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" To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

"To the solid ground
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THURSDAY, MAY 5, 1904.

THE METALLURGY OF STEEL.

The Metallurgy of Steel. By F. W. Harbord, A.R.S.M., F.I.C. With a Section on the Mechanical Treatment of Steel by J. W. Hall, A.M.Inst.C.E. Pp. xxiv+758. (London: Charles Griffin and Co., Ltd.) Price 25*s.* net.

A PONDEROUS volume, profusely illustrated, abounding in detail, probably the best yet published on the whole subject, yet a little disappointing, for, though necessarily to a large extent a compilation, it lacks more than need be that personal touch of the author in selection and presentation which the student so much appreciates. Such is the feeling left by a careful reading of the work. The subject is taken in four parts:—(1) the manufacture of steel; (2) reheating; (3) the mechanical treatment of steel; (4) finished steel. It inspires confidence that the author as a metallurgist has induced the well known metallurgical engineer, Mr. J. W. Hall, to write part iii., and to join with him in the chapter on reheating.

The Bessemer processes, acid and basic, and all their modifications are very well described, and illustrations of various historically interesting as well as typical modern forms of converter are given, in many cases as working drawings with dimensions. The small converters for surface blowing have a special chapter to themselves, and the best known forms are described. It is well that it is so, for they bid fair to revive a little the fading glories of the Bessemer process by their suitability for the making of steel for castings.

The general scheme adopted is to describe the apparatus, then the process, next the reactions of the process, and lastly the thermochemistry, a method which involves some repetition but makes reference easy. The open hearth is similarly treated, such special furnaces as the Siemens new form, the Campbell, and the Wellman tilting furnaces being illustrated in great detail by means of folding plates.

The somewhat sensational Talbot process is carefully considered with the author's special facilities for exact knowledge in this matter. The chapter on steel castings is disappointing, and will serve to illustrate the feeling mentioned above. Few will admit that the beneficial effect of silicon and manganese on castings is due to their removing oxidising gases, or that aluminium in the quantities used increases the fluidity or removes the dissolved oxide of iron. The statement that annealing hard castings counteracts their tendency to fly when cooling is obscure, while the full table of Prof. Arnold's recent results on castings is given without any warning that these results are the basis of a research series, and that the steels are not suitable for commercial work, a point most clearly stated in the original. The weight of Prof. Arnold's authority on practical matters, combined with the relative space taken up by the table, will certainly tend to mislead the student here. The chapter finishes with three tables of three, two and two tests respectively, showing the effect of annealing, &c., yet from a remark in the text, the last two appear to be forgings.

The chapter on crucible steel is difficult to estimate, as to anyone acquainted with the innermost workings of the old crucible steel trade, with its meagre literature, it is almost impossible to judge as to how much a writer might reasonably be expected to know. To the general reader it will be sufficiently interesting, while the beginner in a works could point out many flaws. On entering the gate on a morning the author would find that blister bar is not "cut up," but broken with a hand hammer, giving out quite a musical series of notes as the bars become shorter. The crucible shown would be difficult of manipulation by "the teemer," the real "Sheffield pot" having a well designed and quite artistic shape. The sulphur does increase in melting, and a careful watch must be kept on the quality of the coke, or the rise will be serious, even in high carbon steels, where the carbon, according to the author, expels the sulphur, which somehow in practice it fails to do. The increase of phosphorus, if any, is not detected in ordinary working.

In part iii., on the mechanical treatment of steel, by Mr. J. W. Hall, the excursions into theoretical matters of pure metallurgy are not always happy, but the other parts are treated as one would expect from an engineer of his enthusiasm and experience. The development of various types of mills, examples of modern plant, forging by the hammer and by the press, all seem excellently treated, while the case for and against fluid compression is made very clear. Several Sheffield firms, however, make high speed steels, generally acknowledged to be much more than "nearly equal to those at Bethlehem."

Part iv., by Mr. Harbord, on finished steel, treats of the metal steel itself, its mechanical properties, the relations of iron and carbon, influence of other elements, effect of heat treatment, and the microscopical examination of steel. Mechanical testing makes a good chapter, but why use the erroneous term "tensile strain" instead of "maximum stress"?

To the chapter on iron and carbon many will eagerly turn, because of the paramount importance of the subject in everyday work, its great historical interest, and it may be also because of recent controversy. Perhaps, therefore, one expects too much, but it must be confessed that the author does not seem to have risen to the occasion, and gives only a not too excellent compilation where one expected a sorting out and a grappling with the question. The well known diamond and iron *in vacuo* experiment is taken as proof that carbon can be transferred to iron without the intervention of gas, whereas it is now well known that steel which has ceased to give off gas even at 1000° C. will give off more if heated to 1200° C. After a description of temper (or annealing) carbon, "the existence of this form of carbon has not been confirmed by other investigators" is a rather startling statement. The research on "The Influence of Carbon on Iron," published by Prof. Arnold in 1895, and acknowledged by all to be a classic, is quoted, and with it some recent work of the author's own which only seems to obscure the subject. The author hardly deals fairly with his readers in withholding the tests of his steels as received, and the first three are given here (see sixth report Alloys Research Committee) side by side with ordinary commercial samples taken at random. It will be clear that they are a very undesirable series as a basis for such a research. The three numbers represent maximum stress in tons per square inch, elongation per cent. on two inches, and reduction of area per cent. respectively:—

Harbord ...	30	20	35	Harbord ...	33	13	25
Commercial	30	30	60	Commercial	35	28	47
Harbord ...	31	13	30	Harbord ...	37	6·3	19

The author seems almost alone at the present date in the opinion that "hardening carbon is possibly merely free carbon dissolved in iron," as even Stansfield, Osmond and Stead all favour the idea of carbide dissolved in iron, and the author ought to have told his readers this. He also says that the carbon theory does not explain the critical points in pure iron and loss of magnetism, but surely he must know that the main function of the carbon theory is to explain the hardening of steel. He is hardly up to date on some important matters, as he takes Arnold's mere suggestion of long ago as to Ar_3, and leaves the student to think that this is the present well defined position, whereas both sides are agreed as to Ar_1 being the real carbon change point, and to carbonists it represents the formation or decomposition of a substance corresponding to the formula $Fe_{24}C$. All are agreed also that the purest iron shows Ar_3 and Ar_2, and Prof. Arnold's theory with regard to Ar_2 is quite sufficient to account for the disappearance of magnetism. The author should remember that the serious question is this, "Does a flint hard allotropic iron exist?" The allotropists are more happy than the carbonists in that they have a crisp explanation for Ar_3, if the use of another Greek letter as a prefix can be said to give satisfaction to any practical worker in the field. The solution theory is given in detail, but the author wisely dismisses the application of the phase rule to the problems of steel by reference to original papers, and the student who endeavours to follow these is in need of sympathy if he be well acquainted with the known facts.

The extremely difficult subject of the influence of various elements on steels is well considered, but the author implies that Le Chatelier was the pioneer in our knowledge of sulphide in steel, while everyone should know that that honour belongs to Prof. Arnold. Heat treatment of steel is discussed in twenty-seven pages, and the last chapter deals with the microscopical examination of steel, and several methods of preparation and etching are well described, but Fig. 448 should either be altered to suit opaque objects or removed.

The volume closes with 100 photomicrographs, four useful appendices, and a good index. The structures shown in several of the photomicrographs are not in accord with the writer's experience, but that might be due to abnormal crystallisation in the original steels, which seem to be identical with those used for the sixth report already mentioned. Several errors have been noted, but there is only room to indicate a few as examples:—p. 53, "wild metal which pipes in the moulds"; p. 101, incorrect definition of a heat unit; p. 227, the hardening power of liquids is said to be a function of their specific heats, whereas their conductivities are more important, as witness mercury compared with water; "microphotograph" all through the work instead of photomicrograph. On p. 680 0·8 carbon steel is indicated as saturated, while on p. 681 it is 0·9, and the footnote to p. 684, "The latest research has shown that it should be 0·9," gives a wrong impression. It had no need to be shown after 1895, as Arnold made it quite clear then, and others, perhaps working on impure steels, claimed 0·8, but now they have seen their error and 0·9 is accepted almost universally. On the whole, however, the book is to be recommended as the best available on the metallurgy of steel.

A. McWILLIAM.

FROM THE ANGLER'S POINT OF VIEW.

Trout Fishing. By W. Earl Hodgson. Pp. xviii + 276. (London: A. and C. Black, 1904.) Price 7s. 6d. net.

Fishing Holidays. By Stephen Gwynn. Pp. ix + 299. (London: Macmillan and Co., Ltd., 1904.) Price 7s. 6d. net.

An Angler's Year. By Charles S. Patterson. Pp. xii + 192. (London: W. R. Russell and Co., Ltd., n.d.) Price 2s. 6d.

THE first two of these books are not in any sense books of reference or guides for the angler; Mr. Gwynn frankly states that his object is not instruction but amusement, but it is no ground of complaint that the former as well as the latter is to be found in his descriptions of his fishing holidays. Mr. Hodgson's is a pleasantly trivial book, interesting as giving the views of an experienced fisherman on many points, but no more instructive, in fact, than Mr. Gwynn's in intention. The former is at his best when describing matters of his own observation; "the whustler" would take a lot of beating as a piece of pure narrative, and is almost on a level with Mr. Gwynn's best; it calls for equal admiration in the vigour with which an almost Homeric battle is described, and the delicacy with which a veil is drawn over the undignified end of a noble fish, but it is scarcely possible to extend this admiration to the delicacy with which twenty-one of the author's friends and a daily newspaper are veiled in the obscurity of initialled dashes, which are frequently inadequate as a disguise and always typographically unsightly.

Mr. Hodgson deserves great praise for his effort to figure adequately in colours a series of trout flies, and the result is really very pleasing; we wish we could add really successful, but it seems very doubtful whether the three-colour process is suited to this class of work; the reds, and especially the clarets, are not satisfactory, and a comparison of the different representations given of, *e.g.*, the cow-dung, olive dun, or black gnat seems to show that sufficient accuracy for work of this nature cannot be obtained by the process employed. The excellent reproduction of a picture of a group of brown trout given as a frontispiece may almost serve as a contrast to the figures of flies to show the class of subjects well and ill suited for illustration by this method. It would have been interesting to have had more explanation in the book itself of the flies figured and the reasons for their selection, especially from so ardent an advocate of the wet fly as Mr. Hodgson.

Mr. Gwynn's book is most delightful; we have read much of it before in various periodicals, but nothing is lost in reading it again in book form, and the print and general get-up are so good as to give an additional pleasure to the reader. The proverb which Micky applied to the author's efforts to catch a salmon—to misquote it—*Is fada do leabhar gan bradan,* cannot in any sense be applied to his efforts to write a book; it is the book that is too short, and there is a wonderful store of really useful information not only as to salmon, but as to trout and, in one excellent essay, pilchards.

Unlike Mr. Hodgson's book, Mr. Patterson's "An Angler's Year" contains a large amount of information which should be of the greatest assistance to the beginner. The method by which the author deals with his subject is good; he selects typical days from each month in the year (except March, which he not unfairly regards as "the silly season of angling"), and describes actual experiences of his own, illustrating them with information as to the best gear and method of using it in each instance. Without ever becoming didactic, Mr. Patterson gives a great deal of most useful advice upon many forms of fishing, and is equally interesting whether he treats of trout or conger. There is one addition which would, we think, be appreciated in any future edition, and that is an index, and it really seems an undue economy of space to print advertisements on the back of the title-page and table of contents; still, these are but details (as is the quaint misprint which causes the pike to figure as *Essex lucius*), and in no way affect the value of what appears to us a very practical and useful little book.

It has lately been suggested that there is nowadays too great a tendency to attribute human characteristics to animals; the fisherman certainly tends to attribute them to fish; Mr. Patterson expresses a conviction that the Test trout know more than the anglers; Mr. Hodgson combats at some length the views of those who hold that trout are cunning; both are at issue with Sir Herbert Maxwell as to a trout's sense of colour. The task of approaching the presumed feelings of a fish—especially with a view to deceive—without attributing to it some almost human qualities, even as Mr. Patterson attributes the cunning of the carp to the size of its brain and the fulness of its years, is not easy; there is a tendency almost automatically to put oneself in the place of the fish and to try to look at the world from that standpoint, and to do this one must, to some degree, give the fish human views. Our fish are certainly more interesting a little humanised, and one can feel a real sympathy for M. Guitel's goby and his efforts to find a mate which a mere bald narrative of facts would not evoke; but in reading books on fishing one cannot help wondering whether it is really the fish or only the fisherman who likes some peculiarly compounded paste or some particular tying of a favourite fly. Somehow, while feeling sure that Mr. Gwynn and Mr. Hodgson are right in insisting on the importance of the size of fly used, we yet feel some suspicion that it is the former author and not the fish he angled for that had no taste for worms.

L. W. B.

OUR BOOK SHELF.

Betrachtungen über das Wesen der Lebenserscheinungen. Ein Beitrag zum Begriff des Protoplasmas. By Prof. R. Neumeister. Pp. iv + 107. (Jena: Gustav Fischer, 1903.) Price 2 marks.

THIS is an essay—critical and constructive—on the mechanical and vitalistic interpretations of the phenomena of life. Biology has oscillated from the one position to the other since the days of Harvey. Some progress in the physico-chemical analysis of an abstracted part or process of the organism is made, and hope rises in the biologist's breast that the secret of life is going to be discovered. Always, however, residual phenomena are detected, and there is a retreat to some form of vitalism. Prof. Neumeister gives a scholarly survey of the history, expounding the positions of Johannes Müller, Von Baer, Lotze, Du Bois-Reymond, Fechner, Wundt, Bunge, and many more. His own position, which closely resembles that of Johannes Müller, may be briefly stated as follows:—Truly vital phenomena cannot be interpreted in terms of physico-chemical categories; life is an inter-relation of the physical and the psychical—an inseparable, unknowable inter-relation; there are no forces operative in protoplasm which are not operative in non-living matter, but in all active protoplasm there are psychical qualities of a transcendental character.

Biologists will probably be most interested in the section of the book that deals with protoplasm, and the many conceptions of it that have been suggested, *e.g.* by Nägeli, Kühne, Bütschli, Pflüger, Pfeffer, Verworn, Hofmeister, Hertwig, and Ostwald. Neumeister deals at especial length with the Hofmeister-Ostwald theory, which practically reduces metabolism to a series of fermentations. As a chemical physiologist the author attacks this theory with might and main, and comes to the conclusion that ferments have really nothing to do with the essential activity of protoplasm, their activity is intracellular, not intraprotoplasmic, they are only the "chemical tools" made by and used by protoplasm. What then is protoplasm? A peculiar chemical system of very diverse protein-substances, along with certain other compounds the molecules of which by a unique interaction give rise to psychical and material processes quite inseparable from one another, in a way that we cannot hope to understand. "Ins Innere der Natur dringt kein erschaffener Geist."

J. A. T.

The Fat of the Land. The Story of an American Farm. By J. W. Streeter. Pp. xi+406. (New York: The Macmillan Co.; London: Macmillan and Co., Ltd., 1904.) Price 6*s*. 6*d*. net.

MANY ways have been adopted of teaching agriculture, but we do not think we have before met with an account of the management of a farm thrown into the form of a tale—a romance some readers would be unkind enough to call it. The book describes how an American doctor, warned for reasons of health to abandon a city life, purchased a neglected farm and by a liberal exercise of capital, energy and business capacity, made it both pay its way and provide him at the same time with health and pleasure, so that the family all lived on "the fat of the land." The main text is sound enough, that the farm should be regarded as a factory converting raw material into finished products and that skill and knowledge can always find a satisfactory market by the production of the best, but we doubt if the demonstration will prove convincing or even suggestive to the practical man.

The book reminds us irresistibly of the "Swiss Family Robinson," and bears about the same relation to agriculture as that friend of our childhood did to serious natural history.

Die Dissoziierung und Umwandlung chemischer Atome. By Dr. Johannes Stark. Pp. vii+57. (Braunschweig: F. Vieweg und Sohn, 1903.) Price 1.50 marks.

THIS little book from the fluent pen of Dr. Stark, of Göttingen, is a reprint of three articles in the *Naturwissenschaftliche Rundschau.* Its object is to exhibit a comprehensive view of the application of the electron theory to the group of phenomena which may be characterised as subatomic transformations, and to do this in terms which may be understood by any person of intelligence. On the whole this object is successfully accomplished.

The author shows how the discovery of Röntgen rays and of the Zeeman effect, together with the determination of the mass of the particles forming the kathode rays, have led, in the hands of J. J. Thomson, to an entire change in our ideas of atomic structure. He follows out the bearing of this idea on the phenomena of conduction in metals, in solutions and in gases, and shows how the brilliant researches of Rutherford and of Rutherford and Soddy on radioactivity led them to consider that this phenomenon was caused by the transformation of one element into others, a result which was finally established by the discovery of Ramsay and Soddy that the radium emanation turned into helium.

The book is clearly written, and its value is increased by a chapter of references at the end. It may confidently be recommended to all interested in the recent developments of physical theory.

O. W. R.

Nature's Story of the Year. By Charles A. Witchell. Pp. xii+276; illustrated. (London: T. Fisher Unwin, 1904.) Price 5*s*.

"OBSERVERS of Nature," says Mr. Witchell in his preface, "belong to one of two classes—the scientific and the imaginative." Mr. Witchell himself belongs to the latter category, for, to make use of his own words, he depicts "some curious incidents in Nature in a frame of imaginative colouring." The book will probably give readers a general interest in natural phenomena, for there is no attempt systematically to describe the plant and animal life to be found in the country at different seasons of the year. The author directs attention to anything that happens to have impressed him, and his facts and fancies are expressed in pretty terms.

Essays and Addresses. By the late John Young, M.D., Regius Professor of Natural History in the University of Glasgow. With a Memoir. Pp. xlii+145. (Glasgow: James Maclehose and Sons, 1904.)

THIS small collection of essays and addresses is issued by the committee in charge of the memorials of the late Prof. Young. The biographical sketch with which the volume commences is by Dr. Yellowlees, and it is a pleasing narrative of a well-filled life. The history of the years when Young was on the Geological Survey is particularly attractive, though throughout the narrative the reader is impressed with Young's untiring energy. The committee has selected the following essays and addresses for publication:—"Three English Medical MSS.," "A Discourse," "The Making of a Book," "The Scientific Premonitions of the Ancients," "Jewish Mediciners," and the "Address on the Hunterian Library."

The Globe Geography Readers. Senior. Our World-wide Empire. By Vincent T. Murché. Pp. 392. (London: Macmillan and Co., Ltd., 1904.) Price 2*s*. 6*d*.

THE latest of Mr. Murché's books is one of his best. It provides a simple, interesting account of the countries and peoples of the British Empire which should make the boys and girls who study it interested in different parts of the world. The volume is profusely illustrated with sixteen full-page coloured plates and an unusually large number of black and white pictures There is no rigid adherence to geographical information alone; the historical facts necessary to make up a complete description of a country are included judiciously.

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 Disaster to Submarine A1.

AT the inquest on the victims of the disaster to submarine A1, Commander Bacon is reported to have expressed the opinion that as the result of the collision every soul on board was instantly stunned, since the failure to set in action the mechanism for bringing the boat to the surface could not otherwise be accounted for. It is surprising that this opinion should have been received and adopted without comment by both the coroner and the lay Press, seeing that such a result is contrary to all experience of collisions at sea. The occupant of the conning tower, which was the part struck, was no doubt stunned, probably killed, by the blow, but it is difficult to believe that the same fate should have befallen every other person on board, however remote from the point of concussion.

The fact that the naval authorities can suggest no other reason for the failure to rise to the surface after the collision is not in itself a sufficient justification for the acceptance of an opinion which, from the physiological point of view, is, to say the least, highly improbable, and certainly requires confirmation by experiment.

University, Edinburgh, May 1. E. A. SCHÄFER.

The Life-history of Radium.

EVIDENCE of a convincing nature is rapidly accumulating to the effect that helium may be produced as a result of the disintegration of the radium atom. On the other hand, it has been suggested by Rutherford and others that radium is analogous to the first products of the disintegration of uranium and thorium—to the substances known as uranium X and thorium X—rather than to those elements themselves. Such an idea points to a search for the parent atom, by the dissolution of which radium is formed.

In Prof. Rutherford's recent book on radio-activity, reasons are given for suspecting that in uranium itself we shall find the origin of radium. The atomic weight of uranium is greater than that of radium. Radium is discovered in minerals rich in uranium, and the amount of radium in good pitchblende is about that to be expected on the view that a balance exists between the rate of development of the radium by the uranium present and the rate at which it decays by the ordinary process of radio-activity.

My wife and I have been investigating lately the slight amounts of radium emanation that are almost invariably found in samples of salts and oxides of uranium sold as chemically pure. By the kindness of Mr. H. J. H. Fenton we have been able to examine several specimens of uranium compounds, known to have been preserved in the Cambridge University Chemical Laboratory for periods of from seventeen to twenty-five years. In all cases greater amounts of radium emanation have been obtained from these old specimens than from more recently prepared samples of the corresponding compounds.

It is, of course, possible that a limited number of such results may be accidental, and, in order that indirect evidence of this kind should possess any weight, enough specimens must be examined to enable us to deal with the subject statistically. I should be very grateful if anyone possessing uranium compounds of known pedigree, prepared thirty years ago or upwards, would either test them quantitatively for radium emanation, or send a few grammes of them to me for examination.

If, in most cases, an excess of radium is discovered in the older samples, it would be presumptive evidence in favour of the view that radium is formed by the disintegration of uranium, but the possibility of some general change in the methods of preparation of uranium salts renders even such a confirmation of doubtful validity.

The only convincing evidence would be supplied by tracing the gradual growth of radium in a mass of a compound of uranium. At first sight, it would seem that the time required for such growth would put the possibility of such a confirmation beyond the reach of one human life. But a short calculation shows that the attempt is not so hopeless as might be imagined.

The average life of a radium atom is taken by Rutherford, on a minimum estimate, as about fifteen hundred years. The process of decay occurs in a geometrical progression, and thus in one year about half a milligramme per gramme of radium should disintegrate. On a maximum estimate for the life, the fraction disintegrated per year is 1/100 milligramme. Taking this maximum estimate as the least favourable for our purpose, we see that in one year the one hundred thousandth part will break up.

