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ON LABORATORY ARTS.

On Laboratory Arts. By Richard Threlfall, M.A., Professor of Physics in the University of Sydney. Pp. xii + 338. (London: Macmillan and Co., Ltd. New York: The Macmillan Company, 1898.)

THERE are certain passages in the preface of Prof. Threlfall's valuable contribution which it may be useful to quote before dealing with the book itself.

"It often happens that young physicists are to be found whose mathematical attainments are adequate, whose observational powers are perfectly trained, and whose general capacity is unquestioned, but who are quite unable to design or construct the simplest apparatus with due regard to the facility with which it ought to be constructed. That ultimate knowledge of materials and of processes which by long experience becomes intuitive in the mind of a great inventor of course cannot be acquired from books or from any set course of instruction. There are, however, many steps between absolute ignorance and consummate knowledge of the mechanical arts, and it is the object of the following pages to assist the young physicist in making his first steps towards acquiring a working knowledge of 'laboratory art.' . . . Before all things the means indicated must be definite and reliable. It is for this reason that the writer has practically confined himself to matters lying within his own immediate experience, and has never recommended any process (with one or two minor exceptions, which he has noted) which he himself has not actually and personally carried through to a successful issue. . . . With regard to the question as to what matters might be included and what omitted, the general rule has been to include information which the author has obtained with difficulty, and to leave on one side that which he has more easily attained. . . . Though no doubt a great deal can be done with inferior appliances where great economy of money and none of time is an object, the writer has long felt very strongly that English physical laboratory practice has gone too far in the direction of starving the workshop, and he does not wish, even indirectly, to give any countenance to such a mistaken policy."

The writer of this notice feels so strongly the importance of the subject of the first and last of these extracts that it is only with an effort that he can forego the opportunity which they offer of airing views, and confine himself to the more prosaic duty of review.

The second extract is one which shows that the first essential in a work of this kind is complied with. A mere collection of recipes for making and doing all sorts of things which have been collected from anywhere and everywhere, while not absolutely useless, is necessarily untrustworthy. Without the personal certificate of a man who is both a physicist and a mechanician, no description of a process for making or doing anything will necessarily be the most appropriate or even serviceable at all. With such a certificate, however, any one with but little experience of any particular laboratory art may set to work upon it with reasonable confidence.

The first chapter, of ninety pages, is upon the manipulation of glass and on glass-blowing for laboratory purposes. Of all laboratory arts probably glass-blowing and working, not including the work of the optician, looks more easy when practised by an adept, and seems more utterly and hopelessly impossible when tried for the first time, than any other. It is one which every experimentalist must acquire in some degree, and which as a fact, with a little practice and suitable help, is one of the easiest in which to make progress. Shenstone's well-known little book has been found invaluable by many, the little work of Bolas has recently been reviewed in these columns, and now we have new advice on the same subject, differing in some points, as is to be expected, but the result of personal experience. The subject is one in which any opportunity of watching a glass-blower of skill is worth more than any written instruction, but it is one in which occasional and hurried opportunities of watching a process can be supplemented most usefully by description. Practice, however, is essential, whatever guide to procedure may be attainable.

The writer's experience of the average student is that it is not sufficient to tell him even several times that he must not begin glass-blowing operations upon dusty tubes. The very elaborate description of a really proper way of cleaning a glass tube so as to fit it for the best class of work may, perhaps, induce this individual to take the trouble at least to wash out his tubes.

Prof. Threlfall does not approve of the type of blowpipe usually furnished by the instrument maker, nor apparently does any writer on the subject. He describes a simple form of oxygen blowpipe that is suitable for working lead-glass or unusually infusible glass. For larger work with lead-glass he prefers a system of four blowpipes, the flames of which meet upon the work. The superiority of lead-glass where the nature of the experiment will admit of it is duly insisted on. It is a pity that owing to the miserable blowpipes to be found in laboratories, the beginner never gets a chance of realising how excellent lead-glass really is.

The instructions given for cracking or cutting the larger sizes of glass tubes do not quite accord with the writer's experience. The well-known point of melted glass and the flame-pencil with a very small flame are described as being suitable for leading a crack round a tube. The writer has found with tubes that are not too large, a thick copper wire mounted in a handle and bent at the end into such a form as to make a good long contact with the glass, enables one with a little practice to lead a smooth crack round the tube along any predetermined line with an accuracy and quickness not approached by any other process. It is often possible with a single heating of the wire to "cut" a tube an inch or more in diameter either square across or at an angle as great as 30°, and so nearly in a plane that five minutes' grinding on emery cloth will remove the whole of the old surface.

On p. 49 there is a figure showing an ordinary glazier's diamond mounted on a frame, so as to bear upon the exterior of a rotating tube. No doubt as this is described a tube may be cut that way, and in that case, perhaps, no exception should be taken. But since glass tubes cool from the exterior, the inner surface is ultimately in a state of stretch, while the exterior surface is in a state of compression. As a consequence, glass tubes and vessels in general are far more sensitive to even microscopic scratches inside than out. For this reason, it is preferable to mount the diamond at the side of the end of a stick or metal rod provided with a sliding stock like a marking gauge. With such an instrument, tubes may be cut with facility and accuracy. In the same way it is not easy to cut a circular hole in an ordinary glass shade; but if a glazier's diamond is used on a compass within the shade, the piece will drop out at once.

There are useful instructions on boring holes in glass. Of course, the nature of the tool and of the process depends upon the size of hole and thickness of glass. It will be news to most people, however, to read, after the process of drilling with a file is described :

"It is not, however, necessary to use a file at all, for the twist drills made by the Morse Drill Company are quite hard enough in their natural state to bore glass. The circumferential speed of the drill should not much exceed ten feet per minute. In this way the author has bored holes through glass an inch thick without any trouble, except that of keeping the lubricant sufficiently supplied."

The writer has always believed that a pyramidal end to a drill—that is, a drill of the old-fashioned flat pattern, but with the two faces meeting at the point, not joined by a cross-edge—was the best form for drilling glass, *i.e.* when a diamond drill is not available. Such a drill, made dead hard and well lubricated, certainly drills holes in thick glass with remarkable facility. The success of the Morse twist drill, where such cross-edge is always present, would seem to indicate that there is nothing essential in the pyramid theory. The application of the methods of the mechanical engineer to the work of glass is carried a step further on p. 74, where the reader is told to give up grinding glass to form in many cases where this is the usual practice, but instead to chuck it in the lathe and turn it with a steel tool ground to an edge of 80° and well lubricated. After this, any one who has not worked glass in this way would almost expect to read—the best way to start the Morse drill in glass boring is to use a dead hard and sharp centre punch, and give it a smart blow with a light hammer.

An appendix to the first chapter should be found useful, since the interest in experimenting with Röntgen tubes shows no sign of decaying. Complete and detailed instructions are given for making all the parts of these tubes, for putting them together, for making a suitable pump, and for completing by exhaustion and sealing.

The second chapter is upon glass grinding and optician's work. This is one which the great majority of experimentalists will look upon as outside their practical requirements. There is no doubt that the art of optical grinding, as distinct from mere lapidary performances, is one of the most fascinating for the very few who have laid themselves out to practise it. It, however, is one which cannot be embarked upon in five minutes. A good deal of material and apparatus has to be collected before a start can be made, and at the best the processes seem slow and tedious; they are, moreover, of a kind that cannot be hurried. On the other hand, where the practical physicist finds himself in some outlandish place, it may very well be worth his while to acquire the art of grinding and polishing plane and curved surfaces, and of attaining the skill, if he has the patience, of figuring these with the precision that optical work demands. For those within reach of the real or working optician—quite a distinct type from the shoptician—it is barely worth spending the time for the sake of the work to be done, though it may be for the sake of the pleasure that succeeding in a difficult art will bring to the worker. But this is luxury.

On the other hand, occasions arise in experimental work where it is important to be able to do on the spot and at once some operation of a kind which, taken by itself, the experimentalist would prefer to put in the hand of the instrument maker, but which it may be imperative to perform on the spot, even though the technical success may be inferior to that of a second-rate professional.

The whole series of operations required in making an achromatic object-glass of small size are described, not because any one wanting such a glass would be well advised to make one, but because such a description includes all the ordinary routine of optical work, and a beginner would find it a good training. After this the construction of small lenses and of galvanometer mirrors is described. The author tried making these mirrors of fused quartz and of crystalline quartz, as well as of glass, and has concluded that for the most perfect thin mirrors slices of the crystal are better than anything. In this conclusion the writer of this notice agrees.

The construction of large mirrors and object-glasses for telescopes is dealt with; but in the writer's opinion this, while good enough, is somewhat out of place, for it is not possible to devote enough space to the very wonderful art of testing the surface at the centre of curvature. The formula for the longitudinal aberration of the parabola at this point is not given, nor is the reader warned that the formula of Draper, which is so constantly quoted for this, only gives half the correction.

Sections 68 and 69 should be valuable to many. They are both quotations from Brashear, whose optical masterpieces are known of, if they have not been actually seen by every experimentalist in the world. The first is on the cleaning of dirty object-glasses, and the second on the working of plane surfaces on rock salt.

Some attention is given to the peculiar difficulties of producing optically plane surfaces of any size. Lord Rayleigh's beautiful method of testing the figure by interference with a free surface of water just above it is referred to rather than described. While interference methods of testing are shortly described—and they have the undoubted value that they indicate the magnitude and position of any errors—it is, perhaps, unfortunate that the very handy method of testing the goodness of a plane surface by the use of a telescope and artificial star is not properly described.

The chapter on optical work is really full of valuable information. The fact that some criticism has been offered is perhaps owing to the fact that the subject is one upon which no two people would have quite the same views. The writer must, however, here express his disappointment at not finding any indication of the value of carborundum for these processes. He has never lost an opportunity of trying to collect real experience on this material, practically without success. His own very limited experience is all in favour of the virtues which the makers so forcibly set out. It seems impossible in this country to learn anything about it directly.

The third chapter is on all sorts of things that the manipulator in materials ought to know. The first of these is on Margot's method of coating glass with aluminium and of soldering aluminium, or even glass, by means of its aluminium coat! Prof. Threlfall vouches for the practical ease and success of these processes. He gives full details of the very simple process.

The second is on Boettger's process of depositing bright gold upon glass, just as silver is deposited. This also the author has proved to be satisfactory. The question arises whether it might not be worth while, where colour is not important, to use gold in the place of silver in reflecting telescopes for the sake of the permanence that should in this way be attainable.

The third is on slitting with a disc and diamond dust and making rock sections generally. This, however, does not require particular notice except, perhaps, the curious statement that the author was surprised how difficult it was to learn anything about this art. Vol. iii. of Holtzapffel surely cannot have been in his mind when he wrote this.

A large amount of space is given to the fullest details of the different methods of making and mounting quartz fibres and of their properties. No one with this before him need have any doubt about embarking upon this laboratory art. The writer of this notice had produced the first of some articles on the subject in the *Electrician*, but on seeing Prof. Threlfall's book, felt that the ground was so well and accurately covered that it would be a mistake to go over it again. The curious property of the quartz fibre discovered by Prof. Threlfall, of becoming at ordinary temperatures very slightly *more* rigid as the temperature rises, is referred to; and the suggestion which the writer of this notice also put forward tentatively years ago is made, that chronometer balance-springs made of fused quartz might have some advantage. This curious rise in rigidity with temperature is also noticed by Mr. S. J. Barnett in a valuable paper in the *Physical Review* for February last. Another point referred to by both these writers is the extraordinarily small coefficient of expansion of melted quartz. Benoit gives the extreme coefficients for crystalline quartz as $\cdot 0^{5}72$ and $\cdot 0^{4}133$. Barnett found for three quartz fibres $\cdot 0^{6}3$, and for a rod of fused quartz $\cdot 0^{6}2$. There is one part of the description of the manipulation with quartz fibres where the writer would add to Prof. Threlfall's description. On p. 220 the method of handling the fibre, cutting it off, and mounting it so as to be of the right length is described. Instead of a board to work on, however black it may be, a piece of looking-glass lying flat on the table is infinitely superior. This was suggested years ago by some kind friend, but who it was the writer is ungrateful enough not to remember.

The writer prefers when blowing quartz fibres of extreme tenuity for suspension purposes, not to blow a maze on to some screen, but, using a finer flame, to blow out a single fibre which may often be found joining the two rods, and either thick enough to show colour or generally far too fine to do so, corresponding in fact to the black of the soap-bubble.

Soldering, brazing, silver soldering, all essential every-day arts, are next described well and fully; but whether these descriptions will make these actually easy arts ever seem so to beginners is a question. Perhaps enough is not made of the sweating process carried out without any bit, or any preliminary cleaning or preparation of any kind. On the other hand, under brazing and silver soldering, the great use of a bit made of clean iron wire in showing the melted metal where to go when it does not flash at once, might be added in a future edition.

Insulators and conductors used in the construction of apparatus are next considered. Prof. Threlfall is probably the only person who has turned to useful account the writer's discovery of the superlative insulating properties of rods of melted quartz, even in an atmosphere saturated with water. Their application to a number of electrical appliances is described and figured.

Glass, ebonite, mica, micanite, celluloid, paper, paraffin, wood, slate, and marble are all discussed from the point of view of a constructional material with insulating properties. The electrical and mechanical properties of a large number of alloys, such as platinoid, manganin, &c., close this long and most valuable chapter.

The last chapter is upon electro-plating, chiefly gold, silver, copper and nickel, and upon allied arts. The writer has often heard that the best nickel plating is really cobalt. He hoped to, but did not, find any enlightenment upon this point.

An appendix upon platinising glass concludes the book.

This notice, already too prolonged, and yet insufficient, is enough to show that the experimentalist has now a most useful guide in a large number of processes. It is not possible to describe every process. The personal certificate is what gives value to those that are chosen. It is to be hoped that with Prof. Threlfall's valuable guide, instead of despising them, some of our growing physicists may be encouraged to make themselves familiar with some, at any rate, of those arts which Newton and Faraday cultivated with such astonishing skill and success.

C. V. BOYS.

A NEW TEXT-BOOK OF ZOOLOGY.

A Student's Text-Book of Zoology. By Adam Sedgwick, M.A., F.R.S. Vol. i. Pp. 600. (London: Swan Sonnenschein and Co., Ltd., 1898.)

MR. SEDGWICK has produced the first part of what must prove to be a very useful treatise for University students, if the remaining portions of the work are as well carried out as is the present.

In this volume Mr. Sedgwick gives an account of the Protozoa, Porifera, Cœlentera, Platyhelminthes, Nemertea, Nemathelminthes, Rotifera, Mollusca, Annelida, Sipunculoidea, Priapuloidea, Phoronidea, Polyzoa, Brachiopoda, and Chætognatha. The method adopted is strictly systematic: the larger groups are described and characterised in turn, the enumeration extending as far as families, which are also briefly characterised, important illustrative genera being cited. The work is, in fact, written on the lines of the translation of the "Zoology" of Prof. Claus, which Mr. Sedgwick gave us some years ago; but instead of merely producing a new edition of that work, he has written a new book introducing his own views and his own conception as to what are important facts and useful schemes of classification.

A distinctive feature of the work is the number of excellent woodcuts which Mr. Sedgwick has culled from a very large variety of sources. The text-books of Korschelt and Heider, Perrier, Lang, Claus, Wasielewski and Bronn's Thierreich have been laid under contribution for *clichés*, and the author is to be congratulated on the admirable collection he has brought together. The book is intended to be and is as brief as is consistent with an intelligible exposition. Yet it seems hardly possible that Mr. Sedgwick will be able to complete it in another volume of the same size. He has still to treat of the Echinoderma, the entire series of Arthropoda and the Vertebrata (which he would probably call the Chordata).

There are in the book one or two noticeable and original statements and classificatory innovations which it will be interesting to mention here. Mr. Sedgwick holds, as is well known, special views on the subject of cell-structure. He accordingly defines the Protozoa as "Animals in which there is one nucleus, or, if more than one nucleus, in which the nuclei are disposed apparently irregularly and without relation to the functional tissues of the animal. Conjugating cells of the form of ova and spermatozoa are never formed." In contrast with these the Metazoa are defined as "Animals in which the ordinary (so-called adult) form of the species has more than one nucleus, and in which the nuclei are for the most part arranged regularly and with a definite relation to the functional tissues of the animal (so-called cellular arrangement). Special conjugating individuals of the form of ova and spermatozoa are always formed."

With reference to this it may be remarked that the nuclei of, say, muscular tissue in Metazoa cannot be shown to have any more definite relation to the functional contractile substance than has the nucleus of a gregarine to its functional contractile substance, and the same kind of remark is true in reference to many other active structures in the two groups compared.

It surely is not possible to maintain that conjugating cells of the form of ova and spermatozoa are never formed in the Protozoa when we include (as Mr. Sedgwick does) the Volvocinean Flagellata in that group.

The account of the Protozoa is more complete than is usual in text-books of this size and scope, and the figures of Hæmosporidia and Myxosporidia, borrowed from Wasielewski, are particularly good, though the account on p. 63 of Hæmamœba Laveran i is not quite satisfactory.

Mr. Sedgwick, as might be expected from his own important share in elucidating the subject, is very clear and precise in defining the "cœlom," and in explaining its real nature. He does not, however, as one could have wished, give the actual history of the word "cœlom," and the steps by which the erroneous views of Haeckel, the Hertwigs and other German authorities have been set aside. He says, "formerly the word cœlom was used as synonymous with body-cavity or peri-visceral cavity, and no distinction was recognised between the body-cavity of the Arthropoda and the same structure in such forms as Vertebrata." I think it is worth noting that, as a matter of fact, the word cœlom was introduced by Haeckel in the year 1872, in the first volume of his "Kalkschwämme," p. 468, in the following words :

"Die wahre Liebeshöhle" (contrasted by Haeckel with the digestive cœlenteron of Cœlentera, to which the term "body-cavity" or "Leibeshöhle" was undesirably applied) "welche bei Vertebraten gewöhnlich Pleuroperitonealhöhle genannt wird, und fur welche wir, statt dieses neunsylbigen Wortes die bequemere zweisylbige Bezeichnung Cœlom (κοίλωμα, τὸ, die Höhlung) vorschlagen, findet sich nur bei den höheren Thierstämmen bei den Würmern, Mollusken, Echinodermen, Arthropoden und Vertebraten."

