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

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THURSDAY, JULY 6, 1911.

CANCER AND ITS SUPPOSED CAUSES.

Induced Cell-Reproduction and Cancer. The Isolation of the Chemical Causes of Normal and of Augmented Asymmetrical Human Cell-Division. By H. C. Ross. Being the results of researches carried out by the author, with the assistance of J. W. Cropper. Pp. xxviii+291. (London: John Murray, 1910.) Price 12*s*. net.

MR. ROSS may be congratulated on having written a book singularly unlike most sober scientific treatises. He has been continually on the track of new things, and even the frontispiece, purporting to portray photographically a mitotic figure induced in a large lymphocyte, seems to have been inserted in order to embody a fresh discovery made after the rest of the book had gone to press, though whether others will attach the same significance to the photograph that Mr. Ross himself appears to do, the future will doubtless decide.

The book is written in an interesting and somewhat journalistic style, and the preface contains excellent autobiographical material designed, *inter alia,* to show how "a new method of experimentation with individual living human cells" was accidentally lighted on by the author.

Briefly, the method in question consists in concocting a jelly with agar, to which certain substances, including a dye, are added. Upon this jelly, films of blood containing living leucocytes are spread, which can thus be examined microscopically under various conditions. Ingenious devices for rapid photography are described, and considerable use is made of the photographs so taken in recording the results of observation.

A cytologist will find he has fallen into a rather strange environment when he gets immersed in Mr. Ross's book. He will have much to unlearn, and many new facts to assimilate, before he can hope to emulate the confident progress of his new leader. He will have to recognise that "the word 'nucleus' has a very vague meaning"; that chromosomes are not really of nuclear origin, but that they originate from the Altmann granules, and are formed in the cytoplasm; that the nucleus forms the spindle and the nucleolus constitutes the centrosomic apparatus. He will probably also be astonished to hear that living cells have not hitherto been studied, and consequently that his own reminiscences of observations on Ascaris and many other animals' eggs, to say nothing of plants, in all of which he will seem to remember that nuclear divisions have been followed in the living cell, must be founded on delusion. The zoological investigator will further discover that he owes a larger debt than he was aware of to his botanical colleagues, for Mr. Ross tells us that "most cytological research has been carried out with plant-cells."

Mr. Ross thinks that "from the persistent examination of dead structures, cytology has been rather led away into a maze, from which it will be difficult to extricate it." The main task which he sets himself in his book is to perform this service of extrication and to show what can be accomplished by the study of the living cell in ascertaining the causes which underlie cell division, and especially of cell proliferation. The latter process is obviously of special importance, inasmuch as it lies at the root, not only of the ordinary processes of healing, but also, when it assumes an aberrant character, of malignant disease as well.

The new engine of research, the jelly method, is fully described, and one of the main objects in its use by the author was to control the rate of diffusion of different substances into the cell. There is an excursus on the problem of diffusion, and the net outcome is embodied in formulæ for making what Mr. Ross terms "coefficient" jelly, meaning thereby a jelly in which the rate of diffusion of stain, &c., can be related to a standard in which a particular rate is accepted as unity.

This jelly is made up of a 2 per cent. solution of agar, to which certain proportions ("units") of alkalis, salts, stains (commonly Unna's polychrome methylene blue), and other substances are respectively added. The "units" of each ingredient are so fixed that a doubling of any one of them (*i.e.* two units) will double (or halve) the rate at which any particular substance, the action of which on the cell it is desired to study, will be absorbed. The method is a neat one, but it possesses obvious drawbacks, unless the separate action upon the cell of its constituents in the different strengths employed is fully known. The formulæ employed present an unfamiliar appearance, as the factors are all added together, and the inclusion amongst them of time- and heat-factors, on the basis of units composed of ten minutes and 5° C. respectively seems to assume unusual simplicity in the reactions involved.

By means of this method, however, depending largely on the entrance of the stain to the nucleus, many surprising results were obtained. The addition, for example, of various alkaloids, putrefactive products, &c., led the author to formulate far-reaching conclusions as to the causes underlying cell division, with the result that he believes himself to be justified in announcing the discovery of the main causes that bring about cell division, and induce cell proliferation. The causal agents in question are, of course, chemical, and probably most people who have paid any attention to the matter would agree with Mr. Ross that the fundamental causes of mitosis (nuclear division) are assuredly of a chemical nature. He thinks he has identified certain of these bodies, and this would constitute a most important addition to science if his views as to their action on the living cell should turn out to be as well founded as he imagines them to be.

It is, however, difficult to avoid scepticism on this very point, namely, as to whether the evidence on which the conclusions are drawn is really cogent, and whether the latter are themselves fully warranted.

The author might himself have contributed towards the solution of these crucial difficulties had he seen fit, in addition to the picturesque presentation of his results, to have subjected the foundations on which they rest to a full and wary criticism. For it is clear enough from the account actually given that the cells, even as they were being examined in the jelly, were moriturient. It is stated, over and over again, that under the conditions of the experiments it was not easy to keep them alive for more than ten minutes. It is not, after all, very surprising to learn that all sorts of movements and distortions followed on the application of drugs like atropin, but it is at least uncommon to find that a mitosis can be completely carried through in three minutes. Numerous examples of alleged "mitosis" are described, and photographs are adduced in support of the descriptions. But the photographs themselves are singularly unconvincing, and suggest fragmentation or breaking up of the cell as a whole rather than anything one would expect to see in an actual cell- or nuclear-division. We fail to find any critical guard against misinterpretation of phenomena that might be due to osmotic differences or to the poisonous action of reagents employed.

Nor is one reassured by the account of mitosis (*i.e.* nuclear-division) as referred to in the book. The treatment of the whole subject is suggestive of the enthusiastic amateur who is simply unable, owing to temperament or lack of training, critically to check and examine his own work. Of course, we do not mean positively to assert that such is really Mr. Ross's position, but anyone who puts forward statements on mitosis such as appear on pp. 148 and 149, or again on p. 166, without producing the strongest and most convincing proofs, must not complain if in other directions his views fail to command unreserved acceptance. We are not at all surprised to learn that when Mr Ross attempted to convince his friend of the soundness of his conclusions by demonstrating to them his preparations they all with one consent, as he himself avers, "began to make excuse."

It may be readily admitted that the book contains much that is interesting and valuable by way of suggestion, but we do not regard the conclusions of its author on cell-division and cell-proliferation as sufficiently well founded. J. B. F.

THE EVOLUTION OF LUNAR DETAIL.

Vergleichende Mond- und Erdkunde. By Prof. S. Günther. Pp. xi+193. (Braunschweig: F. Vieweg und Sohn, 1911.) Price 5 marks.

THE resemblance which exists between the surface of the globe and that of the moon, as shown in the irregularities of level and the general character of the superficial formations, has long attracted attention, and much ingenious speculation has been exhibited in tracing a connection and seeking the cause. Fanciful theories exist without number, but men of the highest eminence have occupied themselves with the same theme, being led to it by the fascinating problem of the "Plurality of Worlds." This is the attraction that has induced Prof. Günther to study the subject, or, perhaps it would be more correct to say, to sift and examine what others have written about it. His book is a marvel of research and a triumph of industry. He seems to have examined all that has been written, whether in fact or fiction, bearing on the relations of earth and moon. Mr. H. G. Wells and Jules Verne represent one school of thought; Procter and Flammarion another; the highest authorities, as Darwin, Loewy, and Puiseux, form a third. Every page bristles with notes, and is encumbered by the author's commentaries on those notes. This arrangement perhaps shows greater power of collection than of assimilation. Much of the matter, if worth preserving, could have been incorporated in the text and made the book easier to read.

But however wide the outlook, whether in time or in nationality, problems connected with the physical constitution of inaccessible bodies are likely to remain unsettled, and the discussion prove barren of result. The history of this speculative inquiry is profoundly interesting, but from a philosophical and not an astronomical point of view. We are indebted to the author for the skill with which he has marshalled his facts and the enormous amount of information he has collected, but the moon seems little likely to contribute any fresh facts of importance to the main issue, since the probability in favour of similarity of structure and of evolutionary history is so great. By whatever process we suppose the genesis of the moon to have been accomplished, it would be difficult to imagine a body so near the earth not possessing the same external characteristics and passing through similar geological changes. There is some evidence to show that the actual materials that once formed part of the globe were transferred to our satellite, and this probability is strengthened by the agreement between the density of the moon and that of the superficial rocks on the earth. Indeed, as the author reminds us, there are not wanting those who can point to the exact spot where the catastrophe occurred that in times past tore from the earth the eightieth part of its mass.

The main result of the author's examination is to show the general uniformity of the conviction possessed by all students of the lunar surface that the earth and moon have passed through approximately identical processes of evolution. Prof. Günther reproduces the speculations of ancient Greek philosophers and continues the theme to modern times. Kepler's "Traum von Monde" and the observations of Galileo form connecting links, with the results derived from lunar cartography. In this newer research we start from Hevel and Cassini onwards to the exact methods of Meyer, Lohrmann, Schmidt, and Neville Neison. We are reminded of the artistic work of Nasmyth and Carpenter, of Klein, and of Weinek, and justice is done to their penetrating insight and the ingenuity with which they have pursued their researches. One does not realise how engrossing this subject has proved, how keen has been the attention and the hold it has had upon so many ardent workers, until the whole history is methodically exposed.

Another chapter is devoted to the results of photographic research, in which the pioneer work of De la Rue, Rutherford, and Draper is reviewed, and the history traced down to the admirable series of pictures issued from the Paris Observatory by MM. Loewy and Puiseux. The last four or five chapters are really the kernel of the whole. Herein are considered the formation of lunar craters, the origin of the bright streaks, the debated question of changes of the surface, sufficient in amount to be observed telescopically. The tectonic and orogenic structures here come under review, also, but without much approval, the views of Mr. G. K. Gilbert as to the possibility of the fall of meteors influencing or explaining the external features of our satellite.

SUBSTITUTION IN BENZENE.

Die direkte Einführung von Substituenten in den Benzolkern. Ein Beitrag zur Lösung des Substitutionsproblems in aromatischen Verbindungen. By Prof. A. F. Holleman. Pp. vi+516. (Leipzig: Veit and Co., 1910.) Price 20 marks.

IT is well known that the empiric rules of substitution, which have been evolved from the study of the aromatic compounds, may enable a chemist to predict roughly the result of such a process; it is equally well known that not one of these rules is free from exceptions. The absence of any rational theory underlying the process or the lack of novelty in the methods employed may have deterred chemists in recent years from following up what was formerly regarded as one of the most attractive lines of inquiry; but it is quite certain that very little in the way of systematic study, especially of a quantitative character, has been done in this field of research. Yet the process of substitution possesses not merely a theoretical significance; it has a very considerable technical value. The formation of halogen, nitro and sulphonic derivatives of aromatic compounds are among the most familiar technical operations of the colour-maker.

Though the subject has undoubtedly suffered from neglect, nevertheless a few serious students of the process are to be found. Prof. Holleman and his pupils have for many years devoted themselves to the task of systematising the scattered facts and adding new knowledge by a careful qualitative and quantitative study of these reactions. The volume before us contains not only an account of these inquiries and a critical review of methods and results, but forms at the same time a complete book of reference to all the published observations referring to benzene derivatives containing up to three substituents. It is obviously a work of immense labour, but should be invaluable to those engaged in this fundamental branch of organic chemistry. It may possibly also serve to direct more attention to a topic of so much interest. It is impossible in a limited space to give more than an outline indicating the scope of the book.

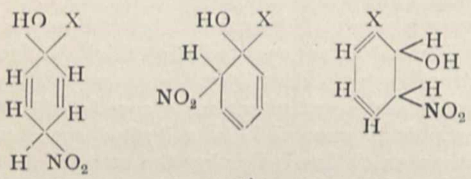
The first three chapters contain an account of substitution methods, the quantitative estimation of the products by chemical and physical means (many of which have been devised in the author's laboratory), and the nature of the di-derivatives formed.

Chapter iv. contains a compendium of results and a critical review of the rules and theories of orientation. Some of the apparent inconsistencies and contradictions in the ordinary rules may be realised from such facts as the following:—The nitration of a halogen derivative of benzene containing an atom of fluorine, chlorine, bromine, or iodine follows the ortho-para rule, yet the amount of ortho compound under similar conditions varies from 12 to 37 per cent. in the four cases. In the nitration of benzonitrile, nitrobenzene, benzoic acid and its esters, all of which are supposed to follow the meta rule, the first gives exclusively a meta compound, whereas the three latter yield an amount of ortho compound varying from 6, in the case of nitrobenzene, to 28 per cent. in that of ethyl benzoate. Whilst aniline and dimethylaniline under certain conditions give meta or meta and para compounds as chief products, dimethylaniline oxide and nitrous acid give mainly ortho and para derivatives. But perhaps the most striking case is that of acetanilide, which, when nitrated in presence of sulphuric acid, gives 80 per cent. of para, but when acted on by nitrogen pentoxide in a solution of acetic anhydride, forms almost exclusively the ortho derivative.

The empirical rules which have been drawn up at different times by Hübner, Nölting, Armstrong, Crum-Brown, and Gibson, and Vorländer, are carefully considered and rejected in turn as inconsistent with the facts, whilst the theories of Armstrong, Flürscheim, and Obermüller are shown to be fundamentally untenable or self-contradictory. The author concludes :—

"Das Endergebniss unserer theoretischen Betrachtungen ist kein erfreuliches; alle Versuche, welche bis jetzt gemacht sind, um die Gesetzmässigkeiten, welche den Ort bestimmen, wo ein zweiter Substituent im Kern eintritt, zu ergründen, sind vollkommen fehlgeschlagen; ja selbst ist es nicht möglich gewesen, die Tatsachen in einer empirischen Regel zusammenzufassen."

After discussing the position taken up by the third entrant group in chapter v., the author develops his own views on the mechanism of substitution. These views, which are published here for the first time, are so eminently simple and rational that chemists may be interested in the following brief outline. Following Kekule's idea that substitution is a succeeding phase of an additive process, Prof. Holleman considers that such a process as nitration, for example, of a compound containing a substituent X produces in the first place one or more of the following three substances :—



para ortho meta

from which the elements of water are subsequently detached. The nature of the predominating compound or compounds will be determined by the accelerating or retarding influence of the substituent X, just as addition of bromine to an olefine will be determined by the substituents already present. If X accelerates the reaction of ortho and para, substitution will be the main result, if it retards, meta substitution (where the double link is unconnected with the X complex) will be the primary effect. If X has no marked effect meta and ortho, meta and para, or all three may be formed.

A work of this kind, which, the author tells us, necessitated the careful perusal of upwards of a thousand original papers, ungrateful and laborious as the task of compilation may have been, will always remain a standard book of reference, for which chemists will feel fully grateful to the author.

J. B. C.

FERMENTS AND FERMENTATION.

Micro-Organisms and Fermentation. By A. Jörgensen. Translated by S. H. Davies. Fourth edition, completely revised. Pp. xi+489. (London: C. Griffin and Co., Ltd., 1911.) Price 15*s*. net.