If in pitchblende, radium is in radio-active equilibrium with its source of supply, the same fraction must be replaced in the year by the disintegration of uranium. In presence of a large excess of uranium, the production of radium would go on at a constant rate. Thus in one year about the one hundred thousandth part of the proportion of radium in pitchblende would be developed in an equivalent mass of uranium.

We find that, using a good electroscope, it is easy to detect with certainty the radio-activity from the radium emanation evolved on heating a milligramme of good pitchblende. In order to produce from uranium an amount of radium large enough to detect by its radio-activity in a reasonable time—let us say one year—it is merely necessary to work with a sufficient quantity of uranium to give, in that time, a mass of radium of which the emanation has an activity equal to that evolved from a milligramme of pitchblende. The requisite quantity of uranium is clearly about $0{\cdot}001 \times 100000 = 100$ grammes. This, as we said, is a maximum estimate; it is probable that less would suffice.

In this manner, by putting on one side a few hundred grammes of some compound of uranium, carefully freed from radium and tested for emanation, it should be possible to detect the growth of radium in a time measured in months, or, on the other hand, to show that it is necessary to look elsewhere for the parent atom of radium.

At the present time we have such an investigation in progress, and trust that eventually we may obtain definite results. But, in the hope that others may undertake a similar task, I venture to place the principles of the method before your readers. On such a fundamental point, several independent experiments are greatly to be desired.

W. C. D. WHETHAM.

Upwater Lodge, Cambridge, April 30.

Graphic Methods in an Educational Course on Mechanics.

THOUGH no one, I venture to think, will gainsay Mr. W. Larden's main contention that "analytical methods give a grasp of the principles of statics, while graphical methods disguise them," yet it should not be forgotten that the analytical treatment has its own set of snares and pitfalls.

Mechanics is a physical science, and like other sciences should be approached from the experimental side. If the initial stages are treated experimentally, the principles underlying the subject will come prominently into view. One need only mention the principle of moments, which every boy has surely grasped, in a general sort of way, long before he has opened a text-book on statics. He has only to carry out a few simple experiments on levers to find out the law for himself in its exact form. Let the beginner hang up two spring balances from nails and then attach a weight by a couple of strings to the hooks of the balances, and he will soon discover for himself whether or not the pulls in the strings are proportional to their lengths.

The graphical treatment lays stress on the empirical and tentative side, which in the symbolical is completely lost sight of. But the superlative advantage of graphical work is its essentially practical character. All cases of a problem can be solved with equal facility. Ladders are not as a rule inclined to the ground at an angle of 60°, coefficients of friction are never quadratic surds, and weights of $\sqrt{2}$ poundals belong to some other world which is not the one in which we live. Again, the question is on a screw jack, and a boy taking $\pi = 22/7$ has worked out an answer to four or five significant figures, and in consequence expects to get greater credit than his more indolent neighbour who has been content with two or three significant figures. Instances might be multiplied; they constitute the daily purgatory of every teacher. Something surely is to be said for a method which avoids these absurdities.

Analytical methods have so dominated the elementary text-book that many boys have the idea that statics is practically useless. They have no notion, for instance, that graphic statics lies at the foundation of bridge construction. Besides, in how many questions in the elementary text-book is the principle involved wholly obscured, because a trigonometrical conundrum is required and not an application of the conditions of equilibrium to give the unknown forces? In a popular text-book one-third of the questions at the end of one of the chapters are of this character. Is it to be wondered at that the average boy gets the idea that mechanics is a subtle epilogue to trigonometry?

Each question treated graphically should be regarded in the light of an experiment, in which the student should get the best result available with the means at his disposal. In any actual problem the *data* themselves are not correctly known, and the *quaesita* are therefore subject to all sorts of cumulative errors. This he quickly finds out by comparing his result with that of his neighbour, and he readily gets a notion of the degree of accuracy that he himself with pencil and ruler is capable of.

Mr. Larden writes:—"a student well trained in analytical methods can always pick up graphical methods rapidly when he needs them for special work." But will he do so? The engineer is not trained in analysis and allowed to adopt a graphical method when a specific problem arises. My experience is that the student, who has mastered analytical methods, is apt to consider graphical work as drudgery, and when called upon to solve a question graphically does not treat it with sufficient respect, and gets an indifferent result. A certain amount of finesse and judgment in choice of scale and of position of the initial force or load is required "to fit the diagram on to a given sheet of paper." This can be acquired only by practice.

Unfortunately it is too true that "graphical work consumes an amount of time that seems out of proportion to the mental training and knowledge of principles gained," but only when applied to too many similar questions. This, however, is misusing, not using the method.

I believe the best results will be obtained when the two methods are used side by side. They are strictly complementary, and the merits of each supply the deficiencies of the other. R. M. MILNE.

R.M. Academy, Woolwich.

Asser and the Solar Eclipse of October 29, 878.

UNDER the date DCCCLXXIX, Asser, in his "Life of King Alfred," gives the following entry:—"*Eodem anno eclipsis solis inter nonam et vesperam, sed propius ad nonam, facta est.*" The oldest manuscript of the Anglo-Saxon Chronicle also notes an eclipse in 879, but it cannot be doubted that in each case the reference is to the eclipse of October 29, 878, which was total in South Wales and southern England. Particulars of the eclipse are given by Mr. Maguire in the *Notices* of the Astronomical Society, vols. xlv., 400, and xlvi., 26. The sun rose totally eclipsed in 73° N. and 42° 8′ W. at about 9.53 local time, and the central line of the eclipse, after passing near Dublin, Aberystwith, Dover and Fulda, went off the earth at sunset about 130 miles south of Moscow at 4.20 local time; St. David's, Winchester and London were within the limits of totality. With regard to the hour of the eclipse, it is needful to consider not only mean time and apparent time, but also natural time, which was the kind of time then in use, according to which the period between sunrise and sunset was conceived to be divided into twelve hours, which were, of course, much shorter in winter than in summer. As the sun rose at London on the day of the eclipse about 7.20, the natural hour would have contained only about 47 minutes of mean time. Mr. Maguire gives the middle of the eclipse at St. David's about 1.12, and at London about 1.18 mean time, and subtracting the equation of time, about 15 minutes, we have 12.57 and 1.3 for the apparent time as shown by a sundial; correcting for natural time, we obtain 1.13 for St. David's and 1.20 for London. Finally, making allowance for the difference of longitude, we see that totality occurred at St. David's at 12.46, and at London at 1.20, according to local time as shown by a waterclock, or some other time-keeper, properly regulated to mark the natural hours. We now have to consider what Asser meant by *Nonam* and *Vesperam*. Those who have written about the passage have taken *Nonam* to be identical with *Nonam Horam*, but probably they have not been right in doing so. It is shown in the "Dictionary of Christian Antiquities" (i. 793) that the day and night were divided into four equal parts, and that each quarter of the day was named after the last hour in it. "None embraces the seventh, eighth and ninth hours; and the last called Duodecima contains the tenth, eleventh and twelfth, ending at Sunset." Asser, however, evidently uses *Vespera* for *Duodecima*. *Nona* is, in fact, noon, the point when the sun is on the meridian, the beginning of the seventh hour, and *Vespera* is the point half-way between noon and sunset, in this case 2.20 mean time and 3.0 natural time. Thus what Asser says is this, that the eclipse was total at a point of time between noon and 1.30 natural time, and we see that the statement is true for any point in England or Wales. If we could be sure that the sentence about the hour of the eclipse was written by Asser of St. David's, it would be a very strong argument, indeed, for the genuineness of the book which is called by his name, for it fixes the moment of the eclipse correctly to within seventy minutes of mean time for any place at which it is possible that the book could have been written. C. S. TAYLOR.

Banwell Vicarage, April 23.

"Abdominal Ribs" in Lacertilia.

IT is usually stated in text-books that among living reptiles only the Crocodilia and Hatteria are furnished with abdominal ribs or parasternum: that is, of course, in the condition of thin pieces of bone lying between the ventral muscles and underlying the true ribs, for no one doubts that the plastron of the Chelonia is the same structure exaggerated. There has been some little confusion between the abdominal ribs and the ventral moieties of the true ribs in Lacertilia, which is cleared up by Dr. Gadow in his contribution to the "Cambridge Natural History." Dr. Gadow correctly observes of the geckos that they possess very long and slender post-thoracic ribs, "which meet each other in the middle line, in this case bearing an extraordinary resemblance to the so-called 'abdominal ribs' of other Reptiles." The statements as to "abdominal ribs" made by M. Boulenger in his catalogue of the lizards in the British Museum appear to me to refer to true ribs. Of the Scincidæ, he remarks that "ossified abdominal ribs are absent." Curiously enough, it is precisely in this group that I find a parasternum. In *Tiliqua scincoides* the ventral musculature is divided by the usual tendinous septa into successive "myotomes," the tendinous intervals being distinctly ossified; there are several pairs of these bonelets which seem to be exactly like those of Hatteria, with which I have compared them. That they are not the ventral moieties of the true ribs is shown by the fact that they overlap the latter, the two series of structures lying at a different plane in the musculature. I intend to make a more detailed communication to the Zoological Society upon this subject immediately. FRANK E. BEDDARD.

Inheritance of Acquired Characters.

REGARDING the "non-inheritance of acquired characters," the following is interesting:—

I was recently visiting a sugar plantation near Ottawa, Natal, and there was shown four fox terrier pups about a fortnight or three weeks old, two of which had been born with quite short tails, and one with a tail shorter than the normal. The fourth pup had a full-length tail. The mother was an ordinary fox terrier with cut tail. When the circumstance of these dogs being born with short tails was first mentioned to me I refused to believe it; but examination showed that the short tails were really naturally short tails and not tails that had been cut, that is to say, the short tails had at their ends the usual tapering vertebræ of a normal dog's tail, and, of course, at this age it was easy to see that the tails had not been cut or bitten off.

Cape Town, April 7. D. E. HUTCHINS.

THE POPULARISATION OF ETHNOLOGICAL MUSEUMS.

SPEAKING broadly, museums may be divided into two main classes, (1) those that are designed to interest and instruct the general public, and (2) those that are intended for specialists. Difficulties and misunderstandings arise when these two objects are not kept apart. The casual visitor is impressed, but scarcely edified, by long series of named specimens, and the specialist does not need popular descriptive labels, but he does require a large number of specimens. The problem that is now before most of our large museums is the conflict of these two interests. Probably the most satisfactory solution will be found in keeping these two classes of collections quite apart. Dr. F. A. Bather, in his suggestive and practical presidential address to the Museums Association (*Museums Journal*, vol. iii., 1903, pp. 71, 110), said, "the functions of museums are three: Investigation, Instruction and Inspiration appealing respectively to the Specialist, the Student, and the Man in the Street. These functions are so distinct that they are best carried out if museums, or the collections of a single museum, be classified on these lines. Such an arrangement is a saving of trouble and expense, and each division can thus be directly adapted to the class of visitors for which it is intended."

The specialist needs all the specimens he can get in a building where they can be safely housed and be readily accessible; he asks for facilities, not for architecture. If once this were fully realised a considerable amount of unnecessary expenditure could be saved. There are many objects that should be preserved for future generations which are neglected by museum curators because they cannot afford to store them, but there would be less excuse for this neglect if the cost of storage could be greatly reduced. At the Liverpool meeting of the British Association Prof. Flinders Petrie advocated the erection of a repository for preserving anthropological or other objects; an outline of his scheme was published in the *Report*, 1896, p. 935, and to the present writer it appears that something of the kind will have to be adopted by most countries, and the sooner this is done the better will it be for science, as objects that should be preserved are continually perishing or are discarded from lack of space in which to house them.

The general public provides most of the funds for the establishment and maintenance of museums, and it may very well insist on having something for its money that it can understand. A museum can be made into an institution of very great educational value without loss of attractiveness if some trouble be taken and if funds are available, and it is very probable that funds would be available if the results were such as could be appreciated by everyone. Our Natural History Museum at South Kensington has set a fine example of what can be accomplished in the way of well mounted birds in their natural surroundings. Probably lack of space and funds has prevented the authorities of the Natural History Museum from constructing large groups of mammals similar to those which form such a splendid feature of the Field Columbian Museum of Chicago, and to a less degree of the American Museum of Natural History, New York.

The pleasure and instruction afforded by the realistic mounting of groups of animals are undoubtedly very great, and not less so are those caused by analogous ethnological groups. The present writer had his first interest in ethnology awakened by the excellent modelled groups of natives in the Crystal Palace, and the wonder and delight these gave to the small boy have never been forgotten. Various museums at home and abroad possess individual figures dressed in appropriate costumes, but it is again to the United States that we have to turn for the most effective development of this art. There are several first class groups of American natives in the American Museum of Natural History, others are to be found in the Field Columbian Museum; especially noteworthy in the latter museum are the groups illustrating the rituals of the Hopi Pueblo Indians, to which the attention of the readers of NATURE was directed a short time ago (NATURE, vol. lxvii., p. 392), and a wonderful case illustrating the domestic industries of the Hopi. It was once the writer's good fortune to be in the company of a couple of Navaho Indians who saw these models for the first time; they could not mask the interest they felt in seeing these representations of their neighbours, and great was their delight in noticing that the model of a particular woman, whose face they recognised, had, like her original, an amputated finger.

FIG. 1.—A Cocopa Indian family of the Sonoran ethnic province, Lower Colorado River, Mexico. They subsist largely by means of agriculture, feeding partly on game and fish, with various seeds, roots and fruits. They dwell in scattered settlements. The men wear skins and the women petticoats made of the inner bark of the willow.

The high-water mark at present reached in this direction is in the dozen groups of lay figures designed by Prof. W. H. Holmes, and first exhibited in the Pan-American Exposition in Buffalo, 1901, to which reference has been made in these pages, and which are now in the National Museum at Washington. These groups present in the most striking manner possible a synopsis of the American aborigines, from the

Eskimo of North Greenland to the wild tribes of Tierra del Fuego. Each lay figure group comprises from four to seven individuals, selected to convey best an idea of the various members of a typical family. The activities of the people are illustrated, and the various products of industry are, so far as possible, brought together in consistent relations with the group. No one who has seen these splendid groups can doubt that this is the best way of illustrating the more salient features of ethnology, especially when these are supplemented, as in Prof. Holmes's scheme, with models made to scale of habitations and of boats, with a limited selection of objects made by the various people, and illustrations of their more important physical characters, such as crania, casts from life, and pictures. An exhibit such as this for all the more important groups of mankind would be of extreme interest and educational value, and would meet all the requirements of the public. If this arrangement were carried out the great bulk of ethnological material, which takes up so much space in large museums, need not be exhibited to the casual visitor.

There are two methods of constructing the lay figures of ethnological groups. The one is to make casts of actual individuals, and the other is to have effigies made by a sculptor. The Chicago groups are examples of the former method, but the Washington groups were made in the following manner:—"The sculptors were required to reproduce the physical type in each instance as accurately as the available drawing and photographs would permit. Especial effort was made to give a correct impression of the group as a whole, rather than to present portraits of individuals, which can be better presented in other ways. Life masks, as ordinarily taken, convey no clear notion of the people; the mask serves chiefly to misrepresent the native countenance and disposition; besides, the individual face is not necessarily a good type of a group. Good types may, however, be worked out by the skilful artist and sculptor, who alone can adequately present these little-understood people as they really are and with reasonable unity in pose and expression."

FIG. 2.—A dwelling group of the Pawnee Indians, a type of the Missouri Valley region. The Pawnee formerly lived in Nebraska. Although their home is in the country of the skin-tent dwellers, they continue to build the ancient northern type of earth-covered aboue with slightly sunken floor.

These groups and the other ethnological exhibits prepared under the direction of Prof. Holmes are figured and described in the annual report of the U.S. National Museum for 1901, published by the Smithsonian Institution in 1903. In the same volume will be found Prof. Holmes's views on the classification and arrangement of the exhibits of an anthropological museum. This essay, which will prove of considerable value to those concerned in this class of work, was previously published in the *Journal of the Anthropological Institute* (vol. xxxii. p. 353).

In his address Dr. Bather dealt mainly with art museums, but he alluded to folk museums, and Mr. Henry Balfour, in his recent presidential address to the Anthropological Institute, advocates the establishment of a national museum to illustrate the evolution of culture in our islands; he, like Dr. Bather, instances what is done in this respect in Scandinavia and Germany. Certainly this is much needed in our country, and immediate steps should be taken to realise it; already much has irrevocably been lost, as there was no institution that cared to preserve the relics of former conditions. In the same address Mr. Balfour gives some valuable suggestions for the arrangement of ethnological museums. Mr. Balfour's address will be printed in the forthcoming number of the *Journal of the Anthropological Institute,* and it will be found to be well worth perusal, as it embodies the long experience of a well-known expert in museum arrangement. It is to be hoped that the time may not be far distant when the educational value of properly arranged ethnological museums will be recognised in this country, and the means will be found to establish them.

A. C. HADDON.

ROUND KANCHENJUNGA.[1]

THIS work of Mr. Freshfield's on a tour round Kanchenjunga comes as a very welcome addition to the literature that deals with the great mountain peaks of the world. Kanchenjunga (28,150 feet) is the third highest measured peak on the earth's surface, Mount Everest being 29,002 feet, and K^2, in the Karakoram range north of Kashmir, 28,278 feet high. At present Mount Everest is hopelessly impossible of access, being in Nepal, a country entirely closed to Europeans; K^2 also lies so far removed from civilisation that it takes weeks of travelling, many days of it over glaciers, to arrive even at its base.

Kanchenjunga, however, can be seen from Darjeeling, and the view of the peak from that place is one of the grandest sights in the world. Kanchenjunga and its attendant peaks form a solitary group of mountains, which divides the province of Sikkim from eastern Nepal, and lies far south of the watershed of the Himalaya.

It is now many years since Sir Joseph Hooker in 1848–1850 made his famous journeys into the country round Kanchenjunga, and obtained leave from the Government of Nepal to travel in the Nepalese valleys on the west and south-west of Kanchenjunga. This leave has never been repeated, and it was not until Mr. Freshfield and his party descended the glaciers on the north of Kanchenjunga and trespassed in the Kanchen valley that Englishmen again set foot in this forbidden land.

[1] "Round Kanchenjunga; a Narrative of Mountain Travel and Exploration." By Douglas W. Freshfield. With Illustrations and Maps. (London: Edward Arnold, 1903.)

Briefly summarised, Mr. Freshfield's tour was as follows:—Starting from Darjeeling he made his way up the valley of the Teesta River, which, running southward, bounds the whole of the Kanchenjunga range on its eastern side; leaving this valley the Zemu River was followed until the Zemu glacier was reached. Here it was that the party were overtaken by the great storm of September, 1899, which "after devastating Darjeeling, swept across Kanchenjunga into Tibet in the form of a premature snowfall, lowering the snow-level nearly 4000 feet and practically closing the highest region." As there was no wind the snow did not drift, but after the storm was over it lay between three and four feet deep round the tents.

Such conditions would have turned back most travellers and stopped any attempts to cross passes more than 20,000 feet high. Mr. Freshfield, however, was not discouraged, and although even a partial ascent of Kanchenjunga was out of the question, still he managed to lead the party over the northern ridge of the Kanchenjunga range and to explore some totally new ground in eastern Nepal. Before doing this he moved north-eastwards to Lhonak. It was from here that the party, together with the baggage train of coolies, crossed over the Jonsong La (20,207 feet). On the west side of this pass lay Nepal, an unknown land. For several days the route lay downwards over glaciers, and it was only after nearly a week spent on the ice and snow that the party finally arrived at the upper grazing grounds of the cattle belonging to the Nepalese village of Kangbachen. Here it was that they connected their route with that of Sir Joseph Hooker, who fifty years previously had visited this valley.

As the party were now in forbidden country, some anxiety was felt as to their reception by the inhabitants, but with the exception of one official no trouble was met with, and as an excuse for the trespass it was pointed out that, driven by the great snowstorm over the pass, the party were seeking their way back to British territory, and that obviously their nearest way was down the Kanchen valley, thence by the Chunjerma and Kang La back to Darjeeling.

From many points of view this work of Mr. Freshfield's is of interest; it is a delightful record of mountain exploration, it is splendidly illustrated, and the descriptions of ice-clad mountains, of tropical forests, and of the great beauty of the atmospheric effects in this great mountain range are all given most admirably by the author. Moreover, many most interesting scientific and geographical problems are discussed.

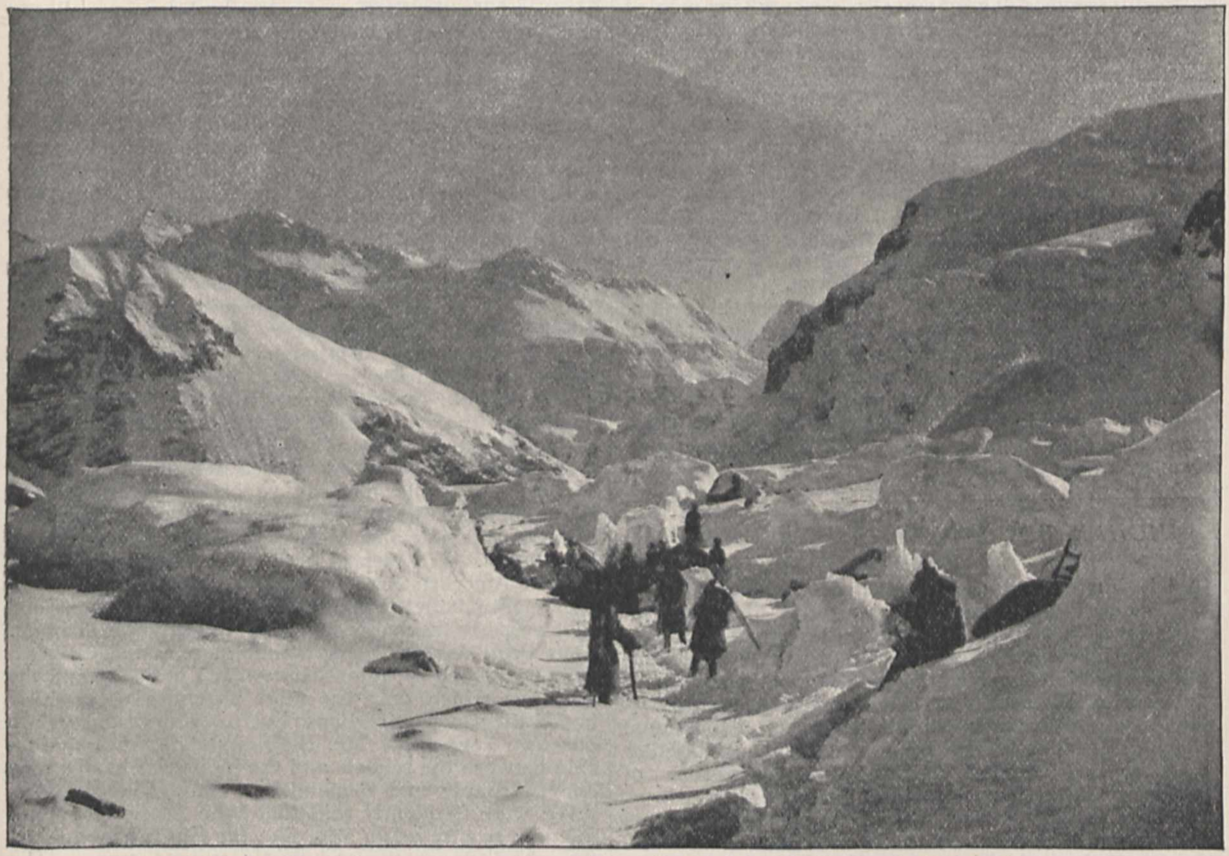

FIG. 1.—Camp below the Jonsong La. From "Round Kanchenjunga," by Mr. Douglas W. Freshfield.