For Haeckel the typical cœlom was the pleuroperitoneal cavity of the Vertebrate. At the time when he wrote, that cavity was supposed to have arisen phylogenetically by a splitting of the mesoblast; hence the failure of Haeckel to distinguish other cavities, such as the hæmocœl of Arthropoda and of Mollusca from the true cœlom. I gather from Hertwig's text-book of Embryology that I was the first to point out that the "schizocœl" (as Huxley called it) of higher Vertebrates could be and should be interpreted (in consequence of Balfour's discoveries in Selachian development) as an enterocœl—a pouch, in this case without lumen—which arises as a solid outgrowth from the enteron, the opening out of its cavity being delayed. Thus the cœlom is now characterised by Sedgwick as "a part of the enteric cavity which has lost its connection with that portion which constitutes the alimentary canal in the adult." The enteric pouches of the Actinozoa are "an incipient cœlom." Further, it is recognised by Sedgwick that "the cœlom, in addition to its mechanical relations, has two most important functions : the one of these is to bud out the reproductive cells, and the other to secrete the nitrogenous waste." The essential cells of the gonads and of the nephridia are parts of the cœlom. Mr. Sedgwick's own researches on the development of Peripatus served more than anything else to establish that the cavity of Arthropods, which I had termed "hæmocœl," is distinct from cœlom, and that there is—quite apart from hæmocœl—a true cœlom in Arthropoda reduced in the adult to nephridial and perigonadial rudiments. My own observations on the pericardium of Mollusca, and on the vascular system of both Molluscs and Arthropods, as well as the work of my pupil Gulland on the coxal glands of Limulus, had tended, before this, to show the existence of "cœlom" distinct from "hæmocœl" in both those groups. Thus the erroneous notions promulgated in the "Cœlomtheorie" of the Hertwigs were superseded. I am distinctly of the opinion that this step forward—viz. the recognition, definition and characterisation of the true "cœlom" as distinct from "hæmocœl"—has been due to English observations and English doctrine, and I think that a full account of the history would be valuable to students.

Mr. Sedgwick necessarily has something to say in this connection concerning the supposed communication of vascular system and cœlom in the Leeches. In his excellent account of those animals (in which he not only discusses Acanthobdella, but introduces Kowalewsky's recent figure of its anterior segments) Mr. Sedgwick lays great stress on Oka's recent observations upon Clepsine, and concludes that "we are bound to hold, provisionally at any rate, that in Leeches, as in other animals, the blood system and cœlom are separate from one another." I quite agree that there are probabilities in favour of Mr. Sedgwick's conclusion. Twenty years ago, and at intervals since then, I have endeavoured to put the matter out of the region of probabilities, but in spite of the careful researches made in my laboratory by A. G. Bourne and others, I have not yet succeeded in so doing. After all, it should be possible, by modern improved methods, to test this question of continuity in *Hirudo* by means of actual injection. There are "other animals," it must be remembered, in which there is free communication between the cœlom and the vascular system, to wit, the not unimportant animals known as Vertebrata.

In his classification of the Mollusca, Mr. Sedgwick has taken his own line, and refused to follow Pelseneer in the separation of the Chitons from the Gastropoda, though he places Neomenia and Chætoderma in a separate class, the Solenogastres, for very good reasons which he sets forth.

The creation of a separate phylum for each of the small groups of Sipunculoidea, Priapuloidea, and Phoronidea is perhaps legitimate in the present state of knowledge, though the questions involved are of a very difficult nature, and the facts known insufficient to give one great confidence in any of the proposed classifications affecting those animals.

Mr. Sedgwick excludes the Platyhelminthes, the Nermertea, the Nemathelminthes, and the Rotifera from the Cœlomata; but he does not argue at any length the question as to whether there are or are not cœlomic rudiments in each of these groups. The perigonadial sacs of Platyhelminthes and Nemertea and their nephridia may be interpreted as modified developments from cœlom, though it would no doubt be difficult to show that they are so. It must, however, be remembered that in such matters the assertion that A *is not* B is as positive and definite a statement, requiring just as full a proof, as the statement that A *is* B.

The chief omission which has to be noted in Mr. Sedgwick's book is that which I have recently pointed out in other works—namely, an insufficient historical account of the discoveries, hypotheses, conceptions and terms (with immediate reference to chapter and verse), the bringing together and explanation of which is the purpose of the writer's labour. Mr. Sedgwick is not so determined to omit history and the names of contemporary workers as are some other writers of text-books. He does not make a profession or virtue of this practice, and in many cases gives an immediate reference to a special memoir, or even cites a naturalist's name, after mentioning an important fact or theory. At the same time, he cannot be said to have done what could easily have been done in this respect without materially increasing the size of his book. Of course, all such references and discussions must be in proportion to the size and scope of the text-book in which they should appear, and Mr. Sedgwick not unfrequently does give a historical reference. But why should he not tell us, for instance, who invented the name Protozoa, what he meant by that term, and how it came to have its present limitations? Why should he not tell us (p. 533) who proposed the separation of Sipunculoidea and Echiuroidea which he adopts? Why should he not give credit to Dr. Hudson for his most interesting discovery of the six-legged Rotifer *Pedalion*, instead of printing Hudson's drawing of his discovery with the label "from Perrier after Gosse?" Mr. Sedgwick very properly states in a footnote that the classification of the Polychæta adopted by him is that of Dr. W. B. Benham, to whose work he refers. It would, I think, have helped many of his readers if he had given some account of the source of classification and terms used by him, in all other instances. Putting aside such suggestions for improvement, I think we must recognise that Mr. Sedgwick's book is a very good one, ably put together, and likely to be extremely useful; it is, in fact, not only the last, but the best zoological text-book—so far as the first volume goes—in the language. E. RAY LANKESTER.

THE ANALYSIS OF ORES.

Methods for the Analysis of Ores, Iron and Steel, in Use at the Laboratories of Iron and Steel Works in the Region about Pittsburg, Pa. Pp. iv + 133. (Easton, Pa.: Chemical Publishing Co., 1898.)

A COLLECTION of the methods in use in the modern laboratories of steel works must be useful if only for comparison, but the present book cannot take rank with standard works such as those by Blair and Arnold. One notes a sameness in the modes of procedure, varied, however, in some instances by questionable modifications, more especially as regards phosphorus determinations.

Sufficient attention has not, on the whole, been given to the exact relative proportions of nitric acid, molybdate, &c. Most of the operators are apparently content to assume that it is sufficient to add, in all instances, measured quantities of the reagents required. This is contrary to the writer's experience: each analysis should be conducted in accordance with the conditions observed at the time; it is not enough to merely add fixed quantities of reagents, but the operator must judge for himself, more especially as regards the use of nitric acid.

In practice the best and most accurate results are obtained by the direct weighing of the molybdate precipitate, using the magnesia method only as a check.

Volumetric methods are useful where rapid determinations are required for check purposes, but are not so trustworthy as the weight method, *i.e.* when proper precautions are taken and the necessary experience gained.

Sulphur.—The evolution method cannot be dispensed with in an ordinary steel works, but is only useful for rough determination; it is little better than a qualitative method, as has been repeatedly demonstrated.

Apparently we have no better method than with aqua regia and subsequent precipitation with barium chloride. It is well known, however, that discordant results are often obtained. At present a rapid and strictly accurate mode of determining sulphur has yet to be devised; this for various reasons well-known to analytical chemists.

As regards the estimation of manganese, nickel, copper, &c., little need be said; there is not much that is novel in the methods, which are fairly good and are such as are usually practised. The same is applicable to carbon determinations, with the exception of barium hydroxide as an absorbent (A. G. McKenna), which the author recommends; as also the complete analysis of chrome iron, which appears a mode of procedure sufficiently accurate for all practical purposes.

Analysis of Ores, &c.—Mr. James M. Camp's method for rapid analysis of blast furnace cinders apparently gives results useful to the blast furnace manager, but the determination of manganese, from Mr. Camp's own showing, cannot be neglected.

The writer has used the colorimetric method both for iron and manganese, especially iron; it is most important to make frequent iron determinations, for obviously iron in the slag is equivalent to loss of metal in the pig-bed. The colour method is rapid, good for iron in slag, and more accurate than the weight process.

Determination of Silica in Ores.—One notes that potassium sulphate or hydrofluoric acid are sparingly used, American chemists relying chiefly on the sodium carbonate method. In this country preference is given to the use of the former; chemical results are considered more accurate, with economy of time.

Determination of Iron.—The bichromate method leaves nothing to be desired as regards slags, ores or minerals in general, but is not very suitable for the accurate determination in iron or steel. Most chemists are content in iron or steel analysis to give the iron by difference, but if a method could be devised whereby the absolutely pure iron could without question be determined within ·001 per cent., such a factor would in the present state of our knowledge be invaluable. Those who have studied the recent developments of the chemistry of iron will understand this.

On the whole, American practice seems inferior to the English; some of the methods quoted are practically obsolete in this country. This applies more especially to manganese determinations—only two chemists when using the gravimetric method for manganese take note of the previous necessary removal of barium when ores are being analysed, to say nothing of other possible impurities.

Very many of the processes given seem devised merely for speedy work, regardless of accuracy; on the other hand, some needless complications have been introduced with consequent loss of valuable time.

JOHN PARRY.

OUR BOOK SHELF.

Electro-physiology. By W. Biedermann. Translated by Frances A. Welby. Vol. ii. Pp. vii + 500. (London: Macmillan and Co, Ltd., 1898.)

MISS WELBY has now completed her translation of this work. The second volume is equal to the first in scientific interest and importance, and the technical difficulties of rendering it into English have been overcome with even greater success.

Prof. Biedermann deals with the main subject of the volume, that of the "electro-physiology" of nerve, much more from a physiological than from an electrical point of view. In every branch of it he is able to give us the results of his own work, or of those of the distinguished colleague with whom he was for so many fruitful years associated at Prague; so that the student who desires to appreciate the experimental basis of Hering's doctrine cannot have a better guide than is here provided for him. It must not, however, be supposed that the work is mainly theoretical; on the contrary, on the subjects of which it treats, it is the best "reference-book" that the physiological worker has at present at his disposal.

In addition to the chapters on nerve, the volume contains a very carefully written chapter on the electrical endowments of the plant-cell, another on electric fishes, and a third on the electrical response of the retina to the stimulus of light. In discussing the first two of these special subjects, Biedermann derives his data chiefly from English sources. In the elaborate and copiously illustrated chapter on electric fishes, the reader will find a complete account of Prof. Ewart's investigations of the development and structure of the electrical organ in the rays; and of Prof. Gotch's researches on Torpedo. In like manner the chapter on the electromotive properties of excitable tissues of plants is mainly based on English researches on *Dionæa*, of which it contains a very full *résumé*. It is a satisfaction to the writer of this notice that the main results of his own investigations have been accepted by his German colleague, and particularly to observe how fully he has appreciated the evidence they afford of the essential identity of the elementary processes of plant and animal life. J. B. S.

Open-air Studies in Botany: Sketches of British Wild-flowers in their Homes. By R. Lloyd Praeger, B.A., B.E., M.R.I.A. Illustrated. (London: Charles Griffin and Co., Ltd., 1897.)

THESE open-air studies should appeal to people who live in the country, and who care about the wild plants around them. A glance through the pages recalls many a country ramble, and a good point about the treatment in the book is that an attempt is made to connect the flora of a locality with the physical conditions which prevail there. It is a pity, however, that the author should have not adopted the names in common use for his plants—*e.g. Scilla festalis* the wild hyacinth or *Volvulus* for *Convolvulus* both look and sound pedantic. Moreover the glossary, which forms a necessary appendix, is sometimes disfigured by misleading statements; thus a carpel is stated to be that part of a flower which contains an ovary. But in spite of occasional slips and blemishes, the positive merits of the book should secure for it a fair measure of success.

The Journal of the Iron and Steel Institute. Name Index. Vols. I–L (1869–96). Edited by Bennett H. Brough. (London: E. and F. N. Spon, Ltd., 1898.)

THE Iron and Steel Institute was founded in 1869, and since its establishment it has done most useful work by arranging periodical meetings for the discussion of practical and scientific subjects bearing upon the manufacture and use of iron and steel. The papers published in the Institute's *Proceedings* are here indexed, and they make a solid contribution to knowledge. The volume contains a short history of the Institute, a list of papers contained in the first fifty volumes arranged chronologically, a list of these papers arranged according to subjects, an index of the authors, and a complete index to the authors of all papers, communications, and abstracts published in the fifty volumes. The complete index will thus be of service in showing the development of the science of iron and steel.

A Simplified Euclid. Book I. By W. W. Cheriton. Preface by Elliott Kitchener. Pp. iv + 111. (London: Rivingtons, 1898.)

SO many simplified Euclids have been published during the last few years, that an addition to their number should seem superfluous. In the one before us the compiler claims that after teaching the subject for some years he thinks that the form he proposes in this book should supply a long-felt want. The method he adopts is to print the proposition exactly as it should be written out by a schoolboy, using sufficient abbreviations to save time in writing without confusing the mind of the pupil. Each proposition is printed on the left-hand side of the page, notes and exercises being printed on the right. The book has many points in its favour.

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.*]

The Origin of the Aurora Spectrum.

PROF. RAMSAY gives the wave-length of the principal line in his new gas as 5566. It will no doubt also occur to others that this is very near the wave-length of the aurora line, which Vogel has measured as 5569. It should be mentioned in connection with this line that Profs. Liveing and Dewar have observed one very near it at 557 in sparks taken in liquid oxygen. The second green line given by Prof. Ramsay as 5557, seems also to have been seen by these observers (*Phil. Mag.*, xxxviii. p. 237, 1894). ARTHUR SCHUSTER.

Manchester, June 10.

The Action of Electric Discharges on Photographic Plates.

REFERRING to the paper on this subject, read on May 16, by Mr. J. A. McClelland, at the Cambridge Philosophical Society, and reported in your issue of June 9 (p. 142), perhaps I may be allowed to mention that very similar experiments, with the deduction that the effect is chiefly due to light, and not to electrolytic or other action, were described by myself in a paper to Section A of the British Association, at its Edinburgh meeting in 1892, and will be found fully reported in the *Electrical Review* for August 26 of that year.

I do not know whether others have observed the fact that when strong sparks from an induction coil or influence machine are allowed to traverse the sensitive surface of an ordinary photographic dry plate, that a dark line, delineating the path of the spark, is immediately produced, and can clearly be seen without any necessity for photographic development. Further, that such lines, though faint to commence with, darken appreciably after a few minutes lapse of time, and still more so in the course of a few hours. This appears to indicate that whatever the precise action of the spark on the film, this action continues after it has once been started. Further, it is a curious fact that these lines, if examined with a magnifying glass, are always found to consist of two dark lines with a light space between them. This is specially noticeable immediately after the spark has passed, the space apparently filling up with lapse of time.

A. A. C. SWINTON.

66 Victoria Street, London, S.W., June 10.

A High Rainbow.

ON Sunday afternoon, May 29, while sitting in my yard, my twelve-year-old son called my attention to a rainbow which he had discovered while lying on his back looking up at the sky. The local time here was 5.40 p.m., and the sun, therefore, about an hour and a half high. The bow was in the west, and about 70 degrees from the horizon, with its convex side to the sun. The colours were fairly well brought out, the red being on the convex side of the arc, and the violet on the concave side. The figure on p. 132 of Tait's "Light" shows a short arc near the zenith, which is a fair representation of what was seen here. I have not read an account of what was seen by Helvetius further than is contained in Prof. Tait's book, and do not know whether the arc seen by him near the zenith showed the rainbow colours. In this case I do not see any of the other halos seen by Helvetius. There were but few very thin clouds, and no rain at all. SIDNEY T. MORELAND.

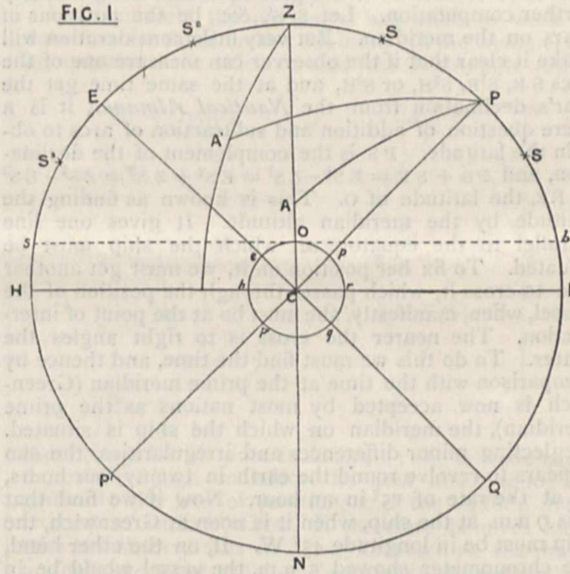
Lexington, Virginia, U.S.A., June 2.

NAUTICAL ASTRONOMY.

IF the compass is the navigator's sheet-anchor, the sextant is certainly his best bower; and just as the former was known, if not generally used in Europe, about a century before Flavio Gioia got the credit of discovering it, so the latter was invented by the transcendent genius of Sir Isaac Newton, more than half a century before it was re-invented by Hadley in 1731. Newton does not seem to have suggested its adaptability for navigational purposes, or if so, it was not sufficiently known or taken up, and I am not aware of any reason to suspect that Hadley knew of Newton's discovery.

The principal use the navigator puts the sextant to is that of measuring the altitudes of heavenly bodies—that is, the angle at his eye subtended between the object and the visible horizon. Now the *rational* horizon may be defined as the plane perpendicular to the plumb-line through the earth's centre, or the circle traced by the meeting of this plane with the celestial concave. The *sensible* horizon is generally defined as a plane parallel to the former through the eye of the observer; but this can only coincide with the *visible* horizon if the eye of the observer is at the surface of the earth—as if he were immersed in the sea, till a horizontal line from his eye would be a tangent to the sphere at that point. But the eye of the observer is always above the surface of the sea; and the more it is raised, the more the visible horizon is depressed, and a correction called "dip" has to be applied to an altitude measured to it, to reduce it to what it would have been had the eye been at the sea-level. Again, before this *apparent* altitude can be used for position-finding, it has to be still further corrected for

FIG. 1



refraction, due to the bending of the rays of light, in passing through the earth's atmosphere, and in the case of sun, moon, or planet for parallax, to reduce it to the angle at *the centre of the earth and to the rational horizon.* Both these corrections are zero when the body is in the zenith, and a maximum at the horizon. Parallax is the angle at the observed body, subtended by the semi-diameter of the earth under the feet of the observer, which will be reduced to a point when the body is in the zenith. If the body has an appreciable semi-diameter, it has to be applied to the altitude of the limb to get that of the centre.

