 28 and 30, with heights from 65 to 44 miles and 56 to 40 miles respectively. These meteors are usually lower in the atmosphere than the Perseids, and move much slower. If we take the radiant of the former shower in 1900 as $339° - 11°$, we shall probably have a position which is certainly within 1° of probable error.

On July 15, at 10h. 13m., a Capricornid fireball was seen at Bristol and four other places. It was a splendid object, about three times brighter than Venus, in the northern part of England. It fell from heights of 51 to 21 miles, along a path of 78 miles; velocity, 16 miles per second.

On July 17, at 8h. 47m., a magnificent fireball appeared over the northern part of England and Scotland. Though the sun had not long set, the brilliancy of the meteor was described as very dazzling, and the nucleus left a streak which remained visible for three-quarters of an hour. The meteor was directed from a radiant at $249° - 20°$ in Scorpio, and fell from a height of 58 to 15 miles, along a path of about 175 miles.

On July 24 another fireball appeared, and was rated at about three times the brightness of Venus. It was seen at Bristol and at several stations in the eastern counties. It fell from 68 to 27 miles, along a path of 103 miles; velocity, 19 miles per second, and was directed from a well-known July radiant at $280° - 15°$.

But the number of brilliant meteors which have recently appeared is so large that the objects cannot be alluded to in detail. Many ordinary shooting stars have also been doubly observed, and these will be tabulated and published at a later period. Among these there was an interesting θ Perseid on July 23, 11h. 13m., with heights of 83 to 59 miles, and a radiant at $30° + 47°$, quite distinct from the true Perseids.

On about August 10–12 the radiant of the Perseids was found far east of its place in July. On August 12, Mr. King, at Leicester, determined the position as $48\frac{1}{2}° + 58°$ from 16 Perseids, and Mr. Besley derived it at $47° + 56\frac{1}{2}°$ on the same night from 4 meteors. On August 16 the writer at Bristol saw 5 Perseids from a radiant at $54° + 58°$.

Though the shower was partially obliterated by moonlight just at the important time, it has this year furnished some interesting materials for discussion as regards its earlier and later stages.

W. F. DENNING.

WHAT PRESSURE IS DANGEROUS ON ELECTRIC RAILWAYS WITH OVERHEAD TROLLEY WIRES.[1]

THE following investigations were set on foot on account of a dissension between the firm of Messrs. Brown, Boveri and Co., Switzerland (Baden), and the authorities regarding the proper pressure for two different electric railways to be worked by three-phase alternating current, namely, the lines Stansstad-Engelberg and Fermatt-Garnergratt, which lines it was proposed to work at a pressure of 750 volts. But this pressure being regarded as dangerous, the authorities refused to allow one exceeding 500 volts to be actually employed.

In these circumstances the firm communicated with Prof. H. F. Weber, of the Zürich Polytechnic, asking him to express his opinion on this matter. In view, however, of his own want of experience on this particular point, Prof. Weber commenced a long series of investigations of the physiological effects of the electric current on the human body, and he used himself as the measuring instrument, thus exposing himself to great danger.

The experiments were made with reference to the special circumstances of the above railways, where the current was supposed to be supplied through two overhead leads, the rails being used as the third conductor of the three-phase system.

Two series of experiments were made corresponding with the cases—

(*a*) A person seizes the two bare leads with both hands simultaneously, or both of the leads fall on a bare part of the human body.

(*b*) A bare part of a person standing on the railway or on a car comes into contact with one of the leads.

The apparatus used in the case of experiments (*a*) consisted of an iron ring wound with 630 turns of wire, through which was sent an alternating current, the frequency of which was 50 per second. The voltage between the first and the last turn was kept at 210 volts. To every thirtieth turn was soldered a copper wire of 10 cm. length, and 6 mm. diameter, and consequently the pressure between the first and the second wire was 10 volts, that between the first and third 20 volts, and so on, up to 210 volts.

Prof. Weber tried these pressures successively on himself, constantly holding with one hand the first wire and seizing with the other hand each of the other wires in succession. The experiments were made three times, his hands being each time wetted to begin with, and afterwards being used dry. The results of each of the three series so obtained were consistent with one another.

When experimenting with *wet hands* he obtained the following results:—

P.D.	Effect.
10 volts.	Very feeble trembling of the muscles of the fingers; the current from hand to hand was measured and found to be 0·001 ampere.
20 volts.	Very considerable trembling of the hands, wrists and forearms; the hands and the arms were able to be moved freely, and the wires could be

[1] By William Rung, C.E., of the firm of Brown, Boveri and Co., Switzerland. Translated from the Danish *Civilengeneer* by F. Lehmann, M. F., Danish C.E.

P.D.	Effect.
	released easily. The current was from 0·0020 to 0·0027 ampere.
30 volts.	The fingers, hands, wrists, the forearms and upper arms nearly paralysed; the fingers or hand could scarcely be moved; serious pains in the fingers, hands and arms, and the experiment not endurable for more than 10 seconds. The wires could, however, be released, but only by using the greatest determination. Current, 0·015 ampere.
40 volts.	The fingers, hands and arms were instantaneously paralysed, and the pain was almost unbearable. The wires could hardly in any case be released. The pain could not be endured longer than 5 seconds.
50 volts.	Again instantaneous paralysis of all the muscles of the fingers, hands and arms; the wires could not be released; the state endurable for 2 seconds at most, whence it was impossible to measure the current.

Having obtained the above results, the experimenter did not find it advisable to let the pressure exceed the 50 volts; the fact that when the hands were wet, it was impossible on 50 volts pressure to release the wires, seemed to prove to him that serious danger was just beginning at this point.

With *dry hands* he formed the following results:—

P.D.	Effect.
40 volts.	The fingers only tingle slightly; the current too feeble for measurement.
	The effects gradually increasing and extending to the arms up to the shoulders, until at
80 volts.	The fingers, hands and arms were almost cramped and aching in every part; great effort was required to release the wires; current from 0·009 to 0·011 ampere.
90 volts.	At the same moment in which the wire is seized, the hands are absolutely paralysed and the wire cannot be released again. The pains in the hands and arms were so violent that they caused the experimenter to call out involuntarily; the effects could not be endured for more than 1–2 seconds.
	The experimenter now went back to 80 volts, and the difference was so great that the effects of this pressure seemed to be extremely feeble relatively to the effects at 90 volts pressure; this fact prevented him from trying the effects of pressure higher still

From these experiments Prof. Weber draws the following conclusions:—

"A simultaneous touching of both of the poles of an alternating current circuit is dangerous as soon as the pressure exceeds 100 volts; and since it is impossible to set one's self free, the case must be regarded as fatal whenever immediate help is not at hand."

These results are consistent with several disasters which have happened in practical life.

In 1896, in Horgen (Switzerland), a man, to prevent his falling down from a ladder, seized with both his hands two non-insulated leads with a P.D. of 240 volts between them, and was immediately killed.

In a mine in Silesia a workman seized in the same manner some non-insulated leads and was killed on account of his being unable to release them, the P.D. being 300 volts.

In the Electric Central Station in Olten a workman, desirous of proving to his companions that a pressure of 500 volts was quite safe, seized both of the leads and was killed instantly.

From this it is obvious that the general opinion of a pressure of 500 volts not being dangerous does not hold good, the limit being much lower. In spite of the great number of disasters which have already happened, the danger does not seem to have been generally appreciated, and workmen and erectors are often seen to deal with leads and apparatus of relatively high pressures in the most careless manner. That disasters have not taken place far more often may be due to the fact that in most cases help has been at hand instantly.

Entirely differing from these are the results of the other series of experiments (*b*). In this case the person is supposed to stand at one of the poles itself, namely, the earth, from which he is, however, rather well insulated by means of his shoes; and, as it will be evident from the results, the danger is in this case very small even at high pressures.

The arrangement used for this series of experiments consisted of twenty glow-lamps, each for 100 volts pressure, connected in series and all well insulated, the total alternating pressure between the first and the last lamp being 2000 volts. The free terminal of the first lamp was earthed, and between every two consecutive lamps a 6 mm. copper wire was soldered to the main connecting the lamps. Between the earth and the first, second, third to the twentieth of the 6 mm. wires, the pressure was consequently 100, 200, 300, . . . 2000 volts.