One important question was as to whether there are peaks higher than Mount Everest lying further to the north in Tibet. Twenty years ago Mr. Graham, from the summit ridge of Kabru, at a height of more than 20,000 feet, asserted that he saw two peaks, one covered with snow and one of rock, further north than Mount Everest, and that they appeared as high, possibly higher, than Mount Everest. This statement has been partly confirmed by native explorers. That high peaks exist there is undoubted, and one was seen from the Chunjerma Pass by Mr. Freshfield. Also more recently a photograph taken by Mr. H. H. Hayden, and published in the *Geographical Journal* (1904, 362), shows these peaks. Mr. Freshfield, commenting on this photograph, says:—"Somewhat to the north-west of Chomokankar (Mt. Everest) appears a great group of peaks; one rock and one snowy summit are conspicuous. These are apparently as yet unidentified and unmeasured. They rise at no great distance beyond Chomokankar, and are probably south of the Tingri Maidan."

During late years much has been written about the effect of rarefied air at high altitudes on the human system. Mr. Freshfield and his party suffered but little inconvenience, even when on the summit of the Jonsong La (20,207 feet). That the effects of low barometric pressure have been much exaggerated is also borne out by the experience of Mr. White, political officer in Sikkim, who says:—"I find that the height is felt most at from 14,000 to 16,000 feet, and that if they (the coolies) once get over that, going to a still higher altitude has very little further effect. Personally the height does not affect me, and I felt perfectly well at 21,200 feet."

The geology of the district is most ably described by Prof. Garwood, by whom also an excellent map of the whole Kanchenjunga range has been made.

Mrs. Le Mesurier has contributed a chapter on Tibetan curios, and in the appendix, besides the exhaustive description by Prof. Garwood of the geological structure and physical features of Sikkim, there is a mass of important and interesting matter collected by the author; on the narratives of journeys made by native surveyors; on the various native names for the highest measured peak (Mount Everest); also a most useful list of books and maps consulted, and last, but not least, a list of photographs taken by Signor V. Sella during the tour of Kanchenjunga.

"Round Kanchenjunga" is a book worth reading from many points of view; it is not merely a tale of mountaineering adventure, but is full of information, artistic description, and new facts. It is a book which undoubtedly will be "serviceable to Alpine climbers and men of science, and not without interest for those who 'love the glories of the world' and count among them great mountains."

HIGHER EDUCATION IN THE UNITED STATES.[1]

ALL intelligent attempts to make known in this country the extent and success of American educational enterprise deserve encouragement. So well considered an effort as that of Mr. Mosely not only merited but has received enthusiastic appreciation. By securing the assistance of educationists representative of successive steps in a complete educational system, Mr. Mosely has been able to bring together in convenient compass authoritative expressions of opinion as to the precise state of each grade of education in the United States, and to provide our new educational authorities with information as to the characteristics of American education which good judges think might with advantage be copied in this country. Similarly, the features of the work of schools and colleges in the States which should be discouraged among us are in this report duly indicated. Mr. Mosely has, too, made arrangements to ensure a wide circulation for the valuable material collected under his auspices. By forwarding to the publishers of the volume the cost of postage and stating his qualifications, any member of an educational authority, any county councillor, local manager, headmaster, or registered teacher may obtain a copy of the book free.

The twenty-six separate reports contained in the volume cover the whole field of education from the kindergarten to post-graduate university study, but it will be possible in this place to refer to a few only of the more important directions in which American practice offers British educationists food for serious reflection. The most prominent place may well be given to an impression received by all the commissioners alike, and recorded first in their joint report; we refer to "the absolute belief in the value of education both to the community at large and to agriculture, commerce, manufactures, and the service of the State" which distinguishes the inhabitants of all the United States. Side by side with this record of their observations must be placed the commissioners' message to their countrymen, which is expressed as a desire "to impress on the British public the absolute need of immediate preparation on our part to meet such competition" as this enthusiasm for education in America will lead us to experience. Evidence of the advances in American education, and also of the sacrifices made in the States to endow and develop colleges and universities, have been frequently laid before readers of NATURE. But though here and there in Great Britain a desire has been manifested to found new universities, and though we are glad to admit that a few of our men of wealth have emulated the example common among American millionaires of giving largely to educational institutions, a general awakening on the part of the nation so far as a thorough belief in education is concerned is still a matter of the future. Meanwhile, the schools and colleges of the United States go steadily on with their work of preparing the rising generation. As Mr. W. P. Groser, who was nominated to the commission by the Parliamentary Industry Committee, says in his report, "England is now competing with American commerce in the making. In the next generation our manufacturers will meet trained men, adding culture to their enterprise and knowledge to their ambition."

Another striking difference between the English and American attitude towards education is appreciated by comparing the relations in the two countries bebetween industry and higher scientific and technical instruction. The report makes it abundantly clear that in America there is complete sympathy between the manufacturers and the college professors, and that properly trained college men are in great demand. Says Prof. Ayrton, "I saw that there actually existed that close bond of union between the industry and the teaching which only the more sanguine of us have hoped they might, perhaps, live to see introduced into our own country." Mr. Blair asserts, "the relationship between the schools and the industries has become one of supply and demand." Prof. Ripper states, "We were frequently told that 'the American manufacturer twenty years ago, like the English of to-day, thought little of the technically trained men. The difference between us now is that the American has changed his opinion, while England appears to be where she was.'" Commissioner after commissioner gives instances of the large proportion of men educated at college who are engaged in great manufacturing concerns in the States. Out of 10,000 employees in the Westinghouse shops and offices, there are 160 college-trained men employed. At the Carnegie Steel Works, where there are 7000 hands, about a hundred technically trained men are engaged, seven of the twenty-three leading officers being college graduates, and similar cases might be multiplied indefinitely.

The same enlightened policy is adopted in the matter of apprentices. Prof. Ayrton was told everywhere, "an engineering apprentice in a factory should be a college trained man," and the foreman of the apprentices at the Westinghouse works informed him, "the engineering apprentices, of whom we have about 150,

[1] "Reports of the Mosely Educational Commission to the United States of America, October-December, 1903." Pp. xxiv+400. (London: Co-operative Printing Society, Ltd., 1904.) Price 1s., post free 1s. 4d.

must be first-class graduates of leading technical schools. We start them on trial at 8*d*. an hour, and if really bright they may be earning 30*l*. a month with us at the end of eight months. We are always on the look-out for bright men; we cooperate with the professors of colleges to get them." "Two of the chiefs of the staff of the Westinghouse Company," says Prof. Ayrton later, "visit all the principal universities, colleges, and technical schools throughout the United States every year for the purpose of seeing the students, and choosing those who are most suitable to work with the Westinghouse Company." College students, too, are encouraged to work in the shops during vacation time, and in this way to supplement theoretical knowledge with practical experience.

Still another way in which the connection between the training given in the technical schools and colleges and the needs of industry is in America made intimate and real is to be found in the conditions of tenure pertaining to professorships. All the practical men engaged in engineering consulted by Prof. Ayrton were unanimous in telling him that "an engineering professor in a college should be actively engaged in the practice of his profession." Or, as he says later in his report, "engineering education in America is directed by those who are doing the engineering work of their country." Prof. Maclean's evidence is in the same direction; he states, "superior men are induced to accept collegiate appointments because of the well-equipped laboratories at their disposal, and because as engineering professors they are given every opportunity and encouragement to do outside work. It is believed that thus they keep in touch with the various lines of progress in their profession." Prof. Ripper, too, adds his testimony to the same effect. He writes, "it is considered vital that the professor should be in the field of practice, otherwise he is liable to become stale and out of date, and to attach exaggerated importance to unnecessary things."

The scepticism of the British manufacturer as to the value of a scientific training in the workshop and factory, his neglect of the technical expert, and his ingrained conservatism are already painfully familiar to men of science. It is unnecessary to insist, in view of what this latest report tells of American enlightenment, that in the absence of an earnest endeavour by British directors of industry to follow the lead of their contemporaries in the States, the results will be disastrous—indeed, fatal—to our commercial supremacy.

To turn now to the extent that science is in America utilised in the service of the State—a matter the importance of which has been urged consistently in these pages. A joint report, signed by the commissioners as a body, places it on record that "the closest connection is being established between theory and practice, the practical bent of the men of letters and science and the breadth of their outlook being very remarkable. The services of experts in various branches of knowledge are, therefore, held in high esteem and are in constant demand." And Prof. Armstrong, in a report brimful of good things, gives numerous examples of the appreciation by the American Government of the services of men of science. To quote one or two of his *obiter dicta* :—"So far as I am aware, there is nothing anywhere to compare with the way in which science is being utilised in the service of the State by the U.S. Department of Agriculture." "There is no question that the research work done under the auspices of the Agricultural Department and in the experiment stations is of the very greatest value, and is contributing most materially to the development of agricultural industry." "One branch of work initiated in the Office of Experiment Stations at Washington of extreme importance, to which reference should also be made, is that relating to the nutrition of man, which has been carried out in various parts of the States under the supervision of my friend Prof. Atwater." If it were necessary, similar examples from these reports could be multiplied a hundredfold.

In a short review it is possible only to touch the fringe of so great a subject. Much of value in the reports has been left completely on one side. But it is greatly to be desired that every man of science, every person engaged in education, whether as administrator or teacher, will study the volume. It is an important and absorbingly interesting contribution to a subject that deserves the immediate attention of every one of our statesmen.

A. T. S.

NOTES.

THE annual conversazione of the Royal Society will be held on Friday, May 13.

WE regret to see the announcement of the death of Prof. E. Duclaux, director of the Pasteur Institute, at sixty-three years of age.

PROF. A. W. WILLIAMSON, F.R.S., is lying dangerously ill at his residence at Haslemere.

INVITATIONS have been issued by the Royal Society of Edinburgh to a conversazione to be held in the rooms of the society on Saturday, May 28.

PROF. HENRI BECQUEREL, of Paris, has been elected a corresponding member of the Berlin Academy of Sciences.

THE deaths are announced of Prof. Leidie (chemistry), of Paris, and Prof. Charles Soret (experimental physics), of Geneva.

IN the *Physikalische Zeitschrift* for April 15, Prof. Th. Indrikson states that he has repeated Sir William Ramsay's experiments showing the spectrum of helium in the emanations from radium, the experiments being in this case conducted in the physical institute at St. Petersburg, where no experiments with helium had previously been made.

IT is announced that an annual subsidy of 35,000 kr. (1950*l*.) for twenty years has been granted by the Icelandic Government for the establishment of a wireless telegraphic connection between Iceland and the Shetland Islands or the mainland of the United Kingdom, and also between the four principal towns of Iceland.

THE council of the Institution of Civil Engineers has made the following awards for papers read and discussed before the institution during the past session :—A Telford gold medal to Major Sir Robert Hanbury Brown, K.C.M.G., a George Stephenson gold medal to Mr. G. H. Stephens, C.M.G., and a Watt gold medal to Mr. Alphonse Steiger. Telford premiums to Mr. E. W. de Rusett, Dr. Hugh Robert Mill, Mr. Alexander Millar, and Dr. T. E. Stanton. A Manby premium to Prof. J. Campbell Brown, and a Crampton prize to Mr. L. H. Savile. The presentation of these awards, together with those for papers which have not been subject to discussion and will be announced later, will take place at the inaugural meeting of next session.

THE Geologists' Association has arranged an excursion to Derbyshire for Whitsuntide. Four days are to be devoted to out-door geology. The party leaves St. Pancras for Buxton on Friday, May 20, and is expected to arrive in London from Derby on Wednesday, May 25. On Saturday, May 21, the excursion will be directed by Messrs. H. A.

Bemrose, E. Sandeman, and H. Lapworth, but for the other three days Mr. Bemrose alone will be the director. The details of the excursion seem to have been carefully planned, and full particulars of these, together with information as to special fares and hotel arrangements, can be obtained from the excursion secretary, Mr. H. Kidner, 8 Derby Road, Watford.

A circular on the present state of the trade in indigo between India and Aleppo, prepared by the reporter on economic products to the Government of India, is noted in the *Journal* of the Society of Arts. It appears that between 600 and 700 chests of indigo are imported into Aleppo from India every year. On account, however, of the competition of German synthetic indigo, this is usually sold by the merchants at a loss. This synthetic indigo has two advantages over the natural product, viz. that it is cheaper and that its price does not vary. The native dyers have found that when natural and synthetic indigo are mixed in about equal proportions, the resulting mixture is more durable and also brighter in colour than the natural indigo. On account of the impetus that has been given to the dyeing industry by the popularity of this mixed dye, much more indigo is used than formerly, and the reduction in the demand for natural indigo has not been nearly so great as might have been expected from the introduction of synthetic indigo.

We have received a copy of the results of the magnetical and meteorological observations made at the Royal Alfred Observatory, Mauritius, in the year 1900, and we note a marked improvement in the form in which the results are now presented, being on the pattern of the Greenwich observations. The routine work has been carried out in a very satisfactory and thorough manner. Photographs of the sun are taken daily, when possible, and the negatives sent to the Solar Physics Committee in London. Meteorological bulletins are supplied daily to the local Press, and copies of monthly results are forwarded to this country and elsewhere. Rainfall observations are now made at about seventy stations, and the results are duly tabulated. Special attention is also given to magnetical and seismological observations.

Mr. J. R. Sutton has contributed to the report of the South African Association for the Advancement of Science a valuable paper containing the determination of mean results from meteorological observations made at second order stations on the table land of South Africa. Observations in Cape Colony are generally made at 8h. a.m. (mean time of the colony), but at some stations other hours are used. The object of the paper is to give materials for reducing these to a common standard of reference. At the cost of a great amount of labour, the author has calculated, from the very complete observations made at Kimberley, the corrections to be applied to means for each hour for all elements in order to obtain the true mean for each month and for the year. He makes suitable reference to the work of the late Mr. Stone, who made a somewhat similar calculation from the Cape observations for 1841–6.

Dr. T. Levi Civita contributes a note to the *Atti dei Lincei* for March 20 on Kepler's equation $nt = u - e \sin u$, and the limits of convergency of the well known expansion of u in powers of e.

Some experiments by Prof. A. Stefanini and Dr. L. Magri on the influence of radium on the electric spark, communicated to the *Atti dei Lincei*, xiii. (1), 6, by Prof. Battelli, lead to the following results:—For discharges between two spheres, or between a positively charged point or sphere and negative disc, the discharge is facilitated by radium for short sparking distances and impeded for longer ones; at these distances the radium influences the positive pole. If the disc is positive and the sphere or point negative, the discharge is impeded at small sparking distances within a limited interval; in general the effect is nil. For certain sparking distances between a sphere and disc it is possible for radium to impede or facilitate discharge according to which electrode is positive.

In the April number of *Climate*, the anti-malarial campaign at Ismailia is described. Dr. Harford discusses sleeping sickness and its cause, and articles of medical interest, reviews and notes complete the contents of this useful journal.

In a pamphlet entitled "Recent Improvements in Methods for the Bacterial Treatment of Sewage" (Sanitary Publishing Co.), Mr. Dibdin describes his multiple surface bacteria beds. The basis of his thesis is that there is no need for sewage to undergo a preliminary anaërobic treatment as in the case of the septic tank process, but that aërobic action alone suffices under the proper conditions. Mr. Dibdin constructs his beds of ridged tiles or of slate débris.

In the *Bulletin* of the Johns Hopkins Hospital for February (vol. xv., No. 155), Dr. Kennon Dunham describes the effects of the Röntgen rays on lower animal life. These differed with the particular species exposed, Chilomonas and two species of Paramæcium being killed after six exposures, each of three minutes' duration on three successive days, while rotifers, Arcella and Cryptomonas were unaffected by this treatment. As regards the different rays, those having the strongest action were found to be directed from the centre of the anode plate in a line perpendicular to its face, and focused by passing through a cylinder of sheet lead. The most destructive rays were produced by a medium low tube excited by a heavy electrical discharge which had been passed across spark gaps or other resistance sufficient to produce rays of great penetrative power, such as will give a clear picture of a deeply seated bone, *e.g.* the hip, in three or four minutes. Dr. Leonard Hirshberg proves by a number of experiments that the species of anopheles mosquito (*A. punctipennis*) so abundant in and about Baltimore does not transmit malaria. There are also other excellent articles, but of purely medical interest.

Messrs. Hepburn and Waterston, in the April issue of the *Journal of Anatomy and Physiology*, continue their account of the histology of the motor-cells and accessory nerve in the spinal nerve-column of the porpoise. Another article in the same *Journal* contains the report of the second of a series of lectures by Prof. A. Robinson on the early development of the ovum and the differentiation of the placenta in various mammalian groups.

In the April number of the *Zoologist* the editor, Mr. W. L. Distant, commences a series of articles on rivers as factors in the distribution of animals, dealing in this instance with their restrictive action. Many instances are noted where rivers form the boundary to the range of species or groups of mammals, a notable case being the limitation of the area of the viscacha by the Uruguay River, although the country to the north appears in every way as well suited to the habits of that rodent as are the pampas to the south.

Three papers on vertebrates constitute the chief contents of the instalment of the *Proceedings* of the Philadelphia

Academy last to hand. In one Mr. J. A. G. Rehn continues his survey of the American bats, dealing in this instance with the genus Dermonotus (Pteronotus), a close ally of Chilonycteris, which, as already noticed in NATURE, formed the subject of his preceding article. Of the other two papers—both by Mr. H. W. Fowler—one is devoted to the description of berycoid fishes, and the second to certain fresh-water fishes from various parts of the United States.

THE presidential address to the Indiana Academy for 1902, which is only just to hand, in the *Proceedings* of that body, is devoted to a survey of the rise and progress of science in Indiana, which date practically from the conclusion of the war of secession. Special attention is devoted to the benefits conferred by science on agriculture, and it is pointed out that, as the result of these investigations, farmers in Indiana will eventually grow only such crops as are best suited to local conditions, and therefore the most remunerative.

THE second part of the first volume of *Records* of the Albany Museum contains five notes by Dr. R. Broom on South African anomodont reptiles. In one of these he discusses the affinities of the pavement-toothed genus Endothiodon, which was placed by Mr. Lydekker among the dicynodonts, but transferred by Prof. Seeley to the theriodonts. The new evidence demonstrates that the endothiodonts are so closely related to the dicynodonts that it is doubtful whether there is any cranial difference between the two groups, except the presence or absence of the palatal teeth.

AN excellent specimen of modern American zoological work is presented in a long and copiously illustrated article on the "Phylogeny of Fusus and its Allies," by Mr. A. W. Grabau, published in vol. xiv. of the *Smithsonian Miscellaneous Collections*. The shells of gastropods, when complete, are admirably adapted for phylogenetic study, since they display the whole growth—from the protoconch onwards—externally. The characters of the protoconch are found to be of prime importance in the group in question, although these must be correlated with the structure of the adult shell. One of the most important results of this line of investigation is the discovery that the genus Cyrtulus, represented by a single species from the Pacific, instead of being inseparable from the Eocene Clavilithes, forms a perfectly distinct type. The well known shells from the Barton Eocene commonly designated in geological works *Fusus longaevus* are shown to indicate at least three specific types of Clavilithes, one of which is regarded as new, under the title of *C. solanderi*.

THE periodic growth of scales as an index of age in the various members of the cod family forms the subject of a very important paper by Mr. J. S. Thomson in the first part of vol. vii. of the *Journal* of the Marine Biological Association. It has long been known that such growths are annual in the carp, and it is therefore probable that the same holds good for salt-water fishes. So far as can be determined by observation and experiment, this induction appears to be well founded in the case of the Gadidæ, and the author is of opinion that, after making all due allowance for individual variation, the age of these fishes can be determined by the number of rings (not the smaller lines) in their scales. Labelling of individual fishes returned to the sea, after their scales have been examined, would afford definite proof of the truth (or otherwise) of the theory. The paper is illustrated with a number of excellent plates.

THE March number of the *Quarterly Journal of Microscopical Science* contains an important paper on the dermal fin-rays (dermotrichia) of fishes, by Mr. E. S. Goodrich, of the Oxford Museum. Such structures may be divided into three types. In the sharks and chimæras these rays (ceratotrichia) are unjointed and composed of a fibrous horn-like substance devoid of bone-cells, and unconnected with the placoid scales found in the skin. In Teleostomi (bony fishes and ganoids), on the other hand, we find small unjointed, horny rays (actinotrichia) on the edges of the fins, which are probably remnants of the ceratotrichia, and, in addition, branched, bony lepidotrichia, developed externally to the actinotrichia, and in primitive forms closely resembling the body-scales. They are probably derivatives from scales which once clothed the fins. Finally, the lung-fishes have jointed, bony rays (camptotrichia) containing bone-cells, and probably representing the lepidotrichia of the teleostomes. In the same issue the editor, Prof. E. Ray Lankester, re-publishes his "Encyclopædia" article on the Arthropoda, one reason for this being that it may readily come under the notice of foreign naturalists. Our readers may be reminded that the author considers the one great feature uniting chætopods, rotifers, and arthropods in a common group is the presence in each body-ring of a pair of hollow appendages—paropodia—moved by intrinsic muscles and penetrated by blood spaces.