In the diagram (Fig. 1), let H E Z P R Q N P' represent a meridian of the celestial concave, and the inner circle the corresponding meridian of the earth; let Z be the zenith, N the nadir, P and P' the poles of the heavens, being the points in the celestial concave, which would be perforated by the earth's axis if indefinitely produced: then H R will represent the rational horizon, the plane of which, passing through C, is normal to the plumb-line Z O N, *s* O *b* will represent the sensible horizon (O being the position of the observer), E Q, the plane of which is normal to P P', will be the equinoctial, whose plane coincides with that of the terrestrial equator. On a meridian from E Q towards either pole, the declination of a heavenly body (corresponding to latitude on the earth) is measured, and from the first point of Aries (the celestial meridian passing through which is the prime meridian of the heavens) right ascension is measured round eastward, instead of east and west, as longitude on the earth.

Now let the reader imagine his eye to be at C, that the earth is a transparent sphere, and that it and its atmosphere are absolutely free from refrangibility, then every point in the celestial meridian would be seen through its prototype on the surface of the earth, and any and every angle at C, measures the same arc of the celestial meridian, and of the one on the surface of the earth. Now, what is true here holds good for every other meridian—every other great circle of the celestial concave, and the one that has the same plane on the earth's surface.

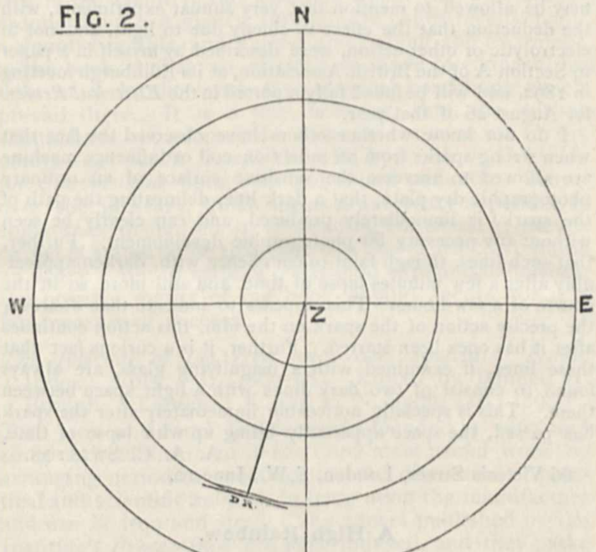
The latitude of a place is the arc of a meridian, intercepted between the place and the equator, consequently *e* O is the latitude of O; but *e* O and E Z are both measured by the angle *e* C O, and E Z = P R, each being the complement of P Z, which accounts for one of the best-known rules in nautical astronomy, viz. that the altitude of the pole = the latitude of the place; so that if there was a star at P, its altitude would give the latitude without any further computation. Let S, S′, &c., be the positions of stars on the meridian. But very little consideration will make it clear that if the observer can measure one of the arcs S R, S^1R, S^2H, or S^3H, and at the same time get the star's declination from the *Nautical Almanac*, it is a mere question of addition and subtraction of arcs to obtain the latitude. P S is the complement of the declination, and $PS + SR = ES^1 - ZS^1 = ES^2 + ZS^2 = ZS^3 - ES^3 = EZ$, the latitude of O. This is known as finding the latitude by the meridian altitude. It gives one line parallel to the equator, on which the ship must be situated. To fix her position on it, we must get another line to cross it, which passes through the position of the vessel, when, manifestly, she must be at the point of intersection. The nearer the cross is to right angles the better. To do this we must find the time, and thence by comparison with the time at the prime meridian (Greenwich is now accepted by most nations as the prime meridian), the meridian on which the ship is situated. Neglecting minor differences and irregularities, the sun appears to revolve round the earth in twenty-four hours, or at the rate of 15° in an hour. Now if we find that it is 9 a.m. at the ship, when it is noon at Greenwich, the ship must be in longitude 45° W. If, on the other hand, the chronometer showed 5 a.m. the vessel would be in longitude 60° E. The Greenwich time may be calculated from a lunar observation, which the perfection of the modern chronometer and the shortening of voyages have driven out of the field. To get the time at ship, we have recourse to spherical trigonometry, or rules and tables based on it, to calculate the hour angle. The sun's westerly hour angle is the apparent time at place (A.T.P.), which is converted into mean time (M.T.P.) by applying the equation of time, which, like declination, &c., is supplied by the *Nautical Almanac*. If the body observed is a star, we get the M.T.P. by adding to the hour angle the star's right ascension, and subtracting that of the mean sun, which is a transposition of the well-known and useful equation, ✱'s hour-angle = M.T.P. + mean ⊙'s R.A. − ✱'s R.A. which we use for time azimuths, and for finding when a body will cross the meridian, for when hour angle = 0

M.T.P. = ✱'s R.A. − mean ⊙'s R.A.

Now, just as the simplest way of getting the latitude is by a body on the meridian, so the best way of calculating the time for longitude is by using the altitude of the sun or a star on the prime vertical (*i.e.* the vertical circle passing through the E. and W. points of the horizon). If, by means of this altitude, or any other way, we could tell the exact instant that the body was on the prime vertical, there being a right angle in the triangle A P Z (Fig. 1), we could calculate the time by right-angled spherics from any two of the three sides, colatitude, polar distance and zenith distance, or their complements latitude, declination and altitude. But in practice, whilst it is easy to get the meridian altitude, it is impossible to be sure of getting the altitude exactly on the prime vertical. It is, however, comparatively easy to observe a body near enough to the prime vertical to be very favourably situated for finding the time by oblique spherics (or formula deduced from it), and thence the longitude; and this, combined with the meridian altitude, is perhaps the simplest and most favourable method of fixing the position at sea. However desirable, it is by no means necessary that the body be near the prime vertical, though, generally speaking, the further it is removed from it, the less favourable the conditions, till at last the triangle becomes an impossible one.

Every particular star is, at every instant of time, in the zenith of some spot on the surface of the earth. At any given instant of time, let Z, in the accompanying figure,

Fig. 2.

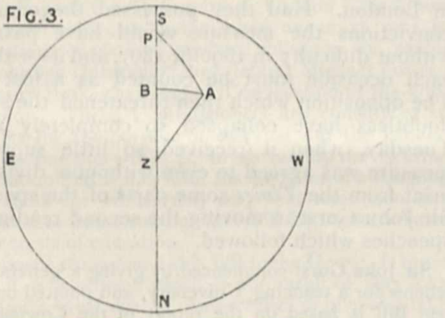


be this spot, as it would be seen from the zenith; then the concentric circles represent circles of equal altitude on the earth's surface, *i.e.* everywhere on the outermost circle the star will be on the horizon (neglecting refraction, &c.). On all places in the next circle the altitude will be 22½°, on the next 45°, &c.; and, of course, there may be an infinite number of imaginary circles between the spot under the star and the outer circle, which brings it on the horizon. Now, it is evident that at whatever point on any of the above circles an observer may be situated, a tangent to the circle at that point will be at right angles to the bearing of the body; but a small portion of the circle may be represented by a similar portion of the tangent, and it is evident that the larger the circle (which is equivalent to the smaller the altitude), the longer the portion of its circumference that may with impunity be treated as a straight line. This straight line is known as "a line of position." The line of position obtained from a meridian altitude differs from all others in this, that the ship is not only on the circle of equal altitude, but on its vertex,[1] and the tangent may be assumed as of infinite length.

1 Compare figure in paper on "Navigation," (p. 104) illustrating composite sailing, where, however, the circles that touch the parallel are great circles.

The line of position by an altitude for time was first discovered by Captain Sumner, who, being doubtful of what latitude he was in, worked an observation with three different latitudes. On projecting these positions on the chart, he found that all three were in a straight line, which produced, led to the Smalls light, whose bearing he thus had, without knowing how far it was away. He steered along the line till he found it. He did not observe, however, that this line was at right angles to the sun's bearing, nor would it have shortened his problem if he had, because it then took as many figures to calculate one longitude and the azimuth as two longitudes with different latitudes. In these days, when azimuths can be taken out of tables by inspection, nearly half the figures are saved by using the azimuth to obtain the line of position.

Thus, no matter what the bearing of a heavenly body, if we can observe its altitude and the corresponding time at Greenwich, it will afford us some information as to the position of the ship. If it is on the meridian, with a minimum of labour we get the latitude in the simplest and most accurate way available to the navigator. If it is not too far in azimuth from the meridian, there are plenty of methods by which the observation can be reduced to the corresponding meridian altitude, and the latitude obtained. If it is on the prime vertical, the line of position will be a portion of a meridian. If it is on any intermediate bearing, the line of position will be at



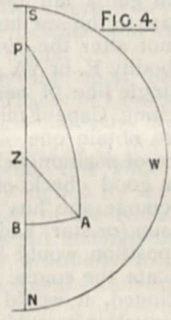
right angles to the bearing of the body, through the latitude by account, and the longitude deduced from it and the observation. Any two lines of position, provided they do not cross at such an oblique angle that the intersection is ill-defined, will fix the position of the vessel. When the star is so far from the meridian, and the time too uncertain to be favourable for working as an ex-meridian, and yet too far from the prime vertical to give an accurate hour angle, the new navigation, originated by the French, and introduced into England by Captain Brent and Messrs. Williams and Walter, R.N.,[1] gives a better line of position than the older methods. By it you calculate the *altitude* for the position of the ship by dead reckoning. If this agrees with the observed altitude (corrected), the line of position is at right angles to the bearing of the star, through the position by D.R. If, however, the observed altitude is, say, 10′ greater than that calculated, the ship must be that much nearer the spot on the earth where it was in the zenith at the moment of observation; so you lay off 10 miles (1 sea mile being practically 1′ of a great circle) from the D.R. position, in the direction of the star, and through this point rule the line of position at right angles to the bearing; or the corrections for the D.R. latitude and longitude may be calculated by trigonometry (see Fig. 2).

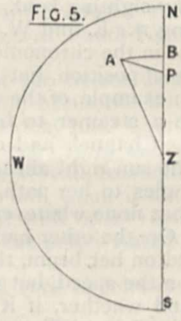
The triangle APZ (see Figs. 1, 3, 4 and 5) is the most important in nautical astronomy. Up to this, I have only referred to it as a means of finding hour angles (angles at P); but not only is it also used for finding azimuths (angles at Z), for if the time be accurately known, we can utilise it for finding the latitude by a star with a large hour angle. To make it clearer, and avoid complicating Fig. 1, I give figures here on the plane of the horizon. In these, let A represent three different stars, and from A let fall a perpendicular on the meridian. Then right-angled spherics can be utilised, and the latitude obtained with fewer figures than by the new navigation. Either before or after the ✱ or ✱s shown in the figure, are obtained for latitude, observe one on or near the prime vertical, for longitude and time, which will give accurately the hour angles of the latitude ✱s, allowing, of course, for any easting or westing made between the observations. Then

$$\sin AB = \sin h \sin p, \tan PB = \cos h \tan p \text{ and } \cos ZB = \sin a \sec. AB$$

h being the hour angle, p the polar distance, and a the true altitude. The sum or difference of PB and ZB = the colatitude. This method is even shorter than it appears at first sight (because the logs. can be taken out in pairs), and is concise and accurate when the data is trustworthy, and, even if the hour angle is doubtful, will give a good line of position.

Unfortunately, the navigator has often to work with data that are more or less doubtful. In the triangle, APZ, he uses the three sides to find the hour angle (P). Of





these the polar distance is accurate; the latitude is often doubtful enough to affect the hour angle, though not generally the line of position, and the altitude may be vitiated in various ways. It therefore behoves him to take his observations in a way that errors, that he can neither detect nor avoid, will neutralise each other. Few human eyes are optically perfect; the best sextants, though beautiful instruments, are not absolutely faultless, and their errors are liable to alter by a knock or jar; the sea horizon is fickle, and refraction uncertain; but the whole of these errors may be minimised, if not absolutely eliminated, in the resulting latitude—for example, by observing (Fig. 1) the ✱'s S^1 and S^2. With about the same altitude, their refraction will probably be similarly affected; the horizon is *generally* subjected to the same influences all round; the personal and instrumental errors may be taken as constant, for the same observer and sextant, at any particular time and place when the altitudes are somewhat similar. Suppose the sum of these errors to be $-2'$, and unallowed for, the effect would be, in each case, to make S^1 and S^2 appear nearer Z than the truth; and while each resulting latitude would be 2′ wrong, the mean would be correct.

Again, in the single altitude problem (Figs. 3, 4 and 5), if the time had been calculated by two stars, one east and the other west, the time and thence the hour angles of the latitude ✱s would be less liable to the foregoing errors; and if the three stars were taken and worked for latitude, each would be a check on the others, and opposite bearings would tend to neutralise errors of

1 "Exmeridian Altitude Tables and other Problems," by these authors, is an excellent work.

altitude. At the same time, single observations very generally give sufficient accuracy for all the purposes of navigation, though they are not to be so absolutely relied on as a systematic set.

I must not conclude without another word on the lunar method of finding the Greenwich time, which I dismissed rather summarily, further back, in favour of chronometers. For long voyages across the ocean, when a vessel is from fifty to one hundred days without sighting land, lunars may still be used, as before the days of steam, not so much for finding individual longitudes as for *rating the chronometers*, and for this purpose it is essential that a series of distances be measured on each side of the moon, and the mean of all the easterly ones, meaned with the average result of those taken to the westward, to eliminate as far as possible personal and instrumental errors. An expert lunarian, who practises regularly, may find out by experience about how much on each side, single distances, measured east or west, would place his ship; otherwise, the result of a single lunar could not be relied on to give the longitude to nearer than $\frac{1}{2}^{\circ}$ or 30′, even if taken under favourable circumstances. In a steamer vibration almost precludes the observation, and the chronometers ought seldom to be 5s., and never 30s. wrong. In ports where there are not time-balls, the chronometer errors can be found accurately with the artificial horizon, observing, if possible, ✳s east and west, or the ☉ and ☽, or even by the ☉ alone. It is generally possible to verify the errors at sea by sighting land, or even to get a fair rate, by observing ✳s E. and W. of meridian to the sea horizon. An error in the chronometer does not alter the *direction* of a line of position, but moves it bodily E. or W.

As an example of the use of a single line of position, suppose a steamer to be approaching Cape Finisterre from the Channel, and only able to obtain one observation, the sun right ahead. The line of position, being at right angles to her path, will be a good check on her speed, but none whatever on the course she has made good. On the other hand, if the sun (or star) had been observed on her beam, the line of position would be no check on the speed, but would indicate the course made good, and whether, if it was continued, it would clear the land. In soundings, a line of position may be combined with the depth of water to fix the position.

The tendency of modern navigation is to become too stereotyped—to do everything by tables, which obscure the mental vision, and to relegate to the bookshelf that knowledge of theory which, combined with practical experience, is the surest guide to the navigator in deciding on the best way of utilising his observations, and which method, in any particular case, will give him the best line of position. If theory is not the only thing that will teach him, that while when the sun culminates near the zenith, he can get good observations for time within a few minutes of its passing the meridian, a Sumner line derived from such observation would be almost useless, owing to the smallness of the circle of equal altitude; it will certainly make him acquainted with the fact in a tenth of the time that unaided experience will. Some of the so-called short methods are only short because of preliminary calculations that are not counted by the authors in the work, and which may all go for nothing if some particular altitude is not obtained, that a passing cloud may render it impossible to measure; or else they involve several vexatious interpolations, which are quite as much trouble, and, if performed mentally, much more liable to error, than taking out and adding up a few lines of logarithms.[1]

[1] Every aspiring young navigator should make himself acquainted with spherical trigonometry, especially with "Napier's Analogies," which combine the brevity of short methods and special tables with the accuracy of pure mathematics. He should also accustom himself to drawing the figures for his problems till he can see the triangle in his mind's eye without a diagram.

Finally, it is better to get several observations of different bodies at (or about) the same time, than two of the same, with the requisite interval for change of bearing, because one of these observations has to be reduced to what it would have been if taken at the same place as the other, and the reduction may be vitiated by errors of the run, as explained in the paper on "Navigation," which it is one of the great objects of nautical astronomy to detect and be independent of. J. F. RUTHVEN.

THE LONDON UNIVERSITY BILL.

ALL friends of scientific and educational progress will be glad that the second reading of the London University Statutory Commission Bill was carried in the House of Commons on Tuesday without a division, and has been referred to the Standing Committee on Law. We are thus brought within sight of a long-delayed and much-needed reform, and all who have assisted in educating public opinion upon the measure, with the object of removing the unreasonable obstruction placed in its way, may congratulate themselves upon the success which their efforts have at last achieved. It is not to the credit of Ministers that a scheme of such deep importance to the best interests of the country should have been permitted to languish for so long a period, seeing that the necessity for establishing a teaching university in the metropolis is admitted by practically all public bodies connected with science and higher education in London. Had they possessed the courage of their convictions the measure would have passed into law without difficulty in 1896 or 1897, and its withdrawal upon each occasion must be counted as a lost opportunity. The opposition which then threatened the scheme would doubtless have collapsed so completely as it did on Tuesday, when it received so little support that the measure was agreed to even without a division. We reprint from the *Times* some parts of the speech made by Sir John Gorst in moving the second reading, and of the speeches which followed.

Sir John Gorst commenced by giving a general history of the scheme for a teaching University, and pointed out that the present Bill is based on the report of the Cowper Commission, which unanimously recommended that there should be no second University in London, and that the necessary modification of the constitution of the London University should be effected by means of a statutory Commission. He continued: "I should like to inform the House of the various bodies by which this scheme has been considered and accepted. It has, first of all, been accepted by the Senate of the University of London by a majority of 22 to 2—practically a unanimous acceptance by the Senate of the University of London. It has been accepted by the Royal College of Physicians, by the Royal College of Surgeons, by the Society of Apothecaries, by University College, by King's College, by the Bedford College for Women, by the twelve medical schools which exist in London, by six theological colleges, by the Society for the Extension of University Teaching, by the Technical Education Committee of the London County Council, by the Corporation of the City of London, by the City and Guilds Institute, by the Polytechnic Council, by the Royal Society, and all the other learned societies in London; and, finally, it has been accepted by the Convocation of the University. I say it has been accepted by the Convocation of the University of London because, by the charter of the University, a particular mode is specified in which the Convocation of the University of London shall express its opinion on the subject. The Convocation expresses its opinion by a meeting at which discussion takes place, and at which a vote is given by the persons there present. Such a meeting of Convocation has been held, and this present scheme has been approved in that legal and formal manner in which the charter of the University requires the opinion of Convocation to be expressed—by a majority of 460 to 239."