IN this translation of the fifth German edition (dated January, 1909) of his well-known textbook, the author has incorporated the main results of investigations made since the appearance of the previous English edition about ten years ago. Although the book has been, to a considerable extent, rewritten, its original characteristics are retained.

Five of the six sections of the work have been enlarged, whilst the sixth, dealing with the pure culture of yeast on a large scale, has undergone marked reduction in volume, possibly because it forms the subject of a separate publication by the author. The illustrations have been increased in number from 83 to 101, nearly all of those found in the previous edition being again given; most of the figures are good, but those numbered 12, 13, and 51 would undoubtedly bear improvement, while Fig. 80 fails to bring out the peculiar bean- or kidney-shape of the spores of *Saccharomyces fragilis*. The bibliography has been revised and supplemented; it is, however, questionable whether a bibliography placed at the end of the book is more convenient than references given as footnotes to the text. One new feature, which will be welcomed by all readers, is the provision of an alphabetical index of subjects.

As the author is a member of the Danish school of micro-biologists, the book would naturally be expected to give, as in reality it does, a prominent place to the investigations of Hansen and his followers on the micro-organisms met with in the brewing industry. At the same time, the more important researches carried out during recent years in Germany and elsewhere are not, as a rule, lost sight of, although no reference is made to the valuable work of Slator on alcoholic fermentation, while the meagre notices given to the results obtained by Ehrlich and by Harden and Young might have been replaced profitably by more extended discussions.

The first chapter, headed "Microscopical and Physiological Examination," deals with such subjects as staining, sterilisation, antiseptics, nutritive substrata, and pure culture methods. In this section the space devoted to technique is very small, and a more detailed description of the methods employed in the author's laboratory would have been of value. In the part dealing with nutritive media, no mention is made of "eau de touraillons," which furnishes an excellent basis for such media, and is largely employed by some of the French investigators. Attention is directed to the stimulating action of small proportions of various poisons on the growth of micro-organisms, but no reference is made to the very thorough researches of Javillier on the influence of zinc on the growth of vegetable organisms, including moulds.

Chapter ii. treats of the biological examination of air and water, chiefly from the point of view of brewery requirements.

In chapter iii., the functions and conditions of growth of zymogenic bacteria are described. A paragraph is given to the nitrifying bacteria, but nothing is said of the very important class of nitrogen-fixing bacteria.

The moulds form the subject of chapter iv., which also deals incidentally with enzymes and with the influences of various external conditions on micro-organisms in general. The occurrence and life-history of most of the commoner moulds are studied, and reproductions are given of some of the excellent drawings made by Brefeld and de Bary.

The fifth chapter, occupying nearly two hundred pages, is concerned with the yeasts, and deals, in addition, with non-sporulating or Torula forms, as well as *Mycoderma vini* and *cerevisiae*. A short account is given of the history of fermentation and of the controversy respecting spontaneous generation. Then follow discussions of the biological relationships of yeasts, variations in the Saccharomycetes, morphology and anatomy of yeast-cells, spore-formation and its application to the analysis of yeasts, and a number of allied subjects. Lastly come descriptions of the more important culture and wild yeasts met with in the brewing, distilling, and wine-making industries.

The last chapter gives a brief account of the methods and apparatus employed in the preparation and transport of cultures of pure yeast for industrial purposes.

The translation has been on the whole well done, although in some cases the English is stiff and the grammar faulty. Rather ugly split infinitives occur in moderately large number, and subject and verb do not always agree in number. Use of the expression "equal molecules of dextrose and lævulose" is difficult to defend, and "sorbite," "mannite," and "albuminoid" are nowadays better termed "sorbitol," "mannitol," and "protein." Very few misprints are noticeable; *d*-methylglucoside (p. 359) obviously refers to α-methylglucoside. T. H. P.

THE FISHES OF AFRICA.

Catalogue of the Fresh-Water Fishes of Africa in the British Museum (Natural History). By G. A. Boulenger, F.R.S. Vol. ii. Pp. xii+529. (London: British Museum (Natural History), 1911.) Price 2*l*. 5*s*.

THE author is to be heartily congratulated on the appearance of the second volume of this great work, which succeeds its predecessor after an interval of two years, a period by no means excessive when the amount of labour involved in a task of this nature is taken into consideration. The present volume completes the account of the carp tribe (Cyprinidæ), containing the great bulk of that group, and likewise includes the whole of the cat-fishes (Siluridæ), several new genera and species being named.

Although no one regards systematic works of this class as the final aim and end of zoological science, their importance and value cannot be overestimated, since it is upon such sure foundations that all superstructures of a more far-reaching and philosophical nature must be based. That it was high time the task of bringing our knowledge of the African Cyprinidæ and Siluridæ up to date was undertaken will be evident by a comparison of the number of species of certain groups recorded in the present volume with that given in Dr. Günther's "Study of Fishes," published in 1880, and based on articles in the ninth edition of the "Encyclopædia Britannica." It is stated, for instances, in p. 573 of the "Study of Fishes" that the total number of species of cat-fishes of the exclusively African genus Synodontis is fifteen, whereas Mr. Boulenger has swelled the list to fifty-seven. Again, Dr. Günther estimated the total number of representatives of the Old World cyprinoid genus Barbus at about 200, while Mr. Boulenger gives a list (inclusive of nine additional uncatalogued species) of no fewer than 194 African members of the genus. Unfortunately comparisons cannot be extended to the total numbers of African Cyprinidæ and Siluridæ recorded by the two naturalists, as Dr. Günther enumerates only those inhabiting Africa south of the Sahara; but, even so, his lists of fifty-two Cyprinidæ and sixty-one Siluridæ (*op. cit.* p. 230) inhabiting Ethiopian Africa are altogether outclassed by Mr. Boulenger.

This great increase in the number of African representatives of the two families has, of course, a most important bearing on previous conclusions as to the place of origin of the two groups. Dr. Günther (*op. cit.* p. 225) suggested that since the majority of the groups of fresh-water fishes common to India and Africa, with the exception of the siluroid Clarias and its relatives, had more representatives in the former than in the latter area, the presumption is that they are of Asiatic origin. Although these conditions are now in many cases reversed, the conclusion will, we think, still hold good in the case of Barbus, the members of which, like many groups of antelopes with ancestral forms in India, would appear to have undergone an unparalleled development when they reached Africa. On the other hand, the abundance of siluroids in the Eocene of the Fayum, where no remains of cyprinoids have hitherto been discovered, points to the conclusion that this group is endemically African. And here it is noteworthy that the connection between the African and South American cat-fishes is now regarded as even less intimate than was the case when Dr. Günther's work was written, the two African species there referred (p. 233) to the South American Pimelodus now being assigned to a genus apart. But to pursue this interesting subject would demand more space than can be given to it in these columns.

If such space were available, we might presume to criticise some details in Mr. Boulenger's "keys"; and we cannot conclude without mentioning that the value of the work would have been increased if the dates of presentation of the specimens were added. The work appears singularly free from misprints.

R. L.

THE INTERNAL-COMBUSTION ENGINE.

Gas Engines. By W. J. Marshall and Captain H. R. Sankey. Pp. xvi+278. (London: Constable and Co., Ltd., 1911.) Price 6*s*. net.

THIS book is the latest addition to Messrs. Constable and Co.'s Westminster series, and is intended to be useful to

"those who, being either purchasers or users of gas engines, wish to know the principles underlying the design or construction and the methods of diagnosing defects when they occur, and the steps to be taken to remedy such defects."

It may fairly be said to have achieved its purpose, and, for the most part, any criticism to which it lays itself open is little more than that to which almost any first edition is liable.

The book (an unusually heavy one to handle for its size) is divided into ten chapters, of which the first three deal with the theory of gas engines and with the Otto and two-stroke cycles of operation. The fourth, fifth, and sixth chapters are concerned with the water cooling of the engine parts, the various methods of ignition, and the principles of operation. Then follows a section on the arrangement of a gas-engine installation and the testing of the plant, whilst the two concluding chapters contain a good description of the various methods used for the governing of the engine and of the use of gas producers.

The book is stated on the title-page to be by the two authors mentioned in the heading to this review, and it is therefore puzzling on pp. 1, 110, and 152 to find "the author" only referred to. Possibly the book was written in sections, and this explanation appears plausible by reason of the curious contradictions that occur in it. Thus, on p. 1 it is stated that "the modern gas engine is a prime mover, perfectly reliable, cheaper in first cost, including the gas producer, than a steam engine and its boiler"; whereas on p. 21, we learn "the capital cost of the gas-engine plant is much greater than that of the steam-turbine plant," and on p. 152 that "the modern gas engine is very nearly as reliable a machine as a steam engine."

These statements should not all of them appear in one publication, as, even if explicable by some verbal modification, they tend to confuse the non-expert readers for whom this book is intended. It is this very use by the non-expert that renders slips in explaining the theory of the engine very unfortunate. On p. 5 an unlucky oversight has led to the statement appearing that if "pressure be kept constant the volume will vary as to absolute pressure." Again, the author or authors in stating (on p. 7) that "only a certain proportion of a given quantity of heat can be converted into work," neglect such a well-known fact that the heat conveyed to a gas to keep its expansion isothermal is all of it converted then and there into work.

These are matters which should be put right in a second edition, and, in view of the very thorough knowledge shown of the practical working of the engine and of the misfortunes to which it is sometimes liable, it is likely that there will be a large number of persons to whom the book will be of such value as to render a second edition a necessity. The description of the mode of working of the Clerk engine is specially good, and the letterpress is well illustrated. The authors commit themselves on p. 94 to an interesting prophecy concerning the probable future development of the gas engine.

"It seems likely," they say, "that in the future large gas-engine plants will be designed with engines working on two-stroke cycles, but, instead of each engine having its own charging pumps, a central set of independently driven pumps for gas and air will be provided"; "these pumps," they suggest, "will probably be of the turbine type, and of greater efficiency than is obtainable with pumps forming an integral part of the engine."

Finally, we welcome the book as an interesting addition to the less ambitious side of gas-engine literature, and foresee a useful place for it in the library of many persons who wish to learn something of this growingly important prime-mover.

RESINS, RUBBER, AND ESSENTIAL OILS.

Allen's Commercial Organic Analysis. A Treatise on the Properties, Modes of Assaying, and Proximate Analytical Examination of the Various Organic Chemicals and Products Employed in the Arts, Manufactures, Medicine, &c. With Concise Methods for the Detection and Estimation of their Impurities, Adulterations, and Products of Decomposition. Edited by W. A. Davis and S. S. Sadtler. Vol. iv., Resins, India-Rubber, Rubber Substitutes, and Gutta-Percha, &c. Fourth edition, entirely rewritten. By the editor and the following contributors, M. B. Blackler, E. W. Lewis, T. M. Lowry, E. C. Parry, H. Leffmann, and C. H. Lowall. Pp. viii+466. (London: J. and A. Churchill, 1911.) Price 21s. net.

THE general characters of the new issue of Allen's work have been described in the notices of earlier volumes reviewed in this journal (NATURE, vol. lxxxv., pp. 37, 365). In this connection therefore it is only necessary to remark that the present volume is notable among its fellows for the rather large proportion of theoretical and descriptive chemistry which it contains.

Many users of the book, however, will probably find this a convenience. Considerable advances have been made in the chemistry of the resins and essential oils since the publication of the earlier editions, and many of the results have not hitherto been brought together. Taken with the numerous references supplied, the articles upon the three groups of products dealt with in the book—namely, resins, india-rubber, and essential oils—form an excellent summary of the general and analytical chemistry of these products. The following are a few out of many interesting matters to be found in the volume.

Thanks largely to the work of Tschirch and his coadjutors, it is now possible to make at least a provisional attempt at a satisfactory classification of the resins. The proximate constituents of these bodies, so far as they are yet known, may be divided into (1) *Resin-esters* and their decomposition products; (2) *Resinolic* or *Resin acids;* and (3) *Resenes;* the last being oxygenated compounds with no very characteristic chemical properties beyond the attribute—a very valuable one—of resistance to the action of alkalies. In many of the resins one or other of these three classes is the preponderating constituent, whence three groups can be distinguished, namely, *ester-resins, acid-resins,* and *resene-resins.* The first group, for instance, includes (among others), gum benzoin, storax, "dragon's blood," ammoniacum, and asafœtida; in the second group are the coniferous resins and copaiba balsam; and in the groups of resene-resins are included myrrh, olibanum, dammar, and Manila copal. From a number of the ester-resins the alcohol of the ester has been isolated, and Tschirch distinguishes two kinds of these alcohols—*resinols* and *resinotannols.* The former are colourless, and give no reaction for tannin when tested with iron salts; the latter are coloured and give a tannin-reaction. For the general analytical examination of resins, the acid, saponification, iodine, methoxyl, and acetyl values are the determinations most frequently useful in forming a conclusion.

The chemistry of the essential oils is no doubt better and more widely known than that of the resins. Nevertheless, in the interests of readers who are not specialists in this branch of chemical work, a summary of the chief facts respecting the composition of the essentials oils may not be unprofitable. These oils, to which the characteristic odours of flowers and plants are nearly always due, are composed of some half-dozen groups of compounds, one or two of the groups predominating in any particular oil. *Terpenes* constitute the bulk of many essential oils, but are seldom the most useful portion. In fact, various so-called "terpeneless" oils are now prepared, the removal of terpenes serving in effect to concentrate the odoriferous properties of the oil in the residual constituents. *Alcohols* of various types—open-chain, aromatic, and sesquiterpene alcohols—with their corresponding *aldehydes* and *ketones*, are the important compounds in many oils. Examples are the alcohol *geraniol* in otto of rose; the aldehyde *citral* in lemongrass oil, and the ketone *pulegone* in oil of pennyroyal. Bergamot, clove, and mustard oils respectively owe their special properties to *esters*, *phenols*, and *sulphur compounds*. Methods by which the proportions of the various constituents may be determined, usually with a fair approach to accuracy, are described at length in the volume, which can be recommended as well worthy of its place in the series.

C. SIMMONDS.

MECHANICS, THEORETICAL AND TECHNICAL.

(1) *Lehrbuch der technischen Physik.* By Prof. Hans Lorenz. Dritter Band, Technische Hydromechanik. Pp. xxi+500. (Munich and Berlin: R. Oldenbourg, 1910.) Price 14 marks.

(2) *Die Theorie der Kräftepläne.* By Prof. H. E. Timerding. Pp. iv+100. (Leipzig and Berlin: B. G. Teubner, 1910.) Price 2.50 marks.

(3) *Vibrations of Systems having One Degree of Freedom.* By Prof. B. Hopkinson. (Cambridge Engineering Tracts, No. 1.) Pp. 54. (Cambridge: University Press, 1910.) Price 2*s.* 6*d.* net.

(4) *Leerboek der Werktuigkunde.* By F. J. Vaes. Vol. i., pp. xii+152. Vol. ii., pp. xiv+224. (Schiedam: H. A. M. Roelants, 1910.) Price 1.40 gulden.