ATTENTION was directed in NATURE (May 17, 1900) to an article by Mr. Lester F. Ward on the "Petrified Forest" of Arizona, and reference was then made to the presence of a petrified trunk which formed a "natural bridge" across a canyon. We have now received an article by Prof. Oscar C. S. Carter on "The Petrified Forests and Painted Desert

FIG. 1.—Agate bridge formed of petrified tree trunk, 111 feet long, spanning ravine in Arizona.

of Arizona" (*Journ.* Franklin Inst., April), and this contains a number of illustrations of the scenery, including the natural (agate) bridge, which we are enabled to reproduce. The silicified trunks of trees are considered to be of Triassic age, and most of them are relics of the denudation of the strata; that represented in the natural bridge is, however, *in situ*. The "Painted Desert" is so named on account of the bright colours of the sandstones, shales, and clays—the rocks being eroded into fantastic shapes, and being coloured blue, yellow, red or green in places; hence the effect in sunlight is brilliant. An illustration is given of pictographs made by cliff dwellers on a face of sandstone near the petrified forest. The silicified tree trunks mostly belong to forms allied to the Norfolk Island pine (Araucaria); other masses resemble red cedar. There are indications that the wood had commenced to decay before it was silicified. Prof. Carter believes that the petrifaction took place in the sandstone and shale, and was due to soluble silicates derived from decomposition of the felspathic cement in the sandstone.

THE delegates of the Clarendon Press have in preparation, and will shortly publish, an authorised translation of "Das Antlitz der Erde," by Prof. Eduard Suess. This English edition of a standard work will be prepared by Dr. Hertha Sollas under the supervision of Prof. W. J. Sollas, F.R.S., and will contain a preface written for it by Prof. Suess.

THE *Electrician* Printing and Publishing Co. announce the early publication of a work by Mr. F. Soddy entitled "Radio-activity: an Elementary Treatise from the Standpoint of the Disintegration Theory." The same company will issue in a few days a book by Prof. S. Lemström, entitled "Electricity Applied to Agriculture and Horticulture."

A NEW edition of an illustrated price list of chemical apparatus has been published by Messrs. Brewster, Smith and Co., of Cross Street, Finsbury Pavement, E.C. The new catalogue contains above four hundred more illustrations than the previous issue, and also full particulars of several new devices of which we have already given descriptions.

THE eighth volume of the new half-yearly series of the *Transactions* of the Leicester Literary and Philosophical Society has reached us. It is edited by Mr. O. T. Elliot. The volume contains the presidential address of Dr. R. Pratt dealing with the subject of "over-strain" and "nervous-breakdowns," which are traced to a wrong use of leisure; four papers read before the society; and the quarterly reports of six of the sections into which the association is divided. We notice that this Leicester society was founded in 1835, and has thus had nearly seventy years of useful work.

ON account of the ease with which gold can be obtained in the pure state, the exact determination of its melting point is an important datum for high temperature measurements. Previous observers have given values ranging from 1061° (Callendar, Heycock and Neville) to 1091° (Barus), the average of the more recent work being 1064° C. In the current number of the *Comptes rendus* a new determination of this constant is described by MM. A. Jacquerod and F. L. Perrot, in which direct comparison with the gas thermometer, with fused silica bulb, is adopted. The heating was carried out in an electrical resistance furnace of special type, giving a complete control over the temperature in the neighbourhood of 1000° C. Owing to the smallness of the coefficient of expansion of silica, the correction for the expansion of the bulb amounts to only 2°, as against 35° to 40° for the same instrument with a platinum bulb. The mean result with the nitrogen thermometer was 1067°.2 C., and the results obtained when the bulb was filled with other gases showed that the coefficients of expansion of oxygen and carbon monoxide are very close to that of nitrogen.

THE additions to the Zoological Society's Gardens during the past week include a Lesser White-nosed Monkey (*Cercopithecus petaurista*) from West Africa, presented by Mr. T. P. Eykyn; two Mountain Ka-Kas (*Nestor notabilis*) from New Zealand, presented by Mr. T. E. Doune; a White-tailed Ichneumon (*Herpestes albicauda*) from Africa, six White-crowned Pigeons (*Columba leucocephala*) from the West Indies, two Large-billed Weaver-birds (*Ploceus megarhynchus*) from India, deposited.

OUR ASTRONOMICAL COLUMN.

COMET 1904 *a*.—Herr M. Ebell has calculated a new set of elements and an ephemeris for Brooks's comet, the former differing slightly from that published by Prof. Pickering. They have been derived from observations made on April 17, 20 and 24, and are given below:—

$$T = 1904 \text{ February } 28{\cdot}8792$$

$$\omega = 50^\circ\ 53'{\cdot}2$$
$$\Omega = 275\ 18{\cdot}5$$
$$i = 125\ 0{\cdot}0$$
$$\log q = 0{\cdot}42950$$

Ephemeris 0h. M.T. Berlin.

1904	α (h. m. s.)	δ (° ′)	log Δ	Brightness
May 2 ...	16 6 56	+52 44·4	0·3556	0·95
6 ...	15 50 5	+54 23·1		
10 ...	15 32 16	+55 44·8	0·3672	0·88
14 ...	15 13 50	+56 47·2		
18 ...	14 55 15	+57 31·9	0·3831	0·81

(Kiel Centralstelle *Circular*, No. 66).

It will be seen from the above ephemeris that the comet is travelling along just inside the southern border of Draco towards Ursa Major, and is becoming fainter. On May 6 it will be very near to, and south of, a small triangle of stars which is situated about 4° south of θ Draconis.

DIMINUTION OF THE INTENSITY OF THE SOLAR RADIATION.—In a communication to the Paris Academy of Sciences M. Ladislas Gorczyński publishes two tables showing, in the first, the mean monthly values of the solar intensity and the absolute humidity, and in the second the maximum values of these two quantities for each month during the years 1901, 1902 and 1903. The tables give the differences between the values for the corresponding month of each year, and show that the diminution in the intensity, which M. Dufour stated (*Comptes rendus*, vol. cxxxvi. p. 713) commenced in December, 1902, really commenced at Warsaw in May of that year. Until more positive evidence as to the effect of the dust ejected from Mont Pelée on the observed solar intensity is forthcoming, M. Gorczyński hesitates to ascribe the diminution to this cause (*Comptes rendus*, No. 5).

THE PERIODICAL APPARITION OF THE MARTIAN CANALS.—In a paper read before the American Philosophical Society Mr. Percival Lowell discusses the 375 drawings of the Martian surface made by him during the opposition of 1903. Having plotted the values allotted to the "visibility" of eighty-five canals, at different periods, with regard to the time of their minima visibilities after the Martian summer solstice, he found that these minima appeared in regular sequence from the North Pole towards the equator. Mr. Lowell believes that the canals are strips of vegetation dependent for their growth—and therefore for their visibility—upon the simultaneous presence of sunlight and water, and he points out that on a planet, such as the earth, where water is constantly present all over the surface, the appearance of vegetation solely depends upon the amount of sunlight received; therefore in the northern hemisphere it simply progresses northward with the sun. On the other hand, he concludes, from his curves, that there is no constant supply of moisture on the surface of Mars, and, therefore, although the sun may have reached the summer solstice, it is not until the snowcap melts and looses the water supply that the vegetation appears. Further, his curves indicate that when loosed the water moves southward at a remarkably steady rate of 53 miles per day, and, as the figure of the planet is shown by its spheroidity to be in a state of fluid equilibrium, he contends that the water must of necessity be conveyed southwards by artificial means.

The curves discussed are reproduced on seventeen plates which accompany the paper in No. 174, vol. xlii., of the *Proceedings* of the society.

ELEMENTS AND COMPOUNDS.[1]

I HAVE the honour of speaking to an audience of many men whom I have long venerated as my intellectual, although not my personal, teachers, and whom I admire as leaders in our common work for science. But however admirable the *present*, I am still more impressed by the thought of the *past* associated with this place. When, not long ago, I was engaged in electrochemical investigations and almost daily sought for information and enlightenment in Faraday's researches, I did not dare to think in my boldest dreams that one day I should find myself standing on the very spot in which he was wont to give the first accounts of the innumerable results of his indefatigable labours, his indomitable zeal, and his inexorable love of truth.

All that the pupil can do in such a case is to imbue himself as completely as he can with the ideas of the master and to try to perform his modest work in the master's spirit. But here arises a new difficulty: what subject ought I to choose? When I look into my own humble efforts, I find everywhere traces of Faraday. So far as relates to electrochemistry, the thing is plain; I think there is no word that I have oftener spoken or written than the word "ion," that word which was uttered for the first time in its modern sense in this very spot. But in other fields in which I have also worked, I feel the influence of his skilful hands and his keen vision. Catalysis, which I have studied during the past ten years, likewise came under his hands; and in the parts of the subject he worked at, the charm of secrecy and inexplicableness has been exchanged for the better qualities of a problem capable of resolution by earnest workers. And in one subject which has engrossed a very great part of my scientific activity, in the question of energy, I find the venerated master again a leader. He was indeed the first scientific man to direct all his investigations in view of the idea of the conservation and the mutual transformation of the various forces, as he called them, or the various kinds of energy, as we call them now.

This is a side of Faraday's mind to which, perhaps, not so much attention has been paid as it deserves. Although doubtless the greatest advance—the discovery of the quantitative proportionality between the energy which disappears and that which originates—was due to Mayer and Joule at a later date, yet the practical perception of this relation was working in Faraday's mind long before. There is indeed a great difference between the intellectual development of a scientific truth to a degree sufficient for the discoverer's *own* work, and to the degree required for its successful transfer to the minds of *other* workers. Faraday contented himself in this case, as well as in others (for example, in his conception of lines of force), with the first step. But that he had reached this step and stood firmly on it, that he used this conception constantly and regularly in his work, is evident from his constant reference to it from the first year of his scientific work onwards. From a closer study of his lectures and papers we learn that in every case he put the question: how can I change a given force into another? This continued to the very end of his work; for the last experiments he made related to the direct conversion of gravity into electricity, and although he did not succeed in his attempt, he was nevertheless convinced of the possibility of the conversion.

Guided by these considerations, I directed my attention to the very earliest problems treated by the master. Even before Faraday held the chair of chemistry here in the Royal Institution, as a youth of twenty-five years of age he practised the art of a lecturer in a small club, the City Philosophical Society, and the first course which he delivered there was on chemistry. In the sixteenth lecture, after a description of the metals, he concluded with the following general remarks:—

"To decompose the metals, then, to reform them, to change them from one to another, and to realise the once absurd notion of transmutation, are the problems now given to the chemist for solution. Let none start at the difficult task and think the means far beyond him; everything may be gained by energy and perseverance." And after a description of how in the course of history the means necessary for the isolation of the metals from their combinations have grown ever more and more efficacious, he mentioned the recent great discoveries of his master Davy as follows:—

"Lastly, glance but at the new, the extraordinary powers which the chemist of our own nation put in action so successfully for the reduction of the alkalies and the earths, and you will no longer doubt that powers still more progressive and advanced may exist and put at some favourable moment the bases of the metals in our hands."

When I try to follow this hint and take for the object of our consideration the question of the nature of the elements and of their compounds, I am aware that I am not the first who has done so in this place. If I am not mistaken, the very first chemist who had the honour of addressing you as a Faraday lecturer, Jean-Baptiste Dumas, lectured thirty-five years ago on the same subject. Nevertheless, I do not shrink from the repetition. Every generation of chemists must form its own views regarding this fundamental problem of our science. The progress of science shows itself in the way in which this is done. Faraday was at this time fully influenced by Humphry Davy's brilliant discoveries, and sought for the solution of the problem in Davy's way. For Dumas, the most important achievement of the science of his day was the systematising of organic chemistry, condensed into the concept of *homologous series*. He therefore regarded the elements as comparable with the hydrocarbon radicles, and tried to arrange them in similar series with constant differences in the numerical values of their atomic weights. It is well known that these ideas finally developed into the great generalisation we owe to Newlands, Lothar Meyer, and Mendeléeff. Although the problem of the decomposition of the elements was not solved in this way, these ideas proved to be most efficient factors in the general development of science.

From what store of ideas will a modern chemist derive the new materials for a new answer to the old question? A physicist will have a ready answer: he will construct the elements in a *mechanical* way, or, if he is of the most modern type, he will use *electricity* as timber. The chemist will look on these structures with due respect indeed, but with some reserve. Long experience has convinced chemists (or at least some of them) that every hypothesis taken from another science ultimately proves insufficient. They are adapted to express certain sides of his, the chemist's, facts, but on other not less important sides they fail, and the end is inadequacy. Learning by this experience, he makes a rule to use only chemical material for this work, and according to this rule I propose to proceed.

Hence, like Dumas, I put the question: what are the most important achievements of the chemistry of our day? I do not hesitate to answer: *chemical dynamics* or the theory of the progress of chemical reactions and the theory of chemical equilibrium. What answer can chemical dynamics give to the old question about the nature of the chemical elements?

The answer to this question sounds most remarkable; and to impress you with the importance I ascribe to this investigation, I will mention the result at once: *It is possible, to deduce from the principles of chemical dynamics all the stoichiometrical laws; the law of constant proportions, the law of multiple proportions and the law of combining weights.* You all know that up to the present time it has only been possible to deduce these laws by help of the atomic hypothesis. Chemical dynamics has, therefore, made the atomic hypothesis unnecessary for this purpose, and has put the theory of the stoichiometrical laws on more secure ground than that furnished by a mere hypothesis.

I am quite aware that in making this assertion I am stepping on somewhat volcanic ground. I may be permitted to guess that among this audience there are only very few who would not at once answer, that they are quite satisfied with the atoms as they are, and that they do not in the least want to change them for any other conception. Moreover, I know that this very country is the birthplace of the atomic hypothesis in its modern form, and that only a short time ago the celebration of the centenary of the atomic hypothesis has reminded you of the enormous advance which science has made in this field during the last hundred years. Therefore I have to make a great claim on your scientific receptivity. But still I do not hesitate one moment to lay the results of my work before you. For I feel quite sure

[1] By Prof. W. Ostwald. Faraday Lecture delivered before the Fellows of the Chemical Society in the Theatre of the Royal Institution on April 19.

that I shall find this receptivity unrestricted; and, moreover, I shall reap another advantage. For I also feel assured that you will offer me the severest criticism which I shall be able to find anywhere. If my ideas should prove worthless, they will be put on the shelf here more quickly than anywhere else, before they can do harm. If, on the contrary, they should contain anything sound, they will be freed here in the most efficacious way from their inexact and inconsistent components, so as to take the shape fittest for lasting use in science. And now let us go into the matter.

The first concept we start from is *equilibrium*. In its original meaning, this word expresses the state of a balance when two loads are of the *same weight*. Later, the conception was transferred to forces of all kinds, and designates the state when the forces neutralise one another in such a way that *no motion* occurs. As the result of the so-called chemical forces does not show itself as a motion, the use of the word has to be extended still further to mean that *no variation* occurs in the properties of the system. In its most general sense, *equilibrium denotes a state independent of time*.

For the existence of such a state it is above all necessary that temperature and pressure shall remain constant; in consequence of this, volume and entropy remain constant too. Now it is a most general experimental law, that the possibility of such a state, independent of time, is dependent on the *homogeneity* of the system. In non-homogeneous bodies, as, for instance, in a solution of different concentration in different places, or in a gaseous mixture of different composition in different places, equilibrium cannot exist, and the system will change spontaneously into a homogeneous state. We can therefore limit our considerations to this state, and we shall consider only bodies or systems of bodies in equilibrium, and, consequently, homogeneous.

Perhaps the possibility of the existence of water in contact with water-vapour might be considered contradictory to this statement, because we have here two different states and no homogeneity. Here we meet with the new concept created by Willard Gibbs, namely, that of a *phase*.

Systems of this kind are formed of homogeneous bodies indeed, but of more than one. The water in our system is homogeneous in itself, and the vapour too, and equilibrium cannot exist until both are homogeneous. But there is a possibility that a finite number of different homogeneous bodies can exist together without disturbing one another. In such a system we must have the same temperature and the same pressure everywhere, but the specific volume and the specific entropy may change from one body to the other.

We call a *phase* every part of the system where these specific properties exhibit the same value. It is not necessary that a phase should be connected to one body only; it may be distributed over any number of parts. In this way the millions of globules of butter in milk form only *one* phase, and the watery solution of casein and milk-sugar forms a second phase: milk is a two-phase system.

Every system consisting of only one phase has two degrees of freedom. This law involves only the assumption that the sole forms of energy involved in the system are heat and volume-energy; we exclude from consideration any effects due to gravitation, electricity, surface-tension, &c. This law is connected with the famous phase rule of Willard Gibbs, but is not identical with it, for it contains no mention at all of the so-called components of the system. Indeed, the law is valid in the same way for any pure chemical element, for example, oxygen, or for any mixture, for example, a glass of whisky and water. If you allow to the latter only one phase, it is impossible to change it in more than two ways, namely, in pressure and temperature.

The existence of such a body in the shape of only one phase is generally limited. If the pressure be lowered at constant temperature, a liquid or a solid will change at last into a gas. Lowering of temperature will change a gas into a liquid and a liquid into a solid. For every one-phase system it is possible to determine a "sphere of existence." This sphere is not necessarily limited on all sides; for gases we do not expect a limit on the side of low pressures and high temperatures, nor for solids on the side of high pressures and low temperatures. But on certain sides every phase has its limits, and most of these limits are experimentally accessible.

What will happen if we exceed the limit of existence of a phase? The answer is most simple: *a new phase will be formed.* The spheres of existence of the different phases therefore limit one another, and the boundary-lines represent the interdependent values of temperature and pressure for the possibility of the co-existence of both phases.

By granting the co-existence of two phases we lose therefore one degree of freedom. At the same time a new variation has arisen from the ratio between the masses of the two phases. For we must not suppose that this ratio is without influence on the state; indeed we find here two radically different cases.

The most general case is, that during the transformation of one phase into another the properties of both are continually changing, and the state of every phase is therefore dependent on the ratio of the two masses. By evaporating sea-water at constant temperature the density of the residue grows continually higher, while the pressure, and therefore the density, of the vapour goes on decreasing. If, however, we evaporate distilled water, we do not find any change in the properties of the residue and of the vapour during the whole transmutation.

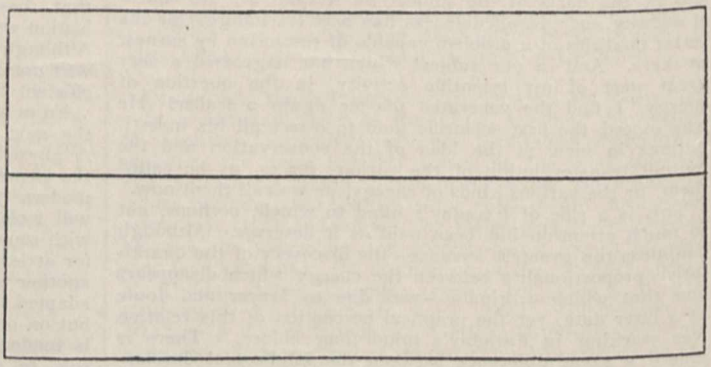

Fig 1.

Bodies of the first description we will call *solutions*, and of the second, *hylotropic bodies*. You will be inclined to call the latter substances or chemical individuals, and indeed both concepts are most nearly related. However, the concept of a hylotropic body is somewhat broader than that of a substance. But the possibility of being changed from one phase into another without variation of the properties of the residue and of the new phase is indeed the most characteristic property of a substance or chemical individual, and all our methods of testing the purity of a substance or of preparing a pure one can be reduced to this one property; anyone may readily convince himself of this by investigating any such method in the light of this description.

If we represent these cases by means of rectangular co-ordinates, taking as abscissæ the part of the first phase converted into the second, and as ordinates pressure or temperature, we get Fig. 1 for hylotropic bodies; they are represented by a horizontal straight line. With a solution we get a continuous line too, but not horizontal and generally not straight. If the ordinates are pressures at constant temperature, and the change is from liquid into vapour, the line will slope downwards as Fig. 2 shows. At other temperatures the lines will be of similar shape, only lying higher at higher temperatures and *vice versâ*. With other changes we obtain similar lines, sloping upwards or downwards as the case may be. For simplicity's sake we will consider in the future only vaporisation; this case gives the greatest possible variety, and we are sure not to omit anything by such a limitation.

What is the general process of change in a solution while it is being vaporised? The answer is quite distinct: *the residue is always less volatile* than the original solution, and *the distillate more volatile*. If there were an example of a solution behaving in the contrary way, then the process of vaporisation at constant temperature would be an explosive one. For the vapour begins to form at a given pressure; if by this the vapour-pressure of the residue were lowered, the vaporisation would continue of itself at a continually accelerated rate until all the liquid would be vaporised at once. It would be, in other words, a *labile* equilibrium. These equilibria are, however, only mathematical fictions, and have no experimental existence. If, on the contrary, the residue has a lower vapour-pressure, then the process is self-limiting, and shows the characteristics of a *stable* equilibrium. With hylotropic bodies we have an *indifferent* equilibrium, because the state is independent of the progress of the transmutation.

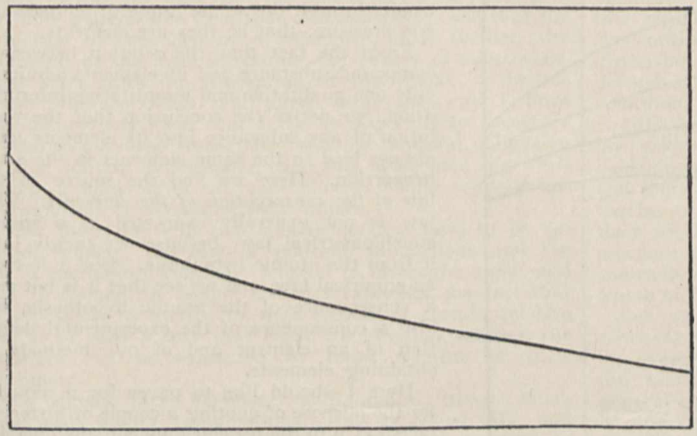

FIG. 2.