Referring to the views of graduates as shown by voting papers, Sir John Gorst said, "Even assuming that the existing graduates of the University of London were unanimous in their objection to the present scheme, I do not know why the personal feelings of London graduates should stand in the way of a great national reform—of a national development of higher education—when in the scheme, as I shall presently show, their rights and interests, such as they are, are most carefully and most securely preserved. There is a further objection brought forward which we shall no doubt hear of from the right hon. baronet, the member for the University of London, and that is a claim that the Convocation of London should have a veto upon any scheme which Parliament may enact for the purpose of developing the University of London. That claim is based upon Article 21 of the charter, which says that if a new or supplemental charter is given by the Crown to the University of London, the power of accepting it shall be exercised by the Convocation of the University. The answer to that is, first of all, that this is a restriction which applies to the charter and not to the action of this House. The Crown may very properly restrain its own power of granting any further charter, but it cannot restrain the power of the Houses of Parliament."

Sir John Gorst proceeded to point out how carefully the objections and fears of those who are opposed to this Bill have been met in the scheme which has been laid before Parliament. He said :—

"I am informed that there is a genera agreement among learned and scientific men, not only in this country, but in the whole of the civilised world, that in the highest parts of progressive science the attainments of students cannot be tested unless the teachers have some voice in setting the subjects of examination. That being the danger to be guarded against, the Bill appoints seven Commissioners by whom the statutes of the new University are to be framed. The Commissioners are Lord Davey; the Bishop of London; Sir William Roberts, a medical doctor and a Fellow of the University of London; Sir Owen Roberts, who is well known as having taken an active part in the spread of modern education; my hon. colleague the senior member for Cambridge University (Prof. Jebb); Michael Foster; and Edward Henry Busk, chairman of Convocation of the University of London. These Commissioners are constituted to frame the statutes for the purpose of carrying out the general scheme of the commission—that is, to so modify the existing University of London that it may fulfil the functions of a teaching University. I think the House may very well trust men like those I have named to frame statutes that will be in accordance with the best interests of education."

"The Government recommend this Bill to the House. It is not their scheme; it is a scheme which is the result of very long controversy and of a great deal of compromise, of give-and-take on the part of the various bodies, and they think it is a satisfactory conclusion of a very long discussed question. It will give a teaching University to London in the only way in which it can be given—namely, by the modification of the constitution of the existing University, and, in doing this, so far from injuring the existing University, it will increase its utility and its reputation."

Mr. Harwood moved an amendment for the rejection of the Bill, and Mr. Yoxall seconded it, but their views received little sympathy.

In speaking against the bill, Sir John Lubbock said those who had opposed the Bill had done so on four main grounds: first, that the result might be to imperil the position of science; secondly, that it might put the country colleges and private students at a disadvantage as compared with the candidates from London colleges; thirdly, that it might tend to lower the standard of the degrees; and, fourthly, that it took away the right at present possessed by his constituents to veto any change which in their judgment would interfere with the great work being carried on in the University. His objections were fully answered by Mr. Bryce, who, in the course of his remarks not only reminded his right hon. friend that Convocation had approved of the scheme, but also said that he should deny that Convocation had any more moral right than legal right to say what should be done with the University of London. He appealed to hon. members present who knew something both of the University of Oxford and of Cambridge, and he did not hesitate to say that the reforms which were passed some forty years ago with the greatest possible benefit and advantage both to the country and those Universities would never have been passed at all if the decision had rested with Convocation. His right hon. friend had set up, on behalf of the London University, a claim which was never listened to for a moment in that House in the case of the ancient Universities of Oxford and Cambridge. He submitted that they were not injuring the existing graduates. They were going to make the University a far more powerful and dignified body, and, incidentally, to enhance the value of her degrees. On a view of the whole matter it could not be shown that any injury at all would be done to the existing University. The work of teaching was incomparably more important than the work of examining. Much superstition attached to the degree; it was not so important as many people were inclined to believe; its value was as a test of teaching and stimulus to study, and the more it was made subordinate to teaching the better for education. For a long time the Bill had been wanted, for many schemes had been tried and had failed, and this scheme had received almost unanimous support from the teaching bodies and the approval of leading scientific men anxious to have a teaching University in London. He could not conceive that there was any foundation for the fear that science teaching or science examination would suffer; that was the last danger into which the new Senate would be likely to fall. All who had the well-being of University teaching at heart, who desired the extension of technical education with better facilities for the humbler classes of the community should unite in support of the scheme, which was approved by both political parties, and he earnestly hoped the House would accept it.

After other speeches the amendment was by leave withdrawn, and the Bill was read a second time, and referred to the Standing Committee on Law.

THE ART AND SCIENCE BUILDINGS AT SOUTH KENSINGTON.

THE agitation against the new departure of the Government in relation to the proposed extensions of the Science and Art Buildings at South Kensington grows apace.

Following upon the Report of the Select Committee of the House of Commons, and the Memorial addressed to Lord Salisbury by the President and Council and many Fellows of the Royal Society, comes still another Memorial, this time from the Royal Academy, and already signed by the President and Council and many members of the Royal Academy, with other representatives of Art, strongly urging that the policy stated in 1890 should be adhered to.

The Royal Academy memorial runs as follows :—

Memorial to the Most Honourable the Marquis of Salisbury, K.G., F.R.S., Premier and Secretary of State for Foreign Affairs.

Whereas in 1890 Parliament voted 100,000*l.* for the purchase of a site at South Kensington upon which to erect suitable buildings for the Science Museum of the Department of Science and Art, and for the extension of its science schools, in accordance with the recommendations of the Royal Commission, over which the Duke of Devonshire presided in 1874, as well as of various committees and other high scientific authorities, and of a Treasury committee appointed in 1889.

And whereas when in 1891 the Government had proposed to erect an art gallery on the site, a memorial, signed by the president and officers of the Royal Society and representatives of the Universities of Oxford, Cambridge, and of many other learned bodies both in London and in the provinces, was addressed to your lordship, showing cause why the site should not thus be allocated.

And whereas the scheme was withdrawn, and it was stated by the late Right Hon. W. H. Smith, M.P., in the House of Commons on April 16, 1891, that the "Government has at disposal more than three acres of vacant land facing the Imperial Institute, and considerable areas beyond to the south of the present Southern Galleries. A portion of these vacant lands can be utilised for the extension of the College of Science and for the future growth of the science collections. Additions to the College of Science must, in any case, take the form of a separate building divided from the present building by Exhibition Road"; while the Chancellor of the Exchequer, the Right Hon. G. J. Goschen, informed the deputation which waited on the Lord President of the Council in May 1891, that "we hope to bring science into one centre fronting the Imperial Institute."

And whereas this arrangement which left the ground on the east of Exhibition Road for the extension of the Art Museum has been generally accepted since 1876, when the Royal Commission for the Exhibition of 1851 offered land and a building with a view of carrying out the recommendations of the Duke of Devonshire's Commission in 1874 to provide the needed accommodation for science at South Kensington.

And whereas we are informed that this arrangement is in danger of being altered by the erection of science buildings on the east side of Exhibition Road.

We, the undersigned members of the Royal Academy and others practising various branches of the arts as a profession, desire most respectfully to express to your lordship our strong opinion that it is desirable to adhere to the former policy, which has been acted upon and publicly acknowledged by the Government since 1890, considering the urgent need of much additional space even for the present art collections of the South Kensington Museum, and the necessity for making some provision for their proper development, we are convinced that any attempt to provide on the east side of Exhibition Road for the necessary expansion of the science buildings will render it impossible to meet the future requirements of the industrial arts, for the promotion of which the South Kensington Museum was founded. We also feel that in praying your Lordship to reserve for art that portion of the land which still remains vacant on the east of Exhibition Road, we are not making an exorbitant demand. The whole plot of ground belonging to the Government on that side is much smaller than that devoted to the Natural History Museum, which only represents one branch of science without either teaching or applications, while the space on the east of Exhibition Road has to provide not only for the Art Museum, but also for the administrative offices of the Department of Science and Art, the Royal College of Art, and part of the Royal College of Science.

We hope to be able to give the full list of signatures next week.

NOTES.

AT the annual meeting of the Royal Society for the election of Fellows, held on Thursday last, the following were elected into the Society:—Mr. H. F. Baker, Prof. E. W. Brown, Dr. Alexander Buchan, Mr. S. F. Harmer, Mr. Arthur Lister, Lieut.-General C. A. McMahon, Prof. W. Osler, Hon. C. A. Parsons, Prof. Thomas Preston, Prof. E. Waymouth Reid, Mr. Alexander Scott, Mr. A. C. Seward, Mr. W. A. Shenstone, Mr. H. M. Taylor, and Mr. James Wimshurst. The certificates of these new Fellows, setting forth the scientific work accomplished by each, were reprinted in NATURE of May 12.

THE ladies' conversazione of the Royal Society was held on Wednesday in last week, and was attended by a large and brilliant assembly. Most of the objects and experiments which were shown at the conversazione were exhibited at the soirée held at the beginning of May; and as these have already been described in NATURE (p. 61), it is unnecessary to refer to them again. The exhibit which attracted the greatest amount of attention was the spectrum of krypton, the new constituent of atmospheric air, discovered by Prof. Ramsay and Mr. Travers.

PROF. H. A. LORENTZ, of Leyden, and M. Émile Picard, of Paris, have been elected, by the London Mathematical Society, honorary foreign members, in succession to the late Profs. Brioschi and Hertz.

WHEN Hutton published the two volumes of his famous "Theory of the Earth," in 1795, he left a third in manuscript, which was declared by his friend and biographer, Playfair, to be necessary for the completion of the subject. Yet this important contribution to science has not only never been published, but seems to have almost passed out of mind. Sir Archibald Geikie last year set inquiries on foot with the view of trying to trace the lost manuscript. A portion of the volume, comprising Chapters iv. to ix., came into the possession of Leonard Horner, who eventually presented it to the library of the Geological Society of London, where it has remained since 1856. But every effort to discover the rest of the work has hitherto failed. At Sir Archibald's request, the Society has agreed to publish the six chapters in its possession, each of which is complete in itself; and he is now engaged in preparing the work for the press. The chapters contain some interesting narratives of Hutton's journeys in Scotland in search of illustrations of his theory. In particular, they include his account of the celebrated visit to Glen Tilt, where he found the granite veins which filled him with such exuberant delight that his guides were convinced he must have discovered a vein of silver or gold. They contain also an account of an expedition into Galloway, and a remarkably full description of the geology of the island of Arran. The volume will be interesting to geologists as a continuation of one of the great classics of their science.

As the two last nominations of foreign knights of the Prussian Order *pour le mérite* have fallen to British subjects, it may be of interest to give a list of the existing members. The Order received its French title from its founder, Frederick the Great, who, as is well known, had a partiality for that language. It was at first given for military services only, but its statutes were remodelled in 1842 by King Frederick William IV., and the class "für Wissenschaften und Künste" was instituted. The German knights of this class, with whom the election into the Order practically rests, are limited to thirty in number, and at present are: A. Menzel, Chancellor; T. Mommsen, Vice-Chancellor; the other members in the order of election being, in the Section of Science: R. W. Bunsen, Max Müller, E. Zeller, T. Noeldeke, J. V. du Vernois, A. Auwers, E. Pflüger, H. Vogel, A. v. Baeyer, O. Fürst v. Bismarck, F. Kohlrausch, H. Grimm, H. Brunner, A. v. Kölliker, H. Usener, W. Hittorf, A. Weber, C. Neumann and Schwendener. In the Section of Art: L. Knaus, A. Achenbach, J. Schilling, R. Begas, F. Schaper, E. v. Gebhardt, H. Ende and A. Hildebrand. The foreign knights, limited to the same number, are, in the Section of Science: O. v. Boethlingk, C. Hermite, Sir G. G. Stokes, N. A. E. v. Nordenskjöld, M. Berthelot, O. v. Struve, Lord Kelvin, Lord Lister, V. Jagic, P. Villari, H. Kern, J. G. Agardh, M. J. de Goeje, G. V. Schiaparelli, F. Imhoof-Blumer, J. H. van 't Hoff, A. O. Kowalevsky, W. Stubbs (Bishop of Oxford), O. Montelius, Sir John Murray and Sir W. H. Flower. In the Section of Art: L. Alma Tadema, G. Verdi, G. Monteverde, E. Wauters, L. Passini and F. Pradilla.

A SPECIAL meeting of the Royal Geographical Society will be held on Monday, June 27, at 4.30 p.m., when Prof. Elisée Reclus will describe his plans for the construction and erection of a great terrestrial globe on the scale of 1 : 500,000 (8 miles to an inch). The president, Sir Clements R. Markham, K.C.B., F.R.S., will occupy the chair. The subject is one which will interest both geographers and engineers.

THE Royal Commission for the Paris Exhibition of 1900 are now prepared to circulate information respecting the exhibition. The classification and rules for exhibitors, together with forms of application for space, can be obtained by applying to the Secretary of the Royal Commission, Paris Exhibition 1900, St. Stephen's House, Westminster, S.W.

IN connection with the seventieth meeting of the Society of German Naturalists and Physicians, to be held at Düsseldorf in September, a series of exhibitions of scientific apparatus and objects has been arranged. An exhibition of objects illustrating the history of medicine and science will be open from July to the end of September. An exhibition of apparatus and photographs illustrating scientific applications of photography will commence in August and continue open until the end of September. New instruments and apparatus will be exhibited from September 17 to September 28, and prizes will be awarded for the best of them. Any machine, apparatus, preparation, or object invented since 1888 may be entered for this exhibition. Objects illustrating methods of instruction in physics and chemistry will be exhibited from September 17 to September 25. Communications referring to the exhibitions should be addressed, Herrn Director Frauberger, Düsseldorf, Friedrichsplatz 3/5.

THE sixty-sixth annual meeting of the British Medical Association will be held at Edinburgh on July 26-29, under the presidency of Sir T. Grainger Stewart. A detailed statement of the arrangements which have been made for the meeting appears in the *British Medical Journal.* An address in medicine will be delivered by Prof. T. R. Fraser, F.R.S. ; an address in surgery will be delivered by Prof. Thomas Annandale ; and an address in psychological medicine will be delivered by Sir John Batty Tuke. The programme of business arranged by the officers of the sixteen sections is long and varied. In addition to the sections in which the business of the annual meeting is ordinarily carried on, there are for the first time this year sections devoted to medicine in relation to life assurance and to tropical diseases, two departments which have grown into positions of great practical importance during the present generation. A considerable number of distinguished members of the medical profession resident in America and the Continent of Europe have accepted invitations to take part in the proceedings.

THERE are at Prague two distinct botanical gardens, one belonging to the German, the other to the Bohemian University. The former is now under the direction of Prof. R. v. Wettstein, the latter under that of Prof. L. Celakovsky.

THE Rev. Arthur C. Waghorne, Bay of Islands, Newfoundland, for nearly twenty-five years a missionary in Newfoundland, offers for sale collections of Labrador and Newfoundland plants, both flowering and flowerless, named by competent authorities.

WE learn from the *Oesterrheichische Botanische Zeitschrift* that M. Philippe Plantamour-Prévost has bequeathed his villa "Mon repos," on the shore of the lake, to the city of Geneva, for the reception of Delessert's herbarium, and for the botanic garden founded by A. P. de Candolle.

IN a note in the *Kew Bulletin*, No. 135, for March 1898, reference is made to the probable success of a process for the artificial manufacture of indigo on a large scale. The Badische Anilin und Soda Fabrik, Ludwigshafen, is now manufacturing "indigo-blue" at a price which very seriously threatens the prosperity of the culture of indigo in India.

ACCORDING to the *Botanical Gazette*, the coming meeting of the American Association for the Advancement of Science at Boston promises to be one of the most notable in the history of the Association. It is the fiftieth anniversary, and special efforts are being made to arrange a worthy celebration. The local committees have been appointed, and the week selected is August 22-27. The local secretary is Prof. H. W. Tyler, of the Massachusetts Institute of Technology.

PROF. JOHN W. HARSHBERGER, of the University of Philadelphia, pleads, in the *Botanical Gazette*, for the establishment of a tropical botanical station in Mexico. The locality especially advocated is a station called Las Canoas, on the Mexican Central Railroad, 144 miles from Tampico. Las Canoas is situated in a beautiful basin-shaped valley 3500 feet above the sea-level. There is an abundant supply of pure water, and the air is clear and bracing. The vegetation is described as of great luxuriance, and the flora is remarkably varied and beautiful. A temporary station could be established here with very little expense, and the virgin forest would supply enough botanical material for years to come.

AN important investigation in connection with mortality is being carried out jointly by the Institute of Actuaries and the Faculty of Actuaries, under the superintendence of Mr. T. G. Ackland, who now has a staff of thirty clerks constantly at work upon a large body of cards containing statistics supplied by assurance companies. The whole of the data relating to the experience in respect of annuitants have been dealt with, and the tables are now in the press. In response to applications made by the Presidents of the Institute and the Faculty, life assurance offices have undertaken to contribute liberally towards the cost of the investigation, which will necessarily be very heavy. The contributions of the companies at present promised or received amount to 10,953*l.*, which sum, it is hoped, will cover the larger portion of the expense, and thus relieve the Institute and the Faculty from any anxiety as to their ability to carry to a satisfactory conclusion this valuable investigation.

FROM a report before us we see that last year was an eventful one in the history of the New York Zoological Society, and it ended in the establishment of the Society as a permanent institution for the promotion of zoological knowledge. All the original objects have been furthered, and noteworthy results have been obtained. The proposal by the Society that 261 acres of land in South Bronx Park should be set apart as the site of the New York Zoological Park, has been unanimously adopted by the Commissioners of the Sinking Fund. The general plan of the Park has been completed and approved by the Park Commissioners. The collections and animal buildings, to cost not less than 250,000 dollars, are to be presented to the City by the Society; and the City is to prepare the ground for occupancy, and to maintain the Zoological Park when established. The sum of 100,000 dollars has been subscribed towards the gift from the Society to the City. This was the amount which had to be raised before the plans could be proceeded with, and work could not be commenced until it was subscribed. Since March 15, 1897, the membership of the Society has increased from 118 to 600 ; but in order to carry out the plans on a scale worthy of New York, the Society should enroll at least 3000 annual members. The Society has decided to systematically foster both the painting and sculpture of animals ; and, with the idea of establishing a school of animal painting and sculpture, provisions for studios have been made in the plans of several of the buildings.

WE are glad to learn from the sixth annual report of the Sonnblick Society for the year 1898 that several improvements have recently been made in the arrangements of this important mountain station, and that the various observations and experiments are carried on with vigour. The meteorological observatory at the summit has now been quite separated from the visitors' refuge which existed in the same building, and a well-equipped station has also been established at the foot of the mountain, at which comparative observations will be regularly made, and will render those at the summit of higher value. These elevated stations are of much scientific interest in connection with the frequent ascents by manned and unmanned balloons for the purpose of investigating the higher regions of the air.