Standing on the ground, Prof. Weber touched the different wires—firstly, merely by a slight touch; secondly, by firmly gripping them in his hand. The experiments were made under two conditions, the experimenter standing firstly on moist gravel soil, and afterwards on clay covered by a thin layer of coal-dust.

Standing *on moist gravel soil*, he obtained the following results:—

P. D.	Effect when the wire was Slightly touched.	Firmly gripped.
800 volts.	Feeble stinging of the skin.	No effect.
	Gradually increasing until at	
2000 volts.	Violent stinging of the skin.	Intense trembling of the fingers.

Standing on clay covered with coal-dust, he obtained the following results:—

P. D.	Effect when the wire was Slightly touched.	Firmly gripped.
200 volts.	Scarcely sensible stinging of the skin.	No effect whatever.
500 ,, 700 ,,	Gradually increasing.	The fingers begin to tingle feebly. Intense trembling of the fingers, hands, arms and ankle-joints.
1000 ,,	Stinging like burning by a flame.	The effects in the fingers, hands, arms and feet not endurable longer than 1 to 2 seconds; difficulty in releasing the wire.
1300 ,,	Same effect.	The fingers, hands, and the arms are entirely paralysed, and the wire cannot be released.

From the last series of experiments it will be obvious that to touch one of the poles is not dangerous as long as the pressure does not exceed about 1000 volts; the intense stinging which appears at the first slight touching serves as a protection against the danger, for the hand is instinctively drawn back rapidly.

The main result of these experiments is, then, that all pressures between 100 and 1000 volts must be regarded as equally dangerous, and consequently there is no reason for not using the higher pressures between 500 and 1000 volts, especially as they lead to greater economy in the working of the electric railway. Further, there is only a very little chance of the passengers or other persons coming into contact with both of the leads. To this danger the employés only are exposed, and being generally people with some electric training, they are acquainted with the danger and may be supposed to be sufficiently careful.

Finally, it is to be remarked that the authorities after these investigations allowed the use of a working pressure of 750 volts as originally proposed.

SEA COAST DESTRUCTION AND LITTORAL DRIFT.

THE increasing number of seaside resorts that are constantly being established all round the coast of this country, and the necessity of protecting the sea front from the devastation of the waves, has led recently to greater interest being shown in the protection of the shores and cliffs.

The means taken to preserve our coasts are as diverse as many of them are ineffectual; and in many cases are designed without any proper consideration being given to the way in which the waves act, or to the physical conditions which have to be dealt with in the management of the littoral drift; while frequently the amenities of the beaches of seaside resorts, and their use and enjoyment by visitors, are impaired by structures as ugly as they are useless to attain the object in view, and in other cases the construction of costly works is rapidly followed by their destruction by the sea.

It is proposed as shortly as possible in this article to state the conditions that have led to the present state of the cliffs and coast, and the conditions under which the material is drifted along the shore. For the purpose of illustration, the coast-line of the south-west of England between Start Point and the Solent has been selected, as this presents features of unusual interest for the study of coast destruction and the drift and accumulation of beach material. The cliffs between these two points consist of a series of rocks of varying degrees of hardness, showing in many places almost vertical faces to the sea, and ranging in height up to 500 or 600 feet. The destruction of these cliffs leads to the deposit, on the beach at their base, of fragments of rock, or inland gravels derived from their summits, which are converted by wave action into shingle, consisting of pebbles of varying character and size, but generally shaped into the form of flattened ovoids, readily distinguishing them from the angular gravels due to glacial drift, or the rounded pebbles rolled down inland rivers.

Originally, no doubt, these cliffs descended to the bed of the English Channel with the same slopes as characterise their land faces, and were washed by the deep water of the sea without the intervention of the sand beaches which now stretch from them, and which, where they exist, have an almost uniform inclination along all parts of the coast.

The present form of these cliffs is due to the destructive action of the waves, or to landslips and weathering from rain and frost. The wearing away has not been regular. Headlands composed of hard rocks project out boldly to low-water mark and beyond, while the softer rocks which formerly adjoined them have been gradually worn away, leaving indents of various shapes and depths.

Some indication of the original position of the coast, and the distance to which it extended beyond the present line, is afforded by the remains of a raised beach, portions of which, consisting of pebbles which have been subjected to marine action, are to be found at Portland Bill on the east of Lyme Bay, at Hopes Nose, near Torquay, at Brixham and in Start Bay on the west.

These beaches are at a much higher level than the present water-line. The direction of this old beach was located by the late Sir J. Prestwich as running in an unbroken line at ten miles outside the present shore between the thirteen- and twenty-fathom contour. There are also patches of gravel near the nine-fathom contour extending all round the bay at about a mile from the shore.

The coast-line has been broken up into three principal indents between the two headlands, namely, Lyme Bay, Weymouth Bay and Bournemouth Bay; the contour of which again is split up into numerous minor bays and coves, the greater number of which have their own peculiar characteristics and contain their own peculiar accumulations of shingle. Two of these shingle banks, namely, the Chesil Bank in Lyme Bay, and that at Hurstcastle at the entrance to the Solent, are, perhaps, the most remarkable accumulations to be found anywhere round the coast of Great Britain; while at Axmouth is one of the most extensive landslips of which there is any record. The fight between sea and land is continuous and unceasing, with the result that the area of this country is gradually being reduced.

The rivers which discharge into the sea along the south coast are few and insignificant in character, and are utterly incapable of transporting from the land the large amount of stones or sand now found on the coast. In some cases they have been blocked up by the littoral drift. A careful consideration of all the circumstances can therefore only lead to the conclusion that some of the results which have been attained must be due to other and mightier forces than those now in existence. These forces may probably be ascribed to the same agencies that gave to this country the shape which it now assumes, and by which the valleys and rivers were scooped out.

At the close of the last great Ice Age the melting of the vast bed of snow which then covered this country must have led to large torrents of water escaping seaward, which would carry with them the debris from the rocks broken up by frost and ice, in the shape of boulders, gravel and sand; and besides leaving deposits in the valleys and those which are to be found on the summit of the cliffs, would carry the degraded material to the sea and form a talus at the level of the water. This deposit, after the wear and tear caused by the waves during long ages, resulted in the present sand beaches.

It is certain that the enormous mass of sand which now covers the littoral of the sea cannot have been deposited by existing agencies. The degradation of the cliffs that takes place is quite inadequate to account for its existence; more especially as only the harder rocks have afforded the material of which the sand is composed, the softer detritus having been carried away in suspension to the depths of the ocean. The sand of the seashore consists almost entirely of grains of quartz of a nearly uniform size, and even where flints abound in the sea cliffs and in the shingle on the beach, this material is conspicuous by its absence in the sand of the shore.

As pointed out in my letter in NATURE of November 30 last, only about one-third of the flints lying on the beaches along the south coast, not only in the part dealt with but also on those bordering on the chalk cliffs extending from Brighton to the North Foreland, and in the large accumulations at the Chesil Bank and Dungeness, are derived from the chalk, the colour of the majority of the flints being different shades of brown, grey, white and red, the former being most prevalent, whereas flints from the chalk are invariably black with a white exterior coating. If the above assumption as to the deposit of land detritus on the coast be correct, it affords a reasonable explanation of this phenomenon.

Another proof that the large accumulations of shingle along the coast are not due to agencies at present operating, is afforded by the fact that the Chesil Bank and Hurst Bank, where the supply of new material drifting along the coast is limited, have not varied materially in shape or increased in size during the time to which the most ancient records relating to them extend, the fresh supply coming from the cliffs being only sufficient in these cases to make up the wear and tear caused by the waves.

The supply afforded by the degradation of the cliffs is after all limited in quantity, and only about equal to making good the waste due to the constant wave action on the shingle. If from any cause an abnormal accumulation takes place on any particular part of the coast, denudation immediately sets in on the coast beyond. Instances of this are afforded by the extension of the Point at Dungeness and the banking up of the shingle on the west side, which has led to a diminished supply all along the Dymchurch and Hythe coasts. The construction of the Admiralty Pier at Dover has led to the denudation of the coast to the northward along St. Margaret's Bay. The pier at Shoreham Harbour for a time denuded the supply at Hove and Brighton; and it is found universally to be the case that where the drift has been stopped by the debris from the fall of the cliffs acting as a groyne extending out to low water, or where artificial works have arrested the progress of the drift, the coast beyond has suffered from denudation.