This being granted, we can ask: if we continue the separation of a solution into a less and a more volatile part by repeated distillation, what will finally become of it? Generally considered, two cases may happen. First the residue may become less and less, and the distillate more and more volatile, and there is no end to the progress. This case we may exclude from experimental evidence of a most general character, for we may take it as a general law that it is impossible to enhance any property beyond all limits, even by the unlimited application of our methods. We must conclude, therefore, that we shall ultimately meet with a *limit of volatility on both sides*, that finally we shall have separated our solution into a least and a most volatile part, and that both parts will not change further by repeated distillation. This is a most interesting result, for it means that *every solution can be resolved into components, which are hylotropic bodies*. For simplicity's sake we have considered only the case that two hylotropic components are generated by the process of separation; generally more than two may be formed, but in every case only a limited number of such components is possible. We may formulate therefore as a general law:—

It is possible in every case, to separate solutions into a finite number of hylotropic bodies.

From the components, we can compose the solution again with its former properties. This is also a general experimental law; if exceptions seem to exist, it is only because the case is not one of true equilibrium. Still we may limit our consideration to those cases where the law holds good. Then we have a relation between the properties of any solution, and the nature and relative quantity of its hylotropic components, which admits of only one interpretation. Every solution of distinct properties has also a distinct composition and *vice versâ*.

If we consider for simplicity's sake solutions of only *two* components, we may represent any property as depending upon the composition in a rectangular coordinate system, the abscissæ giving the composition and the ordinates the value of the property considered. In this way, we get a continuous line of a shape dependent on the particular case chosen.

If we consider the boiling points of all solutions formed by two hylotropic components, the most simple forms of curves (indeed the only experimental ones known) are given by the types I, II, and III, Fig. 3. For any solution, for example, the solution with the abscissa *a*, we can foretell its variation on distillation by the slope of the curve. For, as the residue must be less volatile, the residue will change to the ascending side of the curve. This is for I and III to the right, for II to the left side of the diagram. The change of the *distillate* is the opposite.

If we try to apply this criterion to the points *m* of the curve II and III, where there is a maximum and a minimum of the boiling point, we arrive at no decisive answer, for if the boiling point is already the highest possible it cannot rise, and if it is the lowest possible it cannot fall. We are forced therefore to conclude that the boiling point cannot change at all, that is, that this special solution must behave as a hylotropic body.

This is a well known theorem of Gibbs and Konovaloff, to wit, that a maximum or a minimum, generally spoken of as a *distinguishing point* in the boiling-curve, is necessarily connected with the property of distilling without change in the composition of the solution. A similar law holds good for the transitions from liquid to solid and from solid to gas.

Now this looks like a contradiction; while a few minutes ago we placed solutions in a class exclusive of hylotropic bodies, we have here solutions, that is, mixtures, which behave like hylotropic substances. But the contradiction vanishes if we consider a series of boiling-point curves corresponding to various pressures. We then find that the composition at the distinguishing point does not remain constant under different pressures, but shifts to one side, with alteration of pressure. This fundamental fact was discovered and experimentally developed in an admirable way by Sir Henry Roscoe, and has since proved itself a most important criterion in recognising a chemical individual.

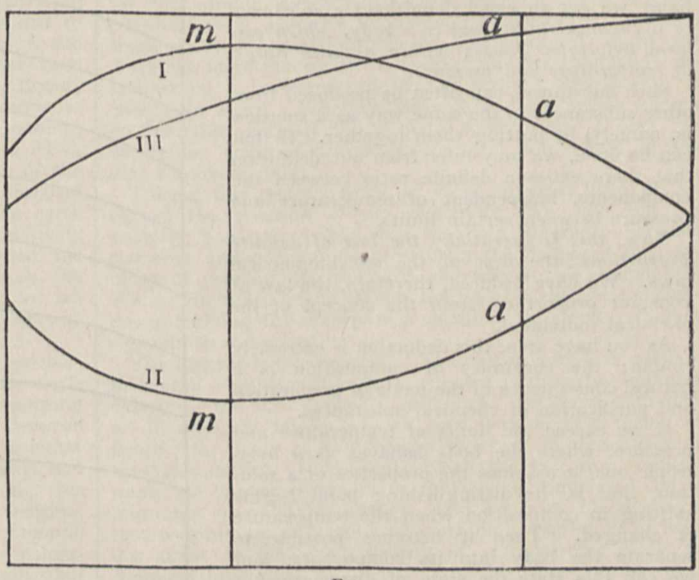

FIG. 3.

By drawing curves corresponding to various pressures, we get therefore generally the diagram shown in Fig. 4, the loci of the distinguishing points forming one curve. Between the infinite possibilities of the shape of this curve we have a distinguishing case again, the case that the curve is a *vertical straight line*. This means that the composition is independent of the pressure. *When this is the case, we call this hylotropic body a substance or a chemical individual.*

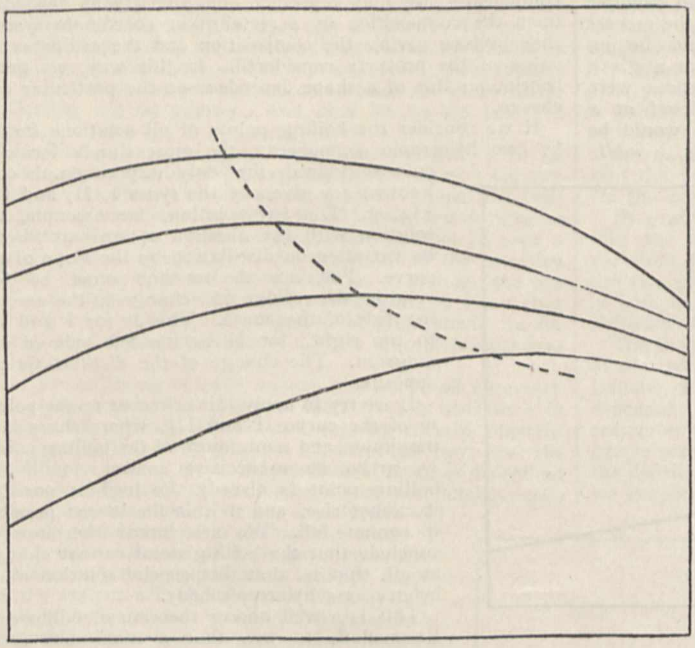

FIG. 4.

Therefore we conclude that a connection exists between solutions and chemical compounds or substances, the latter being a distinguishing case of the former. On the other hand, we get an exact definition: *a substance or a chemical individual is a body, which can form hylotropic phases within a finite range of temperature and pressure.*

Such substances can often be produced from other substances in the same way as a solution is, namely, by putting them together. If that can be done, we may infer from our definition that there exists a definite ratio between the components, independent of temperature and pressure between certain limits.

Now, *this is essentially the law of definite proportions*, the first of the stoichiometrical laws. We have deduced, therefore, the law of constant proportions from the concept of the chemical individual.

As you have seen, this deduction is extremely simple; the constancy of composition is a natural consequence of the mode of preparation and purification of chemical substances.

If we exceed the limits of temperature and pressure, where the body behaves as a hylotropic one, it assumes the properties of a solution, that is, its distinguishing point begins shifting in composition when the temperature is changed. Then it becomes possible to separate the body into its components, and we call this state the state of dissociation of the substance in question. In our graphic representation, the hitherto straight vertical line of distinguishing points turns sideways, Fig. 5. Most substances behave in this way, but there are substances which have never been transformed into solutions or the sphere of existence of which covers all accessible states of temperature and pressure. Such substances we call elements. In other words, *elements are substances which never form other than hylotropic phases.*

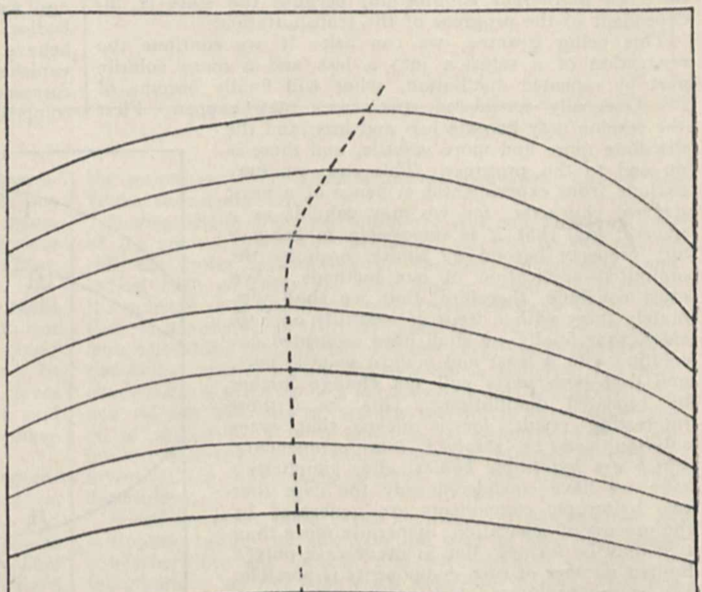

FIG. 5.

From this we may conclude that every body is finally transformable into elements, and into only *one definite set of elements*. For the most general case is a solution. Every solution can be separated into a finite number of hylotropic components, and these again can generally be transferred into a state when they behave like solutions and can be separated further. Finally, the components remain hylotropic through the whole range of temperature and pressure, that is, they are elements.

From the fact that the relation between a compound substance and its elements admits of only one qualitative and quantitative interpretation, we derive the conclusion that the resolution of any substance into its elements must always lead to the same elements in the same proportion. Here we find the source of the law of the *conservation of the elements*. This law is not generally expressed as a special stoichiometrical law, because we tacitly infer it from the atomic hypothesis. But it is truly an empirical law, and we see that it is not only a consequence of the atomic hypothesis, but also a consequence of the experimental definition of an element and of our methods of obtaining elements.

Here I should like to pause for a moment for the purpose of quoting a couple of historical facts. Up to the present moment, the question whether it is possible to deduce the stoichiometrical laws without the help of the atomic hypothesis has only been raised by other investigators in order to deny the possibility. So far as I am aware, there exists only one man who has worked upon the question with the earnest hope of obtaining an affirmative answer. Very few know his name. The man is Franz Wald; he is chief chemist at the iron works in Kladno, Bohemia. His papers on the subject are to be found in the *Zeitschrift* *für physikalische Chemie* and in the *Annalen der Naturphilosophie*.

In the foregoing considerations, Franz Wald has played a great part. To him I owe first the idea that the definition of substances and elements is in a certain sense arbitrary, though very helpful and convenient. This definition is a condensed expression of our methods of separating and purifying these bodies. While, generally speaking, every solution has the same claim to be investigated as these bodies, the latter soon distinguish themselves as standards to which all other cases may be referred. To Franz Wald I owe further the idea that the conception of a *phase* is a far more general one than that of a substance, and that the deduction of the idea of a substance, and, further, the deduction of the laws governing the nature of substances, must start from the conception of the phase. I do not know whether Wald will agree with the way I have manipulated his ideas, but I feel it imperatively necessary to express my deep respect for, and my thankful obligation to, this solitary philosopher, who has prosecuted his work during a long series of years almost wholly without encouragement or sympathy from others.

Now there are still two stoichiometrical laws to be deduced, namely, the law of multiple proportions and the law of combining weights. I prefer to invert the order, and first to deduce the second law. It expresses the fact that it is possible to ascribe to each element a certain relative weight in such a way that every combination between the elements can be expressed by these weights or their multiples.

We suppose three elements, A, B, and C, given, which may form binary combinations, AB, BC, and AC, and besides these a ternary combination, ABC; there shall be but one combination of every kind. Now we begin by forming the combination AB; for this purpose, we must take a certain invariable ratio between the weights of A and B, according to the already proved law of constant proportions. Now we combine AB with C and get the ternary compound, ABC. There will be a certain ratio, too, between AB and C, and we can, if we put A as unity, assign to B and C certain numbers describing their combining weights relatively to A.

Now we begin to combine A with C forming AC, and then we form the ternary combination, ACB from AC and B. According to our law of a relation between elements and compounds, which can be interpreted only in one way, ACB cannot be different from ABC, and, in particular, it must show the same ratio between the relative weights of its elements. Therefore, the ratio of the weights of A and C in forming the combination AC cannot be other than that expressed by the relative combining weights already found in the first way. In other words, it is possible to compute the composition of the hitherto unknown combination AC, from analyses of the combinations AB and ABC. In the same way, we can compute the composition of the unknown combination BC, by help of the numbers obtained by the analyses of two other combinations of the same elements. To resume: the combining weights relatively to A regulate all other possible compounds between the elements concerned. But this is nothing else than the general stoichiometrical law of combining weights, for we can extend our considerations without difficulty to any number of elements.

Lastly, it is easy to deduce the law of multiple proportions from the law of combining weights. If no compounds can be formed except according to their combining weights, then, if there are two different compounds between A and B, we can form the one containing more of B either directly from A and B, or indirectly, combining first A and B to form the lower compound and then combining this with more of B. In applying the law of combining weights, we conceive that the weight of B in the higher compound must be twice its weight in the lower. The same consideration may be repeated, and finally we get the result, that instead of double the combining weight, *any multiple* of it may occur in combinations, but no other ratio.

If we cast a backward glance on the mental operations we have performed in the last two deductions, we recognise the method, the application of which has made the two laws of energetics so fruitful. In the same manner as the difference between the whole and the available energy is independent of the nature of the path between the same limiting points, the product of the chemical action between a number of given elements is independent of the way in which they are combined. If we compare two different ways, we get an equation between the characteristics of the two ways, and this is equivalent to a new law. In our case, this new law is the law of combining weights.

I will put the same idea into somewhat different words. By stating the equation between any two ways, we can get any number of different equations, each representing a new way as an experimental fact. Now, in order that all these equations shall be consistent, there must be some general law regulating the characteristics of the equations. For the consistency of the several equations in the case under discussion, the existence of specific combining weights, independent of the several combinations, is the necessary condition.

This is the main point of the considerations I wish to lay before you this evening. There are some secondary questions as to isomerides or allotropic states of substances, and there are other similar questions, but it would lead us too far to consider them one by one. I have investigated them on the same basis, and I can assure you that I have nowhere found an insurmountable difficulty or an impassable contradiction. All these facts find their proper place in the frame of the same general ideas.

Let me still add some words about the *nature of the elements*, as considered from my point of view. I wish to lay great stress on the fact that here, too, I find myself on the same ground as that on which Faraday built his general concepts during his whole scientific career. There is only one difference, due to the development of science. Faraday ever held up the idea that we know matter only by its forces, and that if we take the forces away, there will remain no inert carrier, but really nothing at all. As Faraday still clung to the atomic hypothesis, he was forced to express this idea by the conception that the atoms are only mathematical points whence the forces emerge, or where the directions of the several forces intersect; here his view coincided with that of Boscovich.

In the language of modern science I express these ideas by stating: *what we call matter is only a complex of energies which we find together in the same place*. We are still perfectly free, if we like, to suppose either that the energy fills the space homogeneously, or in a periodic or grained way; the latter assumption would be a substitute for the atomic hypothesis. The decision between these possibilities is a purely experimental question. Evidently there exist a great number of facts—and I count the chemical facts among them—which can be completely described by a homogeneous or non-periodic distribution of energy in space. Whether there exist facts which cannot be described without the periodic assumption, I dare not decide for want of knowledge; only I am bound to say that I know of none.

Taking this general point of view, in what light do we regard the question of the elements? We will find the answer, if we remember that the only difference between elements and compounds consists in the supposed impossibility of proving the so-called elements to be compounds. We are therefore led to ask for the general energetic properties underlying the concept of a chemical individual, whether element or compound.

The answer is most simple. The reason why it is possible to isolate a substance from a solution is that the available energy of the substance *is a minimum*, compared with that of all adjacent bodies. I will not develop this thesis at length, for it is a well known theorem in energetics or thermodynamics. I will only recall the fact that a minimum of vapour pressure is always accompanied by a minimum of available energy; and we have already seen that a minimum of vapour pressure or a maximum of boiling point is the characteristic of a hylotropic body or chemical individual.

This granted, we proceed to the question regarding the differences between the several substances. Expressed in the most general way, we find these differences connected with differences in their *specific energy content*. Temperature and pressure are not specific, for we can change them at will. Specific volume and specific entropy, on the contrary, are not changeable at will; every substance has its own values of these. We may take therefore these values as the characteristics of the different substances. How many of such characteristics exist I cannot tell. Only for simplicity's sake I will assume that two of them are sufficient. As I will take care not to deduce any conclusions from this number, we shall not be led into error by accepting it.

We place these two characteristics in a system of planar coordinates; then the several elements will be represented by single points in the plane. We lay the plane horizontally and raise from these points ordinates, representing the available energy of each element. Between the points of the elements in the plane are situated the points of all possible solutions, filling up the whole plane. Each of these solutions will also have its available energy, and all the corresponding points in space will form a continuous surface. The form of this surface can be described in a general way. For as each *element* has its point in a *relative minimum,* the surface as a whole will have a shape like the ceiling of a cavern full of hanging stalactites, the end of each stalactite representing an element.

How can we pass from one element to another? Evidently not otherwise than by going over the higher parts of the surface, or the passes separating each stalactite from its neighbours. This can only be done by accumulating an appropriate amount of available energy in the element to be changed. Now the concentration of energy is a task we cannot accomplish *ad libitum,* for the possibility very soon ends. Think, for example, of compressing a gas into a given space. Up to some ten thousand atmospheres the work of compression will go on smoothly, but after that every metal begins to flow like a liquid, and you cannot proceed further. With the concentration of electric or any other energy the task is similar, and so we come to the conclusion that the concentration of energy can be pushed to only a very limited extent. The application of this result to our question about elements is simple enough: we cannot get over the pass between two stalactites because we cannot attain the necessary concentration of energy.

From the history of science we learn that these considerations contain at least some truth, for the isolation of the elements has ever been dependent upon the power of concentrating energy available at that time. The most brilliant example is the application of the voltaic pile to the isolation of the alkali metals by Humphry Davy.

Still I must confess that these last considerations are in a very embryonic state, and I should not have brought them before you if an unexpected application had not lately made itself manifest. Some years ago I explained these views to my old friend Sir William Ramsay, when he asked me how the idea of elements fitted into my conceptions of energy. Then I forgot all about it until Sir William reminded me of it, saying that his perplexing discovery of the transmutation of radium into helium might conceivably find some explanation in this way. This I am convinced of, and the considerations may be pictured in the following manner.

In the corner of our cavern where the elements with the highest combining weight are assembled, the stalactites are very short; and at last they are not really stalactites, but rather regions of different slope in the sloping ceiling. Where the plane is nearly horizontal a drop of water furnishes a picture of the stability of the elements. While hanging at the end of a true stalactite, more or less work must be done to raise the drop over the pass until it flows down another stalactite. But in this corner it will flow of its own accord, and only delay for a short time on the nearly horizontal portions in the ceiling.

Such elements will have only a *temporary existence.* Now we are sure that for the transmutation of one element into another enormous amounts of energy would be required, for the concentrations of energy as yet available have proved themselves insufficient for this purpose. We may expect, therefore, that enormous amounts of energy will be liberated if such an unstable element changes into a stable one. This accounts for the extraordinary quantity of energy developed by radium during its existence. The fact that radium changes into helium, an element with an exceptionally long stalactite (for it is impossible to get even any combination of helium), makes us expect indeed such an unusually great development of energy as is found to occur.[1]

The heat from radium is surely only the last form of the energy developed in its transformation. There are a great many intermediate forms, termed rays or radiations, which have been studied by a band of eminent workers, whose ingenuity and ability have been displayed in the most brilliant way during these investigations. Perhaps I may venture the suggestion that first, other intermediate temporary elements are formed, and that the energy liberated at this transmutation appears first in the shape of *new,* still imperfectly known forms. It is most likely that such forms are originated during the decay of the enormously concentrated energy of radium; at the same time it is probable that we have not yet the means of fixing these forms and so preventing their changing into other more common forms. We should remember that, for example, the conservation of electric energy at a pressure of some thousand volts during some months or years is by no means an easy thing, and I have great doubt if it is possible at all.

But here I must conclude, for I have ventured to intrude on a field where I have not secured my own right of entry by personal work. I see among my audience men who are possessed of an incomparably more minute and comprehensive knowledge of these new realms of science than I. I must ask you, therefore, to take these suggestions in the same spirit as that in which Faraday took his own speculations. They are questions put to nature. If she says Yes, then we may follow the same path a little further. If she says No—well, then we must try another path.

1 Compare Soddy, "The Wilde Lecture," *Mem.* and *Proc.* Manchester Lit. and Phil. Soc., 1904. I am very glad to find that I am in close agreement (except in so far as there is a difference in his accepting the atomistic, while I hold by the energetic point of view) with this most zealous and fortunate worker; indeed, the above statements were written and printed before I saw Mr. Soddy's lecture.

A SMITHSONIAN MAGAZINE OF SCIENCE.

TO provide a medium for the early publication of the results of researches conducted under the auspices of the Smithsonian Institution, and especially for the publication of reports of a preliminary nature, a quarterly issue of the *Smithsonian Miscellaneous Collections* has been commenced. This new periodical has the form of an attractive magazine, and contains papers on a variety of subjects of scientific interest, most of them beautifully illustrated.

The number opens with a description of seventy new Malayan mammals, by Mr. Gerrit S. Miller, jun., based on collections made and presented to the U.S. National Museum by Dr. W. L. Abbott. Mr. C. G. Abbot presents the results of recent studies of the solar constant of radiation, conducted at the Astrophysical Observatory of the Smithsonian Institution, under the direction of Dr. S. P. Langley. Another paper by Mr. Abbot describes the new cœlostat and horizontal telescope of the Astrophysical Observatory, in which are given the results obtained with a device designed by Dr. Langley for the purpose of "churning" a column of air traversed by a solar beam, with the view of reducing the "boiling" or confusion of all parts of the solar image due to variability of the strata of air traversed. Dr. F. W. True presents some photographic illustrations of living finback whales from Newfoundland, these being the first photographs of living whales in American waters that have thus far been published. Brief descriptions of a skeleton of Hesperornis, and a new Plesiosaur, by Mr. Frederic A. Lucas, are given with plates, and Mr. W. H. Holmes illustrates and compares the designs on some remarkable shell ornaments from Kentucky and Mexico.