WE have received from Mr. N. A. F. Moos, the Director of the Bombay Observatory, his report to the Secretary to the Indian Government for the year ending March 31, 1898. This observatory is devoted chiefly to terrestrial magnetism and meteorology, astronomical observations being restricted solely to time observations. All the magnetographs have been in constant action throughout the past twelve months. On June 12 the traces clearly showed the small vibration due to the earthquake on that day ; and on September 21, at 10h. 40m., small disturbance noticed in the horizontal force curve was traced to the earthquake in Borneo. The statement showing the extent to which the various observations have been reduced, and the reductions checked, indicates that these keep good pace with the observations themselves, nearly everything being checked to either February or March of this year.

THE following remarks from a lecture on the aims and methods of pharmacology, recently delivered at Oxford by Dr. W. J. Smith Jerome, and published in the *Lancet*, will interest many scientific investigators:—"Another method by which pharmacological knowledge is to be obtained is that which is generally understood as research. This, I think, is an ideal form of work, and the leisure and acquirements needed for it are, in my opinion, well worth striving after. A laboratory, it is true, may not be an attractive object. It is not usually gratifying to the æsthetic sense; there are apt to be too many and too obvious manifestations of matter apparently in the wrong place, but it possesses, or at least should possess, one of the fundamental attributes of beauty—viz. a fitness for the purpose it is intended to subserve; and if in itself not beautiful, it enshrines what is *par excellence* 'a thing of beauty and a joy for ever.' It enshrines, it is pervaded by, the spirit of truth—truth which serves both as a lamp to illumine and as a beacon to direct, and yet which shines with a pure and steady ray on those alone who seek to follow it in singleness of purpose. The work performed accords most aptly with Matthew Arnold's description of the work of nature. 'Toil unsevered from tranquillity, . . . Labour that in lasting fruit outgrows far noisier schemes, accomplished in repose, too great for haste, too high for rivalry.' And though it must be granted that the methods of the laboratory, like those of nature, are occasionally harsh, it must also be conceded that its results are useful and its aims beneficent. But even into this paradise of toil there enters or may enter one insidious sin—the lust of what is called 'priority.' This must be fought against and overcome, or else, like a gathering cloud, it will, if left unchecked, roll onwards and most surely darken all. And why should it not be fought against and overcome? Each fact discovered in the pursuit of knowledge, discovered it matters not by whom or when, and even when unimportant in itself, may prove a stepping-stone by which that knowledge mounts to other and far higher things. This is the worker's real recompense; it is this pregnant possibility which makes work, honest work, like virtue, its own great reward."

THE current number of the *Annales de l'Institut Pasteur* contains an account, by Dr. Sanarelli, of the preliminary results he has obtained in the use of antitoxic serum in cases of yellow fever. It will be remembered that Dr. Sanarelli was the first to isolate the specific bacillus of yellow fever, and he has since been endeavouring to procure through its agency an efficient antitoxin. Great difficulties have been experienced in rendering animals satisfactorily immune to infection, and it takes from twelve to fourteen months' treatment before a horse can be regarded as vaccinated. Dogs, which have undergone a series of inoculations during a year or more, and are ultimately able to withstand a large dose of the toxin, are still very adversely affected by each fresh inoculation of the virus. So far this anti-yellow-fever serum appears to exert a protective action against yellow-fever microbes, but not against their toxins, and in the present state of the investigations good results can apparently only be hoped for when the serum is employed at a very early period after infection, or as a precautionary measure to ward off the disease; in this latter respect, Sanarelli has obtained some highly encouraging results. The Government of the province of Saint Paul in Brazil have now decided to establish an institute for promoting the further study of the serotherapy of yellow fever, and it is hoped that before long the elaboration of a specific treatment, both curative and preventive, will succeed in banishing a disease which is with justice looked upon as the scourge of the American continent.

THE *Klinisches Jahrbuch*, published by Gustav Fischer of Jena, contains in its last number the report drawn up by Messrs. Kirchner and Kübler on leprosy in Russia. These gentlemen were deputed by the German Government to conduct this inquiry, and made a careful tour of inspection through the Russian eastern provinces right up to St. Petersburg. It is very difficult to obtain an accurate estimate of the number of cases of leprosy in Russia, as compulsory notification of the disease has only been recently introduced, but it is stated to be about 5000. Of late years great energy has been displayed in endeavouring to prevent the spread of infection. Numerous leprosy isolation hospitals have been established, and many of these were visited by the inspectors. They call attention to the fact that the majority of these leprosy establishments have been founded not by the Russian Government, but by the great landed proprietors in the district, and that private munificence helps largely in dealing with cases. The authors express decidedly their firm conviction of the contagious character of the disease, and state that the only hope of stamping it out is to establish institutes for the isolation and treatment of its victims.

MUCH attention has been paid in Italy during the last few years to the pulsations of distant earthquakes, and to the best means of recording them. In a valuable paper contributed to the *Bollettino* of the Italian Seismological Society (vol. iii. No. 9), Prof. Grablovitz compares the different types of instruments now in use for their registration. He deprecates the recommendation of an instrument for universal employment as premature, and as discouraging the improvement of other apparatus. Nevertheless he attempts to clear the ground so far as regards the mode of registration, preferring the mechanical methods used in Italy to the photographic methods used in Germany and England, on account of their comparative cheapness and the greater velocity that can be given to the moving paper. On this last point he lays special stress, as it gives a clearer diagram and enables the time of the different phases to be determined with greater accuracy.

IN the same journal, Dr. Cancani illustrates the value of these remarks by describing the horizontal pendulums recently erected by him at the Observatory of Rocca di Papa, near Rome. These are similar in principle to the instrument employed by von Rebeur-Paschwitz, but are much larger, the distance of the tip of the recording pen from the vertical through the upper fulcrum being 2·70 metres. Each pendulum carries a mass of 25 kg. and has a period of oscillation of 12 seconds. The record is made on a strip of paper which passes under the pens at the rate of 60 cm. an hour. A tilt of one second at right angles to the plane of the pendulum deflects the pens 2 mm. The interesting records of the Calcutta earthquake given by these pendulums is reproduced (on half the natural scale) in NATURE, vol. lvi. p. 346.

THOUGH fishing is carried on at most of the villages and towns around the coast of Jamaica, the amount of fish obtained is far from sufficient to supply the needs of the population of the island. It has long been surmised, however, that the industry is capable of considerable extension, that the waters are teeming with suitable fish, and that with improved modern methods, such as steam-trawling, sufficient fish might be obtained to render the fresh supply more adequate to the needs of the inhabitants, and that native cured fish should in a large measure take the place of the imported article. With this in mind, the Carribbean Sea Fisheries Development Syndicate was formed last year in England, and a steam trawler was chartered to test the possibility of increasing the fishing industry. The operations and results are described by Mr. J. E. Duerden, Curator of the Jamaica Museum, in the *Daily Gleaner* of April 16, and from them it appears that the endeavour to establish a fishery industry in Jamaican waters on the large scale attempted will not meet with success; firstly, on account of the coral nature of the greater part of the sea-floor rendering the use of a trawler impossible; secondly, and more important, because of a general scarcity of fish. It is a curious fact that fish from deep water, on being brought to the surface, are nearly always so distorted by the expansion of the gases within them as to be rendered useless for market purposes. With regard to the scientific results of the experiments, an abundance of material other than fish was obtained, some of which has been presented to the Museum of the Institute of Jamaica, and is briefly described by Mr. Duerden. Perhaps the most remarkable feature of the hauls from a depth of about ten fathoms is the variety, abundance, and size of the sponges. A large, black, massive, almost spherical form occurred in great quantity; specimens 5½ feet round and 20 inches high were often dredged. The small pores were thickly inhabited by a small species of the Crustacean *Alphæus*. Special interest attaches to the re-discovery of the peculiar West Indian genus *Bergia*, concerning the exact scientific position of which there is much doubt. The corals met with in greatest abundance by the trawl were the various species of *Madrepora*. Sometimes large pieces would be brought up, but usually only the small more fragile branches remained entangled in the net. A few other species of corals not obtainable from shallow water were also secured.

THE South-eastern Union of Scientific Societies recently held its third annual congress in Croydon, the Mayor and Corporation having placed the Town Hall at the service of the union for the purpose. The aim of the union is "to win for science such benefits as are found to accrue in manufactures from division of labour; and in trade, commerce, and finance from co-operation." No attempt, however, is made to secure uniformity among the thirty-one societies affiliated to the union. Last year's president of the union was the Rev. T. R. R. Stebbing, F.R.S., and the president-elect, who opened the congress, was Prof. G. S. Boulger. In his presidential address Prof. Boulger directed attention to the position of natural history in this country sixty years ago, with special reference to the character of field work and its organisation; contrasted that position and that character with those of our present-day geology and biology; traced briefly the cause of the difference, and suggested some lines along which future energies should be directed. The address was very appropriate to the occasion, and an instructive statement of the great change which the Darwinian theory had produced in scientific thought. The programme of the congress included papers by Mr. E. Lovett, on "The Folk-lore of Amulets and Charms"; Dr. H. Franklin Parsons, on "The nature of the soil in connection with the distribution of Plants and Animals"; "Entomology as a Scientific Pursuit," by Mr. J. W. Tutt; "Ancient and Modern Dene Holes and their Makers," and "Natural Gas in Sussex," by Mr. C. Dawson; "Place of Geology in Education," by Prof. Lobley; and "Photography in relation to Science," by Mr. J. H. Baldock. There was also a discussion of "Ideals for Natural History Societies, and how to attain them." The meeting was well attended, and should result in increased interest being taken in the study of nature.

THE fifth volume of the elaborate "System of Medicine," edited by Prof. Clifford Allbutt, F.R.S., has just been published by Messrs. Macmillan and Co., Ltd. The contents refer to diseases of the respiratory organs, of the pleura, and of the circulatory system.

AN interesting address upon "Light and Fire Making," delivered by Mr. Henry C. Mercer, has been issued by the Bucks County Historical Society, Doylestown, Pennsylvania. The address contains forty-five illustrations explaining the methods of producing fire by friction of wood, and by striking flint and steel; they also show some of the forms of lamps, candles, torches, and lanterns used in America and elsewhere.

AMONG handy reference volumes must be placed the "Yearbook of Scientific and Learned Societies," published by Messrs. Charles Griffin and Co., Ltd. The new volume contains particulars with regard to the constitution and membership of scientific societies in Great Britain and Ireland, lists of papers read during 1897 before societies engaged in fourteen departments of research, and a good index.

THE fourth edition of Prof. Wiedersheim's "Grundriss der vergleichenden Anatomie der Wirbelthiere" has just been published by the firm of Gustav Fischer, Jena. Since the appearance of the third edition five years have passed, and so much new work in morphology has been done in this period that the book has had to undergo complete revision. Not only has the new material been assimilated, but various changes have been made in the typography, and all references to authors have been placed in the excellent bibliography appended to the volume. Dr. Wiedersheim mentions that the second English edition of his work, adapted from the German by Prof. W. N. Parker, was prepared under his guidance, and the new material in the present German edition was taken into consideration.—The third revised edition of the attractive and exact "Lehrbuch der Botanik für Hochschulen," by Drs. Strasburger, Noll, Schenck, and Schimper has been published by Gustav Fischer. The first edition was published only four years ago, and the fact that three editions have now appeared is a testimony to its value and popularity. Botanists who have a difficulty in reading the German text will be glad to see the English translation which Messrs. Macmillan have lately published.—A large number of questions referring to heredity are discussed in the work entitled "La Famille Névropathique," by M. Ch. Féré, the second edition of which has been published by M. Félix Alcan, Paris. The volume brings together much information on the laws of inheritance in relation to disease, and the numerous references it contains will be found very valuable by students of heredity.—A second edition of a "Syllabus der Pflanzenfamilien," by Dr. Adolf Engler, has been published by the firm of Borntraeger, Berlin. The volume contains brief notes on medicinal and useful plants, and is intended more particularly for use by students of special and pharmaceutical botany.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Miss Nellie Biggs; a Dorsal Squirrel (*Sciurus hypopyrrhus*) from Central America, presented by Miss Trelawny; a Ring-necked Pheasant (*Phasianus torquatus*) from China, presented by Dr. C. Danford Thomas; a Pin-tailed Whydah Bird (*Vidua principalis*) from Africa, presented by Madame Caté; an Eyed Lizard (*Lacerta ocellata*), European, presented by Mr. H. F. Witherby; an Indranee Owl (*Syrnium indranee*) from Ceylon, a Florida Tortoise (*Testudo polyphemus*) from North America, deposited; four Wonga-Wonga Pigeons (*Leucosarcia picata*) from New South Wales, a Naked-throated Bell-bird (*Chasmorhynchus nudicollis*) from Brazil, a Burrowing Owl (*Speotyto cunicularia*) from South America, two Purplish Guans (*Penelope purpurascens*) from Central America, a Sarus Crane (*Grus antigone*) from Northern India, a Four-lined Snake (*Coluber quatuorlineatus*), European; an Angulated Snake *Leptodira annulata*) from Tropical South America, four Azarás (Opossums (*Didelphys azaræ*) from La Plata, purchased.

OUR ASTRONOMICAL COLUMN.

ENCKE'S COMET.—Of the three comets which are due to return this year—namely Encke's, Winnecke's and Wolf's, having periods of 3⅓, 5½, and nearly 7 years respectively—the first seems to have just been found, according to a Kiel telegram dated June 14. Prof. Hussey, telegraphing to Kiel, states that Mr. Coddington, on June 11, 9h. 13·1m. Lick mean time, found a comet, which he terms bright, in position R.A. 16h. 24m. 45·9s. and Declination (south) 25° 14′ 20″, the daily motions in these coordinates being 51′ and 36′ respectively. The comet thus lies in the constellation of Scorpio, a little to the north of the bright star α. A further telegram from Mr. John Tebbutt, dated June 14, states that this observer found the comet on June 12 in position R.A. 16h. 21m. and Declination (south) 25° 52′ at 9h. 22·9m. Lick mean time.

Much interest is attached to this comet, since its period is one of the shortest known. It was first seen in 1818 by that diligent observer Pons, on November 26, perihelion being passed in the following January. It was Encke, however, who undertook to investigate its motions, proving that its period extended over 3⅓ years, and he predicted its return in 1822. At every succeeding return the comet has been observed, and it was even discovered that prior to 1818 it had been three times observed by Méchain, Caroline Herschel, and Thulis in the years 1786, 1795 and 1805. At its last return, in 1895, it was just visible to the naked eye at the time of its maximum brightness.

NEW DETERMINATION OF THE EARTH'S DENSITY.—Herr F. K. Ginzel, in the current number of *Himmel und Erde* (June, Heft 9), describes a new determination of the mean density of the earth by Dr. C. Braun, a former director of the observatory at Kalosca in Hungary. The apparatus used for this purpose was a torsion balance constructed by Dr. Braun himself, and from the description we learn that, excepting the suspension wires, glass globe, chronometer, chronograph, microscopes, and a few small parts, everything was made by himself. The method employed differed mainly from previous determinations in that the torsion balance was enclosed in a glass globe from which all air had been extracted. So complete was the vacuum that after four years no change could be detected. We leave our readers to gather from the above-mentioned source more details regarding the apparatus itself. The observations were begun in the year 1892, and two years later the computations were commenced. After all allowance had been made for corrections the final result gave for the value of the mean density of the earth 5·52765, which nearly corresponds with the best determination made by Prof. Boys. Herr Ginzel, in concluding his article, tells us that, disregarding the very high scientific importance that will be attached to this new determination, if we consider that Dr. Braun is considerably advanced in years, somewhat hard of hearing, and has not been blessed with good health during the last few years, and that he has been thinking over this problem for eleven years in addition to his usual official duties, all will agree in saying that this work is a rare proof of the scientific energy and ideal power of sacrifice for one man.

THE LARGE REFRACTORS OF THE WORLD.—The question of the efficiency of refractors of large apertures has recently been discussed in many articles, and the latest we owe to Prof. G. E. Hale, who deals in *Science* (May 13) with the frequently asked question, "Do large telescopes pay?" Prof. Hale points out the special kind of work to which refractors of large aperture should be employed, and shows that when used by a skilled observer very important work can be accomplished which would be impossible with a small aperture. An instrument, say, of forty inches aperture is more advantageous than one of ten inches, in that, first, it has the power of giving much brighter images, thus rendering faint stars visible. It can, secondly, give an image of a celestial body of measurable dimensions four times as large as that given by a lens of one-fourth its aperture and focal length; and, thirdly, its capacity of rendering visible, as separate objects, the components of very close double stars or minute markings upon the surface of a planet or satellite. Prof. Hale concludes that all the money and time and labour are well spent on refractors of large aperture, and he suggests that further sums might well be expended, particularly in the southern hemisphere, in the establishment of still more powerful instruments.

A propos of large refractors, a fairly complete list of existing large refracting telescopes appears in the current number of the *Observatory* (June), in which are given details concerning the aperture, focal length, location, maker, and date of erection of the various instruments. America comes first as regards the number of instruments and largest size of aperture, followed by France, England and Germany in the order respectively of the number of refractors exceeding 13·4 inches.

THE LEEDS ASTRONOMICAL SOCIETY.—It is always with pleasure that we refer to scientific societies for the promotion and extension of astronomical knowledge, when we know that they are doing useful work in this respect. The *Journal* (No. 5) of the Leeds Astronomical Society for the year 1897 is a good example of the interest displayed by its members in fostering astronomy, and during the past year many interesting papers were read at their meetings. Among these may be mentioned that on the nebular origin of our solar system, by Mr. Barbour, who refers to and extends the significant relationships between the distances and masses of the four superior planets suggested by Mr. Sutcliffe of Bombay, and previously referred to in this column (vol. lvi. p. 424). Other papers read had for their subjects the heat of the sun, the planet Venus, orientation of Egyptian temples, density of the earth, &c. This number contains also an excellent likeness of the retiring president, Mr. Washington Teasdale.

RECENT EXPERIMENTS ON CERTAIN OF THE CHEMICAL ELEMENTS IN RELATION TO HEAT.[1]

THE discovery that different substances have different capacities for heat is usually attributed to Irvine, but there can be no doubt that Black, Crawford, and others contributed to the establishment of the idea. The fact that equal weights of different substances in cooling down through the same number of degrees give out different amounts of heat, may be illustrated by the well-known experiment in which a cake of wax is penetrated with different degrees of rapidity by balls of different metals heated to the same temperature. But for the quantitative estimation of the different amounts of heat thus taken up and given out again, the physicist must resort to other forms of experiment, each of which presents difficulties of its own. Broadly speaking, three principal methods have been used in the past for the estimation of "specific heats." The first is based upon the observation of the exact change of temperature produced in a known mass of water by mixing with it a known weight of the substance previously at a definite temperature above or below that of the water. The second consists in determining the quantity of ice melted when the heated body is brought into contact with it in such a way that no heat from any other source can reach the ice. And the third method consists in observing the rate at which the heated body falls through a definite range of temperature when suspended in a vacuous space.