There is no continuous drift of shingle throughout the whole length of this coast, but the material is confined to the various bays and the banks where it has been accumulated. It has been stated that the land gravel found along the foot of the chalk cliffs at the east end of the English Channel may have been derived from the waste of the gravel beds of the cliffs of Dorset and Hampshire, and that pebbles found on the Chesil Bank have been derived from the coast of Devon; but an examination of the coast shows that under present conditions, at any rate, this is neither the case nor physically possible. For this to have occurred the shingle must not only have passed the numerous headlands which project into the water, but also the approaches to Southampton and Portsmouth, in which the depth of the channels is from five to ten fathoms.

Nearly every bay and cove along the coast here dealt with has its own peculiar accumulation of shingle, which does not travel beyond the projecting headland, and in many cases there are long intervals along the shore where the rock is bare. Thus the shingle in Slapton Bay is of a different character from that found in any other part of the same coast. It consists almost entirely of round white quartz pebbles, resembling peas in shape, and averaging from an eighth to a quarter of an inch in diameter. This shingle not only covers the beach, but has been thrown up into a bank, the top of which is above the level of high tides, and has drifted across a deep indent in the bay, into which two fresh-water streams discharge, entirely closing this from the sea and forming it into a fresh-water lake about two miles long. The quartz pebbles of the Slapton beach do not drift beyond the eastern horn of the bay, and are not to be found in the next recess. All along the Devonshire coast the numerous bays and coves have beaches on which are accumulated shingle derived from the limestone, slate, greenstone, and other rocks which surmount them, while in others there is an absence of shingle and only sand is found. In the bay lying between the headland of the Exe and that at Otterton Point, the beach at Budleigh Salterton is strewn with quartzite boulders and pebbles derived from a large bed contained in the cliffs bordering this part of the bay. These pebbles are of a pink colour, some having marks on them like blood spots. No stones of a similar character are found in the next bay, the drift being stopped by some rock ledges which project out from Otterton Point and form a natural groyne. The shingle in front of Seaton consists almost entirely of the chert and flint derived from the rocks at Beer Head. Beyond this, for several miles there is no continuous bank of shingle, but accumulations are to be found in the bight of the bays, the pebbles being derived almost entirely from the gravel beds in the cliffs. The shingle on the east side of Bridport Harbour is of a different character from that on the west shore, and resembles in size and shape that at Slapton, but the colour of the pebbles is different, these consisting of flint instead of quartz. This small shingle continues all along the coast, and up to the commencement of the Chesil Bank.

The drift of shingle along the shore only takes place above the line of low water, and within the zone covered by the horizontal range of the tide, and it does not accumulate below the line of mean tide level, except where its progress is stopped by encountering an obstruction, and when the quantity has become so great as to extend out into deep water. When the shingle encounters a river of any magnitude, it extends out in a spit across the entrance to the estuary, causing the tides to be diverted from their direct course, and to flow round the end; or else the channel becomes diverted from its course to the leeward, and made to flow in a course parallel with the coast for some considerable distance. Examples of this are found along the coast here dealt with, in the Spits across the estuaries of the Teign and the Exe, and that across Christchurch Harbour, and in the diversion of the streams at Seaton and Charmouth.

The Chesil Bank, which commences near Abbotsbury and extends in a south-easterly direction to the island of Portland, a distance of 10¾ miles, has in its course diverted several small streams, which now flow in a channel running parallel with the bank. The width of this great mound of shingle is about 500 feet, and its height varies from 32 to 53 feet, its top being from 23 to 43 feet above high tides.

Hurst Castle shingle bank extends out from the mainland at the entrance to the Solent for 1½ miles, terminating in a hook-like formation on which stands Hurst Castle, erected in the reign of Henry VIII. The bank slopes down across the Solent for a distance of three miles, leaving only a deep narrow passage between its foot and the Needles, in which is from 4 to 9 fathoms of water. This shingle bank forms on its southern side a steep submarine cliff from 20 to 70 feet in height, the face being very steep and dropping almost suddenly from a dry bank to several fathoms of water. So far as any record exists, this bank has not increased or diminished in size or undergone any material alteration since the castle was built. Eastward of the bank there is no drift of shingle, the foreshore for several miles consisting of a wide belt of alluvial deposit.

Another lesson this stretch of coast appears to teach is, that the theory which has generally received acceptance, that the prevailing direction in which the shingle is drifted along any given coast is always in the same direction as that of the prevailing wind, is not founded on fact. This theory may be said to have been settled on the facts brought forward in a paper read at the Institution of Civil Engineers in 1853 on the Chesil Bank, and the discussion which followed.

A careful examination of the facts mentioned in that article do not appear to warrant any such conclusion, but, on the contrary, tends to disprove it. The local movement of shingle along the south-west coast, and also along the other parts of the seashore, are certainly not uniformly in accord with the direction of the prevailing winds of this country. Approximately, the wind in England blows for two-thirds of the year from the south-west. On the east coast the general direction of the drift is from north to south. On the south-west coast the general direction is from west to east. From Beachy Head to Dover it is north-east; from Dover to the North Foreland, northerly; from the North Foreland to the mouth of the Thames, westerly; and north of the Thames south-westerly. On the west coast, the drift is from south to north, up to the middle of the Irish Channel; and north of this, from south to north; and up the Bristol Channel from west to east. In all these instances the movement is in the same direction as the set of the flood-tide.

Although this is the general direction of the drift, there are numerous instances where, owing to the varying set of the tides, the drift moves in three or four different directions within very short ranges. Taking the example of Lyme Bay and the Chesil Bank, the locality where the prevailing wind theory was established, the facts as given by the author of the paper were: That the prevailing direction of the wind on this part of the coast varied between S.S.W. and S.W.½W., which is practically at right angles to the Chesil Bank. If then the drift is in the direction of the prevailing wind, this should lead to a north-easterly movement. The bulk of the materials of which the bank is composed are stated in the paper to have come from the cliffs to the west of the bank, and therefore must have travelled in a south-easterly direction. At the east end of the bank the shingle derived from the debris of Portland moves in a northerly direction. On the other side of the island of Portland, in Weymouth Bay, the shingle is moved in a south-westerly direction; therefore, within the space of five or six miles, the drift is in three opposite directions, not one of which is in the direction of the prevailing winds, but all of which are in the direction at which the flood-tide strikes the shore. Further, it is correctly stated that the effect of winds from the south-west tends to pull down the bank, which is restored again to its normal condition during calms and north-east winds.

Along all tidal coasts it will be found that the general direction of the drift is the same as that of the flood-tide, and that the beach material in bays is moved in the same direction as that in which the wavelets due to the flowing tide break on the beach. It is not contended that shingle is not drifted by waves due to wind. On the contrary, it is a well-known fact that shingle is frequently drifted, first in one direction and then in the opposite, during the occurrence of gales blowing from different quarters obliquely on the shore, and that the beaches are alternately heaped up with material at one place and denuded at another. This process, however, is only occasional and intermittent, and beyond it there is a regular and continuous drift in one given direction, the main course along the coast being in the same direction as the flood-tide, the building up of shingle banks being most active during calms and off-shore winds.

After a long and careful investigation the writer has satisfied himself that the building up of shingle banks and the regular and continuous drift that takes place along the coast are due to wave action caused by the flow and ebb of the tides. As the great tidal wave moves along the deep water of the channels surrounding the coast, its crest is in advance of the sides, which encounter the friction of the shallower water. The swelling tide therefore meets the shore at an angle oblique to its central course. As the lateral flow of the swelling water comes in contact with the shore it is checked and reflected back, causing a series of small undulations or wavelets, which break at the margin of the water.

Although these waves are small, varying in height, according to the condition of the tide and the slope of the beach, from 6 to 24 inches, they are constant, and never cease during the time that the beach is covered by the tide, the number of them varying from ten to twenty a minute. Allowing a mean of fifteen, this gives a total of 3600 impulses during the period that each tide is acting on the shore. These wavelets are never absent from the shore, except when absorbed by larger waves due to wind. As the wavelets break, the water attains a horizontal movement, and aided by the flood current lifts up and carries forward coarse sand and pebbles in a movement oblique to the coast line, and so gives them a slow but continuous forward movement.