A noteworthy specimen of a Glacial pothole in the National Museum is described by Mr. George P. Merrill, who explains the method by which the specimen was procured. Some notes on the herons of the district of Columbia, by Mr. Paul Bartsch, who made a systematic survey of two heron colonies and conducted experiments with the view of solving some of the problems of bird life, are of special interest. Dr. J. Walter Fewkes gives a preliminary report on an archæological trip to the West Indies in 1903, describing particularly the remarkable objects of stone, bone, shell, wood, and pottery which he collected during the trip, and giving an insight into their various uses. Dr. C. M. Child, of Chicago University, describes the form-regulation in Cœlentera and Turbellaria, of which he made a special study during his occupancy of the Smithsonian table at the Naples Zoological Station, and Dr. Carl H. Eigenmann introduces some new genera of South American fresh-water fishes, and new names for some old genera. Of timely interest is the account of Korean headdresses in the U.S. National Museum by the late Mr. F. H. Jenings, in which are described and illustrated twenty-four varieties of Korean hats and other headgear, including headband buttons and hatpins for topknots.

A brief history of the Hodgkins Fund of the Smithsonian Institution, and of what has been accomplished with its income toward "the increase and diffusion of more exact knowledge in regard to the nature and properties of atmospheric air in connection with the welfare of man," bears the name of Helen Waldo Burnside, and is accompanied with an illustration of the beautiful Hodgkins medal. Mr. A. B. Baker gives an account of a notable success in the breeding of black bears, which is of special interest to those having charge of animal collections. In a contribution on Chinese medicine, Dr. James M. Flint briefly explains the origin of medicine and the theory of disease in the Celestial Empire. The last of the series of articles consists of notes on the rocks of Nugsuaks Peninsula and its environs, Greenland, by Mr. W. C. Phalen, the remaining pages of the magazine being occupied by brief descriptions of various activities of the institution and their results.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The following is a copy of the speech delivered on April 28 by the Public Orator, Dr. Sandys, in presenting Prof. Ostwald, of Leipzig, for the degree of Doctor in Science *honoris causa*.

Viri et rerum naturae in scientiis excolendis et scientiarum illarum in terminis propagandis prospere occupati, non unius tantum populi intra fines angustos cohibentur, sed orbis terrarum totius inter cives merito numerantur. Nuper apud Londinienses Faradaii nostri memoriam oratione luculenta prosecutus est vir scientiarum laude illustris, qui a Germanis olim oriundus, Germanorum ultra terminum orientalem Russorum in imperio natus et professoris officio functus, postea in ipsam Germaniam atque adeo ad universitatem insignem Lipsiensem vocatus, in scriptis suis omnibus Germanorum gravitatem cum Francogallorum stilo lucido consociavit. Idem ne Europae quidem terminis contentus est, auctumno proximo (nisi fallor), velut alter Mercurius, Atlantis nepos facundus, etiam aequor Atlanticum transiturus. Quanta diligentia, memoria quam tenaci, ingenio quam multiplici praeditus, scientiae chemicae et scientiae physicae confinia quam diu quam feliciter lustravit, a collegis magnis sine ulla invidia peregre laudatus, a discipulis plurimis in omni orbis terrarum regione dilectus. Quot opera, inter sese quam varia, scientiae suae explicandae destinavit; idem etiam aliorum labores in Actis a sese tam diu editis quam diligenter in unum collegit, collectos in ordinem quam perspicuum redegit. Nemo mirabitur Actorum illorum librum prope quinquagesimum viri tanti in honorem nuper esse dedicatum, qui abhinc annos fere quinquaginta natus, vitae suae iam per partem dimidiam doctoris nomine decoratus est. Virum talem ad litora nostra honoris causa nuper vocatum, etiam nostro doctoris titulo libenter ornamus.

A COMBINED examination of non-resident candidates for open scholarships, exhibitions, &c., will be held at Trinity College, Clare College, Trinity Hall, Peterhouse, and Sidney Sussex College, Cambridge, beginning on Tuesday, December 6. Candidates will be examined at each college at the same time and by the same papers. Forms of application for admission to the examination may be obtained from any of the Tutors of Trinity College, the Senior Tutor of Clare College, the Master of Trinity Hall, the Senior Tutor of Peterhouse, or the Master of Sidney Sussex College. Entries should be made not later than November 18. Papers will be set in classics, mathematics, natural sciences, moral sciences and history. In mathematics and science the range of subjects included in the examination will be as follows:—*Mathematics*.—Arithmetic, geometry, algebra, trigonometry, elementary statics and dynamics, conic sections treated both geometrically and analytically, and the elements of the differential calculus. *Natural Sciences*.—Physics, chemistry, zoology, botany, physiology, and geology. Candidates for an emolument at Clare College may also offer elementary biology as a subject. Of these subjects no candidate may offer more than three. In making awards, excellence in one subject or in two subjects will be taken especially into account. There will also be (1) a paper of general questions in natural sciences which must be taken by all candidates who offer natural sciences, and (2) an optional paper in mathematics suitable for candidates who offer physics as one of their subjects.

THE Education Bill for Scotland was read a second time in the House of Commons on Monday by a majority of fifty-seven.

A LIST of the courses of lectures proposed for the summer term in the various German-speaking universities and technical schools is given in the *Physikalische Zeitschrift* for April 15.

THE foundation-stone of an extension of the Durham College of Science, Newcastle-on-Tyne, was laid on Monday by Mr. T. G. Gibson. The cost of the new buildings has been provided by a fund of 50,000*l*., raised to commemorate the life of the first Lord Armstrong, whose name the college will henceforward bear.

A COURSE of ten advanced lectures on the "Tracts of the Brain," by Dr. W. Page May, was commenced yesterday at University College, and will be continued on Wednesdays at 5 p.m. The lectures are open without fee to all internal students of the university.

THE following appointments are announced:—Dr. Friedrich Engel, of Leipzig, professor of mathematics in Greifswald; Dr. J. Schubert, of Eberswald, professor of physics, meteorology and geodesy; Dr. K. Hopfgartner, of Innsbruck, professor of chemistry; Dr. K. Schaum, of Marburg, extraordinary professor of physical chemistry; Prof. Paul Behrend, of Hohenheim, professor of organic chemistry; Prof. Lorenz, of Göttingen, ordinary professor of mechanics; and Prof. Roessler, of Charlottenburg, professor of electrotechnics—the last three at the Danzig Technical School; Dr. A. Hagenbach, professor of physics at Aachen; Prof. Moersch, professor of engineering at Zurich; Dr. Wedekind, of Tubingen, and Dr. Otto Dimroth, extraordinary professors.

REPLYING to a question in the House of Commons on April 27, Mr. Brodrick said that papers would shortly be laid on the table relating to the subject of the further maintenance of Coopers Hill College, including the report of the committee which sat last year. In consequence of the strong recommendations of that committee and the evidence brought before them, that efficient candidates for the Public Works Department in India can be provided by other engineering colleges at a less cost to the candidates and to the Indian Government, it has been decided to close the college. No decision, however, has yet been arrived at as to the date of closing, and all possible consideration will be shown to those concerned.

IN his presidential address at the recent annual general meeting of the Institute of Chemistry, Mr. David Howard reviewed the work of the council of the institute during the past year. Among other matters of interest he referred to the work of a special committee appointed to consider the advisability of instituting examinations for technical chemists. Mr. Howard said the most common difficulty at present is how to bridge over the gap between the scientific training and the practical work of the technical chemist. "What the chemist has to learn is to think in tons, not in grams." A large number of well known manufacturers consulted by the committee, while agreeing as to the value of a sound training in chemistry and physics, were emphatic that they did not want chemists trained or examined in the special technology of particular industries. The scheme drawn up by the committee is, as far as possible, based on the opinions of the manufacturers. As Mr. Howard said, it is gratifying to know that in this investigation the institute can rely on the cooperation of so many leaders of industry, among whom are ironmasters, alkali, acid and general chemical manufacturers, brewers, cement makers, and representatives of dyeing, calico printing, and other important industries. A technical chemist possessing all the qualifications suggested by the manufacturers would be at once a competent mechanical engineer, electrical engineer, architect and surveyor, accountant and book-keeper, draughtsman, patent agent, and lawyer, in addition to being a capable chemist, and he would possess also special personal qualities, including the power of organisation, tact and general business capacity. The committee is strongly inclined to think that it is possible so to direct the post-graduate studies of the young chemist that he may adapt himself to technical practice, and thus not only improve his own position, but be better qualified to bear his part in the prevailing struggle of industry.

THE King on April 28 laid the first stone of the new buildings for the Royal College of Science, Ireland, which are situated at Leinster Lawn, Dublin. The ceremony was commenced by the reading of an address by Sir Horace Plunkett, vice-president of the Department of Agriculture and Technical Instruction for Ireland, reviewing the work of the department as a whole, and especially that part of it entrusted to the Royal College of Science for Ireland. Referring to the latter, the address comments on the assistance received by the department from local authorities in the work of developing a system of technical instruction throughout Ireland, and points out the national value of a complete system of education. The King, in reply to the address, expressed his pleasure at performing the ceremony, and continued :—" In these days scientific training is an indispensable condition of success in commercial and industrial life. To be thoroughly effective it requires all the help which research and modern appliances can give. You are therefore wise in providing the improved equipment and the widened opportunity for instruction which this college will henceforth supply. You have told me that the efforts of your department to extend scientific education among the people have been supported by popular sympathy, and by the cooperation of representative public bodies. I am glad to receive this assurance; for without such sympathy and cooperation any scheme of technical instruction, however well devised, must fail to come into close touch with the life of the people, and must fall short of complete success. I agree with you in thinking that a complete system of education is necessary for the full realisation of your aims; and my best wishes go with your efforts to improve the intellectual and material conditions of the country." During his Irish visit the King also took another opportunity of emphasising the value of education in assisting the development of a country. At Kilkenny, in reply to addresses from a number of bodies, including the Kilkenny Agricultural Society, His Majesty said :—" I notice with pleasure the earnest efforts which are now being made for the industrial development of Ireland, and especially for the promotion of the agricultural industry, in which I take great practical interest. Agricultural prosperity, in my judgment, depends largely upon improved educational methods, cooperation, and better facilities for distributing produce. I am glad to know that, along these lines, progress is now being made in Ireland."

SOCIETIES AND ACADEMIES.

LONDON.

Linnean Society, April 21.—Prof. S. H. Vines, F.R.S., president, in the chair.—Mr. Clement **Reid** exhibited drawings by Mrs. Reid of fruits and seeds of British pre-Glacial, inter-Glacial, and Roman plants: 2nd series—Calyciforæ. The most interesting addition to the inter-Glacial flora is the south European *Cotoneaster Pyracantha*, which occurs abundantly on the Sussex coast in deposits which yield also *Acer monspessulanum, Najas minor*, and *N. graminea*. The pre-Glacial Calycifloræ include *Trapa natans*, but the rest of the species yet determined are still living in Britain; many, however, need further examination. The plants from Roman Silchester include the vine, bullace, damson, and coriander.—Dr. O. Stapf, on behalf of Mr. W. B. **Hemsley**, exhibited some specimens of *Primula vulgaris*, Huds., which displayed the phenomenon of phyllody of the calyx in an unusual degree.

Physical Society, April 22.—Dr. R. T. Glazebrook, F.R.S., president, in the chair.—Calculation of colours for colour sensitometers and the illumination of " three-colour " photographic transparencies by spectrum colours: Sir W. de W. **Abney**. In three-colour photography, photographs have to be taken through a red, a green, and a blue screen, the transparencies or prints from which are then viewed. The exact shades and hues of these screens depend on the light which is used for viewing the transparencies or on the colours employed in printing. The present paper confines itself to the former case.—Normal piling as connected with Osborne Reynolds's theory of the universe: Prof. J. D. **Everett**. The paper maintains that, in a struggle for existence between different kinds of closest piling, represented by separate clusters with room to change their arrangements, normal piling possesses great advantages, first, in its six sets of lines of spheres, which serve as battering rams, and secondly, in its four sets of tiers in closest array, which facilitate the coalescence of adjacent clusters.—Note on the diffraction theory of the microscope as applied to the case when the object is in motion: Dr. R. T. **Glazebrook**. According to the Abbe theory of microscopic vision, when a grating is placed on the stage of a microscope and illuminated by plane waves, diffraction images are formed in the focal plane of the object-glass and the images in the view-plane result from these, and this is undoubtedly true. It is proved in this paper that the image in the view-plane may change without an alteration in the *position* of the diffracted images.—An " automatic gas-pump " was exhibited by Mr. C. E. S. **Phillips**. The apparatus is constructed upon a plan which enables the pump, when once set in operation, to continue automatically and to produce as perfect a Torricellian vacuum as is possible.

EDINBURGH.

Royal Society, March 21.—Prof. Flint in the chair.—Dr. J. Erskine **Murray** exhibited and explained a differentiating machine, by means of which the first derivative of a given curve could be traced mechanically. A rod A is pinned at one end to a rectangular frame so as to be capable of revolution in the plane of the frame. A second rod B is retained by guides on the frame so as to be capable of motion only in the direction of its length. A pin in B engages in a longitudinal slot in A, and thus the distance between B and the pin about which A revolves is constant. The displacement of B relatively to the frame is therefore proportional to the tangent of the angle of inclination of A. If the revolving rod A be guided by hand so as to be always tangential to the given curve, a curve the coordinates of which are proportional to the differentials of the original curve is traced out by any point on B. The frame supporting the rods is free to move in direction X at right angles to the rod B. In order to eliminate the *y*-coordinate of the original curve, the board on which the derived curve is traced is free to move in OY but not in OX.—Dr. J. **Halm** gave an account of his spectroscopic observations of the rotation of the sun, which had been carried on at the Royal Observatory, Edinburgh, since 1901. The method employed was essentially that used by Duner, but some simplification and greater steadiness of the apparatus had been secured by the employment of a siderostat and heliometer. The results so far obtained seem to point to a decisive influence of solar activity upon the surface rotation. By arranging the results in two groups, one comprising the observations of 1901–2, a time of sun-spot minimum, and the other those of 1903, at a period of vigorously renewed solar activity, Dr. Halm obtained undoubted evidence of the existence of systematic differences between these two groups of quite unexpected magnitude. The decrease of angular velocity from the equator towards the poles, as observed in 1901–2, agreed very well with that found by Duner in 1887–9, also at a time of sun-spot minimum. But the appearance of spots in 1903 was accompanied by an extraordinary increase of angular velocity in high latitudes. It seemed as if the spots had caused the superficial layers to rotate more in accordance with the law of rotation of a rigid body, a mode of statement, however, which was not to be accepted as involving any physical theory.—In a paper on the viscosity of aqueous solutions of chlorides, bromides, and iodides, Dr. W. W. **Taylor** and Mr. Clerk **Ranken** gave determinations of the relative viscosities of KCl, KBr, KI, HCl, and HBr, in solutions containing 1 mol., 2 mol., and 3 mol. per litre at 0°, 15°, and 25°. The effect of temperature change and concentration on the viscosity was found to be different for the chlorine, bromine, and iodine solutions. The molecular conductivities of the fifteen solutions at 0° were also determined, and showed no greater differences than for solutions of similar concentration at 18°.—In a note on the unit of relative viscosity and on negative viscosity, Dr. W. W. **Taylor** pointed out the disadvantages of expressing the relative viscosities of solutions by taking as unit the viscosity of the solvent at the temperature of experiment. Instead, they should all be referred to water at 0° as standard. Until quite recently only aqueous electrolytes were known to exhibit the phenomenon of "negative viscosity," *i.e.* the viscosity of the solution less than that of the solvent at the same temperature. According as the temperature coefficient of the solution is greater or less than that of the solvent, a solution may exhibit negative viscosity at high temperatures or at low temperatures.—In a paper on the action of chloroform on the heart and arteries, Prof. **Schäfer** and Dr. **Scharlieb** showed, as had been previously proved by Gaskell, McWilliam, Hill, Embley, Sherrington, and others, that chloroform has a powerfully paralysing action upon the mammalian heart, inducing in it a condition in which all irritability is lost, and is only recoverable by washing away the poison by passing a stream of unpoisoned blood or saline solution through the cardiac vessels. They further show that this paralytic condition is not due to vagal inhibition, which is only rarely to be seen in chloroform anæsthesia, and is then probably due to dyspnœa; it is therefore not capable of being antagonised by atropine. Even such a powerful agent as suprarenal medulla, which is one of the strongest cardiac stimulants known, is powerless to provoke contraction in a heart paralysed by chloroform. But sometimes artificial respiration by chest compression may, by inducing some sort of circulation through the coronary vessels, cause the removal of the drug from the heart. No benefit has been obtained by directly "massaging" the heart. The addition of a small percentage of ammonia vapour to the chloroform-laden air used for inhalation is shown to have a markedly beneficial effect upon the result, the heart's force and the blood pressure and respiration being maintained far better than with chloroform alone. Alcohol vapour has a similar but less marked effect. On the other hand, too large a proportion of ammonia vapour is liable to produce instant and permanent arrest of the heart's action. While the respiration usually stops before the heart, in some cases the cessation is simultaneous, and in a few the heart ceases before the respiration. After having completely stopped the heart may after a minute or two recommence to beat, but the respirations rarely begin again spontaneously, except that, as in asphyxia, a staircase of about a dozen respirations may make its appearance long after the ordinary respiratory movements have ceased. These are, however, ineffectual to produce recovery, and if artificial respiration be not resorted to the heart soon ceases permanently. The effect of chloroform upon the arterioles has been determined both in the frog and in the isolated mammalian kidney by perfusion of Ringer's solution containing dissolved chloroform. In the frog, solutions containing from 1 in 200 to 1 in 20,000 produce constriction of arterioles in proportion to the amount of chloroform contained in solution. In the mammal, while stronger solutions (such as 1 in 500) produce powerful constriction of arterioles, dilatation is obtained with weaker solutions (1 in 5000), a strength which in the frog produces marked contraction. This confirms an observation by Dr. C. J. Martin, recently communicated to the Physiological Society. Further experiments are needed to clear up this discrepancy between the results in the frog and mammal.—Mr. G. A. **Carse** communicated a paper on the thermal expansion of solutions of the hydroxides of sodium, barium, and strontium, in each of which the volume of the solution is less than that of the water used in its preparation. In the case of sodium hydroxide the expansion in all cases, whether positive or negative, increased algebraically with rise of temperature. The same was true for strontium hydroxide. In the case of barium hydroxide the expansion was so small and the variation with temperature so slight that nothing definite could be predicated, although all solutions examined agreed in giving negative expansion. With sodium hydroxide the maximum contraction point slowly shifted towards the concentration origin with rise of temperature.—Mr. John **Dougall** presented a complete and elaborate discussion of the analytical theory of the equilibrium of an isotropic elastic plate. The solution was obtained in the first instance for an infinite plate, and was then applied to cases of finite plates.—The Rev. F. H. **Jackson** communicated a theorem relating to a generalisation of the Bessel function.

PARIS.

Academy of Sciences, April 25.—M. Mascart in the chair.—Report presented by the commission charged with the scientific control of the geodesic operations at the equator. A description of the work done during the year 1903, and a sketch of that proposed for 1904 and 1905. Unfavourable meteorological conditions interfered considerably with the work done last year.—M. Bigourdan was elected a member in the section of astronomy in the place of the late M. Callandreau, and M. Gordan a correspondant for the section of geometry in the place of the late Prof. Salmon.—Note on an earthquake at Roustchouk, in Bulgaria, communicated by the French consul.—Observations on the comet 1904 *a* (Brooks), made at the Observatory of Besançon: P. **Chofardet.** On April 19 the comet appeared as a star of the ninth magnitude, with a rounded head 1′ in diameter, and with a central nucleus. There was a slight tail from 2′ to 3′ in length in the direction of the south-west.—Observations on the comet 1904 *a* (Brooks), made at the Observatory of Paris: M. **Salet.**—Provisional elements of the Brooks comet (1904, April 16): G. **Fayet.**—The Leonids in 1903, and the determination of their height by means of simultaneous observations: Maurice **Farman,** Em. **Touchet,** and H. **Chrétien.** The simultaneous observations were carried out at stations 28·7 kilometres apart, and results were obtained for eighty-three meteors. The average height of the first appearance was 103·6 kilometres (extremes 138·5 and 53·9), of disappearance 75·8 (extremes 131·6 and 33·4), the average length of the trajectory being 35·2 kilometres.—On the singularities of analytical functions: L. **Zoretti.**—An attempt at a determination of the difference of longitude chronometrically: Paul **Ditisheim.** The difference of longitude between Paris and Neuchâtel was determined by carrying with special precautions five chronometers between the two places, the mean result being 18m. 28·86s. It is proposed to check this by a new telegraphic determination.—On the fall of water in rivers: Edmond **Maillet.**—On the melting point of gold and the expansion of some gases between 0° and 1000° C.: Adrien **Jacquerod** and F. Louis **Perrot** (see p. 14).—On the atomic weights of hydrogen and oxygen, and on the probable value of their atomic ratio: Ph.-A. **Guye** and Ed. **Mallet.** The method proposed by Vallier for treating a limited number of observations is applied to the reduction of the observations of E. W. Morley on the atomic weights of hydrogen and oxygen. The final value is $O=15{\cdot}8787$ for $H=1$.—Experimental researches relating to some cyclic amines: P. **Lemoult.** The heats of combustion of some amines calculated by means of the formula given by the author in a previous paper show in a few cases wide deviations, and it appeared advisable to re-determine experimentally some of these measurements. The results of determinations made with the Berthelot bomb for xylidine, monoethylaniline, *p*-anisidine, α-naphthylamine, and β-naphthylamine are given, and agree with the figures calculated from the formula within 0·5 per cent.—The formation of hydrogen silicide by direct synthesis from its elements: A. **Dufour.** At a very high temperature, hydrogen and silicon unite directly to form hydrogen silicide. The amount formed is small, and the product was identified by its chemical reactions and its boiling point (−116° C.).—The lead-aluminium alloys: H. **Pécheux.** Alloys containing 93, 95 and 98 per cent. of aluminium were obtained, the properties of which are described.—On colloidal gold: M. **Hanriot.** Colloidal gold, prepared by the method of Henrich, exhibits properties which are inconsistent with the assumption that it consists merely of finely divided gold.—A new indicator and its application to the detection of boric acid: Lucien **Robin.** The indicator proposed is extracted from mimosa flowers by weak alcohol. Its general behaviour is similar to that of phenol-phthalein, with the advantage that it can be used in the presence of ammonia. It gives a characteristic reaction with borates, and may be used for this purpose in the analysis of food products.—The action of magnesium and organo-magnesium compounds on bromophenetol: V. **Grignard.** Bromophenetol reacts readily with magnesium powder, giving ethylene and $C_6H_5O:MgBr$ instead of the normal compound $C_6H_5OCH_2CH_2MgBr$.—On the lactone of oxycrotonic acid and the γ-substituted crotonic acids: M. **Lespieau.**—Researches on the dinaphthopyranic series: R. **Fosse.**—Remarks on some peculiarities of the flora of Long Island: Ph. **Eberhardt.** The views of the author given in previous papers on the influence of the amount of atmospheric moisture on the growth and development of plants have received confirmation from a study of the growth of vegetation on Long Island.—Researches on the browning of the vine: L. **Ravaz.** The browning of the vine is a particular case of impoverishment of the plant brought about by production. It may be avoided by the use of manures rich in potash.—On the evolution of the relief of the plateau of Mehedinti, Roumania: E. **de Martonne.**—On the faults and undulations of the secondary and tertiary layers of the Loir: Jules **Welsch.**—On an albumen extracted from the eggs of fishes: the comparative chemistry of the sexual products in the same species: L. **Hugounenq.**—Autolysis of the tissues of the animal organism and the genesis of morbid phenomena: A. **Charrin.**—The colloidal state of metals in mineral waters; natural oxydases and their therapeutic action: F. **Garrigou.**—On a mechanical apparatus allowing of trepanning and vibratory massage: M. **Bercut.**

DIARY OF SOCIETIES.