THERE is probably no branch of science in which such wide gaps occur between theory and practice as in the study of the motion of fluids. We have, on one hand, the mathematical theory of hydrodynamics, which is limited by the difficulty of obtaining soluble problems and building up integrals of the differential equations of motion subject to given boundary conditions. This difficulty alone restricts the scope of the investigation mainly to the study of perfect fluids, thus immediately introducing a discrepancy between theory and observation. On the other hand, we have the hydraulics of the engineering student, the object of which is mainly to enable numerical calculations to be made regarding such problems as town water supply, efficiency of turbines, pumps, propellers, and ships.

In endeavouring to produce a text-book on technical hydromechanics (1) which should be satisfactory both from a theoretical and from a technical point of view, Prof. Lorenz thus undertook a duty of enormous difficulty. It is not surprising that when he had half finished the manuscript in 1906 he was so dissatisfied with the result that he decided to start afresh, a task which he did not commence until 1908, after re-studying the subject and making a number of original investigations. That such difficulties would exist was shown not only by what he describes as the "step-motherly" treatment of this subject in technical colleges, but also by the absence of any comprehensive treatise covering the required ground. Books on hydraulics generally treated the subject entirely from Bernouilli's principle, combined with the hypothesis of parallel sections, nothing being said about Euler's equations of motion in three dimensions, much less of their modifications for viscous fluids. In one case the treatment of hydraulics was preceded by an introduction on hydrodynamics, an order of treatment likely to confuse rather than enlighten the "applied science" student. On the other hand, we have purely theoretical treatises on hydrodynamics, where practical applications are represented only by half a chapter on the hypothesis of parallel sections, and this Cambridge lecturers, at any rate formerly, told their students to omit.

The order of treatment adopted by Prof. Lorenz is probably the best that could be devised for the object in view. Starting with analytical hydrostatics (including the oscillations of floating bodies, and surface tension) the author next deals with steady, and subsequently with variable, one dimensional motion treated by the hypothesis of parallel sections. These chapters constitute hydraulics proper, and contain applications to flow over weirs and through sluices, pumps, efficiency of turbines, and propellers, long waves in canals, and tides. The extension of the investigation to three dimensional motion leads up quite naturally to Euler's equations of motion of a perfect fluid, problems in irrotational motion, both continuous and discontinuous, vortex motion, and viscosity. In addition to such problems as are commonly treated in hydrodynamics, we have discussions of the oscillations of ships, including a digression on the Schlick gyroscope, approximate theories of the turbine, and of the resistance of ships, and the equations of motion of underground waters. The latter subject might with advantage be brought more prominently than it has been before mathematical students in this country. It affords excellent examples of motion derived from a potential, and the divergence between theory and observation is probably less than in Eulerian irrotational motion. Not the least important feature is a concluding chapter on the history of hydrodynamics. A few points in the book might be improved. The proof of the permanence of irrotational motion on p. 342 scarcely appears conclusive. The equation shows that if the components of spin are ever zero their rates of change will also be zero. This does not prove *without further investigation* (*e.g.* by Stokes's method) that if the components are zero initially they always will remain zero, for a similar argument applied to uniformly accelerated motion from rest would lead to a *reductio ad absurdum*. Does not the author, however, assume this inference? Again, in dealing with flow of a viscous liquid between two parallel plates, would it not be more correct to say that Hele-Shaw first made the experiments, and Stokes subsequently showed that the results were largely due to viscosity, although it was previously thought, and perhaps even incorrectly stated, that these motions represented an actual realisation of two-dimensional flow of perfect liquids?

(2) Prof. Timerding's introduction to "Graphical Statics" contains a praiseworthy attempt to bridge over the gap which so often exists between treatises of a purely theoretical character, and text-books dealing too exclusively with technical applications. In particular the author shows clearly the equivalence which exists between the equations of equilibrium of uniplanar analytical statics and the geometrical constructions of force diagrams, funicular polygons, and stress diagrams of loaded frameworks, such as roof trusses. It is not surprising to find the author compelled to limit the scope of his book to the simplest portions of the subject. By this means he rightly claims to have produced a handbook which is accessible to students possessing a limited knowledge of mathematics (including very elementary analytical geometry but excluding calculus), but which constitutes a self-contained and connected exposition of the subject-matter of which it treats. As the author points out, the book should show to the technical worker that he can derive help and elucidation even from apparently abstract theories, while, on the other side, the mathematician will see with pleasure how densely the path of practice is strewn with the finest flowers of theory.

(3) "Vibrations of Systems having One Degree of Freedom" is a tract of a rather more elementary character than the series of "Cambridge Mathematical Tracts" which have proved so useful already. It might afford a useful supplement to existing text-books for such students of physics as have attended a suitable course of preparation in the elements of the calculus and differential equations. The equations of free and forced oscillation with one coordinate are discussed, and are applied to such problems as the rolling of ships and its measurement by the pendulum. The main difficulty is for students to find time to read such tracts when they have so much other subject-matter to study; this difficulty can probably be overcome by placing the book in the hands of the lecturer rather than of the pupil. There is certainly much to be said for requiring students to get a clear understanding of vibrations with one degree of freedom before introducing them to the more general problem where there are several coordinates. So much for the use of the book by students of pure science. As for its use for the class of students for which it appears to be primarily intended, we can only express the opinion that the book tends in exactly the same direction as the two German works above reviewed, and we hope it will be as successful as they are in making such students appreciate the value of theoretical knowledge.

(4) "Leerboek der Werktuigkunde" (=Lehrbuch der Werkzeugkunde) is a text-book on elementary statics and dynamics treated without the calculus for use in the Dutch higher schools, and by private students. Both in the methods of treatment and exposition, and in the worked-out exercises, examples and questions for examination, this book closely resembles our numerous English school books on elementary mechanics. Its contents include uniformly accelerated motion, component and resultant velocities, relations between force, mass and acceleration, composition of coplanar forces and couples, centres of gravity, ordinary examples on equilibrium of bodies resting against one another, the principle of work, circular and harmonic motion, elementary moments of inertia, impact, and the so-called mechanical powers. This course of study is so familiar to English readers that further description is unnecessary. It may be noticed that while the familiar "first and second systems of pulleys" appear in the last-named section, allowance is made for passive resistances, such as friction, which is not always done in English books. On the other hand, the author still follows old customs in trying to deal with composition of velocities without the necessary explicit references to relative velocity, which last receives somewhat meagre attention, a plan which experience shows to lead to frequent mistakes on the part of students.

G. H. B.

OUR BOOK SHELF.

History of Biology. By Dr. L. C. Miall, F.R.S. (The History of Science Series.) Pp. vii+151. (London: Watts and Co., 1911.) Price 1*s*. net.

This is a wise and instructive book, such as we have learned to expect from Prof. Miall. It is scholarly but restrained, so that the reader is not overwhelmed with too much learning. It is a model of terseness, yet it has that picturesqueness of illustration which is necessary if a history is to grip the ordinary mind. A book like this, which commands our warmest admiration, could not have been written except by one who had studied the history of biology for a long time, and at first hand.

After a brief reference to the biology of the ancients, the long cessation of scientific inquiry that followed, and the revival of knowledge, Dr. Miall sketches the history in five periods. The first (1530–1660) saw the fresh start of botany and zoology, the beginning of experimental physiology, the exploration of new lands. The second (1661–1740) was the period of the early microscopists, of discussions as to the meaning of fossils, of early comparative anatomy and taxonomy, and of inquiry into the sexes of flowers. The third 1741–1789) was the time of Linnæus and the Jussieus, of Réaumur, of the rise of the genetic or historical method as illustrated by the works of Montesquieu and Buffon, of inquiries into animal intelligence and instinct, the metamorphoses of plants, the function of the green leaf, and so on. The fourth period (1790–1858) is illustrated by Sprengel and the fertilisation of flowers, by Cuvier and palæontology, by Chamisso and alternation of generations, by von Baer and embryology, by the cell-theory, and by the investigation of the higher cryptogams. The fifth period is illustrated by Darwin and Pasteur.

At the end of the book there is an interesting chronological table—a good lesson in itself. There is reason for regret that students often take relatively little interest in the historical development of the science which they pursue. The excuse sometimes offered, that they have no time for "historical studies," is made impossible by a book like this, short and illuminating. It shows us, with singular success, how "Biology, which in the sixteenth century sent out only a few feeble shoots, has now become a mighty tree with innumerable fruit-laden branches. The vigour of its latest outgrowths encourages confident hopes of future expansion." J. A. T.

Catalogue of the Lepidoptera Phalænæ in the British Museum. Vol. x. Plates cxlviii–clxxiii. (London: Printed by order of the Trustees, 1911.) Price 20s.

VOL. x. of Sir George F. Hampson's great work on the moths of the world was issued in November, 1910, and was reviewed in NATURE for February 23, 1911 (p. 539). It was published in advance of the plates, which were not quite completed, but which appeared in May of the present year. They number twenty-six, and on each plate thirty-two species are figured, making a grand total of 842 species figured out of 1222 described (except a few described in vols. viii. and ix.) in vol. x., and if we add to this number the 214 species figured in the text, we find that only a few species are described and not figured, and even of these most are recognisably figured elsewhere. The enormous number of species of insects makes this of great importance, and the close resemblance and frequent dull colouring of many species often makes it difficult to point out their characters by description alone. With these excellent illustrations it should be easy to identify most, if not all, the species represented, and it is to be regretted that we have not yet a sufficient series of illustrated works on other orders of insects besides Lepidoptera.

The Mechanism of Weaving. By Thomas W. Fox. Pp. xxii+604. Fourth edition. (London: Macmillan and Co., Ltd., 1911.) Price 7s. 6d. net.

SINCE the first edition of this book was reviewed in NATURE on December 13, 1894 (vol. li., p. 149), 130 pages and twenty-six new illustrations have been added. Some parts of the work have been rearranged and others enlarged. The sections dealing with dobbies, Jacquards, figuring harnesses, card-cutting, picking, multiple shuttle boxes, letting off, taking up, beating up, loom adjustments, and reeds have all been extended; and those on gauze, lappets, and swivels have been rewritten. Descriptions of terry weaving, the automatic supply of weft to looms, and warp stop motions have been included in this edition, and also an index.

Die Grundlehren der höheren Mathematik. By Prof. G. Helm. Pp. xvi+420. (Leipzig: Akademische Verlagsgesellschaft, 1910.) Price 13.40 marks.

THIS is much more like an English school class-book than usually reaches us from Germany. Practically it is a revision course of pure mathematics for what we should call a degree standard. There are chapters on differential and integral calculus, analytical plane and solid geometry, differential equations, interpolation, and the elementary theory of vectors. The author has had in mind the requirements of technical students, and his illustrations of theory are mainly of a practical kind. Finally, there are nearly 400 diagrams, so that the "appeal to the eye" has not been neglected. M.

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 Duration of Geological Time.

THERE is at the present time a great discrepancy in the numerical values given for geological time by the various methods employed. Considering the period which has elapsed since the commencement of the Cambrian, the evidence afforded by the study of radio-active minerals suggests that its length is of the order of 500 million years. Arguments derived from the study of sedimentation give, according to recent writers, a space of time for the same period not exceeding 50 million years, and the method based on the salinity of the ocean gives a similar figure. It is with regard to sedimentation that I wish here to make a few remarks.

It is evident that at the present time the volume of post-Eozoic sediments in existence is greater than ever it has been, and also that it is slowly increasing at the expense of the igneous and pre-Cambrian rocks now exposed to denudation. Knowing the rate of this denudation and the total volume of these sediments, all of which must necessarily have had their origin in igneous and pre-Cambrian rocks, the period represented by the fossiliferous strata is given by a simple process of division.

Situated as I am in Africa, without any geological literature, I can only give the most approximate estimates of the necessary factors with which to illustrate the method and arrive at the required length of time.

Sollas gives the thickness of the post-Eozoic sediments as 250,000 feet, or approximately 50 miles.

If we suppose all the sediments to be deposited at the maximum rate, they will form a regular bed along the continental shores for a distance of 35 miles seawards. Taking 100,000 miles as the average shore line of the Cambrian and subsequent periods, we arrive at the total volume of sediments—175×10^6 cubic miles. This spread over the whole globe represents a thickness of seven-eighths of a mile.

The average rate of continental denudation is probably 1 foot in 5000 years, or 1 mile in 26 million years. The continental area is 56 million square miles, one quarter of which is occupied by igneous and pre-Cambrian rocks, *i.e.* 14 million square miles. The rate of denudation of the latter is therefore 14 million cubic miles in 26 million years. The time for all the sediments to collect at that rate would be 325 million years. This figure is only an indication of the order of the time elapsed. It requires to be corrected for the following factors:—

A. Factors tending to decrease the estimate.

(1) Exposed surface of igneous and pre-Cambrian rocks may have been greater in former ages.

(2) Marine denudation of igneous and pre-Cambrian coasts.

(3) Greater density of igneous rocks than of sediments.

B. Factors tending to increase the estimate.

(1) Average land area has probably been about 0·8 that of to-day.

(2) Average continental mean height has probably been below that of to-day.

(3) Recent glaciation has laid bare many pre-Cambrian areas.

(4) Present is a volcanic period, increasing the weathering capacity of rain.

(5) Present climates are of maximum variability.

(6) Parts of land area are subject to deposition, *i.e.* negative denudation.

(7) Unconformities not represented by sediments elsewhere.

(8) Some sediments at great depths may have become igneous rocks.

It is hoped that, with the increase of accurate quantitative knowledge of former conditions, and of the processes at work in denudation and deposition, the above factors may be verified and allowed for in making an estimate of the antiquity of sediments.

The estimate here given is probably too low in the light of these corrective factors, and it is interesting to notice how much more closely it agrees with the results of the totally independent method based on radio-activity than do those deduced from the facts of sedimentation in the usual way.

ARTHUR HOLMES.

Mosuril, Portuguese East Africa, May 6.

Breath Figures.

THE two interesting letters on breath figures by Lord Rayleigh and Dr. Aitken (NATURE, May 25 and June 15) seem to me to contain a statement of the cause of this phenomenon as well as the data necessary to support it.

Thus it is shown that a blow-pipe flame, burning sulphur, sulphuric acid, hydrofluoric acid, and caustic soda give these breath figures, while heat and alcohol flame give no such result. The conclusion apparent from these chemical data is that when the glass is coated with a film having an affinity for water, breath figures are formed.

Coal gas contains sulphur, and a blow-pipe flame gives sufficient sulphuric acid to form a film on glass; burning sulphur gives similar acid products, and both yield breath figures.

Sulphuric acid, hydrofluoric acid, and caustic soda are each capable of dissolving glass, which implies wetting and a certain amount of penetration; washing does not immediately remove this, and a film of acid or alkali is left capable likewise of forming breath figures.

In ammonia solution we have a strong alkali which cannot dissolve glass in the caustic soda sense; when it is allowed even to stand on a glass plate no breath figure is formed, but when it is well rubbed in a faint figure is produced.

If breath figures, from blow-pipe flames, say, be soaked in ammonia solution and washed, they may be gradually destroyed—by neutralisation of the acid in the superficial pores of the glass—until breath outlines only exist. These lines correspond to the lines of greatest acid penetration, and would be represented by charred lines on a piece of wood.

This gradual destruction of the figures on gradual neutralisation of the acid conclusively shows that these figures are neither due to cleanliness nor dust, as has been suggested.