The process of intermixture with water was used by the earlier experimenters in the last century, and some of the best results extant have been obtained by this process, which, however, is not so easy as it appears when the highest degree of accuracy is desired.

Lavoisier and Laplace in 1780 devised the ice calorimeter which bears their name, and in a most interesting memoir, which is reprinted among Lavoisier's works, they show that they were familiar with the idea which in modern times is known as the principle of the conservation of energy. In this memoir they give the results of experiments in which the specific heats of iron, mercury, and a few other substances are estimated with a very tolerable approach to accuracy. Although many of the metals were known to them, it would not have been possible, had they persisted in this work, to make the discovery which was reserved for Dulong and Petit thirty-five years later, for the atomic theory had not been conceived and no atomic weights had been determined.

Dulong and Petit (*Ann. Chim.*, 1817, vii. p. 144) seem to have used at first the method of mixtures, and to have found by direct experiment that the specific heat of solids (metals and glass) increases with the temperature. They also studied (after Leslie) the laws of cooling of bodies; and two years after the publication of their first paper on the subject, they (Petit and Dulong, *sic*) arrived at the remarkable general expression which is associated with their names (*Ann. Chim.*, 1819, x. 395).

1 A discourse delivered at the Royal Institution, Friday evening, May 13, by Prof. W. A. Tilden, D.Sc., F.R.S.

After pointing out that all the results of previous experiments, except those of Lavoisier and Laplace, are extremely incorrect, they describe their results obtained by the method of cooling, conducted with many precautions to avoid error.

COPY OF TABLE BY DULONG AND PETIT (*Ann. Chim. Phys.*, 1819, x. 403).

Specific heats		Atomic weights (o = 1)	Atomic weight × specific heat
Bismuth ...	... ·0288	13·30	·3830
Lead ..	... ·0293	12·95	·3794
Gold ...	... ·0298	12·43	·3704
Platinum ...	... ·0314	11·16	·3740
Tin ...	... ·0514	7·35	·3779
Silver ...	... ·0557	6·75	·3759
Zinc ...	... ·0927	4·03	·3736
Tellurium	... ·0912	4·03	·3675
Copper ...	... ·0949	3·957	·3755
Nickel ...	... ·1035	3·69	·3819
Iron ...	... ·1100	3·392	·3731
Cobalt ...	... ·1498	2·46	·3685
Sulphur ...	... ·1880	2·011	·3780

The statement of the law is best given in the words of the authors (p. 405):

"Les atomes de tous les corps simples ont exactement la même capacité pour la chaleur."

Here the question rested till resumed, many years later (1840), by Regnault, who in his first memoir (*Ann. Chim.*, 73, 5) points out the difficulties which attended the acceptance of the statement of Petit and Dulong in the form in which they gave it. He then discusses the three principal experimental methods, viz. (1) fusion of ice, (2) mixture, (3) cooling, and decides in favour of the second, which he used throughout his researches. The general form of the apparatus used by the great physicist has been a model for the guidance of successive experimentalists since his time.

Another quarter of a century elapsed before the question of the specific heat of the elements was resumed by Hermann Kopp. His results were communicated to the Royal Society, and are embodied in a paper printed in the *Philosophical Transactions* for 1865. After reviewing the work of his predecessors, he describes a process by which he has made a large number of estimations of specific heat, not only of elements but of compounds of all kinds in the solid state. Concerning his own process, however, he remarks that "the method as I have used it has by no means the accuracy of that of Regnault" (p. 84).

In 1870 Bunsen introduced his well-known ice-calorimeter. This is an instrument in which the amount of ice melted by the heated body is not measured by collecting and weighing the water formed, but by observing the contraction which ensues when the ice melts, contained in a vessel of special form. The results obtained by Bunsen himself are uniformly slightly lower than those of Regnault for the same elements.

Since that time experiments have been made by Weber, Dewar, Humpidge, and others in connection especially with the influence of temperature in particular cases.

Setting aside the elements carbon, boron, silicon and beryllium, as providing an entirely separate problem, the question is whether the law of Dulong and Petit is strictly valid when applied to the metals. Kopp, in his discussion of the subject, came to the conclusion that it is not; but the grounds for this conclusion are unsatisfactory, since neither the atomic weights nor the specific heats were at that time known with sufficient accuracy.

It has been customary to assume that the divergencies from the constant value of the product, At. Wt. × Sp. Ht., are due partly to the fact that at the temperature at which specific heats are usually determined, the different elements stand in very different relations to their point of fusion; thus lead at the temperature of boiling water is much nearer to its melting-point than iron. It has also been attributed to temporary or allotropic conditions of the elements. As to the relation to melting-point, the specific heats of atomic weight seem to be practically the same in separate metals and alloys of the same which melt at far lower temperatures. For example, the atomic heat of cadmium is 6·35, of bismuth 6·47, of tin 6·63, and of lead 6·50; while the mean atomic heat in alloys of bismuth with tin, and lead with tin, ranges from 6·40 to 6·66 (Regnault), which is practically the same.

Again, while the melting-point of platinum is at a white heat, and it becomes plastic at a low red heat, the specific heat at this lower temperature is very little less. Many other metals change considerably in properties at temperatures far removed from their melting-points, without substantial change in their capacity for heat.

As to allotropy it is a phenomenon which is comparatively rare among metals, and in the marked cases in which it occurs we have no information as to the value of the specific heats in the several varieties (such as the two varieties of antimony and the silver zinc alloy of Heycock and Neville), and they may be left out of account. Bunsen compared the so-called allotropic tin obtained by exposing the metal to cold for a long time, and found it ·0545 against ·0559 for the ordinary kind (*Pogg. Ann.*, 141, 27). In dimorphous substances, such as arragonite and calcite, there is often no difference. Regnault found for these two minerals ·2086 and ·2085 respectively.

The differences between metals hammered and annealed, hard and soft, were also found by Regnault to be very small (*Ann. Chim.* [3] ix.):—

Hard steel	·1175	Same, softened ...	·1165
Hard bronze	·0858	Same, softened ...	·0862

Kopp came to the conclusion, *first*, that each element in the solid state and at a sufficient distance from its melting-point has *one* specific or atomic heat which varies only slightly with physical conditions; and, *secondly*, that each element has essentially the same specific or atomic heat in compounds as it has in the free state. This last is practically identical with the statement which is known as Neumann's law. With Kopp's conclusions I agree, but from some of Regnault's results, coupled with my own, the effect of *small* quantities of carbon and, perhaps, of sulphur upon the specific heats of metals is greater than has been supposed.

If we take the results of Regnault and of Kopp, and combine them with the most accurately known atomic weights, the products are still not constant.

ATOMIC WEIGHTS MOST ACCURATELY KNOWN (1897), COMBINED WITH SPECIFIC HEATS.

	A.W. (H = 1)	S. H. Regnault	S. H. Kopp	At. Ht. Regnault	At. Ht. Kopp
Copper	63·12	·09515	·0930	6·01	5·87
Gold	195·74	·03244	—	6·35	—
Iron	55·60	·11379	·1120	6·33	6·23
Lead	205·36	·03140	·0315	6·45	6·47
Mercury liq.	198 49	·03332	—	6·61	—
– 78° to + 10° sol.	198·49	·03192	—	6·34	—
Silver	107·11	·05701	·0560	6·11	6 00
Iodine	125 89	·c5412	—	6·81	—

The law of Dulong and Petit is therefore only an approximation, but this may perhaps be due to impurity in the materials used. That is the problem which I have endeavoured to solve.

The introduction of a new method of calorimetry by Prof. J. Joly, and the excellent results obtained by the author in the use of the differential form of his instrument (*Proc. R. S.*, 47, 241), led me to think that with due attention to various precautions, such as exact observation of the temperatures and practice in determining the moment at which the increase of weight due to condensation is completed, results of considerable accuracy might be obtained.

The problem is to find two elements very closely similar in density and melting-point which can be obtained in a state of purity, and then to determine with the utmost possible accuracy the specific heat of each under the same conditions. The two metals cobalt and nickel were selected for the purpose. They were examined by Regnault, but the metals he used were very impure.

The cobalt employed in my experiments was prepared by myself. For the nickel I am indebted to Dr. L. Mond. Both were undoubtedly much more nearly pure than any metal available in Regnault's time. The results obtained are as follows:—

SPECIFIC HEATS OF COBALT AND NICKEL. PURE FUSED.

Cobalt S.G. $\frac{21^\circ}{4^\circ}$ 8·718	Nickel S.G. $\frac{21}{4^\circ}$ 8·790
·10310	
·10378	
·10310	·10953
·10355	·10910
·10373	·10930
·10362	
Arith. mean ·10348	·10931
Atomic heat	?

Further experiments will be made, because a single well-established case of this kind is sufficient to decide the question. Already, however, I feel certain that Kopp's conclusion is right, and that the law of Dulong and Petit, even for the metals, is an approximation only, and cannot be expressed in the words of the discoverers. For although the exact values of the atomic weights of these two elements are not known, it is certain that they are not so far apart as would be implied by these values for the specific heats, even assuming that the value for nickel is, as I believe, slightly too high.

Two other examples of somewhat similar kind are shown by gold and platinum, copper and iron.

SPECIFIC HEATS OF GOLD AND PLATINUM. PURE FUSED.

Gold S.G. $\frac{18^\circ}{18^\circ}$ 19·227	Platinum S.G. $\frac{18^\circ}{18^\circ}$ 21·323
·03052	·03147
03017	·03150
·03035	·03144
Arith. mean ·03035	·03147
Atomic heat 5·94	6·05

SPECIFIC HEATS OF COPPER AND IRON. FUSED.

Copper (pure) S.G. $\frac{20^\circ}{20^\circ}$ 8·522	Iron S.G. $\frac{15^\circ}{15^\circ}$ 7·745
·09248	Contains ·01 % C.
·09241	·11022
·09205	·11037
·09234	
Arith. mean ·09232	Arith. mean ·11030
Atomic heat 5·83	6·13

For the gold I naturally applied to my colleague Prof. Roberts-Austen. The platinum I prepared from ordinary foil by re-solution, and reprecipitation as ammonic chloride, &c. Both metals were fused into buttons before use. The atomic heats come closer together than those of cobalt and nickel.

Copper and iron differ considerably in melting-point, but both at the temperature of 100° are far removed from even incipient fusion. The copper was prepared from pure sulphate by electrolysis, the iron by reduction of pure oxide in pure hydrogen. Notwithstanding all our care, it was disappointing to find it contained ·01 per cent. of carbon, the source of which I am at a loss to explain. This iron is purer than any examined by Regnault or Kopp.

The differences observed between Co and Ni, and between Au and Pt, are manifestly not due to allotropy or to differences of melting-point, which in these cases can have no effect on the result.

So large a difference must be due to peculiarities inherent in the atoms themselves, and differences of atomic heat are to a certain extent comparable with the differences observed in other physical properties which, like specific volume, specific refraction, &c., are approximately additive.

If we try to think what is going on in the interior of a mass of solid when it is heated, the work done is expended not only in setting the atoms into that kind of vibration which corresponds to rise of temperature—that is, it makes them hotter—but partly in separating the molecules or physical units from one another (= expansion), and partly in doing *internal* work of some kind, the nature of which is not known. A difference between metals and non-metals has been brought out by the researches of Heycock and Neville, who find that metals dissolved in metals are generally monatomic; whereas it is generally admitted that iodine, sulphur and phosphorus in solution are polyatomic. It is, moreover, remarkable that although in respect to specific heat each element in a solid seems to be independent of the rest with which it is associated, when the separate elements are dispersed in vapour some rise in separate atoms like mercury, some in groups of atoms I_2, S_6, As_4, P_4, and these groups, as the temperature is raised, are simplified with very varying degrees of readiness.

Sulphur vapour, for example, diminishes in density from 7·9 at 468°, to 4·7 at 606° (Biltz), that is, from about S_7 to S_4, and iodine from density 8·8 at 253°, to 5·6 at 1570° (V. Meyer), that is, from about I_2 to $\frac{3}{4}$ I_2, but the dissociation of As_4 and P_4 begins only at much higher temperatures, while with mercury there is no corresponding change.

But, although these groups are taken as the chemical molecules, the physical unit in the solid is certainly the atom, whether united by combination or mere mixture.

The two metals, cobalt and nickel, with which I began my inquiry, have nearly the same atomic weight, but they differ from each other remarkably in chemical properties. For example, nickel forms a compound with carbonic oxide; on the other hand, cobalt produces many remarkable ammoniacal compounds, to which there is nothing corresponding among the compounds of nickel.

Having put aside the common excuses for the observed divergencies from the law of Dulong and Petit, we are compelled to look round for some other hypothesis.

The constitution of carbon compounds is now explained by a hypothesis concerning the configuration of the carbon atom introduced by Van t' Hoff and Le Bel twenty-five years ago, and which is now accepted by the whole chemical world. It seems not unreasonable to apply a similar hypothesis to the explanation of those cases of isomerism which have been observed in certain compounds of the metals, notably chromium, cobalt, and platinum. This has already been done by Prof. Werner of Zürich. Of course, as there is no asymmetry, there are no optical differences in the pairs of compounds thus represented. If the constitution of compounds can be safely explained by such hypothesis, this implies peculiarities in the configuration of the individual constituent metals around which the various radicles are grouped in such compounds, and hence peculiarities in the behaviour of such metals in the elemental form may possibly be accounted for. For the atom of cobalt, Prof. Werner employs the figure of the regular octahedron. For nickel, therefore, which differs from cobalt, especially in yielding the remarkable carbonyl compound discovered by Mond, and by not yielding ammines like those of cobalt, and in other ways, a different figure must be chosen. This, however, is for the present a matter of pure speculation.

SCIENCE IN THE THEATRE.

THE assimilation of nature on the stage! To what extent is assimilation possible, and what are the necessary methods and appliances for obtaining a satisfactory assimilation? This practically was the subject of a very valuable paper prepared for the Society of Arts by Mr. Edwin O. Sachs, the architect, which led to an animated discussion at the crowded meeting before which it was read. The title of Mr. Sachs' paper, it is true, was briefly "Stage Mechanism," but he went far beyond the mere description of the various appliances that can be used for obtaining certain scenic effects, and, more especially in his introduction, treated the subject on broad lines.

Though the presentation of drama and opera with some attempt at realistic surroundings is now accepted as a matter of course in all civilised countries, it can but rarely be said that the attempts are successful. In fact, only of recent years has the London manager been able to give us the presentation of indoor scenes with some claim to merit, and this only by building up his various scenes piecemeal in a most cumbersome way, which is all that is possible where the changes of scene are few and the "run" long. As to the presentation of scenes out of doors, the London manager has most lamentably failed, no matter how well painted individual canvases may have been, or how tricky the arrangements of individual scenic effects. A sky that looks like so much blue calico hanging on a wash-line, a horizon with angles, a tree that looks like a piece of card-board, or a moon which suddenly rushes into the sky and then remains stationary, are all anomalies, and form only a few of the innumerable details which tend to make a scene incongruous.

Now according to Mr. Sachs, who fully recognises the attempts that have been made from time to time by Sir Henry Irving, Mr. Beerbohm Tree, Sir Augustus Harris, and others

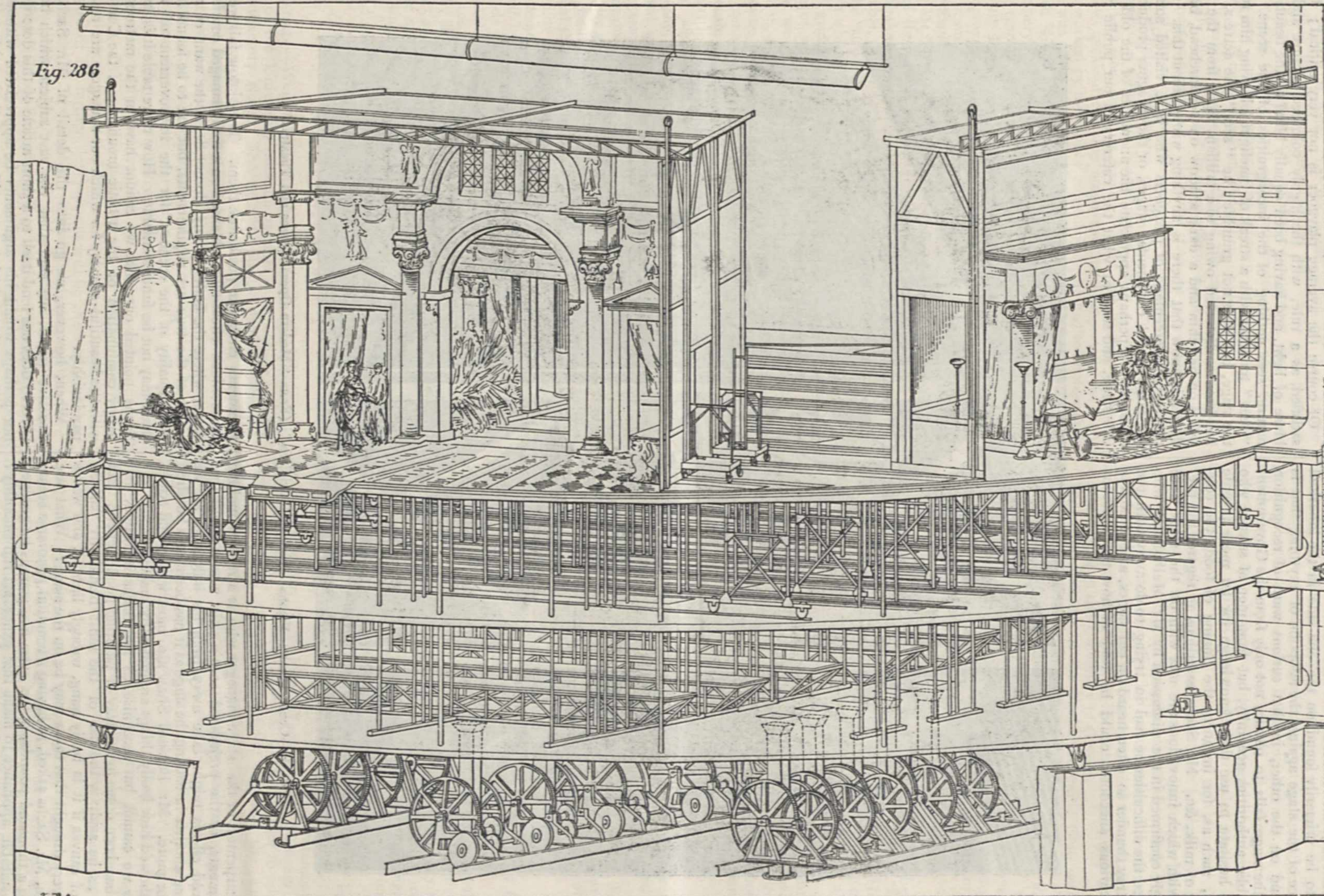


FIG. 1.—Electric Turntable Stage. (From "Stage Construction.")