The constant murmur that is heard on a shingle beach on days when there is a total absence of wind, and when the sea is perfectly smooth and calm in the offing, attests the fact that the pebbles on the face of a shingle beach are in constant movement. These tidal wavelets are capable of moving and pushing up the face of a shingle bank pebbles weighing from 1 or 2 ounces up to 5 or 6 lbs. A calculation as to the mechanical power of the water contained in an average sized wavelet shows that the kinetic energy developed amounts to 165 foot lbs., which is capable of lifting 9900 pebbles, each weighing 4 ounces, to a height of 1 foot.

W. H. WHEELER.

RECENT STUDIES IN GRAVITATION.[1]

THE studies in gravitation which I am to describe to you this evening will perhaps fall into better order if I rapidly run over the well beaten track which leads to those studies, the track first laid down by Newton, based on astronomical observations, and only made firmer and broader by every later observation.

I may remind you, then, that the motion of the planets round the sun in ellipses, each marking out the area of its orbit at a constant rate, and each having a year proportional to the square root of the cube of its mean distance from the sun, implies that there is a force on each planet exactly proportional to its mass, directed towards, and inversely as the square of its distance from the sun. The lines of force radiate out from the sun on all sides equally, and always grasp any matter with a force proportional to its mass, whatever planet that matter belongs to.

If we assume that action and reaction are equal and opposite, then each planet acts on the sun with a force proportional to its own mass; and if, further, we suppose that these forces are merely the sum totals of the forces due to every particle of matter in the bodies acting, we are led straight to the law of gravitation, that the force between two masses M_1 M_2 is always proportional to the product of the masses divided by the square of the distance r between them, or is equal to

$$\frac{G \times M_1 \times M_2}{r^2}$$

and the constant multiplier G is the constant of gravitation.

Since the force is always proportional to the mass acted on, and produces the same change of velocity whatever that mass may be, the change of velocity tells us nothing about the mass in which it takes place, but only about the mass which is pulling. If, however, we compare the accelerations due to different pulling bodies, as for instance that of the sun pulling the earth with that of the earth pulling the moon, or if we compare changes in motion due to the different planets pulling each other, then we can compare their masses and weigh them, one against another and each against the sun. But in this weighing our standard weight is not the pound or kilogramme of terrestrial weighings, but the mass of the sun.

For instance, from the fact that a body at the earth's surface, 4000 miles, on the average, from the mass of the earth, falls with a velocity increasing by 32 ft. / sec.^{2}, while the earth falls towards the sun, 92 million miles away, with a velocity increasing by about $\frac{1}{4}$ inch / sec. ^{2}, we can at once show that the mass of the sun is 300,000 times that of the earth. In other words, astronomical observation gives us only the acceleration, the product of G × mass acting, but does not tell us the value of G nor of the mass acting, in terms of our terrestrial standards.

To weigh the sun, the planets, or the earth, in pounds or kilogrammes, or to find G, we must descend from the heavenly bodies to earthly matter and either compare the pull of a weighable mass on some body with the pull of the earth on it, or else choose two weighable masses and find the pull between them.

All this was clearly seen by Newton, and was set forth in his "System of the World" (third edition, p. 41).

He saw that a mountain mass might be used, and weighed against the earth by finding how much it deflected the plumb line at its base. The density of the mountain could be found from specimens of the rocks composing it, and the distance of its parts from the plumb line by a survey. The deflection of the vertical would then give the mass of the earth.

Newton also considered the possibility of measuring the attraction between two weighable masses, and calculated how long it would take a sphere a foot in diameter, of the earth's mean density, to draw another equal sphere, with their surfaces separated by $\frac{1}{4}$-inch, through that $\frac{1}{4}$-inch. But he made a very great mistake in his arithmetic, for while his result gave about one month, the actual time would only be about $5\frac{1}{2}$ minutes. Had his value been right, gravitational experiments would have been beyond the power of even Prof. Boys. Some doubt has been thrown on Newton's authorship of this mistake, but I confess that there is something not altogether unpleasing even in the mistake of a Newton. His faulty arithmetic showed that there was one quality which he shared with the rest of mankind.

Not long after Newton's death the mountain experiment was actually tried, and in two ways. The honour of making these first experiments on gravitation belongs to Bouguer, whose splendid work in thus breaking new ground does not appear to me to have received the credit due to it.

One of his plans consisted in measuring the deflection of the plumb line due to Chimborazo, one of the Andes peaks, by finding the distance of a star on the meridian from the zenith, first at a station on the south side of the mountain, where the vertical was deflected, and then at a station to the west, where the mountain attraction was nearly inconsiderable, so that the actual nearly coincided with the geographical vertical. The difference in zenith distances gave the mountain deflection. It is not surprising that, working in snowstorms at one station, and in sandstorms at the other, Bouguer obtained a very incorrect result. But at least he showed the possibility of such work, and since his time many experiments have been carried out on his lines under more favourable conditions. Now, however, I think it is generally recognised that the difficulty of estimating the mass of a mountain from mere surface chips is insurmountable, and it is admitted that the experiment should be turned the other way about and regarded as an attempt to measure the mass of the mountains from the density of the earth known by other experiments.

These other experiments are on the line indicated by Newton in his calculations of the attraction of two spheres. The first was carried out by Cavendish.

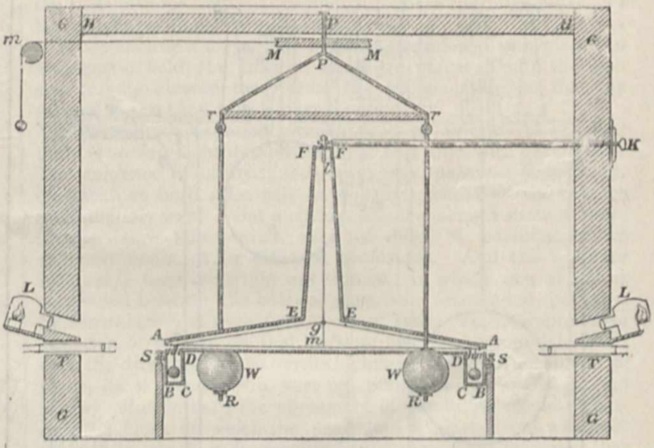


FIG. 1.—Cavendish's apparatus.

In the apparatus (Fig. 1) he used two lead balls, B B, each 2" in diameter. These were hung at the end of a horizontal rod 6' long, the torsion rod, and this was hung up by a long wire from its middle point. Two large attracting spheres of lead, W W, each 12" in diameter, were brought close to the balls on opposite sides so that their attractions on the balls conspired to twist the torsion rod round the same way, and the angle of twist was measured. The force could be reckoned in terms of this angle by setting the rod vibrating to and fro and finding the time of vibration, and the force came out to less than 1/3000 of a grain. Knowing M_1 M_2 and r the distance between them and the force $G\ M_1\ M_2/r^2$, of course Cavendish's result gives G, or knowing the attraction of a big sphere on a ball, and knowing the attraction of the earth on the same ball, that is, its weight, the experiment gives the mass of the earth in terms of that of the big sphere, and so its mean density. This experiment has often been repeated, but I do not think it is too much to say that no advance was made in exactness till we come to quite recent work.

By far the most remarkable recent study in gravitation is Prof. Boys' beautiful form of the Cavendish experiment, a research which stands out as a model in beauty of design and in exactness of execution (Fig. 2). But as Prof. Boys has described his experiment already in this theatre (*Proc. R.I.*, xiv. Part ii. 1894, p. 353), it is not necessary for me to more than refer to it. It is enough to say that he made the great discovery, obvious

[1] A discourse delivered at the Royal Institution of Great Britain on Friday, February 23, by Prof. John H. Poynting, F.R.S.

perhaps when made, that the sensitiveness of the apparatus is increased by reducing its dimensions. He therefore decreased the scale as far as was consistent with exact measurement of the parts of the apparatus, using a torsion rod, itself a mirror, only 2" long, gold balls, *m m*, only ¼" in diameter, and attracting lead masses, M M, only 4¼" in diameter. The force to be measured was less than $1/5 \times 10^6$ grain.

The exactness of his work was increased by using as suspending wire one of his quartz threads. It would be difficult to over-estimate the service he has rendered in the measurement of small forces by the discovery of the remarkable properties of these threads.