This explanation enables one to predict that Dr. Aitken's suggested experiment of burning pure hydrogen in dustless air would give breath figures, while pure (dusty) hydrogen burning in pure (dusty) oxygen would give no figures, the reasons being that pure hydrogen burning *in air* gives sufficient nitric acid to produce figures, while pure hydrogen burning *in pure oxygen* produces no acid, and would produce no figures.

Caeteris paribus, it may be inferred that pure quartz glass would not give figures with sulphuric acid, but with hydrofluoric acid and caustic soda.

If the rays from radium can produce breath figures on glass, it constitutes another cause.

Glasgow. GEORGE CRAIG.

A Zenith Halo.

WILL you permit me to quarrel with your correspondent for the heading "A Zenith Rainbow," attached to his letter from Bruges, published in NATURE of May 11, p. 349? The phenomenon described was not a rainbow, as Mr. Gold has taken pains to point out. The heading is unfortunate, for two reasons: first, because it tends to confirm the prevalent misuse of the word "rainbow," and, secondly, because it will probably lead to the improper classification of Mr. Kreyer's letter in bibliographies.

The terminology of atmospheric optics is in a state of dire confusion, even among scientific men, but all the latter are agreed in calling the phenomenon in question a halo. Mr. Gold follows Pernter and most other writers in terming this particular halo an "arc of contact." However, this name, as well as the common alternative, "tangent arc," is objectionable, for the reason that the halo thus designated is by no means always in contact with, or tangent to, the halo of 46° (or the position which the latter would occupy if present). On this subject see M. Besson's article "Le halo du 21 décembre 1910; un arc tangent qui n'est pas tangent," in *La Nature* of March 11, 1911, p. 248. In the picture that accompanies M. Besson's article, the "tangent" arc is shown separated from the halo of 46° by an interval of about 3°.

Another common name, "circumzenithal arc," is open to the objection that this halo is but one of many that are central at the zenith.

The only accurate and distinctive name for the phenomenon is "upper quasi-tangent arc of the halo of 46°."

Statistics of the frequency of the various halo phenomena are misleading. Mr. Gold states, on the authority of Pernter, that the arc in question had been observed only about seventy times up to 1883. Besson, "Sur la théorie des halos," records 111 observations of it in ten years (1898–1907) *at Montsouris alone*. If systematic observations of halos were made all over the world, the frequency of such phenomena would doubtless be found to be far greater than is now generally supposed.

C. FITZHUGH TALMAN.

U.S. Weather Bureau, Washington, May 22.

PROBABLY no one will be inclined to dispute Mr. Talman's proposition that systematic observations would largely increase the apparent frequency of the phenomenon mentioned.

With reference to the terminology, it is, as he points out, unfortunate that the terms "arc of contact," "tangent arc," should have come into general use for a bow which is not always in contact with the halo. I cannot, however, agree that Mascart's term, "quasi-tangent arc," is a satisfactory substitute. It was, I believe, intended to meet those cases when the arc is present at approximately 46° from the sun, but without the 46° halo. It does not fit cases for low or high solar altitudes when the arc is more than 46° from the sun. I think it would be better, instead of trying to indicate all the peculiarities of the phenomenon by its name, to use a term such as "auxiliary arc," if the present names are to be abandoned.

The phenomenon is described by Bravais as "un véritable arc-en-ciel," and this may account for the less appropriate use of the term "rainbow."

E. GOLD.

Meteorological Office, South Kensington, London, S.W., June 2.

Jelly Rain.

ON the morning of Saturday, June 24, the ground here was found to be covered with small masses of jelly about as large as a pea. There had been heavy rain on Friday night, and it was raining at 7 a.m., when, so far as I can ascertain, the phenomenon was first seen. On being examined microscopically the lumps of jelly turned out to contain numerous ova of some insect, with an advanced embryo in each. The egg itself is very minute—an elongated oval 0·04 mm. in length. Yesterday and the day before many larvæ emerged, and were obviously those of some species of Chironomus, though colourless, having no hæmoglobin, as is the case with the larvæ of *C. plumosus*. Not being an entomologist, I am at a loss to understand how these egg-masses could have appeared where they did unless they were conveyed by the rain, as it does not seem likely that the midges would have laid their eggs on pavements, gravel paths, tombstones, &c., even had they been wet; nor has any large number of adult insects been seen in the locality. It would be interesting to hear whether the same thing was observed elsewhere, and whether the phenomenon often occurs. Showers of algæ, small snails, and even frogs have been recorded from time to time, but I cannot recall a like instance to the above.

Eton, Bucks, June 30. M. D. HILL.

NOTES ON THE HISTORY OF THE SCIENCE MUSEUM.

THE recent discussions relating to the Science Museum have brought to the front several important questions connected with the utilisation of land for public purposes connected with science and the arts. They have occurred, too, at a time when the Royal Commissioners for the Exhibition of 1851, who in times past have behaved so generously in selling their land at a nominal price, have now parted with the last square yard of it which can be used for the high purposes determined upon by the late Prince Consort when it was placed at his disposal by them.

It seems desirable, therefore, to bring together as briefly as possible the facts touching the various allocations of the land which have been made from time to time. In this way we shall be able to touch upon some of the circumstances which have arisen regarding museum sites during the last half-century. Further, we shall be led to recognise the vast benefits which have been conferred upon the nation by the Commissioners' action.

The present site of the new Victoria and Albert Museum to the east of Exhibition Road was the first thus devoted in 1858, after the partnership between the Commissioners and the Government had been dissolved, chiefly to the purposes of Art, although a small Patent Museum ("the Boilers") had before that year been erected by the Government at a cost of 15,000*l*.

Next came the plot on the south face of the main square of the Commissioners' estate, facing Cromwell Road. This was the largest plot conveyed to the Government for national purposes, and its transfer was made memorable by a remarkable speech by Lord Palmerston concerning the Cabinet's decision to purchase it, and the uses to which it was proposed to apply it. In this speech (*Hansard*, June 15, 1863), Lord Palmerston, the Prime Minister, took pains to show the generous action of the Commissioners. Some extracts from this speech may be given.

"Sir, I rise to propose the Vote of which notice has been given, for the purchase of land and buildings on the site of the Exhibitions of 1851 and 1862. This City of London may, without exaggeration, be called the commercial capital of the world. It ranks high among the great political centres of civilised nations, and in point of wealth and population it may very fairly be stated to exceed any other European city. But the very circumstances which I have mentioned—the great wealth and great population of the City—have tended progressively to impair the architectural and ornamental character of the town. Our streets are narrow, our open spaces few and small, our public buildings are not many, and, respecting those which do exist, differences of opinion prevail as to their propriety of ornamentation and architectural design. We have not, in this town, what are to be found in many smaller towns upon the Continent, a great number of splendid palaces belonging to individuals. When we have mentioned Northumberland House, and, perhaps, Lansdowne House, if we are called on to enumerate other great ornamental constructions, we shall be driven to the, no doubt, very beautiful collection of apparent palaces—the clubs in Pall Mall, many of which are imitations of beautiful palaces on the Continent. In all the Italian towns, at Prague, and in most German towns, there are large piles of ornamental buildings which represent the wealth and taste of the nobility of those countries. What is the reason of that? What is the reason of the inferiority of this city as compared with other first-rate towns, in regard to the conditions of the space occupied by and the character of the buildings? The great run of the private houses of London may really be termed mean. I am not speaking of those more lately constructed, which are on a better plan; but the old red-brick houses of London are low, they are destitute of architectural ornament, and may be said to be mean in their character. What is the cause? It arises from the great value of the ground—from the immense competition which the wealth of the metropolis causes for the small spaces of ground. People are unable to buy a large quantity of ground on which to construct a house, and, having paid dearly for such a portion of the land as they require, they have smaller disposable means for the erection of ornamental and handsome structures. The price of land in London is very great. I will just mention a few instances to show the value that attaches to the surface in this great town. . . . Therefore I say that the natural progress of wealth and civilisation tends to add greatly to the value of land to be covered by buildings in the interior of towns; and admitting that there are certain requisites which are necessary for the development of the public establishments and buildings, the question arises where the land for such purposes can be acquired, and whether we should look for it in the centre of the town, where everything is covered with valuable property, or whether we should embrace the opportunity of acquiring it at certain greater distances, but still within reach for all the purposes to which it is to be applied. Well, we hold that the land held at Kensington by the Commissioners of 1851 does afford the means of providing for our immediate and prospective wants, and we are able to get land there for our immediate purposes on terms infinitely cheaper than those on which land can be acquired nearer the centre of the metropolis."

Having shown how the question was dominated by the price of land in various parts of the metropolis, Lord Palmerston passed to the then requirements of the Government. The chief of these was an expansion of the Patent Museum before referred to, and also of the British Museum.

In 1859, two years after the establishment of the Patent Office Museum, the Commissioners of Patents laid a Report before Parliament, in which the following passage occurs:—

"It is intended to make the Patent Office Museum an historical and educational institution for the benefit and instruction of the skilled workmen employed in the various factories of the kingdom, a class which largely contributes to the surplus fund of the Patent Office in fees paid upon patents granted for their valuable inventions. Exact models of machinery in subjects and series of subjects, showing the progressive steps of improvement in the machines for each branch of manufacture, are to be exhibited; for example, it is intended to show in series of exact models each important invention and improvement in steam propellers (steamboat propulsion) from the first engine that drove a boat of two tons burden to the gigantic machinery of the present day, propelling the first-rate ship of war or of commerce. The original small experimental engine that drove the boat of two tons burden, above referred to, is now in the museum."

Add to these illustrations of applied science similar illustrations of the instruments used in the advance of pure science, and we have a picture of what is required in the Science Museum of to-day.

Lord Palmerstone thus referred to the needs of this museum:—

"Now, the question is, what do we want? What are the requirements that press on the Government? In the first place, we want a Patent Museum. Anyone who considers the value of a great collection of models and inventions to those employed in the mechanical and productive arts of the country must know that it is of great importance that they should have access to a repository in which they can find everything connected with that particular department of industry to which they have devoted themselves. In America, a country not supposed to be addicted to unnecessary ornament, but where a great disposition is shown to practical improvement, there is a Patent Museum which covers eleven acres. Well, we do not propose a museum of such dimensions. I think that about three acres will be sufficient for all present needs in regard to a Museum of Patents."

He then passed on to the British Museum requirements :—

"Then we want an addition to the British Museum. The question then arises where that addition is to be found—whether the land is to be had by purchasing land in immediate contiguity to the British Museum, or by the purchase of land at Kensington, as we propose. Calculations have been made that eight acres are required, but that is, I think, more than is necessary. I think that five acres would be a nearer approximation, and three acres have been named as the smallest amount of space that is required."

Next we have a reference to the National Portrait Gallery, room for which was eventually found elsewhere :—

"We have got together, at some expense and trouble, a most interesting collection of portraits of distinguished men connected with the history of the country. They are now placed in a house where they cannot be seen, and it is urgently desirable to have a better building in which to place that Portrait Gallery, Then we have a Museum at Kensington, full of most valuable and instructive productions, and a Committee of the House of Commons that sat two or three years ago strongly recommended additions to that institution."

"Now, we calculate the cost of these various augmentations—supposing that the land were bought in the metropolis and at the rate which it now bears —as follows :—If eight acres are taken for the British Museum, the cost of land will be 390,000*l.*, and the buildings 824,000*l.*, making a total of 1,214,000*l.* If five acres only are required, the land will cost 240,000*l.* and the building 567,000*l.*, making a total of 807,000*l.* Supposing the lowest estimate of three acres to be sufficient, the land will cost 150,000*l.* and the building 300,000*l.*, making a total of 450,000*l.* I then take the Patent Museum, which will require three acres. The land is set down at 100,000*l.* and the building at 100,000*l.*, making together 200,000*l.* . . . The Portrait Gallery will require half an acre, and we calculate will cost 25,000*l.* for land, and 25,000*l.* for the building, or together 50,000*l.* These sums would come to the following total :—If you take eight acres for the British Museum, the total for all these buildings will be 1,514,000*l.*; if you take five acres, 1,107,000*l.*; if three acres, 750,000*l.* Assuming that these are wants which Parliament may think it proper to meet, these would be the sums you would require if you took land now occupied by houses in any central part of town. Now, the proposal that we make is one which the Committee will see is a very economical one. By the plan which we recommend we should have much more space and at far smaller expense. The arrangement that we propose is, that the public should purchase seventeen and a half acres. (Several hon. Members: Sixteen.) No—seventeen acres of the land belonging to the Commissioners, which is now covered with the building in which the Exhibition took place. For that land the Commissioners are willing to take 120,000*l.* My hon. friend will admit that to get seventeen acres of land at about 7000*l.* per acre, for which we should pay 50,000*l.*, 60,000*l.*, or 70,000*l.* an acre elsewhere, is a considerable advantage."

It will be gathered from this speech what an enormous saving had been effected by paying such a low price for the land. The plot in question was sold for half its then value, thus presenting the public with 120,000*l.* In the conveyance a covenant was inserted restricting the use of the land to purposes connected with Science and the Arts.

In 1863 the only land to be obtained on these low terms was the large plot purchased from the Royal Commission of 1851, capable of containing the Patent Museum, the Natural History Museum, and other institutions; but by 1869 there was another plot available for the building of a Natural History Museum. This plot consisted of land reclaimed from the Thames near Hungerford Bridge by the construction of the Embankment. As no action had yet been taken on the Cabinet decision of 1863, referred to in Lord Palmerston's speech, concerning the Natural History Museum, it was suggested that it should be built here, and a Select Committee was appointed to inquire into the matter. Their first report was published on May 10, 1869 (Report of Select Committee on Hungerford Bridge), and this was soon followed by a second.

These reports and their accompanying plans are a mine of information, especially in relation to the then stated requirements of biologists with regard to the natural history collection.

It has already been shown that the demands for space for these collections before the Government in 1863 were three acres and eight acres, and that Lord Palmerston compromised with five acres, which were to be provided for out of the sixteen and a half acres purchased from the Royal Commission of 1851.

In the interval between 1863 and 1869 further inquiries had been made, as will be gathered from the following extracts of the evidence (second Report) :—

"Examination of Prof. Owen, p. 107.

"2343. [Mr. Cowper.] Will you state, according to your present views, what area you think necessary for properly providing for the natural history collection?—Mr. Hunt, in 1863, went carefully into all those details and questions with me and ultimately embodied them in a plan, which is printed in a Parliamentary paper. He arranged the building for present actual wants on a space of three acres, and I asked for two additional acres for later additions, looking forward to the next thirty years."

"2344. Is that your present view of the subject?—It is so."

"Examination of Prof. Huxley, p. 112.