(assisted by such eminent painters as Mr. Burne-Jones, or Mr. Alma Tadema), the reason for the anomalous scenes we see to-day is to be primarily found on the one side in the inherited prejudice of the stage against the adoption of anything that is new; and on the other, in that curious want of recognition which the stage fails to obtain, not only from the Government and public authorities generally, but from men of science who do not hesitate to use their knowledge for far more prosaic matters, such as, for instance, the tinning of food, the condensing of milk, &c. Mr. Sachs' assertions as regards the prejudice with which innovations are met with on the stage were amusingly confirmed in the discussion by Mr. Mulholland, who explained the difficulties he had in trying to do away with the tin-tea-tray thunder so often heard on our stages, and of course many curious anecdotes could be told of how the ignorant stage-carpenter, or the stage-manager who is afraid of making experiments, or the prejudiced scenic artist who is afraid that improved effects might take away from his influence, all vie with one another in leaving the stage as it has been for a hundred years or more. Mr. Bernard Shaw also most wittily and scathingly showed how badly things are generally managed.

Now we cannot but recognise, as Mr. Sachs indicated, that much has been done in the way of painting good scenery. But what, as he said, is the use of the most beautifully painted piece of canvas if it is badly hung, wrongly lighted, and waves with every draught that there may be on the stage? What use is there, Mr. Sachs asked, in having a beautiful panorama scene, costing a thousand pounds, if the extra five pounds be grudged for a suitable appliance to make that panorama run smoothly? Mr. Moul, of the Alhambra, argued that there seldom were any spare five pounds. But there should be. Why not let the panorama scene cost ten pounds less and have the appliances?

Of course the average playgoer is not very critical; he is satisfied, as a rule, with the highly coloured picture and the blaze of light, and having been equally blind to the beauties of nature, sees nothing of the incongruities of the scene. He "sees" an actor with a streak of limelight following him round the stage, but does not grumble; he "sees" the actress, with her features distorted owing to a brilliant light from the footlights on her chin and a dark shadow on her forehead, but he does not know that there is anything wrong about this. Only that small percentage of playgoers who have visited some of the large model continental stages, or the Wagner productions at Bayreuth, perhaps appreciate the anomalies of the old English stage, and scoff at what the caterers of our public entertainments choose to put before them. They know full well the harmonious effect often obtained on a well-managed continental stage, where the faults, if any, do not lie in the want of recognition of the true art requirements, but are to be found in the poor quality of the scenery, for the improvement of which there may not be funds available. How regrettable it is, as Mr. Sachs pointed out, that we cannot have in the metropolis a happy combination of the artistic mounting of the Continent with the beautiful scenery for which our managers are ready to pay lavishly.

When, however, we go into the detail of Mr. Sachs' instructive paper, we find that the vast subject which he has covered does not lend itself to a short article of this description, nor perhaps would his arguments be appreciated without the many illustrations which he was able to put before his audience

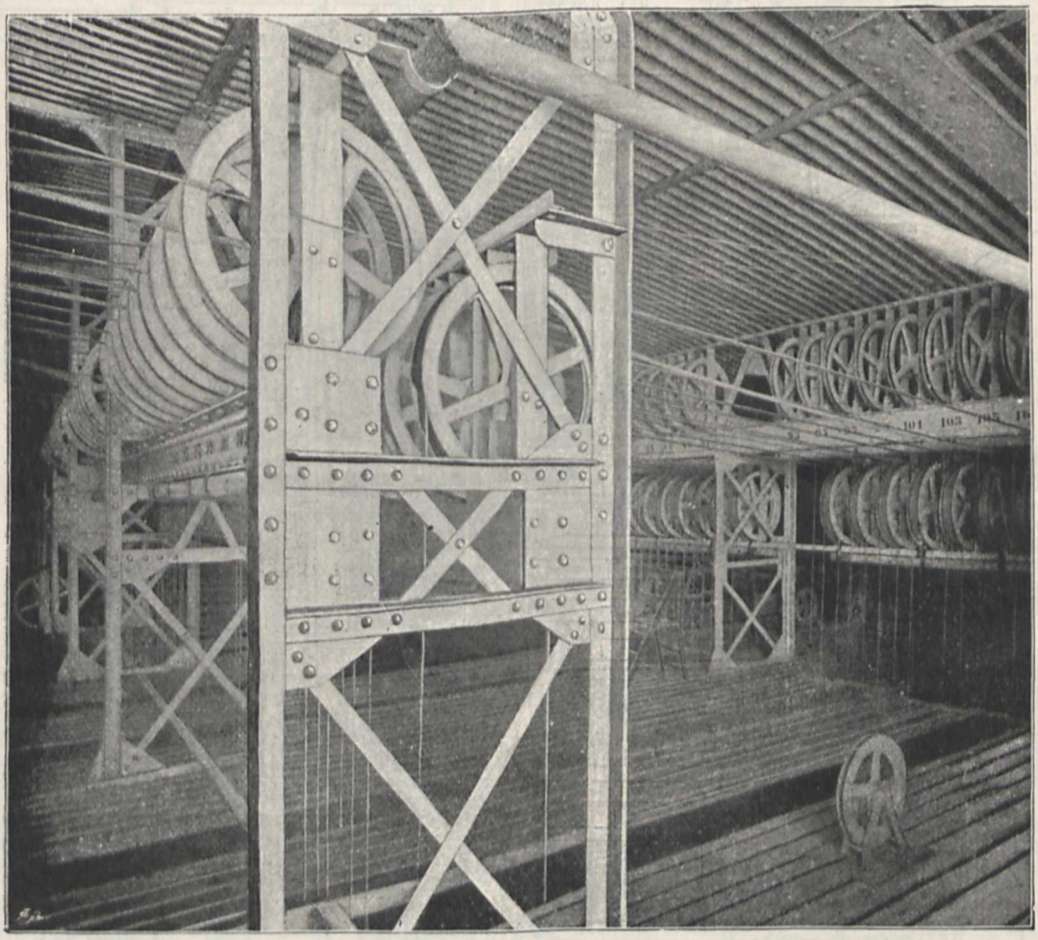


FIG. 2.—Court Theatre, Vienna. View of "Gridiron." (From "Modern Opera Houses and Theatres.")

at the Society of Arts. Yet we would point out that, in the first place, he divided the stages he had under consideration into (1) wood stages, (2) wood-and-iron stages, and (3) iron stages; and that he then again subdivided them according to the power used for moving the scenery, or obtaining certain effects, be it manual labour, hydraulics, or electricity.

In speaking of the wood stage of the metropolis, Mr. Sachs naturally does not omit to refer also to the wood stage of the Continent, which is but little better than our own; nor when he spoke of the wood and iron stage of Paris did he omit to speak of our "Palace" Theatre of Varieties, which is the solitary example of a theatre in this country in which a combination of wood and iron is to be found. When Mr. Sachs, however, came to speak of the iron stage, and more especially the iron stage worked by hydraulics or electricity, he had to confess that there was not a single iron stage to be found throughout the United Kingdom, that there was no stage worked by electric machinery, and that the only appliances in which hydraulics are being employed in this country were some so-called "bridges" at Drury Lane. But on the continent, the iron stage, with all its improvements for lighting, for showing a curved horizon, and —to summarise—for giving some semblance of nature, is already to be found in considerable numbers and of considerable variety.

By Mr. Sachs's courtesy we are able to show two illustrations— one of the great electrical turntable stage for Munich, so useful for Shakespearean drama, where a quick change of scene is desirable, and the other of a hydraulic stage at Vienna worked on the suspended system. In the first case a general view is shown which well describes itself. In the latter case a view of the "gridiron" is shown, which plainly indicates the modern forms of wiring adopted.

But we cannot go further into the technical detail of the question, and we only trust that Mr. Sachs's words will have had some effect on the many managers and stage engineers who had come to hear him, not forgetting Herr Kranich, from Bayreuth, one of the leading exponents of scenic mounting on true art lines.

But whatever may have been the influence of Mr. Sachs's advocacy, we would end by quoting him where he said "that the real secret of perfect scenic art lies in illusion, *i.e.* in visual deception, or in not allowing the eye of the spectator to discern the means whereby the semblance of reality is obtained; mere actuality will not accomplish this—crude realism alone would then result."

What the scenic artist and the stage-manager must attempt, according to Mr. Sachs, is to obtain a successful illusion; and this, he argues, is obtainable, not by any great *radical* reform, as desired by irresponsible faddists, but a *practical* reform of the methods and appliances which are to-day used on the stage of the metropolis, and which are, unfortunately, quite a hundred years too old.

Why should not our stage have the full benefits of science and art as practised now on the approach of 1900 A.D., instead of the makeshifts with which the world was satisfied at the beginning of the last century?

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Want of accommodation in more than one department of the University museum renders it impossible to carry on satisfactory work. The extracts printed below, from the report of the delegates of the museum, tell of a condition of "hope deferred, which maketh the heart sick." Prof. J. Burdon-Sanderson reports:—"The Regius Professor of Medicine takes this opportunity of expressing his bitter disappointment that another year has been allowed to pass without any step having been taken towards providing adequate accommodation for the teaching of medical science in the University. It is in his judgment to be feared that if the reasonable requirements of the medical school continue to be disregarded, its further development will be checked, and that the progress of those departments of teaching which have common interests with it will be seriously interfered with." Prof. R. B. Clifton, Professor of Experimental Philosophy, says: "Some electrical apparatus has been placed in the room formerly allotted to the professor as a private laboratory, and with that in the room devoted to the electrical work of the preliminary classes, it is now possible to offer some, though very restricted, facilities to Honour students who wish to gain experience in the methods of measuring electrical quantities. The professor and demonstrators have now, however, no place in which they can carry on research; and all attempts to undertake work of this character must in future be abandoned. After twelve years of fruitless effort to obtain extended accommodation for Honour students, and the means of providing for the increasing number of those working for the preliminary examination—a class of students not contemplated when the laboratory was designed—it is probably quite useless to trouble the delegates with any further application for assistance in this direction." It will be difficult for men of science on the Continent and in the United States to believe that so little encouragement is given to scientific work in the University of Oxford.

The 191st meeting of the Junior Scientific Club was held in the physiological lecture-room of the museum on Friday, June 10. After private business, Mr. V. H. Veley, F.R.S., read a paper on *Coleothrix methystes*, the active microorganism which Mrs. Veley and himself recently discovered in "faulty" rum, and, it is hoped, will shortly form the subject of a monograph. After the paper a discussion took place, in which Dr. Ritchie and others joined.

CAMBRIDGE.—Mr. A. E. H. Love, F.R.S., of St. John's College, has been appointed University Lecturer in Mathematics in the room of Mr. Glazebrook, resigned.

The Senior Wrangler this year is Mr. R. W. H. T. Hudson, of St. John's College, son of Prof. W. H. H. Hudson, of King's College, London. Miss Cave-Browne-Cave, of Girton, is bracketed fifth wrangler.

The Vice-Chancellor announces that donations amounting to over 6000*l.* have been received for the University Benefaction Fund, started last year. A large number of the donations are ear-marked for the Medical School. A bequest of 10,000*l.* has also fallen to the University, but it is assigned to the foundation of a prize or scholarship in memory of the late Dr. Allen, Bishop of Ely.

Mr. C. F. Hadfield, of Trinity, and Mr. R. C. Punnett, of Caius, have been nominated to the University tables at the Naples Zoological Station; and Prof. E. W. MacBride, of St. John's, to the table at Plymouth.

The General Board propose that Mr. W. N. Shaw, F.R.S., should be appointed assistant-director of the Cavendish Laboratory for the ensuing year, in the place of Mr. Glazebrook.

DR. R. A. HARPER has been appointed professor of botany at the University of Wisconsin.

AT a meeting of the Court of Edinburgh University on Monday a letter was read from a benefactor of the University, intimating that he is prepared to give to the University such a sum as may be necessary, but not exceeding 10,000*l.*, to build and equip a laboratory and class-room to be used exclusively for the teaching of public health, the site of the proposed building to be provided by the University.

THE foundation-stone of a separate department for instruction in the technology of the leather industries, was laid at the Yorshire College, Leeds, on Monday. The ceremony was performed by Mr. A. B. Kent, Warden of the Skinners' Company of London, who have provided 5000*l.* in order to establish this department, and will contribute towards the working expenses.

THE new laboratories of physiology and pathology at the University College, Liverpool, will be formally opened on October 8. The laboratories have been erected and equipped in the most adequate way for study and research by the Rev. Thompson Yates, at a cost of 25,000*l.* Lord Lister, President of the Royal Society, has consented to perform the opening ceremony; and the Victoria University will take advantage of his visit to Liverpool to confer upon him the honorary degree of doctor of science.

AT the Science and Art Department on Friday last a conference was held of organising secretaries and other representatives of local organisations which have been recognised by the Department as responsible for science and art instruction within their several districts. The Vice-President of the Committee of Council on Education (Sir John Gorst) presided, and the conference was attended by representatives from a number of ounties. Various matters connected with the administrative arrangements between the local authorities and the Department were considered and decided.

AT the instance of the Headmasters' Conference, the Headmasters' Association, the Headmistresses' Association, and the Conference of Catholic Schools, a Bill dealing with the subject of secondary education will be introduced into Parliament this Session. The Bill proposes to transfer the powers relating to secondary education now vested in the Charity Commission, the Science and Art Department, and the Education Department to one central authority under the Committee of the Privy Council on Education, and to establish local secondary education authorities to administer areas not less than those of a county or a county borough. It is contemplated that the reconstituted Education Department will consist of two sections, for secondary and primary education respectively, these two sections being under one permanent secretary, who will be advised by chief assistant secretaries in regard to each of these two chief divisions of departmental work. The Bill further provides for the registration of secondary schools according to their different types and of teachers qualified to teach. The residue under the Local Taxation (Custom and Excise) Act, 1890, is to be allocated to education, and in the case both of residue and of Imperial grants now paid through the Science and Art Department such portions as the Treasury shall determine are to be allocated to secondary education and to technical instruction respectively.

THE new buildings of Reading College, under which name the University Extension College at Reading will in future be known, were opened by the Prince of Wales on Saturday. The College was established in 1892 as a direct outcome of Oxford University Extension work. Mr. H. J. Mackinder was appointed Student of Christ Church, Oxford, his appointment being made "with a view to giving system and completeness" to the educational work of one of the University Extension centres. His services were offered to Reading, and were accepted; and, largely owing to his efforts during the past six years, the College has advanced to the position it now occupies. The first home of the College was restricted to an ancient building, formerly part of the Hospital of St. John, attached to the Abbey of Reading. The accommodation was soon found to be insufficient for the increasing number of students. Mr. Herbert Sutton, chairman of the Council, purchased the vicarage of St. Lawrence, adjoining the Hospitium, and the acquisition of this property enabled certain necessary enlargements to be made, including the building of a dairy institute. The cost of the College properties and buildings exceeds upwards of 20,000*l.*; and it was this amalgamation of old and new buildings in one central educational organisation, to be known as Reading College, that the Prince of Wales formally opened on Saturday. In responding to the toast of "The Royal Family," at the luncheon after the opening ceremony, the Prince of Wales remarked:—"In the work we have done to-day, we have inaugurated an institution which has for its object the advancement of higher education, especially in those branches more particularly connected with science, art, and agriculture. To me this is particularly interesting on account of the early associations which render it a matter of interest to know that the new College owes its inception and encouragement to the University of Oxford, and to Christ Church, my old College. The presence of the Vice-Chancellor of Oxford and of the Dean of Christ Church, as well as the attendance of many other eminent men from Oxford, is a proof of the interest they take in this movement. Let me mention that the heads of colleges and the Hebdomadal Council have satisfied themselves of the high standard of efficiency of the education in Reading College, and have agreed with great liberality to affiliate Reading College to the parent University to the extent of conferring on it the privilege of allowing students, after spending three years at Oxford and passing certain scientific examinations there, to proceed to Reading, where one year's further study in the science and practice of agriculture should count as part of their University career, and entitle them to the B.A. degree on the completion of their full course. This proposal, although supported by a large and influential University, was, on a division, rejected by two votes, the numbers being 47 to 45. The interest which I take in University Extension teaching, which now includes agriculture, leads me to hope that another year may see the adoption of the important policy advocated by the important bodies to which I have alluded, and that its provisions may be carried through the subsequent stages to render it law."

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 12.—"The Electrical Response of Nerve to a Single Stimulus investigated with the Capillary Electrometer." Preliminary communication. By F. Gotch, M.A., F.R.S., Professor of Physiology, University of Oxford, and G. J. Burch, M.A. (Oxon).

By means of a very sensitive capillary electrometer the authors have obtained photographic records of the electrical response in the sciatic nerve of the frog when excited by a single stimulus. The records differ in character according to the condition of the nerve. In uninjured nerve a rapid displacement of the meniscus in one direction is followed by a corresponding displacement in the other direction. In nerve which is the seat of a persistent electromotive change, whether through local injury or the passage of an appropriate polarising current, the record shows that the initial rapid displacement is succeeded by a prolonged after-effect of similar sign. The records are sufficiently pronounced to allow of the calculation of the E.M.F. of the potential difference between the electrometer contacts causing the initial displacement; this may reach as much as 0·032 volt, and attains its maximum very rapidly. In fresh nerve at 6° C. the first indications of such electrical change occur 0·002 second after the single stimulus has been applied at a distance of 30 mm. from the capillary contacts. The after-effect develops more slowly, taking from 0·006 to 0·01 second to culminate, its maximum E.M.F. is only one-tenth that of the initial change, and it subsides slowly; it is present in every nerve when one of the capillary contacts lies upon the cross section of the nerve.

"On the Magnetic Susceptibility of Liquid Oxygen." By Profs. J. A. Fleming, F.R.S., and James Dewar, F.R.S.