One of the chief difficulties in the measurement of these small gravitational pulls is the disturbances which are brought about by the air currents, which blow to and fro and up and down inside the apparatus, producing irregular motions in the torsion rod. These, though much reduced, are not reduced in proportion to the diminution of the apparatus.

A very interesting repetition of the Cavendish experiment

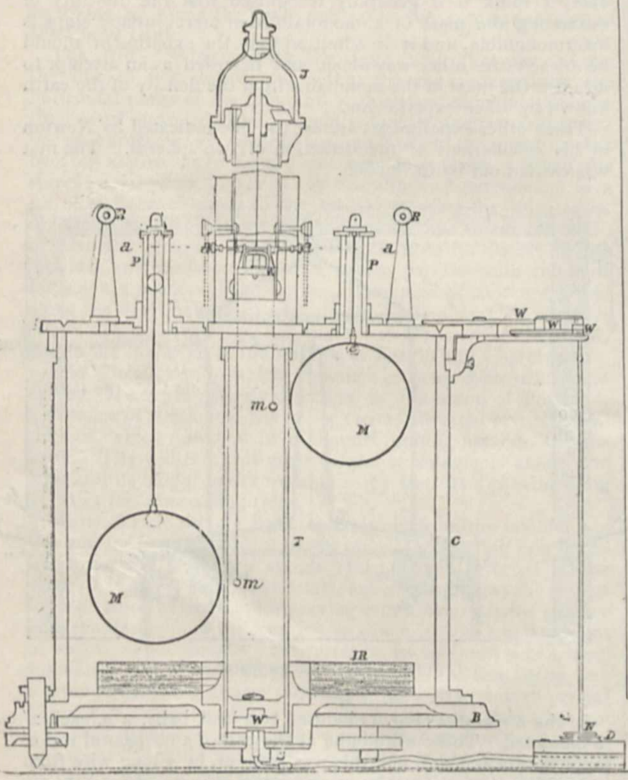


FIG. 2.—Boys' apparatus.

has lately been concluded by Dr. Braun (*Denkschriften der Math. Wiss. Classe der Kais. Akad. der Wissenschaften Wien*, lxiv. 1896) at Mariaschein, in Bohemia, in which he has sought to get rid of these disturbing air currents by suspending his torsion rod in a receiver which was nearly exhausted, the pressure being reduced to about $\frac{1}{200}$ of an atmosphere. The gales which have been the despair of other workers were thus reduced to such gentle breezes that their effect was hardly noticeable. His apparatus was nearly a mean proportional between that of Cavendish and Boys, his torsion rod being about 9" long, the balls weighing 54 gms.—less than two ounces—and the attracting masses either 5 or 9 kgms. His work bears internal evidence of great care and accuracy, and he obtained almost exactly the same result as Prof. Boys.

Dr. Braun carried on his work far from the usual laboratory facilities, far from workshops, and he had to make much of his apparatus himself. His patience and persistence command our highest admiration.

I am glad to say that he is now repeating the experiment, using as suspension a quartz fibre supplied to him by Prof. Boys in place of the somewhat untrustworthy metal wire which he used in the work already published.

Prof. Boys has almost indignantly disclaimed that he was engaged on any such purely local experiment as the determination of the mean density of the earth. He was working for the Universe, seeking the value of G, information which would be as useful on Mars or Jupiter or out in the stellar system as here on the earth. But perhaps we may this evening consent to be more parochial in our ideas, and express the results in terms of the mean density of the earth. In such terms, then, both Boys and Braun find that density 5·527 times the density of water, agreeing therefore to 1 in 5000.

There is another mode of proceeding which may be regarded as the Cavendish experiment turned from a horizontal into a vertical plane, and in which the torsion balance is replaced by the common balance. This method occurred about the same time to the late Prof. V. Jolly and myself. The principle of my own experiment (*Phil. Trans.*, 182, 1891, A, p. 565) will be sufficiently indicated by Fig. 3. A big bullion balance with a 4-foot beam had two lead spheres, A B, each about 50lbs. in weight, hanging from the two ends in place of the usual scale

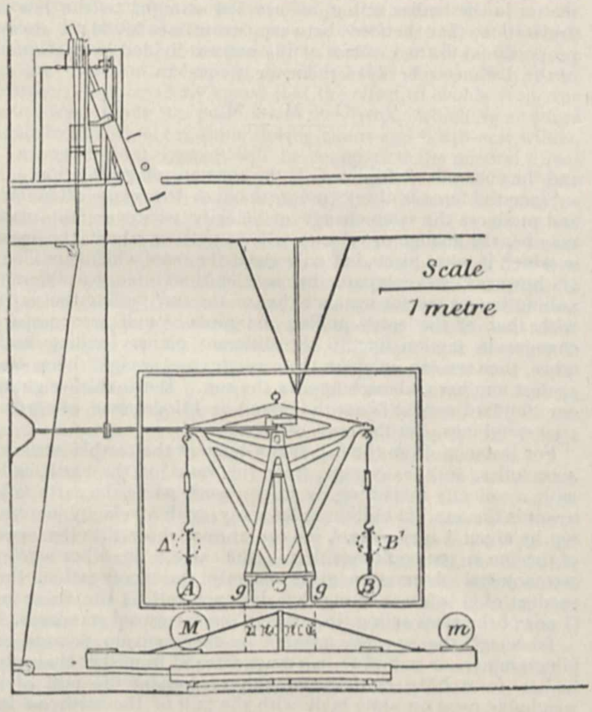


FIG. 3.—Common balance experiment (Poynting).

pans. A large lead sphere, M, 1' in diameter and weighing about 350 lbs., was brought first under one hanging weight, then under the other. The pull of the lead sphere acted first on one side alone and then on the other, so that the tilt of the balance beam when the sphere was moved round was due to twice the pull. By means of riders the tilt, and therefore the pull, was measured directly as so much increase in weight. This increase, when the sphere was brought directly under the hanging weight with 1' between the centres, was about ⅛ mgm. in a total weight of 20 kgm., or about 1 in 100,000,000. If, then, a sphere 1 foot away pulls with $1/10^8$ of the earth's pull, the earth being on the average 20,000,000 feet away, it is easy to see that the earth's mass is calculable in terms of the mass of the sphere, and its density is at once deduced. The direct aim of this experiment, then, is not G, but the mass of the earth.

It is not a little surprising that the balance could be made to indicate such a small increase in weight as 1 in 100 million. But not only did it indicate, it measured the increase, with variations usually well within 1 per cent. of the double attraction, or to 1 in 5000 million of the whole weight, a change in weight which would occur merely if one of the spheres were moved $\frac{1}{40}$ inch nearer the earth's centre. This accuracy is only attained by never lifting the knife edges and planes during an experiment, thus keeping the beam in the same state of strain throughout, and, further, by taking care that none of the mechanism for moving the weights or riders shall be attached in any way to the balance or its case, two conditions which are absolutely essential if we are to get the best results of which the balance is capable.

Quite recently another common balance experiment has been brought to a conclusion by Prof. Richarz and Dr. Krigar-Menzel ("Anhang zu den Abhandlungen der Königl," *Preuss. Akad. der Wissenschaften zu Berlin*, 1898) at Spandau, near Berlin. Their method may be gathered from Fig. 4. A balance of 23 cm., say 9-inch beam, was mounted above a huge lead pile about 2 metres cube, and weighing 100,000 kgm.