"2422. [Mr. Tite.] Probably three acres might include it all?—Yes. I reckon that five times the space now occupied by the bird-room in the British Museum (taking that space at 15,000 square feet) would suffice for the erection of a building in which the largest zoological collection that can ever be formed may be displayed and preserved in a manner most advantageous to the public and to men of science. Thus, for zoology, I ask, say, an acre and three-quarters; I should provide another 15,000 square feet for the fossils, and half as much for the mineralogical collections; and half as much for the botanical collection, if any such collection is to be taken to the new site. This makes a sum total of about two acres and a half, and half an acre for margin, offices, and residences, and the like, and I believe that ample provision will be made not only for all present, but for all future, needs of a great national natural history museum. In saying that, I think the building ought to be of only one storey, and top-lighted; I mean so far as it is devoted to museum purposes. But I think that the museum galleries might be conveniently supported on a side-lighted ground floor, which would afford ample room for the library and offices.

"2423. [Mr. Layard.] Of course, when I asked you the question about the space, I meant to include all the natural history collections, together with the collections of mineralogy, zoology, geology, osteology; in fact, everything that appertained to the department of science in the British Museum?—My first answer had reference entirely to the zoological collection; but if you wish to add the other collections I should say, speaking roughly, another couple of galleries, making, say, seven altogether, would be sufficient, but that is, of course, a mere estimate, and a very moderate one."

The plans show that the building proposed on this site covered about 3¾ acres, and there was little, if any, room for expansion, as the District Railway and wide roads had to be provided for, and it was proposed that the latter should bound the area available for museum purposes.

Ultimately this scheme was given up, and the South Kensington site was fixed upon, the building covering the same ground—3¾ acres—as that proposed for the Hungerford Bridge site.

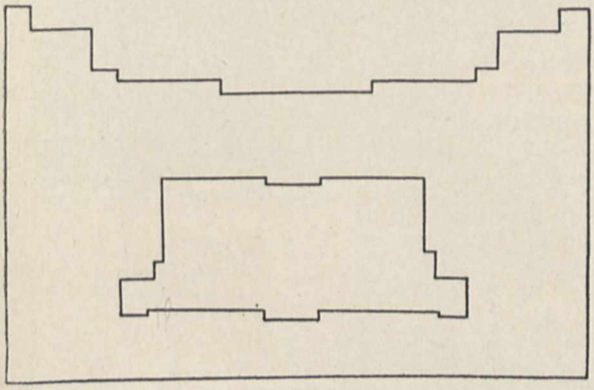


FIG. 1.—Plan of plot and building erected on it.

It would seem that between 1863 and 1869 the question of the Patent Museum had dropped into the background, for the plan ultimately adopted for the Natural History Museum was allowed to sprawl over nearly the whole of the plot as shown in the copy of the map published in the sixth Report of the Commissioners of 1851 (1879). It thus put difficulties in the way of using the unoccupied land. I give another plan, which shows that two museums of the same plan and size, say a Natural History Museum and a Patent Museum, could have been built on the land, leaving some eight acres for future extensions. Of course, if this had been carried out, any desirable change in the plan might have been made.

It would seem also that all the data collected in 1863 and 1869 had either been forgotten or shown to be worthless, for there is nothing that I know of in the shape of public documents to show, until long afterwards, of what part of the 16½ acres bought the British Museum Trustees might consider themselves to be in possession. The land all round the museum was necessarily planted and laid out as gardens, because at the time it was not used for building purposes.

The first thought seems to have been given to this matter in 1881, after the erection of the building. A fence was erected to cut off the land to the north, and the new museum then found itself in the centre of a square containing more than 12½ acres. Shade of Owen! shade of Huxley! The first had asked for three acres, and two more to cover the expansion of thirty years, and the latter three acres, in which "the largest zoological collection that can ever be formed may be displayed and preserved in a manner most advantageous to the public and to men of science."

The second thought was given to this subject in 1899, when a new north boundary was considered. This added one and a half acres more land, making more than fourteen acres in all.

There is no doubt that the Government *then* allocated this land for Natural History Museum purposes. In the recent "Correspondence" (Cd. 5650, p. 1) a letter of 1910 is printed, quoting a letter of 1899 "with reference to the boundary line between the ground which it was then contemplated should be allotted for the use of the Natural History Museum and that provided for the use of the Education Department (Science Museum).

"It was then arranged that a boundary should be fixed, as shown upon the plan, which was forwarded to your predecessor at that time, and it was decided that all the land to the south of that land should be regarded as ear-marked for the future of the expansion of the Natural History Museum."

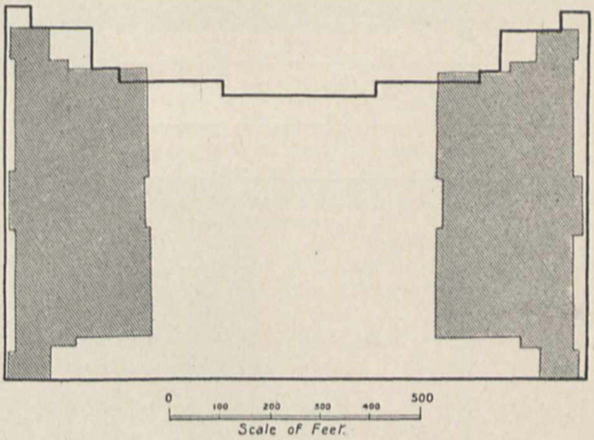


FIG. 2.—Plan showing what might have been.

As the building covers 3¾ acres, this allocation provided 10¼ acres for future expansion.

Incredible as it may seem, this is more by an acre than the area of the parent institution in Bloomsbury at the present time; an area required to garner all the collections on all subjects except natural history, made since 1753, say during a century and a half. Until 1907 it occupied seven and a half acres. In 1907 five and a half acres additional were bought, making thirteen in all.

To state this fact is to show that something had gone wrong somewhere. Had someone blundered?

This does not seem to be impossible, for the Government has recently been led to reconsider the matter. We read in the "Correspondence" referred to:—

"The Treasury and this Board [of Works] have had no desire to disturb the arrangement then [in 1899] arrived at so long as the occupation of this land by the Natural History Museum does not affect injuriously the interests of any other Department."

"The land and the Museum Buildings being vested in the Commissioners of Works, are the property of his Majesty's Government, and they are bound, therefore, in the interests of the public, as a whole, to consider without prejudice whether the time has not now arrived when some modification of that boundary [that of 1899] should be made."

The "Correspondence" goes on to say:—

"There can be no doubt that, whatever shape the new Science Museum may ultimately take, it will be necessary to build up to the boundary line, and it is probable that, in order to safeguard both the Science Museum and the Natural History Museum from fire, and to lay out the ground to the best advantage, it may be necessary to construct a road, to be used privately between the two buildings."

In later "Correspondence" (Cd. 5673, p. 3) a block plan is given in which the revised boundary is indicated by the proposed road.

This proposed road, if constructed, involves the proposed to construct the new private road for the special purposes of the Science Museum. This new road will have to run from Exhibition Road to Queen's Gate, a distance of 1170 feet, and will absorb *at least* three-quarters of an acre of ground. The architects of the Imperial Institute and the Imperial College between them absorbed three acres in their new road running between the same termini.

It must be pointed out that the proposed new road will run parallel to, and only fifty feet away from, the existing road, which has served the purposes of the Natural History Museum for the last thirty years. Curiously enough, this road is not shown on the block plan.

Even a Quartermaster-General proud of his department would scarcely recommend the construction of two parallel roads fifty feet apart, especially where every square foot of space is so precious and is being so hotly contested.

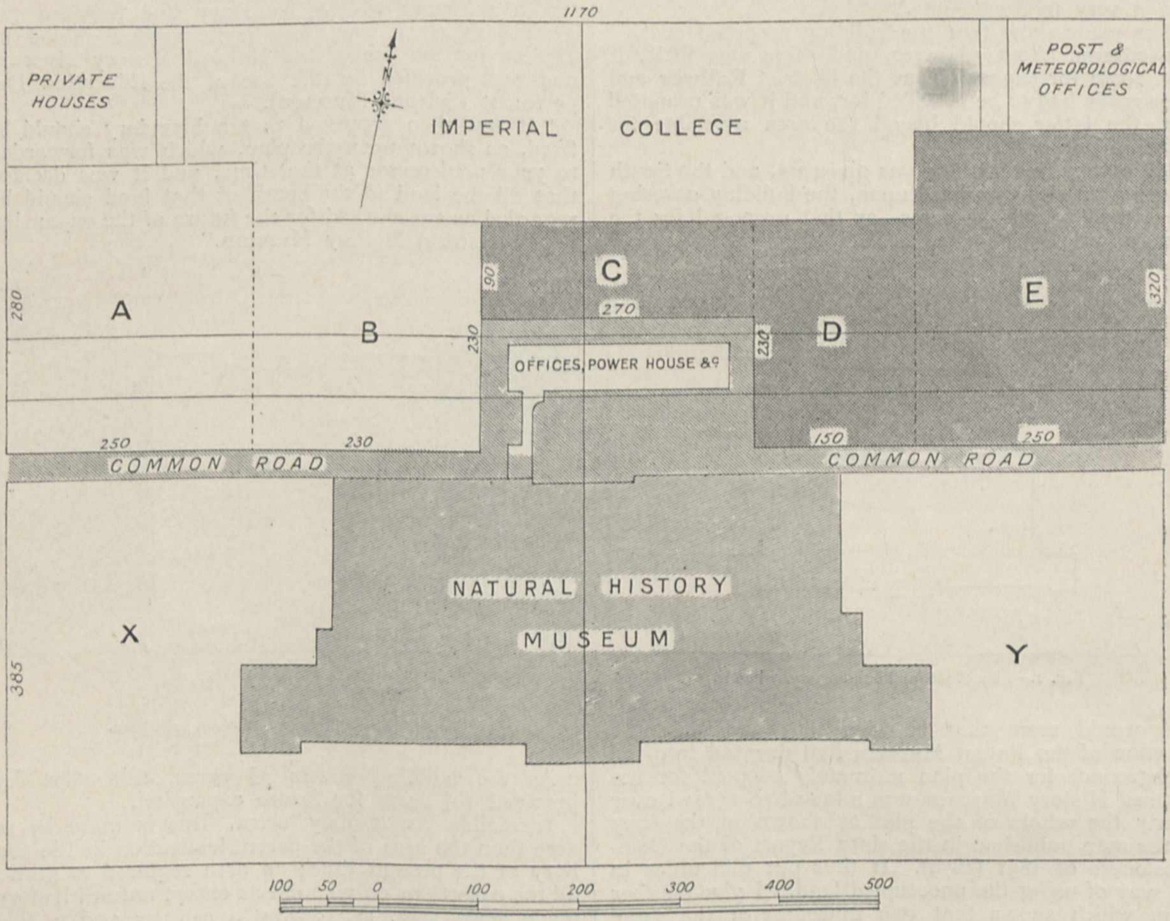


FIG. 3. Plan showing utilisation of the existing road and spirit museum building.

destruction of the building of the spirit museum. It is the inflammable nature of the contents of this building which has been objected to, and not the building itself, which might from its position be conveniently applied to many uses for one or both museums.

This action of the Government in changing the boundary has withdrawn the one and a half acres additional land included in the boundary in 1899; but that still leaves the Trustees in possession of twelve and a half acres, two and a half times what was asked for in 1869 by their responsible officer to provide for a thirty years' expansion.

The newly defined frontier is really the old line of fence erected in 1881, to the north of which it is

The question arises whether the existing road is not really the best boundary. This I referred to in my letter to *The Times* of May 30. It would serve the purposes of the two museums as regards fire precautions and other matters; no sacrifice of space for a new road, no breaking up of frontages, and no destruction of the spirit museum would be necessary.

Further, the existing road when the boundary was thus established could be made to enclose the *body* of the *spirit* museum, which might remain for use as offices, workshops, or other convenient use, in a central backyard, away from any frontage; this, as well as the road which would be carried round it, would be common to the purposes of both museums.

In the accompanying plan (Fig. 3) I have shown gardens, until they are built over, as an embellishment of the south front. I append photographs showing the present condition of the ground and how it would appear if the Science Museum were built to

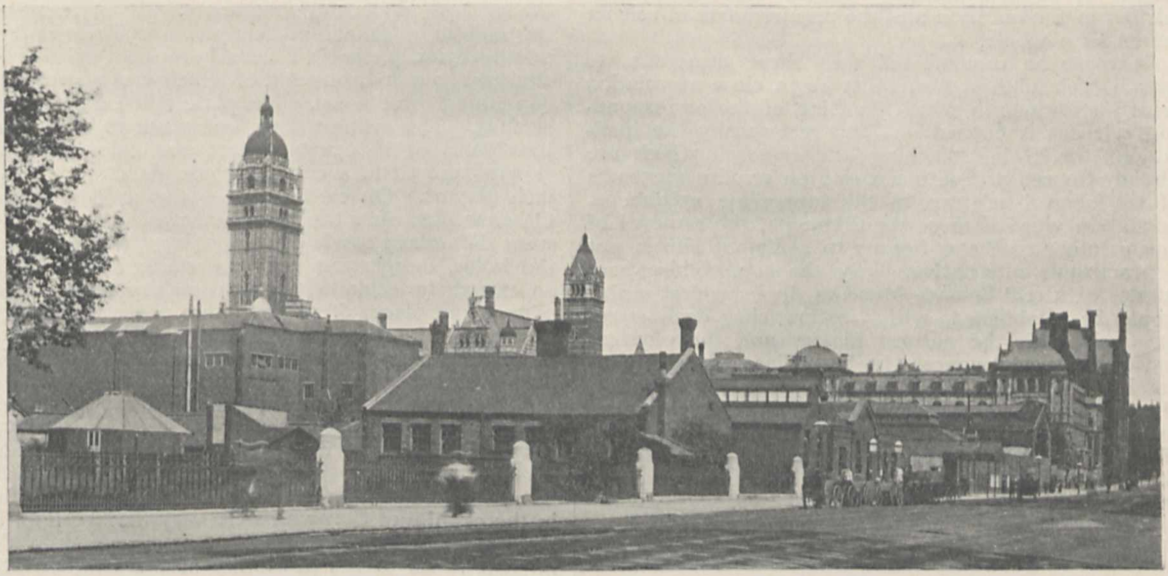


FIG. 4.—Things as they are.

blocks A, B, C, D, E, which could then be used for the Science Museum; the area is six acres.

With regard to the Natural History Museum, it may be stated that with the three boundaries above referred to the included areas are as follows:—

	Feet	Acres
Boundary fixed in 1889	1170 × 530 =	14·2
Proposed road	1170 × 470 =	12·6
Existing road	1170 × 415 =	11·1

harmonise with the Victoria and Albert Museum across Exhibition Road.

In the photograph the frontage is shown broken by the road, but there is no necessity for this if for any reason it would be better to continue it for the purposes of either museum.