THURSDAY, MAY 5.

ROYAL SOCIETY, at 4.—Election of Fellows.—At 4.30—Experiments on a Method of Preventing Death from Snake Bite, capable of Common and Easy Practical Application: Sir Lauder Brunton, F.R.S., Sir Joseph Fayrer, Bart., F.R.S., and Dr. L. Rogers.—A Research into the Heat Regulation of the Body by an Investigation of Death Temperatures: Dr. E. M. Corner and Dr. J. E. H. Sawyer.—(1) A Note on the Action of Radium on Micro-organisms; (2) Further Note on Some Additional Points in Connection with Chloroformed Calf Vaccine: Dr. A. B. Green.—On Certain Physical and Chemical Properties of Solutions of Chloroform in Water, Saline, Serum and Hæmoglobin. A Contribution to the Chemistry of Anæsthesia—Preliminary Communication: Prof. B. Moore and Dr. H. E. Roaf.

LINNEAN SOCIETY, at 8.—British Freshwater Rhizopoda: J. Cash.—On Coloration in Animals and Birds: J. Lewis Bonhote.—On the Cranial Osteology of the Fishes of the Families Mormyridæ, Notopteridæ and Hyodontidæ: Dr. W. G. Ridewood.

RÖNTGEN SOCIETY, at 8.30.—The Röntgen Society; its Past Work and Future Prospects: J. J. Vezey.—Some Experiments with Alpha Rays: F. H. Glew.

CHEMICAL SOCIETY, at 8.—The Slow Combustion of Ethane: W. A. Bone and W.E. Stockings.—Note on the Hydrolysis of Starch by Diastase: J. S. Ford.—The Reesin Acids of the Coniferæ. Part I. The Constitution of Abietic Acid: T. H. Easterfield and G. Bagley.—The Action of Radium Rays on the Halides of the Alkali Metals, and Analogous Effects produced by Heat: W. Ackroyd.—The Dynamic Isomerism of Glucose and of Galactose. Solubility as a means of Determining the Proportions of Dynamic Isomerides in Equilibrium: T. M. Lowry.—A Study of the Substitution Products of *ar*-Tetrahydro-α-naphthylamine, *ar*-4-Bromotetrahydro-α-naphthylamine and *ar*-Tetrahydro-α-naphthylamine-4-sulphonic acid: G. T. Morgan, Miss F. M. G. Micklethwait, and H. B. Winfield.—The Additive Products of Benzylideneaniline with Methylacetoacetic Ester and Acetoacetic Ester: F. E. Francis and Miss M. Taylor.

INSTITUTION OF MINING AND METALLURGY, at 8.—Discussion on Laboratory Equipment (conclusion).

FRIDAY, MAY 6.

ROYAL INSTITUTION, at 9.—Anthropoid Apes: Dr. P. Chalmers Mitchell.

PHYSICAL SOCIETY, at 8.—An Experiment with Lubricating Oil: W. A. Price.—Some Instruments for the Measurement of Large and Small Alternating Currents: W. Duddell.—Exhibition of Apparatus from the National Physical Laboratories.

MONDAY, MAY 9.

ROYAL INSTITUTION, at 5.—General Monthly Meeting.

FARADAY SOCIETY, at 8.—Studies in Viscosity: Dr. C. E. Fawsitt.—The Electrolytic Oxidation of Anthracine: Alberto Fontana and F. Mollwo Perkin.

SOCIETY OF ARTS, at 4.30.—The Majolica and Glazed Earthenware of Tuscany: Prof. R Langton Douglas.

TUESDAY, MAY 10.

ROYAL INSTITUTION, at 5.—Meteorites: L. Fletcher, F.R.S.

SOCIETY OF ARTS, at 8.—Crystalline Glazes and their Application to the Decoration of Pottery: W. Burton.

WEDNESDAY, MAY 11.

SOCIETY OF ARTS, at 8.—Early Painting in Miniature: R. R. Holmes.

GEOLOGICAL SOCIETY, at 8.—On some Quartzite-Dykes in the Mountain Limestone near Snelston (Derbyshire): H. H. Arnold-Bemrose.—Phenomena bearing upon the Age of the Lake of Geneva: Dr. C. S. DuRiche Preller.

THURSDAY, MAY 12.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—I the discussion on Messrs. Merz and McLellan's paper is concluded at the meeting of May 5, Messrs. Parsons, Stoney and Martin's paper on the Steam Turbine as applied to Electrical Engineering will be read and discussed.

MATHEMATICAL SOCIETY, at 5.30.—Some Mathematical Instruments: C. Cooke (communicated by Major P. A. MacMahon).—On the Evaluation of Certain Definite Integrals by Means of Gamma Functions: A. L. Dixon.—Generalisations of Legendre's Formula

$$KE' - (K - E)K' = \tfrac{1}{2}\pi:$$

A. L. Dixon.—Note on the Integration of Linear Differential Equations: Dr. H. F. Baker.

SOCIETY OF ARTS, at 4.30.—British Grown Tea: A. G. Stanton.

FRIDAY, MAY 13.

ROYAL ASTRONOMICAL SOCIETY, at 5.

MALACOLOGICAL SOCIETY, at 8.—List of Mollusca collected during the Commission of H.M.S. *Waterwitch* in the China Seas, 1900–1903, with Descriptions of New Species: Surgeon K. Hurlstone Jones, R.N., and H. B. Preston.—On a Carboniferous Nautiloid from the Isle of Man: G. C. Crick.—Notes on the Genus Anoma: E. R. Sykes.—New Land Shells from New Zealand: Henry Suter.

CONTENTS.

SUPPLEMENT TO "NATURE."

LORD KELVIN ON OPTICAL AND MOLECULAR DYNAMICS.

Baltimore Lectures on Molecular Dynamics and the Wave Theory of Light. By Lord Kelvin, O.M., G.C.V.O., P.C., F.R.S. Pp. xxii+694. (Cambridge: University Press, 1904.) Price 15*s.* net.

IN the autumn of the year 1884 Lord Kelvin delivered at Johns Hopkins University a course of lectures "On Molecular Dynamics and the Wave Theory of Light," mainly *extempore,* which, having very fortunately been reported stenographically by Mr. A. S. Hathaway—one of his band of auditors, the famous "twenty-one coefficients"—were issued to the world unrevised in a papyrograph volume at the end of the same year, and have since been known as the "Baltimore Lectures." The report, being nearly *verbatim,* showed how comparatively slight were the immediate preparations that Lord Kelvin had made for some portions of his task, and thus had the great advantage of revealing the procedure and attitude of an investigator of transcendent genius in face of regions of his subject more or less new to him.

One result, in fact, of his enthusiasm for the new aspect of optical propagation revealed by the phenomena of anomalous dispersion, and of the wealth of mechanical illustration which, taking his audience into collaboration, he provided for this hitherto rather abstract subject, was to start a period of enlivened interest, illustrated by memoirs by Lindemann and others, abroad and in this country; this has now reaped a reward in fruitful comparison of the theory with experimental data obtained over the enormous range of six or seven octaves by Langley, Rubens, and other pioneers. Irrespective of this phenomenon of anomalous dispersion in the spectrum near a region of absorption of the light, Lord Kelvin's own estimates of molecular dimensions had already ruled out the earlier attempts of Cauchy and his followers to base dispersion on mere loading of the æther by massive molecules. On such a theory of inert molecular masses the proportional dispersion per octave could depend only on the ratio of the intermolecular distance to the wave-length, no other magnitudes coming into consideration, and it must depend on the square of this ratio; thus the actual 1 per cent. dispersion for glass would be explicable by about 10 molecules per wave-length, while with the real number, about 10^4, the circumstances would be practically the same as for a uniformly distributed load, which would give no dispersion at all. The modern theory of dispersion thus must rest on an investigation of the interaction between the forced internal vibration of the molecules, conditioned by their own proper periods, and the periodic impressed vibration of the wave-motion which produces it. So long as the internal dynamics of the molecule remain unexplored, only general principles can be applied, and it matters little to the argument whether it is conducted in terms of a mechanical conception of radiation or in terms of the electric theory; in either case only the general frame into which the facts are to be fitted can be supplied by theory. On a mechanical view, mere loading may produce refraction but not dispersion; so on the electric view, even if there were no free periods in the ordinary sense, there would remain an index of refraction equal to the square root of the dielectric coefficient.

In the preface to the present volume Lord Kelvin states that he chose for his lectures the subject of the wave-theory of light with the object of accentuating its points of failure, thereby intending to stimulate the activities of his audience towards extending further "the floods of new knowledge splendidly enriching the whole domain of physical science" that had flowed from the theory.

"We all felt that difficulties were to be faced and not to be evaded; were to be taken to heart *with the hope of solving them if possible*; but at all events with the certain assurance that there is an explanation of every difficulty, though we may never succeed in finding it. It is in some measure satisfactory to me, and I hope it will be satisfactory to all of my Baltimore coefficients still alive in our world of science, when this volume reaches their hands; to find in it dynamical explanations of every one of the difficulties with which we were concerned from the first to the last of our twenty lectures of 1884."

The sentences quoted contain the key to much (though far from all) of Lord Kelvin's mathematical investigation of the last twenty years. The result is this magnificent volume of more than 700 pages, which in its variety of contents and width of grasp forcibly recalls the original "Thomson and Tait" of forty years ago, except, indeed, that the form of a treatise being now dispensed with, the author is at liberty to go directly for the various subjects of interest that hold his attention without any necessity for preliminary didactic expositions.

The earlier lectures are reproduced here with additions within brackets, but the author soon found that it was easier to re-write the greater part of the material. His expression of distrust of "the so-called electromagnetic theory of light" (p. 45) stands as in the original. Along with it is the interesting statement that he had worked out for himself, as early as the year 1854, the result that an electric impulse is propagated along a cable with a velocity of the order of that of light, and that it only required a knowledge of the ratio of the electric units to lead to the result that for an air dielectric it would agree with the velocity of light in air. An investigation of such linear propagation, on the lines now familiar, and thoroughly developed by Heaviside, is inserted as the last appendix (L). The first published determination of this velocity is contained, as is well known, in Kirchhoff's memoir of 1857; there the result is deduced on the basis of Weber's theory of moving electrons, which act on each other instantaneously at a distance, the law of attraction involving their velocities as well as their distance apart. Neither there nor in the ordinary modern investigation for cables is there any reference to transmission of electric effects across space with finite speed; that makes no difference for the case of enclosed cylindrical dielectrics of diameter small compared with the wave-length, for with them it is only the adjacent parts of the electric distribution and current on the conducting boundaries that are sensibly effective as regards the internal state of each element, and their mutual influences are adjusted in times which are in any case inappreciable in an estimate of the times required for transmitting effects along the cylinder. For wider cylinders or shorter waves, the Weberian formula of Kirchhoff gives a result differing from the velocity of radiation, of which Maxwell was of course well aware, while Lord Kelvin's approximate treatment is no longer applicable. We now know that, to transform the Weber-Kirchhoff formulæ into those of the modern electron theory, it is only necessary, in the integral expressions for the vector potential of the current and the scalar potential of the charges, to consider each element as propagated through space with the velocity of radiation instead of being transmitted instantaneously.

One of the great historical difficulties in optical theory, above referred to, was that of embracing the phenomena of propagation in crystals and of reflection from transparent bodies within the dynamics of ordinary elastic solid media. This problem was resolutely attacked by Green nearly sixty years ago with a brilliant but unsuccessful result, and no success in adapting his analysis was achieved by anyone else until some three years after the Baltimore lectures were delivered. Then Lord Kelvin produced his theory of an elastic medium with finite rigidity but perfectly labile as regards compression, and characteristically illustrated it by material structures, such as a mass of foam *in vacuo*, which resist distortion but are insensitive to shrinkage of volume. If luminiferous media were elastically like this, the necessity of continuity of displacement normal to a reflecting interface would no longer press upon the theory, for the two media would stretch locally in the direction of the normal without reaction on the other stresses, just as much as might be required. And it was promptly pointed out by Glazebrook that the arrangement which thus allowed Fresnel's laws for reflection was also competent to explain propagation in crystals by the simple expedient of making the inertia æolotropic, while the lability as regards compression is again all that is wanted to obtain the ascertained laws of MacCullagh's theory (Lord Kelvin's rotational æther) or the electric theory for crystalline reflection. In fact, one advantage accruing with the electric theory is that it dissects the accepted and unique formal analysis of propagation of light into a series of linear relations between various vectors, each of which has a distinctive name and quality; and according as we take one or other of these vectors to represent a displacement of an elastic medium, we have the various mechanical theories of Fresnel, MacCullagh, and Sarrau and Boussinesq, differing in the types of interfacial continuity that they require, but algebraically the same; the exact duality between the systems of Fresnel and Sarrau is in this way open to direct inspection. Such, then, was Lord Kelvin's solution of 1887, in which all media were taken to be labile as regards compression, the type which Green had rejected under the idea that it was intrinsically unstable; this classical objection Lord Kelvin at once removed by the illuminating remark that the medium only required to be held fixed at an outer boundary to prevent any internal collapse. There was something unnatural about this, as its author admitted, and it now appears that an æther so constituted would still absorb much condensational energy from vibrators; but here in 1904 comes the further crucial remark that only one of the two media need be thus labile in order to confer the requisite freedom for reflection without interference from compressional waves; the æther itself may remain incompressible, as Green took it to be, but the interaction of æther and molecules in every material body is to be such as always to make it labile or inelastic for compressional disturbance. It need not, however, be absolutely labile if Fresnel's laws are to be satisfied only within experimental limits. And here the remarkable peculiarity of highly refractive substances like diamond, the "adamantine property" discovered by Airy, which replaces an abrupt change of phase in passing the polarising angle by a gradual though rapid one, comes into consideration; if only the velocity of propagation of the condensational wave in material media is a small complex quantity, the complexity will introduce just the gradual change of phase that is required in order to include that property. Moreover, if this velocity is a pure imaginary, there will be no loss of energy involved; this happens for a granular or discrete medium whenever there are periods of free internal vibrations among its constituent granules that are longer than the period of the waves under consideration. If we cannot include the adamantine property as introduced in this way through total reflection for compressional waves, no resource is known except that of gradual transition at the surface; this Lord Rayleigh has shown to be the main cause for water, as careful cleansing of the surface almost entirely removes the phenomenon.

In this chain of simple, yet brilliant and attractive, ideas, Lord Kelvin has gradually forged a reconciliation between fact and theory that would probably have been received with universal acclaim thirty years ago. Nowadays, as regards most people, the need has ceased to be so strongly felt; for better for worse most of us are now wedded to the electric theory of light, the creation of Lord Kelvin's most famous disciple, which forms a consistent scheme of the relations of electricity and radiation, perfectly definite and unambiguous with the large simplicity of nature itself, that has led into no essential contradiction with fact, though it has many times predicted phenomena of the most essential and fundamental kinds.

Not that there is any difference of opinion as to the value of the electric theory. Lord Kelvin would doubtless agree that, as a new mode of grouping of the relations, it has placed them in a most fruitful light, and shown the directions of natural development. He would perhaps say that it is a successful description rather than an explanation, and he would probably desire to modify the terms of the description in order to bring it closer to the train of dynamical ideas in which he would search for the explanation. And here we are at the parting of the ways. Is it incumbent on us to treat the æther as strictly akin to the material bodies around us? or may we assign to it a constitution of its own, to be tested by its success in comprehending the complex of known relations of physical systems? This is not the occasion to follow up that question. It would appear that Lord Kelvin cannot grant that such a constitution has been determined until it has made clear in full detail the mode of connection of the atom with the æther, so that a precise mechanical model of it could be imagined; whereas, on the other side, it may be held to be the merit of the scheme that it evades such a hopeless task, and defines physics as relating to the surrounding field of æthereal activity of the molecules rather than to the molecules themselves, which must remain in many respects inscrutable—a consummation that would hardly have been attempted had not the illuminating conception of Lord Kelvin's vortex-atoms shown the way.

The plan along which Lord Kelvin now finds it most hopeful to pursue ultimate physical synthesis admits the existence of "electrions," freely mobile through æther simply because two media can be superposed independently in the same space, which exert direct *force at a distance* upon the æther as also does the matter itself, that the forces are so enormous as sensibly to compress or expand the æther around these nuclei, and that the source of electric, chemical and elastic action is thus to be found. This conception is developed over many pages with the power and conciseness that are familiar to his readers; it remains a question for the future whether it will prove to be a fruitful theory; it certainly forcibly illustrates many deep molecular phenomena, and demands, and will doubtless receive, very careful study.

The point of view is illustrated in p. 300, in treating of the spheres of activity of the various kinds of molecules, where Lord Kelvin states that this "is a most interesting subject for molecular speculation, though it or any other truth in nature is to be explained by a proper law of force according to the Boscovichian doctrine which we all now accept (many of us without knowing what we do) as the fundamental hypothesis of physics and chemistry." When one reflects that to Lord Kelvin, more than to anyone else except Faraday, has been due the stimulus to replace artificial mathematical attractions by activity propagated according to simple relations, this sentence may perhaps be taken as expressing his belief that in probing into the details of the dynamics of the unexplored molecules we are still practically confined to the partial but fruitful conception of mutual forces.

Thus in the appendix entitled "Aepinus Atomized," a definite foundation is postulated by taking the electrion to be a very minute negative ionic charge, and an atom to involve a positive ionic charge rigidly distributed through a much larger sphere, but in normal condition neutralised by one or more electrions inside it, which may be occasionally shot out as kathode rays; and electrions and atoms can be wholly or partially superposed in the same space without mutual deformation. On this basis the statical configurations of electrions in the spheres, that can represent neutral atoms, are discussed and are applied to the dielectric quality of matter and its æolotropy in crystals, to the intricate and elegant details of the pyroelectric and piezoelectric quality in the latter, and in more general terms to the nature of conduction and its striking relation to temperature, so different in pure metals and in nonmetals and alloys. In further appendices the same conception is applied to crystalline dynamics, where auxiliary Boscovichian laws of pure attraction are also introduced, because Lord Kelvin thinks a purely electric basis is too narrow, even when not restricted to spherical nuclei as here. The whole is developed on all sides with marvellous directness and facility in tracing out crystalline groupings in space, which, however, make it difficult reading, though relieved by frequent flashes when a vivid analogue of some ascertained experimental relation appears. It is a conception such as this that Lord Kelvin has in mind in his postulate, above referred to, that material bodies are labile to optical compressional waves. The free molecular vibrations that must correspond to a bright line-spectrum do not come in for consideration; nor does the now burning question of actual dissociation in typical chemical atoms.

The task of making a review of a book like the present one can at best be very imperfectly executed. The book is largely a new creation. It surveys a vast range, all the cognate subjects on which the author feels that he has something new to communicate—laws of diffusion of gases, transparency of the sky, detailed dynamics of optical chirality, motion of molecules through æther, front of a wave-train in a dispersive medium, the finiteness of the universe, atomic theory of electricity, regelation and plasticity of ice, waves and ripples on water and their dispersion, crystalline structure and iridescence, partition of energy in molecular systems, crystalline dynamics on Boscovich's principles, electric and magnetic screens. Instead of putting the question, Is this subject clearly and strikingly expounded? one has rather to ask, Is this new departure or revolutionary idea justified by its results? Any off-hand decision is, of course, impossible. When one is in difficulty over inscrutable or irreconcilable phenomena, it will be a book to turn over to see what the premier authority has to say on the subject in hand; for what he says is not lightly thrown from his pen, it is the work of twenty years, and withal it forms a consistent whole. In the remarks here made about only a few of the many themes of which it treats, it is the obviously revolutionary element that has attracted attention. There is, however, one very serious criticism as to which there can be no question. This book of seven hundred pages—dealing in concise manner with nearly all the most intricate topics of dynamical and molecular theory, with the cross references and recurrences to previous passages that are involved in twenty years of preparation—is without an index, and the detailed table of contents does not meet the want. The thanks of the scientific world will surely go to the veteran author, now by a happy choice Chancellor of the university which he has so long adorned, for this splendid gift, which stimulates and educates even where it fails to convince, and bears on every page evidence of profound and unwearying thought. J. L.

BLOOD RELATIONSHIPS.

Blood Immunity and Blood Relationship; a Demonstration of Certain Blood-relationships amongst Animals by Means of the Precipitin Test for Blood. By George H. F. Nuttall, M.A., M.D., Ph.D. Pp. 444. (Cambridge: University Press, 1904.)

TSCHISTOWITSCH was the first to observe that if a rabbit were subjected to repeated injections of serum from an animal of a different species, it reacted to the introduction of the foreign proteid by forming and accumulating in its blood a substance which, when added to a solution of the particular serum injected, gave rise to a precipitate. These experiments at once aroused considerable interest, and were confirmed and extended by a number of observers on account of their importance in relation to the processes whereby the organism protects itself against the introduction of proteid poisons and micro-organisms by the formation of so-called anti-bodies.