Two pans were supported from each end of the beam, one pan above, the other pan below the lead cube, the suspending wires of the lower pans going through narrow vertical tubular holes in the lead. Instead of moving the attracting mass, the attracted mass was moved. Masses of 1 kgm. each were put first, say, one in the upper right-hand pan, the other in the lower left-hand pan, when the pull of the lead block made the right hand heavier and the left hand lighter. Then the weights were changed to the lower right hand, and the upper left hand when the pulls of the lead pile were reversed. When we remember that in my experiment a lowering of the hanging

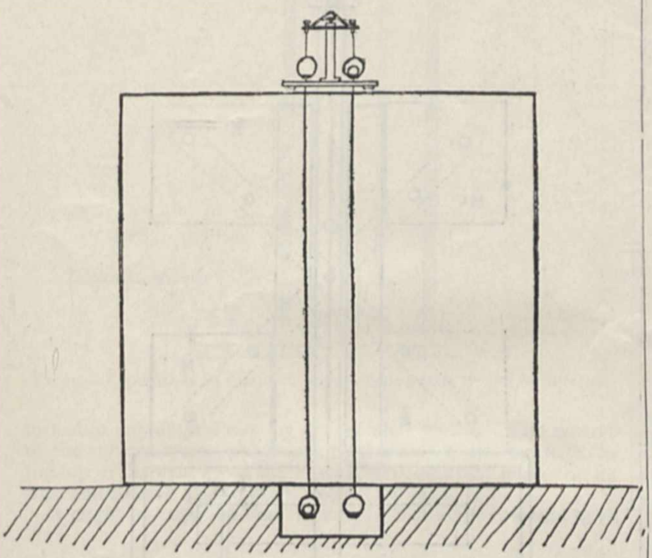


FIG. 4.—Common balance experiment (Richarz and Krigar-Menzel).

sphere by $1\frac{1}{2}$ inches would give an effect as great as the pull I was measuring, it is evident that here the approach to and removal from the earth by over 2 metres would produce very considerable changes in weight, and, indeed, these changes masked the effect of the attraction of the lead. Preliminary experiments had, therefore, to be made before the lead pile was built up, to find the change in weight due to removal from upper to lower pan, and this change had to be allowed for. The quadruple attraction of the lead pile came out at 1·3664 mgm., and the mean density of the earth at 5·505.

This agrees nearly with my own result of 5·49, and it is a curious coincidence that the two most recent balance experiments agree very nearly at, say, 5·5; and the two most recent Cavendish experiments agree at, say, 5·53. But I confess I think it is merely a coincidence. I have no doubt that the torsion experiment is the more exact, though probably an experiment on different lines was worth making. And I am quite content to accept the value of 5·527 as the standard value for the present.

And so the latest research has amply verified Newton's celebrated guess that "the quantity of the whole matter of the earth may be five or six times greater than if it consisted all of water."

I now turn to another line of gravitational research. When we compare gravitation with other known forces (and those which have been most closely studied are electric and magnetic forces) we are at once led to inquire whether the lines of gravitative force are always straight lines radiating from or to the mass round which they centre, or whether, like electric and magnetic lines of force, they have a preference for some media and a distaste for others. We know, for example, that if a magnetic sphere of iron or cobalt or manganese is placed in a previously straight field, its permeability is greater than the air it replaces, and the lines of force crowd into it, as in Fig. 5.

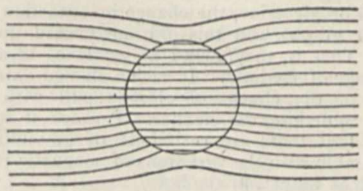


FIG. 5.—Paramagnetic sphere placed in a previously straight field.

The magnetic action is then stronger in the presence of the sphere near the ends of a diameter parallel to the original course of the lines of force, and the lines are deflected. If the sphere be diamagnetic, of water, or copper, or bismuth, the permeability being less than that of air, there is an opposite effect, as in Fig. 6, and the field is weakened at the end of a diameter parallel to the lines of force, and again the lines are deflected. Similarly, a dielectric body placed in an electric field gathers in the lines of force, and makes the field where the lines enter and leave stronger than it was before.

If we enclose a magnet in a hollow box of soft iron placed in a magnetic field, the lines of force are gathered into the iron and largely cleared away from the inside cavity, so that the magnet is screened from external action.

Now common experience might lead us at once to say that there is no very considerable effect of this kind with gravitation. The evidence of ordinary weighings may, perhaps, be rejected, inasmuch as both sides will be equally affected as the balance is commonly used. But a spring balance should show if there is any large effect when used in different positions above different media, or in different enclosures. And the ordinary balance is used in certain experiments in which one weight is suspended beneath the balance case, and surrounded, perhaps, by a metal case, or perhaps by a water bath. Yet no appreciable variation of weight on that account has yet been noted. Nor does the direction of the vertical change rapidly from place to place, as it would with varying permeability of the ground below. But perhaps the agreement of pendulum results, whatever the block on which the pendulum is placed, and whatever the case in which it is contained, gives the best evidence that there is no great gathering in, or opening out of the lines of the earth's force by different media.

Still, a direct experiment on the attraction between two masses with different media interposed was well worthy of trial, and such an experiment has lately been carried out in America

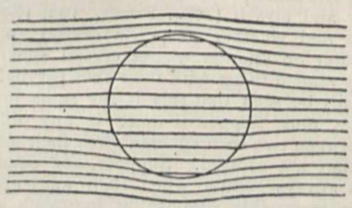


FIG. 6.—Diamagnetic sphere placed in a previously straight field.

by Messrs. Austin and Thwing (*Physical Review*, v. 1897, p. 294). The effect to be looked for will be understood from Fig. 7. If a medium more permeable to gravitation is interposed between two bodies, the lines of force will move into it from each side, and the gravitative pull on a body, near the interposed medium on the side away from the attracting body, will be increased.

The apparatus they used was a modified kind of Boys' apparatus (Fig 8). Two small gold masses in the form of short vertical wires, each ·4 gm. in weight, were arranged at different levels at the ends virtually of a torsion rod 8 mm. long. The attracting masses M_1 M_2 were lead, each about 1 kgm. These were first in the positions shown by black lines in the figure, and were then moved into the positions shown by dotted lines. The attraction was measured first when merely the air and the case of the instrument intervened, and then when various slabs, each 3 cm. thick, 10 cm. wide and 29 cm. high, were interposed. With screens of lead, zinc, mercury, water, alcohol or glycerine, the change in attraction was at the most about 1 in 500, and this did not exceed the errors of experiment. That is, they found no evidence of a change in pull with change of medium. If such a change exists, it is not of the order of the change of electric pull with change of medium, but something far smaller. Perhaps it still remains just possible that there are variations of gravitation permeability comparable with the variations of magnetic permeability in media such as water and alcohol.

Yet another kind of effect might be suspected. In most crystalline substances the physical properties are different along different directions in a crystal. They expand differently, they conduct heat differently, and they transmit light at different speeds in different directions. We might, then, imagine that the lines of gravitative force spread out from, say, a crystal sphere unequally in different directions. Some years ago, Dr. Mackenzie (*Physical Review*, ii. 1895, p. 321) made an experiment in America in which he sought for direct evidence of such unequal distribution of the lines of force. He used a form of apparatus like that of Prof. Boys (Fig. 2), the attracting masses being calc spar spheres about 2 inches in

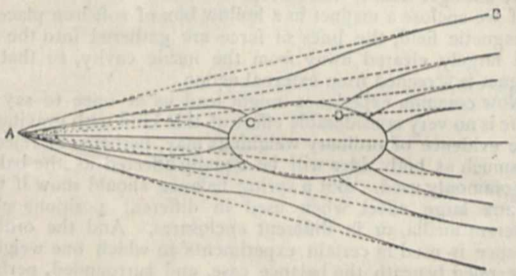


FIG. 7.—Effect of interposition of more permeable medium in radiating field of force.

diameter. The attracted masses in one experiment were small lead spheres about ½ gm. each, and he measured the attraction between the crystals and the lead when the axis of the crystals were set in various positions. But the variation in the attraction was merely of the order of error of experiment. In another experiment the attracted masses were small calc spar crystal cylinders weighing a little more than ½ gm. each. But again there was no evidence of variation in the attraction with variation of axial direction.

Practically the same problem was attacked in a different way by Mr. Gray and myself (*Phil. Trans.*, 192, 1899, A, p. 245). We tried to find whether a quartz crystal sphere had any directive action on another quartz crystal sphere close to it, whether they tended to set with their axes parallel or crossed.