If the blocks C, D, E, were built on first, the combined area of the building, just over three acres, would be a little less than that of the Natural History

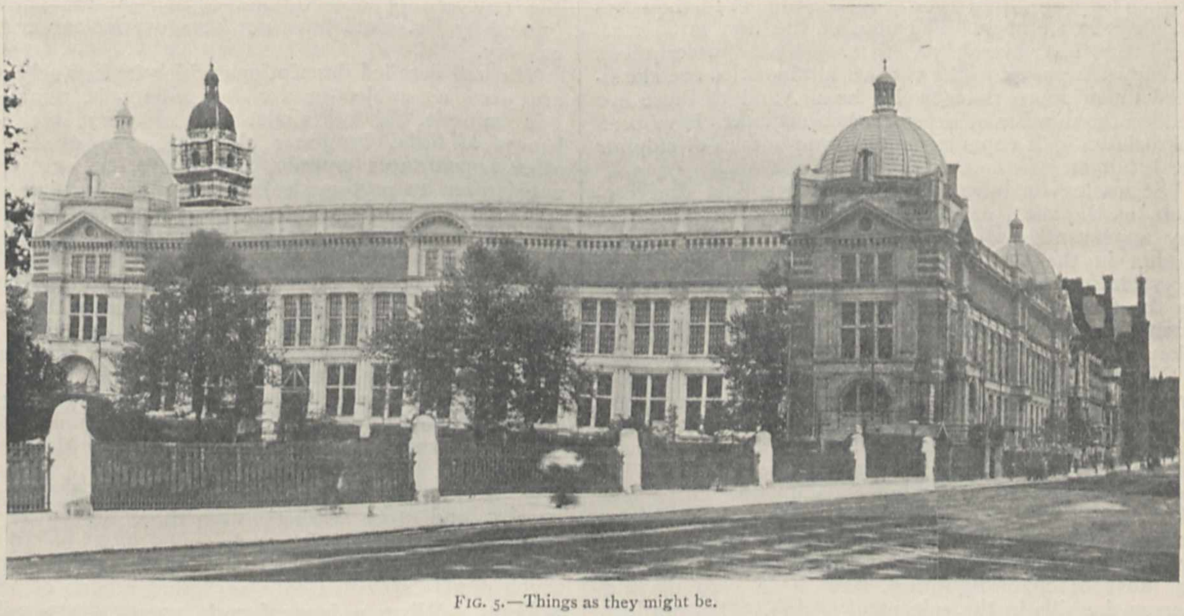


FIG. 5.—Things as they might be.

If the existing road boundary were chosen the road could not only be used for the purposes of both the Natural History and Science Museums, but part of the latter could be built along it, thus utilising the Museum; the blocks A, B, might remain for expansion of the Science Museum, as the equivalent of the blocks X, Y, in the case of the Natural History Museum.

I believe that if such a scheme as this were put forward as a compromise, those who declined to consider it as a way out of the present *impasse* would put themselves in the wrong, in the minds of straight-thinking people who know the history of the question and the requirements of science taken as a whole.

It must be remembered that these museums, to obtain their highest use, must be in close association with institutions in which teaching of the corresponding sciences is carried on, and reciprocally the institutions for higher teaching and research which are already housed at South Kensington require museums of the several sciences in the immediate neighbourhood. In view of these requirements, the removal of one or other of the museums to a distant site is not a practicable alternative. By the compromise here suggested a real Science Museum, in its widest sense, would be established, with two branches dealing respectively with the natural history and the physical and mechanical sciences, in immediate contiguity to the Imperial College. It only wants a consideration of the many memorials presented to the Government since 1858, and of the recommendations of the Duke of Devonshire's Royal Commission of 1874, to be perfectly certain that in the future the two museums will be under one master instead of two.

NORMAN LOCKYER.

EARTHQUAKES AND LUMINOUS PHENOMENA.

IN vol. xiv., No. 6, 7, and 8, of the *Bollettina della Societa Sismologica Italiana,* we find a very long paper by Dr. Ignazio Galli on the collection and classification of luminous phenomena observed at the time of earthquakes. After an introduction, he considers that which might be excluded and the difficulties first met with in the formation of a catalogue of the phenomena he discusses. The illustrations which he gives of luminosities and other strange phenomena which have appeared at or about the time of earthquakes are 148 in number. The date of the first is 89 B.C., and the last March 30, 1910. These descriptions occupy 184 pages. The various luminosities are classified under more than twelve heads, and to these are added the number of times that earthquakes have been associated with vapours, smoke, and odours of sulphur or bitumen.

Seismologists have known for years past that certain earthquakes are said to have been accompanied by appearances of the Aurora Borealis, glimmering lights in the sky, fire-balls, *ignis fatui,* lightnings, corrustations and emanations from the soil, but this is the first time so large a collection of these phenomena have been brought together for their consideration.

When resident in Japan the present writer made many experiments extending over some years on electrical and magnetic phenomena associated with seismic disturbances. He also collected material from all parts of the world which bore upon these associations. One conclusion arrived at is that it is an undoubted fact that at the time of certain large earthquakes, as, for example, the one which in 1906 destroyed Valparaiso, curious lights which, in this instance, were compared to those of chain lightning, have been seen playing across the hills in the epicentral region. Observations of this nature led the writer to make experiments at Shide, in the Isle of Wight, and at the King Edward VII. Mine at Camborne, in Cornwall. The object was to determine whether there was or was not at the time of a large earthquake a practically instantaneous transmission of energy to distant regions other than that recorded by seismographs. It was observed, and still is observed, by many persons that the face of a very large chalk pit at Shide exhibits, after dull damp days, a flaring luminosity. In a chamber at the end of a tunnel in this pit, a cylinder carrying photographic paper was installed. This cylinder was enclosed in a box, one end of which was a metal plate containing three holes. The plate touched a flat chalk surface. The cylinder took one week to turn; therefore parts of the paper before the holes were very slowly exposed to a chalk surface about 3/16th of an inch distant. On certain weeks the results were nil. Other weeks, after the development of the paper, there were three dark bands corresponding to the position of the holes, suggesting that the chalk had acted like an extremely feeble light. Another experiment was to place small pieces of photographic paper in envelopes, a certain number of which had a small glass window; these were placed against the face of the chalk. The image of the windows was frequently obtained, but nothing more than the effects of damp was found upon the others.

The conclusion arrived at was that the photographic effects were in no way connected with radio-activity, but they were probably electrical. The effects obtained in the granite of Cornwall were very marked and, like those observed in the Isle of Wight, varied in their intensity. As to the possibility of these effects being due to micro-organisms, a number of investigations were made, but there were no indications that organisms obtained from the chalk surfaces were connected with luminosity.

Whether these observations throw light upon differences in climate observed at different places, even though they may be near to each other, is a matter for conjecture, but future researches may show that the well-being of living things on the surface of our earth is more dependent upon its radiations than has hitherto been imagined.

I venture to refer to these experiments to show that the outcome of observations similar to those catalogued by Dr. Galli have not been overlooked in this country.

The 148 detailed descriptions which he has collected are used as subject-matter for twenty-six analyses. For example, did lightnings, thunderstorms, meteors, beams of light, luminous clouds, hot vapours, and other appearances precede, accompany, or were they noted after an earthquake? Dr. Galli says that sixteen of these analyses are nothing but the analytical *résumé* of the various phenomena which have been observed, and they therefore possess a real value which cannot be sensibly altered by any report that is ill-founded or untrustworthy. The remaining ten are provisional conjectures which await the judgment of physicists and seismologists. They will be confirmed or contradicted by future observations. If they fall, either partly or entirely, they will at least have the merit of having put the question as to certain probable causes of luminous phenomena connected with earthquakes. At the same time, as one heartily wishes, they may suggest hypotheses which are better, broader, and more synthetic than those the writer of the paper has brought forward.

Dr. Ignazio Galli is to be congratulated on his work, which directs attention to a neglected branch of seismology. When a face of rock 100 or more miles square is rudely pushed over another face, equal in area, it seems reasonable to suppose that such an adjustment should be accompanied by luminous and other phenomena.

JOHN MILNE.

NOTES.

THE arrangements for the meeting of the International Association of Seismology are now nearly complete. The following foreign States will be represented:—United States, France, Russia, Austria, Germany, Hungary, Belgium, Switzerland, Spain, Greece, Italy, Holland, Rumania, Servia, Bulgaria, and probably also Japan and Norway. At the opening meeting on July 18 the Lord Mayor of Manchester and the Vice-Chancellor of the University will welcome the delegates, and Prof. Schuster, as president, will deliver a short address. On the same day the Lord Mayor will hold a reception in the Town Hall. The council of the University will give a dinner, and Dr. Shaw, the director of the Meteorological Office, will invite the guests to an excursion to view the observatory at Eskdalemuir. Among British men of science, the following have signified their intention of being present:—Sir George Darwin, Dr. Milne, Prof. Perry, Prof. Lamb, Prof. Knott, Prof. Love, Mr. Oldham, Dr. Shaw, and Dr. G. W. Walker.

EARLY on the morning of Saturday, July 1, a school of cetaceans, numbering apparently between fifty and sixty head, was stranded on the beach at Mount's Bay, near Penzance. Judging from an illustration in *The Daily Mirror* of July 3, these cetaceans may be identified with the black-fish or pilot-whale (*Globicephalus melas*), which derives its name of ca'ing whale from the habit of entering bays or inlets in schools, and thus becoming stranded, as in the present instance. Although such events are not uncommon in the Faröes and Shetlands, they are rare further south, and the Mount's Bay case appears to be unique. When the tide rose several of the stranded creatures succeeded in making their escape, but those unable to get away, if not already dead, were shot. Such stranded cetaceans are, we believe, Crown property, and application was accordingly made to the Board of Trade for permission to obtain specimens for the Natural History Museum. This being granted, a taxidermist was dispatched by the night mail on Monday with instructions to prepare skeletons and make preparations of some of the viscera. The largest specimen is stated to be about 25 feet long.

THE fact that on July 3 eleven aëroplanes crossed the Channel from Calais to Dover between 4 and 5.15 a.m. without a mishap is a striking proof—if proof be now needed—of the growing trustworthiness of men, motors, and machines. Accidents rarely occur now to experienced pilots, though their experience is small compared with what is considered necessary to make a first-class motorist. The oldest European pilot now flying has barely three and a half years to his credit, while it is well known that double that time scarcely suffices to produce a racing motorist of the highest rank. The eleven aëroplanes included eight monoplanes: Morane (Védrines), two Déperdussins (Vidart and Valentine), Sommer (Kimmerling), two Blériots (Lieut. Conneau and Garros), R.E.P. (Gibert), and Train (Train); and three biplanes: two Maurice Farmans (Renaux, with a passenger, and Barra), and Bristol (Tabuteau). It is worthy of remark that all the machines had Gnome motors except those of Gibert, who used a 60 horse-power R.E.P., Barra, who had a 70 horse-power Panhard, and Renaux, who had a 60 horse-power Renault. The occasion was the seventh stage of the circuit of Europe—from Paris *via* Liége, Spa, Liége, Utrecht, Brussels, Roubaix, Calais, Hendon, Calais, back to Paris again, a total distance of about 1025 miles. At the time of writing, omitting Valentine, Train, Barra, and Tabuteau, all the above mentioned have completed all the stages up to Hendon. The total times of the first four from Paris stand at present as follows:—Conneau, 51h. 43m. 5s.; Garros, 55h. 37m. 7s.; Vidart, 67h. 47m. 57s.; and Védrines, 70h. 20m. 20s.

AN archæological discovery of some interest has been made at Corfu. Excavations carried on by M. Versakis, the local Ephor of Antiquities, on the site of the ancient city (Palæopolis) of Korkyra, at the expense of the Greek Archæological Society, have resulted in the discovery of fragmentary sculptures belonging to an early temple. The most important fragments are those of a Perseus and Medusa group, which reminds one very forcibly of the metope sculptures of Selinus. The remains of colour on them are reminiscent of the brilliant painting of the early sculptures from the Athenian Akropolis now in the Parthenon Museum. There are other fragments of sculpture, all of interest. The discovery having been made during the stay of the Emperor William at the Achilleion this year, naturally attracted the eager attention of his Majesty, who at once consulted Prof. Dörpfeld, the director of the German Archæological School at Athens, with regard to carrying on the excavations himself. This has now been arranged, and Prof. Dörpfeld will carry on M. Versakis' work at the expense of H.I.M. Prof. Dörpfeld is of opinion that the remains belong to a temple of Apollo, dating to the seventh century B.C., probably. The resemblance of the style of its sculptures to that of the Selinuntine metopes is interesting in view of the fact that both Selinus and Korkyra were colonies of Corinth.

THE International Congress of Naval Architects and Marine Engineers, which is being held in celebration of the jubilee of the Institution of Naval Architects, was opened by the Duke of Connaught on Tuesday, July 4, many delegates of foreign Ministries of Marine and foreign societies being present in the assembly. The congress was to have been held last year, but was postponed in consequence of the death of King Edward. The council of the institution, to do honour to their guests from abroad, has made the following additions to the list of honorary members:—The King of Norway, the King of Spain, the King of Sweden, Prince Henry of Prussia, the Archduke Ferdinand, the Duke of Connaught, the Duke of Genoa, Prince Roland Bonaparte, Lord Rayleigh, Admiral Dewey, Admiral Togo, and Admiral Ijuin. An account of the proceedings of the congress will appear in a subsequent issue of NATURE.

THE fortieth meeting of the French Association for the Advancement of Science will be held this year at Dijon from July 31 to August 5, when M. Charles Lallemand will be the president. The sections of the association and their presidents are as follows:—M. E. Belot, mathematics, astronomy, geodesy, mechanics; M. Galliot, navigation (civil and military), engineering; Prof. Hurion, physics; Prof. Georges Lemoine, chemistry; Prof. Violle, meteorology and physics of the globe; Prof. Collot, geology and mineralogy; Prof. Quéva, botany; Prof. Bataillon, zoology, anatomy, physiology; Dr. Henri Martin, anthropology; Dr. Paul Courmont, medical science; Dr. Delherm, medical electricity; Prof. Grimaud, odontology; M. Lucien Magnien, agriculture; M. Auguste Chevalier, geography; M. Paul Razous, political economy and statistics; Prof. Beauvisage, pedagogy and teaching; Prof. Jules Courmont, hygiene and State medicine; Dr. Simon, archæology. Inquiries may be addressed to the secretary of the association, Dr. Desgrez, 28 rue Serpente, Paris.

THE third International Congress for Sanitary Dwellings is to be held this year, on the occasion of the International Hygiene Exhibition at Dresden, on October 2–7. The congress is to be divided into nine sections, as follows:—Group A: *General*.—Section i. Town-planning (building, forms of country settlement, garden cities, width of streets, height of building). ii. Construction of buildings (planning, distribution of space, building material, foundations, basement, kitchens, lavatories, floors and ceilings, staircases, lifts and roofs). iii. Internal arrangements (lighting, heating, ventilation, furnishing). iv. Sanitation (cleaning, removal of refuse, disinfection). Group B: *Dwelling Houses*.—v. Dwelling houses in towns. vi. Dwelling houses in the country. Group C: *Special Kinds of Dwellings*.—vii. School buildings, boarding-schools, prisons, hotels, lodging-houses, hospitals, convalescent homes, baths, churches, theatres, and other public buildings. viii. Work-rooms and workshops, means of communication and transit (railways, tramways, ships, vehicles, &c.). Group D: ix. Legislation, executive, statistics, &c. Prof. Renk is the president of the congress, and Dr. Hopf the general secretary, and all inquiries should be addressed to him at Dresden, Reichsstrasse, 4. Other communications and money-orders should be addressed: Kongresskanzlei, Zimmer 156, neues Rathaus, Dresden.