The interest of the observations is not, however, confined to the doctrine of immunity, for fuller knowledge of the phenomena has shown them to have important applications to both forensic medicine and zoology. The value to the former was pointed out by Uhlenhuth and others, who directed attention to the fact that the serum of an animal previously subjected to repeated injections of human serum forms a very sensitive test for the same, and can therefore be used for the detection of human blood. The importance of precipitin phenomena to the zoologist has been particularly insisted upon by Dr. Nuttall, and the present volume is largely concerned with results of interest from this point of view

When the precipitins were first discovered, it was concluded that the reaction was strictly specific, and that the serum of an animal injected with human serum only formed precipitates with the serum of man, and one injected with ox-serum only when added to the serum of the ox. Nuttall and Uhlenhuth showed, however, that no such hard and fast line could be drawn. Indeed, the development of our knowledge of the specificity of the precipitin reaction is in great measure due to the work of Dr. Nuttall and to that of his pupils, Drs. Graham Smith and Sangar. However, although not strictly specific, a precipitin precipitates the serum of the same species of animal as that used in its preparation more readily and in greater amount than that of animals of other species, and the difference is least marked when the animals are closely related, as in the case of the horse and the donkey. From these results, Dr. Nuttall conjectured that the varying degree to which a precipitin reaction occurred might afford a valuable indication as to blood relationship.

The present volume contains the results of experiments, undertaken by the author in conjunction with Drs. Graham Smith and Sangar, with a large number of anti-sera upon the blood of 586 different species of animals.

The book is divided into two parts. Part i. is devoted to a condensed summary of our knowledge on anti-bodies in general. It commences with a brief but clear account of Ehrlich's theory regarding the formation of anti-toxins and anti-bodies generally. This is followed by a series of paragraphs on ferments and anti-ferments, cytotoxins, hæmolysins, bacteriolysins, agglutinins, &c., which in style suggests the pages of a technological dictionary. Short sentences, each pregnant with some fact, and with reference attached, follow one another in bewildering succession. Many of these are contradictory, and it is to be regretted that there is no summing up by the author at the end of each paragraph.

This portion of the book does great credit to the author's industry and scholarship, but it makes impossible reading, and is only serviceable to one knowing the subject and wanting the references. After fifty pages one is glad to reach the end of part i., and to come to the subject-matter proper of the book, viz. the precipitins.

Part ii. commences with the methods for obtaining precipitating anti-sera. The style now leaves little to be desired, and this account is delightfully clear and complete, so that anyone wishing to repeat the experiments could hardly fail for want of adequate instructions. Sections ii. and iii. contain nearly all that is known of the nature of precipitin reactions and the effects of heat, peptic and tryptic digestion, filtration and putrefaction, upon both precipitins and precipitable substances. On p. 126, however, the statement is made that "no measurements of the amount of precipitin during the growth of immunisation have as yet been made which would correspond to those made upon antitoxin." One can only presume that this paragraph was written prior to the publication of von Dungern's quantitative experiments with the precipitins obtained by the injection of crab-plasma.

Section iv. deals with the specificity of the precipitins. After historically reviewing the views of different experimenters on this subject, and showing that increased knowledge has fully confirmed his earlier contentions against the absolute specificity of precipitin reactions, the author expresses himself as in entire agreement with the remark of Linoisier and Lemoiné: "Là où on a cru voir une action spécifique, un examen attentif ne permit de voir qu'une action particulièrement intense."

Section v. treats of precipitins obtained by the injection of other proteids from bacteria, milk, and higher plants. In section vi. are given in tabular form the results of 16,000 tests of 30 anti-sera with the bloods of a large number of animals. This particular series is not quantitative, and was presumably made before the author had devised his quantitative method, the reactions being entered as "full," "marked," "medium," "faint," and "nil." This is followed by a later series of 500 experiments made in conjunction with Strangeways with a quantitative method devised by the author, whereby the dilution of the serum and the time of reaction being constant, the actual volume of the conglomerated precipitate is measured in an ingenious way. The volume of the precipitate, with the homologous serum, is taken as the unit, and the volumes obtained with the sera of other animals are expressed in percentages of this unit.

The method and the interest of the facts brought to light by it will be clearer from two short examples.

Amount of precipitate obtained by adding anti-human serum to the serum of man and apes (expressed as percentages) :—

Man	100
Ourang	80
Cynocephalus mormon	50
Cercopithecus petaurista	50
Ateles vellerosus	25

Amount of precipitate obtained in a similar way by adding anti-horse serum to the serum of horse, donkey, zebra :—

Equus caballus	100
Equus asinus	84
Equus grevyi	58

Tested in this way the indications of blood relationship between man and the ourang are comparable to those between the horse and the donkey. The serum of other mammalia gave but traces of precipitate with the above anti-sera, and that of other vertebrates none at all.

In these precipitin-phenomena we have perhaps a really physiological test of blood relationship, and that, as the author suggests, "a common property has persisted throughout the ages which have elapsed during the evolution of animals from a common ancestor in spite of differences of food and habits of life." Anomalies do undoubtedly occur when working with any particular anti-serum, so that all conclusions must be controlled by experiments with anti-sera prepared from different individuals. Section viii. contains the results of 2500 similar tests, undertaken by Graham Smith, in the application of the method to the lower vertebrates and invertebrates. These will be of no less interest to zoologists, but space prevents our entering upon further particulars.

The ninth and last section deals with the practical application of precipitin reactions to legal medicine. As the precipitable substance in sera is a relatively stable body, is very resistant to the action of putrefactive organisms, and is not destroyed by drying, the detection of human blood by this means is not confined to stains of recent origin. Indeed, Graham Smith and Sangar have examined a large number of articles from the collection of the Criminal Investigation Department, Scotland Yard, and have succeeded in identifying human blood stains which were thirty years old.

The fact that anti-human serum forms precipitates to some extent when added to the serum of monkeys does not seriously diminish the forensic value of the precipitin test for human blood, for the plea that suspected bloodstains were of simian origin would seldom be raised and hardly ever substantiated.

The volume concludes with an excellent bibliography on precipitins and allied subjects which occupies sixteen pages!

In addition to containing the methods and experimental results whereby the author and his associates, Drs. Graham Smith and Sangar, have tested and developed the precipitin reaction as an indication of blood relationship, the book contains practically all that is known on the subject of precipitins up to the present time, and will therefore be indispensable to anyone desiring to become acquainted with or to work upon this subject.

CHARLES J. MARTIN.

THE MOON.

The Moon. A Summary of the Existing Knowledge of our Satellite, with a Complete Photographic Atlas. By Wm. H. Pickering. Pp. xii+102; 100 illustrations. (New York: Doubleday, Page and Co., 1903.) Price 10 dollars net.

IT has so long been taught that the moon is a world on which nothing ever happens that it may come as a surprise to many to learn that the probability of frequent changes in the lunar surface is now seriously advocated. The author of this book, who is a well known American astronomer, is convinced that there are daily alterations over small areas which cannot be explained either by shifting shadows or varying librations, and therefore infers that there are real changes in the surface detail. The observations on which this conclusion is based are collected in the present volume, which also includes a more general account of our satellite, and contains the first complete photographic atlas which has yet been published.[1]

To make a thorough study of the moon, Prof. Pickering some years ago suggested the use of a telescope of great focal length, and, as so frequently happens in America in such circumstances, the generosity of two anonymous donors enabled him to try the experiment. The instrument actually employed was a 12-inch objective of 135 feet focal length, giving a direct image of the moon nearly 16 inches in diameter, and to obtain the advantage of such "steady" atmospheres as can only be found in low latitudes it was taken out to Jamaica and set up at Mandeville, 2080 feet above sea-level. The long telescope tube was erected on the side of a convenient hill with its axis in the direction of the pole, and light was reflected into it at the lower end by a clock-driven mirror. The instrument was so far successful that all the negatives for the atlas were obtained within seven months.

The atlas shows the lunar surface in sixteen sections, each of which is exhibited under five different conditions of illumination, and there is in addition a good picture of the full moon, with the necessary key maps, besides other illustrations of interest. Although the photographs are not all of the finest definition, the completeness of the series gives them a special value, and the atlas will doubtless prove extremely useful to all who are engaged in lunar studies.

Apart from the photographs, the chief interest of the book lies in the observations and arguments which are put forward in favour of lunar activities. The moon is so near that no improbably great area need be affected to make a change visible to an observer on the earth, but any real variations are liable to be

[1] The atlas is also published in the *Annals* of the Harvard Observatory, vol. li., 1903.

masked by the varying conditions of illumination. This difficulty does not, of course, disappear even when series of photographs are under examination; in the words of the author (p. 91):—

"It was soon found that for certain regions, notably those in the northern half of the disc, the change in appearance produced by the difference of lighting rendered it absolutely impossible to identify the same formation upon the plates taken at (lunar) sunrise and sunset and those taken at noon."

Photographs at intermediate phases were accordingly taken, and by aid of these the connection can be traced.

Photographs, indeed, introduce another difficulty. Slight changes in exposure and development were found sometimes to produce very misleading results, and it is pointed out that the only safe procedure is to confirm all suspected changes by extended observations under different conditions with the telescope. There is, however, no reason to suppose that the author is unfamiliar with the many pitfalls, and the interesting results of his labours may therefore be received with some confidence, or at least as demanding careful investigation by other observers.

Attention was directed by the author ten years ago to the variability of many of the dark spots which are dotted over the lunar surface, the three in Alphonsus being probably familiar to most observers. The view then expressed that these are patches of organic growth resembling vegetation, which spring up and die during the long lunar day, still seems to give the only simple explanation of the appearances observed. The spots are said to be darkest near full moon, when shadows are geometrically impossible, and a real change in the reflecting surface therefore seems to be highly probable.

On the question of active lunar craters, the chief facts relating to Plato and the much discussed case of Linné are summarised, and an account is given of phenomena observed in the crater forming the source of Schroter's valley which bear a striking resemblance to those accompanying the active eruption of a terrestrial volcano. Part of the description reads:—

"Dense clouds of white vapour were apparently rising from its bottom and pouring over its south-western crater wall in the direction of Herodotus" (p. 40).

The changes in this "vapour column" are said to be visible with a 6-inch telescope under ordinary atmospheric conditions, so that the reality of the phenomenon need not long remain in doubt, whatever explanation may be adopted. The author evidently believes that there is an actual emission of vapour, and he points out that as water cannot exist as a liquid on account of the rarity of the lunar atmosphere, it would take the form of snow or hoar frost.

Many of the changing appearances of lunar details are, in fact, attributed to deposits of snow and hoar frost which melt under the influence of the sun's rays, and are re-deposited when those rays are withdrawn. Among other evidence that there is snow on the moon, two photographs of the full moon are reproduced, one representing it as ordinarily seen, while the other is intended to exhibit the principal snow-covered areas; as these are differently printed copies from the same negative, the illustration is anything but convincing in the absence of details as to the printing processes. Other examples are more satisfactory. Linné, for instance, is surrounded by a white halo, which is stated to be not only now permanently smaller than it was thirty years ago, but to change with the altitude of the sun in a manner analogous to the seasonal variations of the polar caps of Mars. In this case the author had the happy thought to inquire if there were any variation during a lunar eclipse, the idea being that the withdrawal of sunshine for a couple of hours or so might produce an appreciable increase in size. Such an enlargement appears to have been established at the Lowell Observatory in 1898, and by the author himself in 1899, 1902, and 1903; another observer, Mr. Saunder, however, seems to have been somewhat doubtful as to the reality of the slight increase which his measures indicated in the eclipse of 1903, and as his observations would make the halo twice as great as those which the author made on the same occasion, further observations of this kind are evidently desirable.

It is also considered probable that many of the remarkable changes which have long been recognised in the craters Messier and Messier A are to be accounted for by varying depositions of snow.

Permanent deposits of snow in the craters themselves are believed to furnish an adequate explanation of the striking brightness of such craters as Aristarchus, and even the long bright streaks, such as those which radiate from Tycho, are attributed to the same substance. The long streaks are considered to be composed of a multitude of smaller snow streaks issuing from small white craterlets, usually less than a mile in diameter, many of which show a tendency to occur along lines which are probably cracks or lines of weakness in the lunar surface.

The "riverbeds" and lunar "canals," which the author has detected, present many features of interest, and the latter may be of special importance in view of the light which they may throw on the nature of the corresponding features of the planet Mars.

While some of his researches tend to modify the prevalent idea that the moon is a dead world, the author has no revolutionary views to put forward as to the general character of the lunar formations. He says:—

"There seems, indeed, to be no feature found upon the moon which is not presented by the Hawaiian volcanoes, and there is no feature of the volcanoes that does not also have its counterpart upon the moon. Even the cause of the bright streaks upon the moon . . . is partly illustrated by Hawaii" (p. 25).

Sufficient has been said to indicate the interesting character of this work, but its value as a contribution to science can scarcely be gauged until independent observations of the unexpected phenomena have been made. It is fortunate that some of the investigations suggested are within range of very modest instruments, even as low as 4 inches aperture.

KINSHIP AND MARRIAGE.

Kinship and Marriage in Early Arabia. By the late W. Robertson Smith. New Edition, with Additional Notes by the Author and by Prof. I. Goldziher, Budapest. Edited by Stanley A. Cook, M.A. Pp. xxii+324. (London: A. and C. Black, 1903.) Price 10*s.* 6*d.*

THIS new edition of a masterly work should be welcomed by all who take an interest in the study of primitive man, a study which, it is no paradox to say, has more practical bearing than academic history on the social problems of the future. Before his death Robertson Smith made corrections and added notes to the first edition of 1885, which are now incorporated. As anthropologists and orientalists know, the essay is an application of the theories of J. F. McLennan to early Arabia, conducted with the originality, insight, logical clearness and brilliance of exposition which are inseparable from the name of Robertson Smith.

Beginning with an exposure of the easy methods of the Arabian genealogists, he proceeds to argue that "female kinship" was once the rule. The strong Arab sense of blood-unity "can only have come from female kinship" and from a state of society where children were reckoned to the tribal kin, but not to a particular father. He regards the *mota* marriages, common in the time of Mohammed, as a last relic of McLennan's *beena* marriage, in which the husband goes to live with his wife's people. This system of *beena* or *sadica* marriage with female kinship and totemism was broken up by the growth of the idea of the family (*dar*), the result being male kinship and *baal* marriage, in which the husband has "dominion." The change was made through "marriage by capture," followed by marriage by purchase. But there is also to be explained the acceptance of male kinship in a state of society where there was "no notion that a man should keep his wife strictly to himself." The only possible explanation lies, the author thinks, in Tibetan polyandry, in which a group of brothers bring to their common home a common wife. This must have been preceded by Nair polyandry, in which a group of brothers is entertained in her home by a common wife. The whole doctrine of the paternal system implies that this polyandry was quite widely spread. Lastly, bars to marriage before Islam were made on female kinship alone; the early Arabians and northern Semites possessed totemism and exogamy.

How far the author might have modified his conclusions is an idle speculation. Criticism of one who has taught us all is especially invidious in the case of a book which in substance is nearly twenty years old. But it is only fair to science to point out that recent research has found grave objections to McLennan's theory of social development and to many of his "universal institutions" themselves. Much also of McLennan's evidence was bad; the author quotes (p. 98) one of his examples of "marriage by capture," which is nothing of the kind. The best authorities contradict the statement on p. 262 as to the prevalence of such "marriage" in Australia, and that on p. 267 as to "marriage by capture" being followed by exogamy. Objections may be raised to the suggestion that *beena* marriage with adoption into the woman's kin are proved by Genesis ii. 24—"a man shall leave his father and mother and shall cleave unto his wife, and they two shall be one flesh"; to the old idea that early man considered animals to be men in disguise; to the view that the Arabs "practised" cannibalism, and that "promiscuous" behaviour at religious feasts is a survival of polyandry; and to the acceptance of metronyms in the genealogies as proofs of female kinship, while patronyms are rejected.

Recent speculation, however, is but beginning to reconstruct the development of the primitive social organism. The great value of this book is to prove that the early Semites followed the same lines of development, whatever they were, as other races, and to provide the best exposition of the prevalent theory.

ERNEST CRAWLEY.

SYLVICULTURE.

Schlich's Manual of Forestry. Vol. ii. Sylviculture. Third edition. Pp. viii+393. (London: Bradbury, Agnew and Co., Ltd., 1904.) Price 8*s.* net.

IN NATURE of July 23, 1891 (vol. xliv. p. 265), Sir Dietrich Brandis, K.C.I.E., reviewed the first edition of the above volume. He then prophesied a great future for Prof. Schlich's work. That the prophecy was not a vain one has been amply proved by the test of time. The book reached the second edition in 1897, and has now passed into the third. There is no preface to this edition, but the arrangement of the former editions has, on the whole, been retained; however, the subject-matter has been somewhat differently classified. The present volume consists of four parts—each part is divided into chapters and sections, which are further subdivided as occasion demands. Part i. deals with the foundations of sylviculture—this was formerly part iv. of vol. i. of the "Manual." Part ii. comprises the formation and regeneration of woods. Part iii. is devoted to the tending of woods, while part iv. consists of sylvicultural notes on British forest trees.

The author has condensed a marvellous amount of information into a small space. At the same time, each subject is dealt with at sufficient length to be quite intelligible to the student and practical forester. This is largely due to the admirable way in which Prof. Schlich has arranged his matter. One subject leads on quite naturally to another, so that there is no needless repetition and overlapping.

The author assumes that the student has already made some progress in other branches of science upon which sylviculture depends—"the forester requires to be well acquainted with the manner in which soil and climate act on forest vegetation, in order to decide in each case which species and method of treatment are best adapted, under a given set of conditions, to yield the most favourable results. The detailed consideration of the laws which govern this branch of forestry finds a place in the auxiliary sciences, such as physics, chemistry, meteorology, mineralogy and geology." Why not botany? especially plant physiology, the *bed-rock* upon which true scientific sylviculture must be founded. It has been for long a criticism of foresters in this country that they are insufficiently acquainted with the life and form of plants —with botany, in fact—and the pages of this book seem to justify the criticism, at least there is occasionally a looseness of expression regarding botanical points which should not appear in a manual for students such as this. Take, for instance, the statement, "the atmosphere overlying the soil furnishes certain nourishing substances—heat, light and moisture." (p. 7), or again, "certain plants (Leguminosæ) can take nitrogen direct from the air by means of tubercles or nodules" (p. 11). The mention of the name Acacia up to p. 52 of the book instead of False-Acacia is botanically wrong and misleading, and the statement that elm does not ripen its seed in the north of England (p. 66) is also wrong because botanically unqualified. The identification of mistletoe with *Loranthus europoeus* (p. 324) is, we take it, a slip.

As regards sylviculture the book has been entirely brought up to date, and is eminently practical and suggestive. It may, with every confidence, be warmly recommended alike to the student, landed proprietor, forester and nurseryman. All doubtful or controversial matter has been carefully avoided, and every view stated, or method recommended, is founded upon the author's own direct observation and experience, as well as on that of others.

The various sylvicultural systems are clearly and concisely described, and their advantages and disadvantages amply criticised, so that the forester need have no difficulty in choosing the one best suited to his own locality and the objects of management. In the important sections dealing with the raising of plants in the nursery, much valuable and useful advice is given. The ultimate success of a wood depends, to a large extent, upon the health and vigour of the plants from which it originated—hence it is very important that young seedlings should be grown and handled with the greatest possible care. On p. 191, Prof. Schlich gives a timely warning to nurserymen in regard to the pernicious practice of laying down seedlings, when they are pricked out, into shallow trenches, involving the bending of the root-system to one side—a defect from which the tree does not recover for many years. He says, "unless nurserymen give up that vicious practice they must be prepared to see landed proprietors revert to the system of home nurseries."

Part iv. of the volume is replete with information. In fact, it is a condensed volume on sylviculture in itself. The notes on the Douglas Fir have been considerably extended, but in regard to the fungus enemies of this species, *Phoma Douglasii* might have been included, as this disease has been known in Scotland now for several years.

ENGINEERING IN SOUTH AFRICA.

The Engineer in South Africa. By Stafford Ransome, M.Inst.C.E. Pp. xx+319. (Westminster: Archibald Constable and Co., Ltd., 1903.) Price 7*s*. 6*d*.

AT the close of the war the author was appointed by the *Engineer* to visit all the British possessions in Africa south of the Zambesi River, and to write frankly and fully to that journal on the various problems which have been evolved by recent events. The result was a series of articles on "South Africa from an Engineer's Point of View." These articles were of a highly interesting nature, and were much appreciated at the time.

The volume before us combines the most interesting portions of these articles with much additional matter as well as most of the illustrations. Mr. Ransome is well known as a successful author of books of this type, and we are not surprised at the able way he handles the subject.

Any man seriously thinking of going to South Africa, be he an artisan or a trained engineer, should most certainly obtain a copy of this book; the information given on the cost of living and travelling, as well as on the prospects of employment, is very much to the point.

Chapter vi. deals with the labour question, a subject very much to the fore at the present time. Our author, after pointing out the prohibitive cost of white unskilled labour, discusses three alternatives, which are as follows:—(1) the importation of Asiatic labour; (2) the trusting to Providence to induce the Kaffir to work; (3) the taking of measures to make the Kaffir work, his conclusion being that the third alternative should be adopted, and that legislation should be introduced to this end. Chapter xiii. deals with the theory and practice of the railways, one of the most interesting in the book. The railway mileage at present open for traffic is 5457, under construction 2636, making a total of 8093 miles. Our author has much to say about the long delivery and high prices paid for railway plant when ordered in Britain, and no doubt has formed these views from conversations with men on the spot; he also compares American delivery of such material to our detriment. It is only fair to point out that the average locomotive built in Britain for these railways is the most expensive of its kind; its design usually emanates from the colony, and the locomotive builder here has to do what he is told. On the other hand, the American locomotive builder works with a much freer hand in every way. He supplies what he thinks best, and is not handicapped by a rigid specification; no wonder he can deliver sooner!

Judging from chapter xiv., the harbours of British South Africa are in a bad way, more especially those in Cape Colony, where for political reasons their development has been remarkably slow; and the author very reasonably argues that since the majority of imports are likely to be for the Transvaal, the harbours further up the coast are more likely to develop in the future; this applies to the Port of Natal, Durban.

Mr. Ransome gives us an excellent description of diamond mining in Kimberley in chapter xvi., tracing the development of the De Beers Company from the commencement, and explaining the various methods from beginning to end, and the same can be said of chapter xvii., which has for its subject "Underground at the Rand Mines."

This volume is of interest to all connected with South Africa, and Mr. Ransome may be congratulated on the production of so excellent a book.

N. J. L.