It may easily be seen that this is the same problem by considering what must happen if there is any difference in the attraction between two such spheres when their axes are parallel and when they are crossed. Suppose, for example, that the attraction is always greater when their axes are parallel, and this seems a reasonable supposition, inasmuch as in straightforward crystallisation successive parts of the crystal are added to the existing crystal, all with their axes parallel. Begin, then, with two quartz crystal spheres near each other with their axes in the same plane, but perpendicular to each other. Remove one to a very great distance, doing work against their mutual attractions. Then, when it is quite out of range of appreciable action, turn it round till its axis is parallel to that of the fixed crystal. This absorbs no work if done slowly. Then let it return. The force on the return journey at every point is greater than the force on the outgoing journey, and more work will be got out than was put in. When the sphere is in its first position, turn it round till the axes are again at right angles. Then work must be done on turning it through this right angle to supply the difference between the outgoing and incoming works. For if no work were done in the turning, we could go through cycle after cycle, always getting a balance of energy over, and this would, I think, imply either a cooling of the crystals or a diminution in their weight, neither supposition being admissible. We are led, then, to say that if the attraction with parallel axes exceeds that with crossed axes, there must be a directive action resisting the turn from the crossed to the parallel positions. And conversely, a directive action implies axial variation in gravitation.

The straightforward mode of testing the existence of this directive action would consist in hanging up one sphere by a wire or thread, and turning the other round into various positions, and observing whether the hanging sphere tended to twist out of position. But the action, if it exists, is so minute, and the disturbances due to air currents are so great, that it would be extremely difficult to observe its effect directly. It occurred to us that we might call in the aid of the principle of forced oscillations, by turning one sphere round and round at a constant rate, so that the couple would act first in one direction and then in the other, alternately, and so set the hanging sphere vibrating to and fro. The nearer the complete time of vibration of the applied couple to the natural time of vibration of the

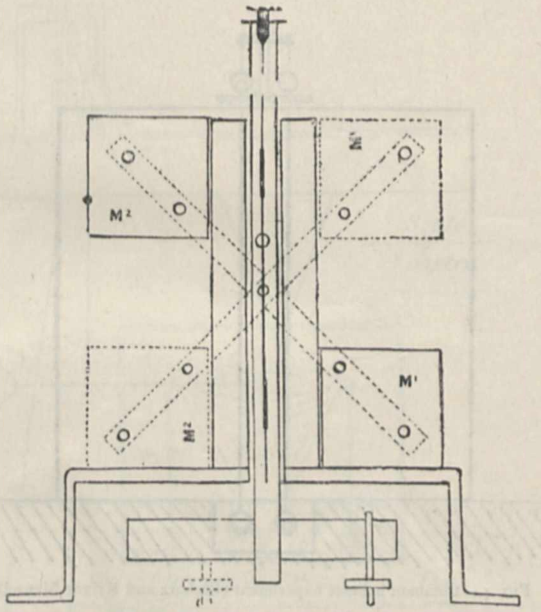
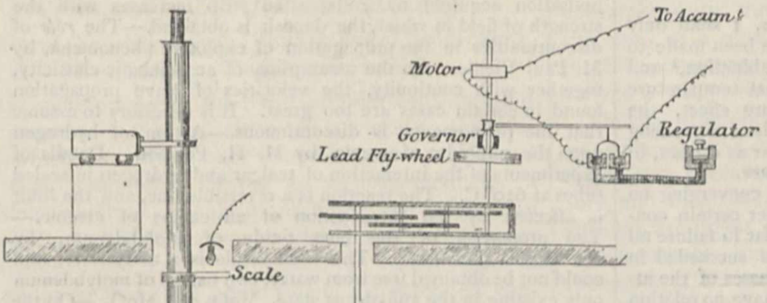


FIG. 8.—Experiment on gravitative permeability (Austin and Thwing).

hanging sphere, the greater would be the vibration set up. This is well illustrated by moving the point of suspension of a pendulum to and fro in gradually decreasing periods, when the swing gets longer and longer, till the period is that of the pendulum, and then decreases again. Or by the experiment of varying the length of a jar resounding to a given fork, when the sound suddenly swells out as the length becomes that which would naturally give the same note as the fork. Now, in looking for the couple between the crystals, there are two possible cases. The most likely is that in which the couple acts in one way while the turning sphere is moving from parallel to crossed, and in the opposite way during the next quarter turn from crossed to parallel. That is, the couple vanishes four times during the revolution, and this we may term a quadrantal couple. But it is just possible that a quartz crystal has two ends like a magnet, and that like poles tend to like directions. Then the couple will vanish only twice in a revolution, and may be termed a semicircular couple. We looked for both, but it is enough now to consider the possibility of the quadrantal couple only.

Our mode of working will be seen from Fig. 9. The hanging sphere, ·9 cm. in diameter and 1 gm. in weight, was placed in a light aluminium wire cage with a mirror on it, and suspended by a long quartz fibre in a brass case with a window in it opposite the mirror, and surrounded by a double-walled tinfoiled wood case. The position of the sphere was read in the usual way by scale and telescope. The time of swing of this little sphere was 120 seconds.

A larger quartz sphere, 6·6 cm. diameter and weighing 400 gms., was fixed at the lower end of an axis which could be



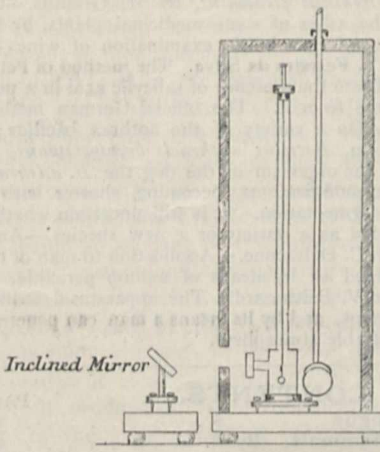


FIG. 9.—Experiment on directive action of one quartz crystal on another.

turned at any desired rate by a regulated motor. The centres of the spheres were on the same level and 5·9 cm. apart. On the top of the axis was a wheel with 20 equidistant marks on its rim, one passing a fixed point every 11·5 seconds.

It might be expected that the couple, if it existed, would have

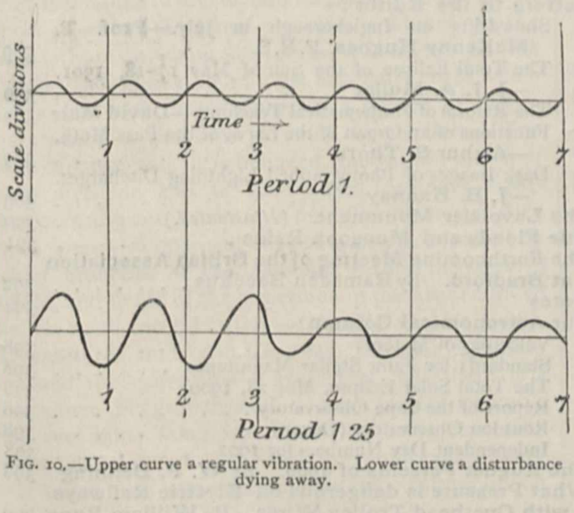


FIG. 10.—Upper curve a regular vibration. Lower curve a disturbance dying away.

the greatest effect if its period exactly coincided with the 120 second period of the hanging sphere—*i.e.* if the larger sphere revolved in 240 seconds. But in the conditions of the experiment the vibrations of the small sphere were very much damped, and the forced oscillations did not mount up as they would in a freer swing. The disturbances, which were mostly of an impulsive kind, continually set the hanging sphere into large vibration, and these might easily be taken as due to the revolving sphere. In fact, looking for the couple with exactly coincident periods would be something like trying to find if a fork set the air in a resonating jar vibrating when a brass band was playing all round it. It was necessary to make the couple period, then, a little different from the natural 120 second period, and, accordingly, we revolved the large sphere once in 230 seconds, when the supposed quadrantal couple would have a period of 115 seconds.

Figs. 10 and 11 may help to show how this enabled us to eliminate the disturbances. Let the ordinates of the curves in Fig. 10 represent vibrations set out to a horizontal time scale. The upper curve is a regular vibration of range ± 3, the lower a disturbance beginning with range ± 10. The first has period 1, the second period 1·25. Now cutting the curves into lengths equal to the period of the shorter time of vibration, and arranging the lengths one under the other as in Fig. 11, it will be seen that the maxima and the minima of the regular vibration always fall at the same points, so that, taking 7 periods and adding up the ordinates, we get 7 times the range, viz. ± 21. But in the disturbance the maxima and minima fall at different points, and even with 7 periods only, the range is from + 16 to − 13, or less than the range due to the addition of the much smaller regular vibration.