So far as is at present known, no fresh cases of plague among the rats have been discovered in the last few days at the riverside wharves at Wapping and Shadwell (see NATURE, June 29, p. 592). *The Times* special correspondent in the issue of June 28 pleads for an organised inquiry into the incidence of plague among the rats in England. He points out that in East Anglia, where plague was present last September, no action had been taken by several of the rural district councils, and that in London only spasmodic attempts at rat destruction have been undertaken. He also condemns as unwise the attitude of the Local Government Board in attempting to suppress knowledge of these recurring outbreaks of plague among the rats. No other country has so much at stake as Great Britain, with her vast carrying trade, and a persistent, elaborate, and prolonged inquiry into the radius of rat-infection is urgently called for.

THE issues of *Travaux de Soc. Imp. des Naturalistes de St. Pétersbourg—C.R. des Sciences* from January to March contain several articles on various groups of invertebrates, for the most part in Russian, notably two by Mr. K. Derjugin on the scientific results of the cruise of the schooner *Alexander Kowalevsky* in the Kola-Fjord during 1908–9.

To the *Sitzungsberichte* of the Vienna Academy of Sciences for December, 1910 (vol. cxix., part i.), Mr. J. Tandler contributes an important memoir on the pectoral muscles of mammals, based on MS. left at his death by the late Dr. E. Zuckerkandl. The anatomy and relationships of these muscles are described in representatives of the various orders, from the opossum to the gorilla and man.

IN reference to the paragraph on African dinosaurs on p. 390 of NATURE of May 18, Mr. F. A. Lucas writes to say that the longest rib of a dinosaur from the western United States, described in 1904 by Riggs under the name of *Brachiosaurus altithorax*, measures 2·74 m., or somewhat longer than the corresponding bone of the African species. The length of the femur is 2·03 m., and that of the humerus 2·04 m. The latter is a little less than the length of the African humerus, but serves to show that the femur is not necessarily the longest of the limb bones. In Mr. Lucas's opinion Brachiosaurus and the African dinosaur may prove to be related.

THE migration of the godwit from eastern Siberia to New Zealand forms the subject of an article by Mr. R. W. Reid in the July number of *Chambers's Journal*. The birds leave the extreme north of New Zealand early in April, and reappear usually in the first weeks of October; and they appear to spend a couple of months in the far north, four months in travelling, and the remaining half of the year in their southern home. They thus enjoy two summers in succession. When they arrive in New Zealand the godwits are in their winter plumage, but those which remain when their fellows fly north assume the brilliant summer dress, although they never breed. These non-migrating birds thus wear a summer livery in winter. It would be interesting to ascertain whether the Antarctic affords suitable breeding-grounds for the species.

MUSK-RATS (genus Fiber) form an exclusively North American group of rodents, with a distribution extending from the neighbourhood of Bering Strait to Arizona and the Gulf of Mexico. With such a range, it is remarkable that they have not hitherto been detected in north-eastern Asia, either in the living or fossil condition. Full details of their taxonomy, distribution, habits, and commercial importance are given by Mr. N. Hollister in a memoir published by the U.S. Department of Agriculture as No. 32 of the North American Fauna. The typical species is divisible into a number of local races, but the Newfoundland musquash (*F. obscurus*) is recognised as a distinct species on account of its inferior size, relatively large hind feet, dark colour, and small and weak skull and teeth. A form from a small area on the coast of Louisiana is likewise ranked as a species (*F. rivalicius*), although it differs from the typical *F. zibethicus* mainly by its somewhat inferior dimensions, duller colour, and darker under-parts. Three extinct species are recognised. In spite of incessant pursuit for the sake of their fur, which is steadily increasing in value, musk-rats continue to hold their own, and, according to the author, will probably become in the future the chief American fur-producer.

INHERITANCE of heterostylism and colour in *Primula sinensis* have provided the subject of extensive experiments by Mr. R. P. Gregory, who presents his latest results in *The Journal of Genetics* (vol. i., No. 2). Colour has to be considered independently for the flowers and stems; when absent from the flowers they are white, and when absent from the stems they are green. Colour may also be distinguished as full or pale. Full-coloured flowers are only produced on deeply coloured stems, although in the horticultural class "Sirdar," where the petals are full-coloured but the pigment occurs in dots, the stems are green. Full colours are dominant to pale; magentas are dominant to reds, and both colours to blue; whites may be dominant or recessive to colours.

A MORE than ordinary amount of critical investigation has been undertaken by Dr. A. S. Hitchcock and Miss A. Chase in the compilation of the monograph on North American species of Panicum, published as vol. xv. of the Contributions from the United States National Herbarium, as may be surmised from the list of herbaria consulted, and is confirmed by examination of the details supplied. The authors take a restricted view of the genus, which is founded on the species *Panicum miliaceum*. In addition to the section of Eu-Panicum, under which seventy-five species are collected, a small subgenus Paurochætium with six species, and a large subgenus Dichanthelium, comprising 110 species, are segregated. The two larger sections are again subdivided into groups named after a leading species. Members of the Dichanthelium section are characterised by distinct vernal and autumnal habits.

THE question having been raised whether the timber from teak plantations is as strong as that from natural forests, specimens from trees grown in Burma were subjected to a series of tests, with the results recorded by Mr. R. S. Pearson in Forest Bulletin No. 3 published by the Government of India. The samples were, of course, similar in dimensions, and care was taken to get them as uniform as possible with respect to moisture content. Only in the case of transverse strain did the timber of natural-grown teak give a higher coefficient, and then the difference was small enough to be negligible for practical purposes. Another Bulletin (No. 2), issued with the former, provides a set of diagrams and curves, prepared by Mr. F. A. Leete, to show the relation between age, girth, number of stems, and height of teak trees in fully stocked plantations. The data may be used to determine whether any given area is suitably stocked or if thinning is required.

IN *La Géographie* for April M. C. Rabot summarises his own work and that of others relating to the superior limits in altitude of forest trees in Scandinavia, and comes to the conclusion that the retreat of these upper limits is to be attributed to climatic variation or, more exactly, a lowering of the summer temperature, which has been estimated at 2·5° C.

IN the Transactions of the Royal Society of Canada Dr. W. Bell Dawson describes in general terms the scope of the investigations carried on by the Tidal Survey of Canada. An important portion of the work has been the establishment of local bench-marks and the determination of their relation to mean sea-level, since no general system of levels yet exists in the country. A number of these have now been fixed, and are described in a recent (1906) report of the marine department, but their connection by lines of precise levelling is still wanting.

MESSRS. S. NAKAMURA and K. HONDA contribute a lengthy paper to the Journal of the College of Science at Tokio University on seiches in some lakes of Japan. Starting with a Sarasin limnimeter, a simpler type of portable instrument was found desirable, and each of the authors produced one which was found to be well suited to the investigation in hand. Large-scale records of the curves obtained with these instruments are given in facsimile, and these show the very marked oscillations of the water set up by violent thunderstorms passing over the lake while the record was being taken.

IN the *Arkiv för Matematik, Astronomi och Fysik* of Stockholm, Band 6, No. 40, Mr. O. A. Åkesson puts forward a method for determining the direction of ice-drift in the neighbourhood of the Pole. Supposing such drift to be rectilinear and uniform, he utilises four altitudes of the sun taken in pairs at about six hours' interval, as long a period as practicable separating the first and last pair. The Greenwich times of the observations are supposed to be known, and, following the method employed by Prof. Charlier, the position of the first point and the direction and rate of drift are obtained. An example from the voyage of the *Fram*, when ice-bound from February, 1893, to August, 1895, is given in illustration of the method.

AT a meeting of the Research Department of the Royal Geographical Society, Dr. Strahan presented the fourth report on the investigation of British rivers, which dealt with the Exe, the Creedy, and the Severn. Gauge readings and discharges had been taken frequently, and an interesting determination of the bottom-load moved by the Exe in each of the past seven years was included. The rainfall has also been studied to obtain the ratio of the run-off for the river system and for different tributaries. This most important investigation can only advance slowly on account of the lack of local observations having any considerable continuity and being satisfactorily comparable. Even under the present arrangements, it is difficult to ensure brief flood waves being adequately recorded at all stations, but the results testify the importance of the inquiry and the need for continuing it on as wide a basis as possible.

THE meteorological charts for July issued by the Meteorological Committee contain, *inter alia*, timely and useful notes on the cyclonic storms of the Indian Ocean. During that month the south-west monsoon dominates the weather conditions over the Bay of Bengal and the Arabian Sea, and storms over the head of the bay are of comparatively frequent occurrence; the force of the monsoon in the Arabian Sea attains at times thirty to forty-four miles an hour. In the South Indian Ocean cyclonic storms are (so far as available observations show) practically non-existent in July; only one trustworthy storm track is shown there on the chart during a 38-year period. The interesting synoptic weather charts over the North Atlantic for June 8–14 show that a cyclonic area which was formed off Newfoundland on June 11 moved steadily eastward, and at the close of the period was beginning to influence the weather on our extreme western and south-western coasts.

AN interesting contribution to *Symons's Meteorological Magazine* for June, by Mr. R. C. Mossman, now attached to the Argentine Meteorological Office, refers to the probable rainfall in the north-east of England during the present summer. On comparing the rainfall at Cordoba for the first quarter of the year for the thirty-three years 1878–1910 with that of the north-east of England for the third quarter, it is seen that the values are generally the reverse of each other, *i.e.* when the rainfall at Cordoba is in excess of the normal during the first quarter, it is in defect over the north-east of England in the third quarter, and *vice versa*. During the first quarter of this year the rainfall at Cordoba was only 46 per cent. of the normal. So far as these statistics go, therefore, there is a distinct suggestion that the present summer (July to September) will have a rainfall in excess of the normal over the north-east of England.

THE summary of the weather for the first six months of the present year, which has just been issued by the Meteorological Office, shows that the mean temperature was slightly in excess of the average in all parts of the United Kingdom, taking the period as a whole. The absolute highest temperature in any district during the six months was 84°, in the north-east of England, and 80° has been exceeded in every district except in the north of Ireland and in the Channel Islands, whilst the lowest temperatures are below 20° in every district except in the Channel Islands, the absolutely lowest reading being 11°, in the east of Scotland. The largest aggregate rainfall for the six months January to June inclusive is 24·02 inches, in the north of Scotland, which is 0·58 inch more than the average; but this is the only district with an excess of rain except the east of England, where the aggregate was 10·33 inches and the excess 0·15 inch. The smallest aggregate rainfall in any district is 8·31 inches, in the Midland counties, where the deficiency for the six months amounts to 3·30 inches. The greatest deficiency is in Ireland, amounting to 3·97 inches in the north and 3·88 inches in the south. In the south-east of England, which includes the London area, the aggregate rainfall was 9·97 inches, which is 1·38 inches fewer than the average. The greatest number of rainy days is 106, in the north of Scotland, and the least 73, in the Midland counties. The total duration of sunshine was nowhere very different from the average; there was an excess in all districts except in the north-east of England, the Midland counties, and the Channel Islands, but the deficiency nowhere exceeded twenty hours. The rainfall for the first month of summer was generally in excess of the average; but the difference from the normal was nowhere very great except in the north-east of England, where the excess amounts to 2·20 inches. The duration of bright sunshine for June was generally in excess of the normal. At Greenwich the mean temperature for June was 60·8°, which is 0·5° above the average of the previous sixty years; the first half of the month was much warmer than the latter half, and the rain during the latter half was very much heavier; the aggregate for the month was 0·1 inch more than the normal, whilst the duration of bright sunshine was about twenty-five hours more than usual.

Some interesting photographs are described by M. Flammarion in the June number of *L'Astronomie*. Apropos of Prof. R. W. Wood's recent experiments in photographing the lunar surface in ultra-violet light, M. Flammarion shows pictures of ordinary objects taken with the sun's invisible radiations, infra-red, and ultra-violet. In one set, taken in full sunlight with the less refrangible rays, white trees are seen projected against a dead-black sky, giving the appearance of a nocturnal snow scene. Another pair of photographs, one ordinary and one ultra-violet, are simultaneous photographs of a man standing; his shadow, very pronounced in the ordinary photograph, has disappeared, through lack of contrast, in the second. But perhaps the most striking pictures are a similar pair in which the camera was pointed towards a landscape so that a part of the latter is seen through a glass window. In the ultra-violet picture the window and the landscape beyond have both disappeared, the window-glass being entirely opaque to the shorter radiations solely transmitted by the screen and the quartz objective.

As a preliminary to the study of the magnetic properties of some slightly magnetic alloys, Mr. H. C. Hayes, of the Jefferson Physical Laboratory of Harvard University, has investigated the errors to which the cooling curves taken in the metallurgical study of an alloy are subject. As these errors are obviously greater the more rapid the cooling, Mr. Hayes has constructed a new crucible which allows the process to be accelerated at will. It is of steel, and to prevent contamination of the melt is lined with a thin coating of lime, which is applied in a special way. The crucible and its contents can be quickly transferred from the furnace to a tank of water. Using an Einthoven string galvanometer with thermo-junctions of copper-constantan, the author finds that with the usual arrangement of the couple in a protecting tube of quartz or porcelain the lag of the thermo-junction behind the temperature of the melt may, during a rapid cool, exceed 400° C. By using a copper tube deposited electrolytically on the end of the constantan wire, and protecting it by means of a thin coating of lime, he finds that the lag is reduced to a very small quantity, even for the most rapid cooling curves.

A copy of a report on the analysis of potable spirits by the Government analyst for Western Australia has reached us. Its chief feature is a table setting out in detail the analytical results obtained in the examination of eighty-seven specimens of whisky, of known origin and age, selected from distilleries and other places in Scotland. The author considers, in reference to the evidence given before the recent Royal Commission on Whisky, that there is really less difficulty than was supposed in forming analytical standards for distinguishing between various classes of whisky, namely, "pure malt, pot still" whisky, "grain, patent still" whisky, and "blended" whisky, a mixture of the two. He suggests that definitions should be based, not upon any one constituent, but upon three taken conjointly, the furfural, esters, and higher alcohols being the three chosen. Further, not only should the quantities of each of these reach a stipulated minimum, but the three should bear a certain proportional relation to one another. On these principles definitions are formulated for (1) pure malt pot-still whisky, and (2) for blends containing, respectively, not less than 75 and not less than 50 per cent. of the pure malt pot-still product. The actual figures proposed as standards presume that the particular analytical methods described are employed, and it is admitted that they would exclude some spirits from their proper classes, though the number would be few. There is nothing new in the principle of forming a judgment on the character of a spirit by considering all three of the constituents indicated; probably most alcohol analysts do it, and certainly some have done so for years past. The data now given, however, obtained as they are from a considerable number of whiskies all analysed under strictly similar conditions, will be very useful in any discussion of standard values, whether the particular limits suggested by the author are accepted or not. A similar series dealing with Irish whiskies would be valuable.