May 26.—"Note on the complete Scheme of Electrodynamic Equations of a Moving Material Medium, and on Electrostriction." By Joseph Larmor, F.R.S., Fellow of St. John's College, Cambridge.

This paper (in continuation of previous memoirs) undertakes in general form the exact expression of the electrodynamic relations of moving media which are polarisable, or are in motion through the æther. No foundation is available from which to investigate the modification that the ordinary equations of MacCullagh and Maxwell must then undergo, without going back to molecular theory. When that is done the crucial point in the investigation is the transition from a theory concerned with the individual molecules to a mechanical theory concerned only with the element of volume: this requires a separation between the influence of neighbouring molecules which affects only the structure of the material at that place, and the influence of the matter in general which induces polarisation and mechanical strain in the structure. It is shown that to express the influence of magnetic polarisation of the material, and also the influence of convection of electrically polarised material, these agencies must be replaced analytically by equivalent distributions of electric current. The resulting scheme of equations is wide enough to include the whole field of electrical and optical phenomena in continuous bodies, whether fixed or in motion, of of which various cases are again incidentally considered.

Physical Society, June 10.—Mr. Shelford Bidwell, President, in the chair.—Dr. S. P. Thompson described and exhibited a model illustrating Max Meyer's theory of audition. Max Meyer abandons the audition theory of Helmholtz, and contends that analysis takes place in the ear otherwise than by resonance of the Corti organ. Imagine a jointed system, like a hand, to be oscillated from one end, *i.e.* from the finger-tips. A small motion affects only the top joints, but a large motion affects the whole structure. Such a structure is the membrane of the inner-ear. It widens towards one end, and is effectively damped by the contained liquid. Wave-motions of different amplitudes run along it to different distances before they are extinguished; these distances are recorded by nerves, and are thereby communicated to the Corti organ. In the model, the compound-wave to be analysed is cut out on the edge of a disc of zinc, so that, as the disc revolves, the motions are communicated to a frame-work. If the frame is thus moved through more than a certain distance, a displacement occurs which sets a second frame in motion, and so on to a third and fourth. The depth to which the motion penetrates is indicated by a series of glow-lamps connected electrically to the frames. Prof. Ayrton said it had for some time past occurred to him, when considering the way in which an expert telegraph clerk reads siphon-recorder signals on a long cable, that it might be possible to analyse waves without the supposition of a resonating apparatus. The clerk interprets not so much the motions to one side or other of the zero-line, as the rate of change of velocity, *i.e.* the acceleration of the siphon. This had been recognised in the design of those relays for long cables, where the lever makes contact when the received current exceeds a certain value, and breaks contact when the current falls below a certain minimum. Messrs. Siemens had adopted a relay in which the lever was carried on the suspended coil of a D'Arsonval galvanometer by a pivot with a small amount of friction. If contact was made, the coil could, nevertheless, continue its motion in a given direction. If that direction altered, contact was immediately broken, and the lever passed over to the opposite stop, thereby reversing the local circuit. It was possible that, in the process of hearing, something akin to this took place, the ear behaving as a mechanism responsive, not by resonance to the complete waves, but by its sensitiveness to changes of direction of the received impulses. Dr. S. P. Thompson thought that a mechanism similar to the relay described by Prof. Ayrton was contained in the telautograph of Elisha Gray; it was a "Prony" mechanism. In the acoustical problem the ear was probably sensitive to abrupt changes of shape in the waves as well as to reversals. In the case of mistuned octaves, something is heard that suggests "revolving" in the ear, indicating a cyclic change. In this regard it was necessary to take into account the phase-relations as well as the relative intensities of the component tones.—Mr. E. H. Barton then read a paper on the attenuation of electric waves along a line of negligible leakage. It forms a sequel to a paper communicated to the Physical Society and printed in their *Proceedings* of December 1897 and January 1898. Shortly after the publication of the earlier results, Mr. Oliver Heaviside drew attention to Lord Rayleigh's high-frequency formula for the "effective resistance" of wires to alternating currents, and suggested that the formula might be approximately applicable to the case; but he thought the experimental value of the attenuation would be considerably higher than the one derived from calculations. Mr. Barton here repeats the work, with special precautions as to the mode of insulating the parallel copper wires through which the wave-train proceeds. The value of the attenuation constant deduced from these experiments is 0·000013. By applying Lord Rayleigh's formula for the effective-resistance of the circuit, and using this value in Mr. Heaviside's expression for the attenuation, the calculated constant is 0·0000062. To account for the discrepancy, the author points out that the effective-resistance formula was originally developed for a wire placed at a considerable distance from other parts of the circuit, and for currents following the harmonic law. Whereas, in the experiments the conditions are (1) wires 1·5 mm. diameter, only 8 centim. apart, and (2) the waves are propagated in the form of a damped train, with the large end leading; they are extinguished after ten or a dozen vibrations. Mr. Oliver Heaviside (communicated) pointed out that, as there was human interest in error, it might be worth mentioning that at first it was supposed the previous experiments of Dr. Barton made the index of the attenuation factor to be six times that of the long-wave theory for simple periodic waves. And it was hard to account for so large a discrepancy. The discovery of an error in the figures, reduced the result from six to two. The small depth of the surface-layer of effective conduction, and the distance apart of the wires, seemed now to make it improbable that Dr. Barton's first reason (1) was adequate to account for the doubling of resistances. The second (2) was of course a substantial reason for increased resistance. A third one, Mr. Heaviside suggested, was the external resistance at the boundary of the waves. A combination of the second and third reasons, with a little of the first, might account for most of the extra attenuation observed, and, if more was wanted, one could "try the K.R. law." Mr. Appleyard said it was rather to be regretted that, in all the experiments, the distance between the wires had been the same, *i.e.* 8 cms. By taking a few different values (1) might have been checked. Lord Rayleigh's formula for the effective-resistance, involved the square-root of the magnetic permeability of the wires. The author had, throughout, used copper, a paramagnetic metal, and had assumed $\mu = 1$. It would be of advantage to try other metals. Mr. Barton, in reply, said he would make further experiments with the two conductors at different distances apart, and he would also try iron wires. With iron, the thickness of the surface-layer of the effective conductor was about one-thirteenth that of copper. Iron should therefore give a greater value of the attenuation than copper. —Mr. A. Griffiths then read a paper on diffusive convection, a phenomenon analogous to caloric convection. The differences of density that produce convection-currents are not due to changes of temperature, but to variations in the quantity of dissolved substance per unit volume. The author has devised an apparatus consisting of a vessel divided horizontally by a diaphragm, through which pass two vertical tubes of unequal lengths. A solution of copper-sulphate, maintained at constant strength, is placed in the lower compartment. The upper compartment is filled with water. Diffusion takes place up the tubes. One tube is 4 cm. long; the other is 4·05 cm. The tops of the tubes are exactly at the same level. Up the longer tube, and down the shorter, diffusive convection occurs at the rate of 5 cm. per year. This flow *increases* the quantity of copper-sulphate transmitted by the long tube by about 2 per cent., and *diminishes* that transmitted by the shorter tube by about the same amount. Consequently, the resultant increase due to the motion is only a fraction of 1 per cent. To detect the flow, the author employs a second piece of apparatus, in which the upper ends of the tubes are separated by a capillary, containing coloured liquid. By this means the motion is considerably magnified. Dr. S. P. Thompson asked whether, in a case where a large tube was used in determining the velocity, the viscosity of the liquid would not play a very much less part than with narrow tubes. Mr. Griffiths explained that viscosity was not important until very small tubes were considered, *e.g.* those of the order 0·001 mm. diameter.—The President proposed votes of thanks to the authors, and to Dr. Max Meyer for lending the Society his model.—The meeting then adjourned until June 24.

EDINBURGH.

Mathematical Society, May 13.—Mr. J. B. Clark, President in the chair.—The following papers were read:—On the second solutions of Lamé's equation, by Mr. Lawrence Crawford (communicated by Mr. J. W. Butters); on the insolation of a sun of sensible magnitude, by Mr. A. Ritchie Scott; the singular solutions of a certain differential equation of the second order, by Mr. Hugh Mitchell.

PARIS.

Academy of Sciences, June 6.—M. Wolf in the chair.—New photographic studies of the surface of the moon, by MM. Lœwy and Puiseux. A discussion of the data contained in the third part of the photographic atlas of the moon.—On a new absolute electrodynamometer, by M. Marcel Deprez. In the system described, the forces due to the action of the current are simple algebraic functions, rigorously and without approximation, of the dimensions of the fixed and movable circuits.—On a new constituent of the atmosphere, by MM. William Ramsay and Morris W. Travers (see NATURE, p. 127). M. Berthelot observed that the green ray of krypton coincided almost exactly with the bright green line of the aurora borealis. He suggested the name *eosium* for the new element.—On the propagation and deformation of the tidal wave which ascends rivers, by M. Partiot. The curve of the experimental results obtained on the Gironde and Garonne are compared with five formulæ; of these, that suggested by M. Boussinesq agrees best with the experiments.—On surfaces of total constant curvature, by M. C. Guichard.—On the systems of differential equations which satisfy the quadruply periodic functions of the second species, by M. Martin Krause.—On discontinuous functions which are allied to continuous functions, by M. R. Baire.—On the determination of the order of interference fringes, by MM. A. Perot and Ch. Fabry.—On the rotatory power of quartz in the infra-red, by M. R. Dongier. A comparison of the experimental results with those calculated from a formula given by M. Carvallo.—On the discharge of a Leyden jar, by M. R. Swygedauw.—Comparison of the Hertzian field in air and in oil, by M. Albert Turpain. In a resonator kept in a plane-perpendicular to the direction of the wires the wave-lengths vary with the nature of the dielectric; if the resonator is in the same planes as the wires, the wave-lengths are independent of the nature of the dielectric.—On resonators, by M. Oudin. The resonator now used consists of a solenoid of bare copper wire wound round a cylinder of paraffined wood, the high frequency current being produced by the arrangements of Hertz, of Tesla, or of d'Arsonval. This resonator creates a very intense alternating field, a Geissler tube being lit up at two metres distance. The discharge resembles in appearance that of a statically charged body, and causes lesions of the skin similar to those produced by the X-rays.—Visibility of the blind spot in the retina, by M. Aug. Charpentier. The experiments cited show that the spot where the optic nerve enters the retina, although insensible to light and blind in the proper sense of the word, is really represented in space by positive visual sensations occupying the same place, as if it were replaced in the eye by a real piece of retina in continuity with the rest of the membrane.—Quality of the fifteen vowels of the French language, by M. Monoyer.—Action of ammonium persulphate upon the silver in photographic negatives and the utilisation of this action, by MM. Lumière and M. Seyewetz. By means of a 5 per cent. solution of ammonium persulphate it is possible to reduce an over-exposed photograph in a manner not possible with the reagents previously suggested for this purpose, the persulphate acting first upon the most opaque portions of the negative, and leaving the half-shadows untouched.—On the causes of the imperfections in radiographs brought about by the use of reinforcing screens, by M. A. Londe. Comparative photographs were made with five screens, the platinocyanide of barium and of potassium, sulphide of zinc, Becquerel's violet sulphide, and Kahlbaum's screen. Whilst some of these increased the rapidity of action of the X-rays, it was always at the expense of clearness of definition, the image being accompanied by a kind of halo. Hence these screens cannot be employed in delicate work.—On the constitution of the ternary alloys, by M. Georges Charpy. A microscopical study of the bismuth-lead-tin and copper-tin-antimony alloys.—On the yttrium earths contained in the monazite sands, by M. O. Boudouard.—On the carbonic acid of the atmosphere, by MM. Albert-Levy and H. Henriet. The differences occasionally observed between the amounts of atmospheric carbon dioxide as determined by potash and baryta respectively, may possibly be due not to a different absorptive power for the gas with the two reagents, but to a slow oxidation of the organic matter present in the air which proceeds with different velocities in the two cases.—On a crystallised hepta-acetate of ouabaine, by M. Arnaud. Obtained by the action of acetic anhydride in presence of zinc chloride upon ouabaine.—On some acetals of pyrocatechol, by M. Ch. Moureu.—Nitration of cellulose and its hydroxy- and oxy-derivatives, by M. Léo Vignon.—A new mucin extracted from an ovarian cyst, by M. Charles Lepierre.—On the Holothuria collected by the *Travailleur* and *Talisman*, by M. Rémy Perrier.—On the embryogeny of *Serpula infundibulum* and *Hydroïdes pectinata*, by M. Albert Soulier.—Polymorphism in an Annelid (*Dodecaceria concharum*), by MM. Félix Mesnil and Maurice Caullery.—On the sexuality and relations of the Sphacelariaceæ, by M. C. Sauvageau.—On the palæozoic layers on the southern declivity of the Montagne-Noire, by M. J. Bergeron.—Characteristics of the bitumenous schist of the Bois-d'Asson (Basse-Alpes), by M. C. Eg. Bertrand.—On the transport of the sick, by M. Bonnafy. A discussion of the relative merits of State hospital-transports or ships chartered from the mercantile marine for this purpose.

DIARY OF SOCIETIES.

THURSDAY, JUNE 16.

ROYAL SOCIETY, at 4.30.—Observations on Stomata: Francis Darwin, F.R.S.—Note on the Attenuation and Exaltation of the Virulence of the Organism of Texas Fever: A. Edington.—Mathematical Contributions to the Theory of Evolution. V. On the Reconstruction of the Stature of Prehistoric Races: Prof. K. Pearson, F.R.S.—On some Expressions for the Radial and Axial Components of the Magnetic Force in the Interior of Solenoids of Circular Cross Section: C. Coleridge Farr.—On the Source of the Röntgen Rays in Focus Tubes: A. A. C. Swinton.—On the Constituents of Argon: Prof. W. Ramsay, F.R.S., and M. W. Travers.—And other Papers.

LINNEAN SOCIETY, at 8.—Observations on the Seasonal Variations of Elevation in a Branch of Horse-Chestnut Tree: Miller Christy.—On Pantopoda collected by Mr. W. S. Bruce in Franz-Josef Land: G. H. Carpenter.—Morphological Relationships of the Actiniaria and Madreporaria: J. E. Duerden.—On some Fossil Leporines: Dr. C. I. Forsyth Major.

CHEMICAL SOCIETY, at 8.—Ballot for the Election of Fellows.—Preparation of a Standard Acid Solution by Direct Absorption of Hydrogen Chloride: Dr. G. T. Moody.—Researches on the Terpenes. III. Halogen Derivatives of Fenchene and their Reactions. IV. On the Oxidation of Fenchene: J. A. Gardner and G. B. Cockburn.

SATURDAY, JUNE 18.

GEOLOGISTS' ASSOCIATION (London Bridge, L.B.S.C.), at 12.25.—Excursion to Crowborough. Directors: G. Abbott and R. S. Herries.

MONDAY, JUNE 20.

ROYAL GEOGRAPHICAL SOCIETY, at 8.30.—Tirah: the Geographical Results of the Recent Afridi Campaign: Colonel Sir T. Hungerford Holdich.

SOCIETY OF CHEMICAL INDUSTRY, at 8.—Aluminium as a Heating and Reducing Agent (in the Production of Chromium and other Metals): Dr. Hans Goldschmidt and Mr. Claude Vautin.

VICTORIA INSTITUTE, at 4.30.

TUESDAY, JUNE 21.

ZOOLOGICAL SOCIETY, at 8.30.—Remarks upon Series of Specimens of Lepidosiren and other Fishes obtained in Paraguay: J. Graham Kerr.—Report on the Collection of Fishes made by Mr. J. E. S. Moore in Lake Tanganyika during his Expedition 1895-96; with an Appendix by Mr. J. E. S. Moore.—On the Scorpions, Spiders, and *Solpugæ* collected by Mr. C. Steuart Betton in East Africa between Mombasa and Uganda: R. I. Pocock.

ROYAL STATISTICAL SOCIETY, at 5.—Annual General Meeting.

ROYAL PHOTOGRAPHIC SOCIETY, at 8.—Photographic Images: Captain W. de W. Abney.

WEDNESDAY, JUNE 22.

GEOLOGICAL SOCIETY, at 8.—Post-Glacial Beds exposed in the Cutting of the New Bruges Canal; T. Mellard Reade.—High-level Marine Drift at Colwyn Bay: T. Mellard Reade.—Observations on the Geology of Franz Josef Land: Dr. Reginald Koettlitz.—Notes on Rocks and Fossils from Franz Josef Land brought home by Dr. Koettlitz, of the Jackson-Harmsworth Expedition, in 1897: E. T. Newton, F.R.S., and J. J. H. Teall, F.R.S.—On the Corallian Rocks of Upware: C. B. Wedd.

FRIDAY, JUNE 24.

PHYSICAL SOCIETY, at 5.—Exhibition of an Apparatus illustrating the Action of Two Coupled Electric Motors: Prof. Carus-Wilson.—Exhibition of Weedon's Expansion of Solids Apparatus: J. Quick.—On the Theory of the Hall Effect in a Binary Electrolyte: Dr. F. G. Donnan.

SATURDAY, JUNE 25.

GEOLOGISTS' ASSOCIATION (Liverpool Street Station, G.E.R.), at 9.30 a.m. —Excursion to Sudbury. Director: Dr. J. W. Gregory.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Grundriss der Vergleichenden Anatomie der Wirbelthiere: Prof. R. Wiedersheim, Vierte, Gänzlich Umgearbeitete Auflage (Jena, Fischer).—The Wonderful Century: A. R. Wallace (Sonnenschein).—Royal University of Ireland Exam. Papers, 1897 (Dublin, Ponsonby).—University Extension College, Reading, Calendar 1897-98, 3rd edition (Reading).—The Cubomedusæ: F. S. Conant (Baltimore, Johns Hopkins Press).

PAMPHLETS.—Lessons in Domestic Science: E. R. Lush, Part 1 (Macmillan).—The Romanes Lecture, 1898: Types of Scenery and their Influence on Literature: Sir A. Geikie (Macmillan).

SERIALS.—Zeitschrift für Wissenschaftliche Zoologie, lxiii. Band, 4 Heft (Leipzig).—Physical Review, February, March, April (Macmillan).—Die Vertheilung der Erdmagnetischen Kraft in Österreich-ungarn: Prof. J. Liznar, ii. Theil (Wien, Gerold).—American Journal of Science, June (New Haven).—Sechster Jahres-Bericht des Sonnblick-Vereines für das Jahr 1898 (Wien).—Himmel und Erde, June (Berlin).—Bulletin de la Société Impériale des Naturalistes de Moscou, 1897, No. 4 (Moscou).—Journal of the Institution of Electrical Engineers, June (Spon).—Brain, Part 81 (Macmillan).

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