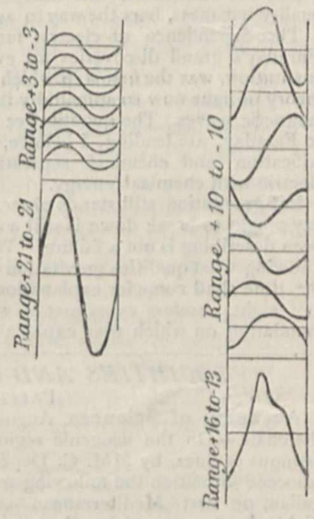


FIG. 11.—Results of superposition of lengths of curves in Fig. 10 equal to the period of the regular one.

In our experiment, the couple, if it existed, would very soon establish its vibration, which would always be there and would go through all its values in 115 seconds. An observer, watching the wheel at the top of the revolving axis, gave the time signals every 11·5 seconds, regulating the speed, if necessary, and an observer at the telescope gave the scale reading at every signal, that is, 10 times during the period. The values were arranged in 10 columns, each horizontal line giving the readings of a period. The experiment was carried on for about 2½ hours at a time, covering, say, 80 periods. On adding up the columns, the maxima and minima of the couple effect would always fall in the same two columns, and so the addition would give 80 times the swing, while the maxima and minima of the natural swings due to disturbances would fall in different columns, and so, in the long run, neutralise each other. The results of different days' work might, of course, be added together.

There always was a small outstanding effect such as would be produced by a quadrantal couple, but its effect was not always in the same columns, and the net result of about 350 period observations was that there was no 115 second vibration of more than 1 second of arc, while the disturbances were sometimes 50 times as great.

The semicircular couple required the turning sphere to revolve in 115 seconds. Here, want of symmetry in the apparatus would come in with the same effect as the couple sought, and the outstanding result was, accordingly, a little larger.

But in neither case could the experiments be taken as showing a real couple. They only showed that, if it existed, it was incapable of producing an effect greater than that observed.

Perhaps the best way to put the result of our work is this: Imagine the small sphere set with its axis at 45° to that of the other. Then the couple is not greater than one which would take 5¼ hours to turn it through that 45° to the parallel position, and it would oscillate about that position in not less than 12 hours.

The semicircular couple is not greater than one which would turn from crossed to parallel position in 4½ hours, and it would oscillate about that position is not less than 17 hours.

Or, if the gravitation is less in the crossed than in the parallel position, and in constant ratio, the difference is less than 1 in 16,000 in the one case and less than 1 in 2800 in the other.

We may compare with these numbers the difference of rate of travel of yellow light through a quartz crystal along the axis and perpendicular to it. That difference is of quite another order, being about 1 in 170.

As to other possible qualities of gravitation, I shall only mention that quite indecisive experiments have been made to seek for an alteration of mass on chemical combination,[1] and that at present there is no reason to suppose that temperature affects gravitation. Indeed, as to temperature effect, the agreement of weight methods and volume methods of measuring expansion with rise of temperature is good, as far as it goes, in showing that weight is independent of temperature.

So while the experiments to determine G are converging on the same value, the attempts to show that, under certain conditions, it may not be constant, have resulted so far in failure all along the line. No attack on gravitation has succeeded in showing that it is related to anything but the masses of the attracting and the attracted bodies. It appears to have no relation to physical or chemical condition of the acting masses or to the intervening medium.

Perhaps we have been led astray by false analogies in some of our questions. Some of the qualities we have sought and failed to find, qualities which characterise electric and magnetic forces, may be due to the polarity, the + and −, which we ascribe to poles and charges, and which have no counterpart in mass.

But this unlikeness, this independence of gravitation of any quality but mass, bars the way to any explanation of its nature.

The dependence of electric forces on the medium, one of Faraday's grand discoveries for ever associated with the Royal Institution, was the first step which led on to the electromagnetic theory of light now so splendidly illustrated by Hertz's electromagnetic waves. The quantitative laws of electrolysis, again due to Faraday, are leading, I believe, to the identification of electrification and chemical separation, to the identification of electric with chemical energy.

But gravitation still stands alone. The isolation which Faraday sought to break down is still complete. Yet the work I have been describing is not a failure. We at least know something in knowing what qualities gravitation does not possess, and when the time shall come for explanation all these laborious and, at first sight, useless experiments will take their place in the foundation on which that explanation will be built.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 13.—M. Maurice Lévy in the chair.—On the neogenic regions of Lower Egypt and the Isthmus of Suez, by MM. C. Depéret and R. Fourtau. Of the Miocene formation the following were recognised:—The Burdigalian, or first Mediterranean stratum, by the presence of *Echinolampas amplus*, *Scutella Innesi*, *Lovenia*, *Cidaris avenionensis*, *Amphiope truncata* and other fossils; the Vindobonian, or second Mediterranean stratum, by a blue lime containing *Pecten cristato-costatus* and numerous echinoderms. Of the Pliocene, in the neighbourhood of Cairo, are layers of yellowish sand containing *Clypeaster aegyptiacus* and other characteristic fossils.—The area of the basins of Russia in Asia, by M. J. de Schokalsky. The map is made upon the scale of 1 in 4,200,000, and the area evaluated by means of a sheet of celluloid divided in square millimetres. The area found is 16,085,000 sq. kilometres.—On a hypsometric map of European Russia, by M. J. de Schokalsky. The previous map of M. de Tillo was limited by the latitude 60° N.; the present map includes the whole of European Russia upon a scale of 1 in 15,300,000.—Observations of the Borelly comet, made at the Observatory of Algiers with the 31·8 cm. equatorial, by M. F. Sy.—A new arrangement of apparatus serving to measure geodesic bases, by M. Alphonse Berget. Ruled plates of iron floating in a bath of mercury are used instead of the ordinary scales. The method has the advantage of securing without trouble the absolute horizontality of the rules; two consecutive rules are necessarily in the same horizontal plane, since their mercury baths are connected; there is no correction necessary for the flexure of the rules, and the temperature correction is much more certain.—Stereoscopic vision of curves traced by a phase apparatus, by M. Marc Dechevrens.—Properties of magnetic deposits obtained in a magnetic field, by M. Ch. Maurain. Iron was deposited in a magnetic field either from a solution of ferrous and ammonium chlorides, or from a solution of ferrous sulphate in sodium pyrophosphate. It was found that the intensity of magnetisation of different layers of the deposit growing in a uniform field has the same value, and that the uniform magnetisation acquired by a deposited strip increases with the strength of field in which the deposit is obtained.—The *rôle* of discontinuities in the propagation of explosive phenomena, by M. Paul Vieille. On the assumption of an adiabatic elasticity, together with continuity, the velocities of wave propagation found in certain cases are too great. It is necessary to assume that the phenomenon is discontinuous.—Action of hydrogen upon the sulphides of arsenic, by M. H. Pélabon. Details of experiments of the interaction of realgar and hydrogen in sealed tubes at 610° C. The reaction is a reversible one, and the limit is affected by the introduction of an excess of arsenic.—The properties of the blue oxide of molybdenum, by M. Marcel Guichard. The blue oxide is a molybdate, and could not be obtained free from water, two oxides of molybdenum only existing in the anhydrous state, MoO_2 and MoO_3.—On the colouring matter of *Echinus esculentus*, by M. Griffiths.—On the composition of the ashes of some medicinal plants, by M. Griffiths.—On a cause of error in the examination of wines for salicylic acid, by M. J. Ferreira da Silva. The method of Petlet and Grobert will indicate the presence of salicylic acid in a pure wine that is really free from it. The official German method gives exact results.—On a variety of the anthrax bacillus; a short asporogenic form, *Bacillus anthracis brevigemmans*, by M. C. Phisalix. In the organism of the dog the *B. anthracis* undergoes important modifications, becoming shorter with a rapid and complete segmentation. It is still uncertain whether this should be regarded as a variety or a new species.—Antihepatic serum, by M. C. Delezenne.—Application to man of the regeneration of confined air by means of sodium peroxide, by MM. A. Desgrez and V. Balthazard. The apparatus described weighs twelve kilograms, and by its means a man can penetrate easily into an irrespirable atmosphere.

[1] Landolt, *Zeit. für Phys. Chem.*, xii. 1, 1894. Sanford and Ray, *Physical Review*, v. 1897, p. 247.

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