The first part of the fifth volume of the Journal of the Institute of Metals has been received. The first section of the book, which runs to 423 pages, contains a full account of the annual meeting held on January 18 last, and reported in the issue of Nature for January 26 (vol. lxxxv., p. 429). The report of the council for the year 1910 is included, and the presidental address by Sir Gerard A. Muntz, Bart., entitled "'The Reason Why': the Quest of the Institute of Metals," is printed in full. The second section is devoted to abstracts of papers relating to the non-ferrous metals and the industries connected therewith; while the third contains the Memorandum and Articles of Association and a list of members.

Messrs. J. and A. Churchill have in preparation, for publication early next year, a new annual entitled "Who's Who in Science," to be edited by Mr. H. H. Stephenson, and to contain the names of leading representatives of science throughout the world, as well as statements of their chief works.

OUR ASTRONOMICAL COLUMN.

Wolf's Comet, 1911*a*.—In No. 4508 of the *Astronomische Nachrichten* Prof. Max Wolf states that he rediscovered his comet, on June 20, on a plate taken for that purpose. The position, for 1855·0, was 18h. 43·7m., +13° 25′, at 12h. 4·9m. Königstuhl M.T., on June 19. 18h. 43·7m., +13° 25′, at 12h. 4·9m. Königstuhl M.T. The object has a stellar nucleus of magnitude 15, or brighter, lying eccentrically in an extremely faint nebulosity of 20″ diameter. According to M. Kamensky's ephemeris, the positions for July 6 and 14 are 18h. 31·9m., +15° 0·5′, and 18h. 24·5m., +15° 15′, respectively.

THE RADIAL VELOCITIES OF NEBULÆ.—Prof. Campbell, writing to the *Astronomische Nachrichten* (No. 4508), urges that as many radial velocities of nebulæ should be determined as is possible.

Keeler's observations of the radial velocities of twelve planetary nebulæ show that these objects probably have very high actual velocities, something of the order of 25 km. per second. On the other hand, observations of the Orion nebula, and others, indicate that these extended nebulæ have very low radial velocities, or are at rest relatively to the stars of our system. If these facts can be accepted as a generalisation, many problems connected with stellar evolution are opened up. For example, it is suggested that the extended, amorphous nebulæ are masses of primordial substance antecedent to the stellar state, whereas the quickly moving planetary, or condensed, nebulæ are the results of collisions, or near approaches, &c., of rapidly moving aged stars. Some support is given to this suggestion by the observation that helium (early) stars show but small radial velocities (6 km.), whereas the older, reddish stars give much higher (16 or 17 km.) velocities.

THE INTERFEROMETER IN THE STUDY OF NEBULÆ.—In the June number of *The Astrophysical Journal* (vol. xxxiii., No. 5, p. 406) MM. Fabry and Buisson describe an apparatus they have employed in the spectroscopic study of nebulæ. This apparatus is compact and light, and can readily be attached to any telescope of sufficient aperture for the work. They have used it with the Marseilles refractor of 10 inches (26 cm.) aperture and 10 feet (3·1 metres) focal length, and have secured useful observations of the Orion nebula; but a reflector of large relative aperture would be much better for the work, because of its achromatism.

The method is capable of giving very accurate measures for the wave-lengths of the different nebula lines, and, therefore, using the hydrogen lines should give the radial velocity with great precision. By finding the variations of wave-length from one part of the nebula to another, the circulatory movements of the gas might be determined.

THE ROTATION OF STARS ABOUT THEIR AXES.—In a note published in No. 7, vol. lxxi., of the Monthly Notices (p. 578), Prof. George Forbes makes an interesting suggestion on the possibility of determining the rotation of stars about their axes and the direction of the axes. In the case of the sun this rotation causes a displacement of the lines in the limb spectra, which in the case of the integrated light of the stars would become a broadening of the lines. But if the star is an Algol variable the light from each limb is periodically arrested by the eclipsing satellite, and the spectral lines should then show broadening on one side only. When the other end of the bright star's equator is eclipsed, broadening on the other side of the line would result. Relatively this would amount to a periodical displacement of the lines about a mean position, such as has been observed in some cases, and a measure of the displacement might provide material from which the direction of the star's axis and the velocity of equatorial regions of the star could be approximately deduced.

THE TAILS OF COMET 1910*a*.—From Prof. K. Pokrowski, of the Dorpat Observatory, we have received an interesting monograph dealing exhaustively with the several tails of comet 1910*a*. From fifty-nine observations made by various observers—many of which were recorded in these columns—Prof. Pokrowski determines the positions of different points in the tails, and then calculates the time at which the matter forming them was ejected and the value of the repulsive force which drove them away from the nucleus. He thus finds that the chief tail conforms to Bredichin's second type, the repulsive action being about equal to the gravitational attraction; the western branch of the tail was probably ejected, under rather greater repulsive force, from the nucleus about January 18.

The second, smaller tail, belongs to the third type, being formed under a repulsive force considerably less than the gravitational repulsion by particles which left the nucleus about January 16. The anomalous tail, directed towards the sun, is likened to those of comets 1844 III., 1862 III., and 1882 II., where the particles left the nucleus with a very small velocity, in this case about 0·5 km. per second.

CONFERENCE ON EDUCATION AND TRAINING OF ENGINEERS.

THIS conference, organised by the Institution of Civil Engineers, held its meetings at the rooms of the institution on June 28 and 29, under the presidency of Mr. Alexander Siemens. The subjects were discussed under three sections, general education, scientific training, and practical training, the chairmen of these being respectively Mr. Anthony G. Lyster, Dr. W. C. Unwin, and Mr. R. Elliott-Cooper. There were very large attendances at all of the meetings.

In opening the conference, the president stated that the council of the institution had endeavoured to define the scope of discussion by declaring that its object was to consider the methods of preparation to be adopted by those who contemplate entering the engineering profession in compliance with conditions laid down by the by-laws for election into the institution. In addition to a sound general education, to a competent knowledge of science and to practical training, there is the necessity common to all professions, which Prof. Max Müller defines thus:—" . . . No science and no art have long prospered and flourished among us, unless they were in some way subservient to the practical interests of society . . . that interest depends on the practical advantages which society at large derives from these scientific studies." This principle may be expressed by saying that a young engineer should be educated so as to become a dividend-earner for his employer, for this is the most trustworthy indication of his merit. He should possess some knowledge of business methods and of law, and he will find one or more modern languages very useful in obtaining remunerative employment. No definite resolutions would be taken on the subjects under discussion, as the object of the conference is rather to form opinion than to arrive at and to record definite conclusions.

Sir Wm. White directed attention to the danger of too much devotion to the theoretical side. At one time training in engineering science could scarcely be obtained, but there was a modern tendency to go too far in the other direction at the expense of the practical side of the engineer's training. The evils of coaching for examination purposes were also pointed out by the same speaker. Sir J. Wolfe Barry justified the introduction of admission to the institution by examination, and considered that in the engineer's course of training the general education of an accomplished gentleman should not be lost sight of.

Mr. Anthony G. Lyster considered that education, to be of real value, should not only furnish information and knowledge, but should also train and expand the intelligence and develop that type of character which fits a man to lead the best and most useful life. The demand for special training for the engineer becomes increasingly urgent, and unless he is to be debarred from our universities or public schools, it behoves the authorities to bring their educational standards into line with modern requirements. There is no desire that members of the engineering profession should be engineers and nothing else; on the contrary, every opportunity should be given to the best type of man that the university and public school can produce to start with the best intellectual equipment as an engineer.

In the general education section, Dr. James Gow, headmaster of Westminster School, opened the discussion on literary education and engineering by stating that his experience as examiner in Latin for the institution showed that Latin is seldom included in the preliminary education of an engineer, or that boys who intend to be engineers pay little attention to this subject. It is probable that, where Latin is neglected, no very close study is given to any other language. He did not contend that literary studies are of any direct or immediate use to the engineer; his work must be largely deductive and mathematical, on which a literary education has no bearing. But presumably there are occasions when he is called upon to make use of chemistry or geology, or some other deductive science, and he is not properly equipped unless he has a fair knowledge of these sciences. Now it is notorious, at least to schoolmasters, that a boy who passes from the classical side to the modern side of a school has an immense advantage in inductive science over those who have been educated entirely on modern lines. Latin, as taught with grammar and dictionary, is inductive science almost in the abstract. Again, the engineer is doubtless often called upon to command a gang of foreign workers. The sooner and the better he learns their language the more easily he will control them and direct their labour. The language may be wholly alien to any that is spoken in Europe, yet the necessary elements of language must always be the same. It is undoubtedly the business of the engineer to understand contracts, and to give and receive orders accordingly. These transactions require a careful and exact appreciation of words by whomsoever used. It is too often forgotten that the whole profession of lawyers lives, in the main, on the inability of other people either to say what they mean or to understand what is said to them. Dr. Gow suspects that the time is at hand when it will be advantageous to the engineer to mingle the *utile* with the *dulce*, to discern not only what is mathematically possible, but also what is artistically impossible, not only what is cheap, but also what is nasty. For this purpose some general culture is necessary, such as makes a man liberal in mind and sympathetic to the common run of his fellows. Dr. Gow considers that the best plan is to give a boy a general education, mainly literary, up to sixteen years of age, and at that point to watch him closely and put him to what he wants to learn. If he is clever he will be successful; if he is not clever he will, at least, be happy and proud of his calling.

Prof. Silvanus P. Thompson initiated a discussion on the extent to which mathematical and scientific subjects should share with other subjects of literate education the attention of schoolboys who intend to enter later the engineering profession. In the present chaos of secondary education, the schools of the type which chiefly furnishes boys to the engineering profession are almost wholly destitute of any organisation adapted to that end. Not one, so far as the speaker knew, has any definite educational goal to set before the majority of its boys. In general, schoolmasters devote their energies to preparing a few scholarship candidates, and have no definite educational aim whatever for the bulk of the boys. Until this hopeless state of things is radically altered, and until the goodness or badness of a school is adjudged, not by the triumphs of a few, but by the proportion of all its scholars whom it brings to a maturity test, British education will continue to be in a bad way. In all German secondary schools there is a perfectly definite goal before every boy in the school. Before he reaches the topmost class he will have to pass the Einjährige examination, or pass out disgraced. Three years later, if he passes the Abiturient examination, the way is open for him to any university and to any professional career; otherwise he is marked as unfit for professional life. Here, with us, the State (save in Scotland) has not yet organised the secondary schools. Each university wastes its energies over holding matriculations and the like. Almost all the professional bodies hold amateur matriculations or preliminary examinations of their own. The result, educationally, is muddle, waste, inefficiency. The schoolmasters, in despair at the multitude of twenty conflicting matriculations, fix upon none, and let chaos work.

Prof. Thompson is in agreement with the recommendations of the report of the committee of the Institution of Civil Engineers, given 5½ years ago, on the studentship examination. These briefly are:—(1) specialisation at school is undesirable; (2) a leaving examination for secondary schools, similar to that in existence in Scotland and in Wales, is desirable throughout the United Kingdom; (3) instruction in mathematics should be by somewhat modified methods; (4) a general knowledge of elementary physics and chemistry or natural philosophy is preferable to the pursuit in detail of some particular department in science.

At school, the first object of science teaching should be to evoke interest, not to impart the facts or data of science, still less to systematise their rediscovery. All that has its place later. Even the driest subject can be made thoroughly attractive by a live teacher who handles his subject in a human way. A bad teacher can make even electricity as dull and distasteful a subject as the conjugation of irregular verbs. One difficulty which has been explained by masters of progressive tendencies in seeking to introduce mathematical reforms such as that which has come about in the past few years has been the stupidity of inspectors, who have not yet grasped the importance of the reforms. The benumbing influence of all the older Cambridge traditions is also felt. Bad teaching is responsible more than anything else for distaste for mathematics. A really capable teacher will make his boys enthusiastic over matters that in the hands of others are deadly dull.

The greatest change which has come over the teaching of mathematics is the almost complete disappearance of Euclid. Prof. Thompson is not sure whether the loss is not greater than the gain. The teaching of Euclid was in one respect absolutely invaluable. If approached rightly, after practice in geometrical drawing, the study of Euclid constitutes an unrivalled training in methodical and cogent reasoning. But Euclid is gone, and there has been no satisfactory substitute for it. It is the opinion of Prof. Thompson that boys nowadays are less capable of following a sustained train of thought than they used to be.

In the conflict of subjects, one is apt to lose sight of the fact that training in thinking and in the correct expression of thought is more essential than the study of any particular subject. In all studies—science, mathematics, language, or literature—there should be cultivated precision in the use of words and cogency in modes of thought. These things are vastly more important in the ultimate making of a professional engineer than the acquisition of a hoard of scientific facts. The secondary school must not degenerate into a house of cram.

Sir John Wolfe Barry agreed with Dr. Gow that specialisation should not commence too early. A general training should be given in early life, as it was impossible to attain to such later. His own early training had not been directed towards engineering, and he considered that this had been to his advantage.

Mr. Theodore Reunert, of Johannesburg, hoped that the conference would lay down a definite course of training for engineers. In other professions parents could obtain definite information regarding the course of study and training through which their sons must proceed; in engineering no such information is available.

Dr. W. H. D. Rouse thought that specialisation before sixteen or seventeen was wrong. In his opinion, Prof. Thompson had not spoken too strongly in regard to the evils of examinations. Examinations were generally of an undesirable character, and their continued multiplication was a great evil. Prof. Thompson's remarks about German examinations being conducted by cooperation between the State and the teachers were of value, and he considered that such was the only way that was fair to the teachers.

Mr. W. Whitaker, F.R.S., could not confirm the view that culture could be obtained only in early life. Dr. Gow had emphasised the educational value of literature; there was nothing like leather, and he noticed that Dr. Gow was a Doctor of Literature. Mr. Whitaker agreed with Prof. Thompson's remarks on the teaching of Euclid. He believed that English was taught very badly, and that scientific men in this respect were as badly trained as the majority of people, and worse than most.

Dr. R. Mullineux Walmsley thought that the lack of a definite educational aim in England was due to defective public opinion. In this respect matters were much better in Scotland. The statement which had been made that universities had to hold matriculation examinations because they wanted the fees was true, and if the London University gave its examinations up it would be bankrupt—a scandalous state of affairs.

Prof. W. S. Abell considered Euclid to be the basis of the English system of mathematics, and that the Institution of Civil Engineers should insist on its being taught.

Colonel J. E. Capper said that a mechanic might be able to lay a railway, but an engineer, in addition, should be able to say if it was necessary to lay it at all. To answer such questions, a general literary education was necessary.

Mr. T. H. Bailey suggested that Prof. Thompson had missed the aim of public-school life, which was not to turn out professional men, but to give a general grounding and moral training.

Both Dr. Gow and Prof. Thompson in their replies agreed on the undesirability of having a rigid examination for all students. But schoolmasters certainly desired the laying down of a general course which might be available for the information of parents.

Prof. Alfred Schwartz opened the question of specialised entrance examinations for university or college courses of study in engineering, and is of opinion that a satisfactory standard of general education should be attained, and should not include specialised subjects germane to engineering science. This examination should admit to all faculties. The secondary-school training of the engineer should be on as liberal lines as possible, since his subsequent training is largely materialistic. What is wanted in the engineering courses is a supply of students with a wide mental outlook, whose faculties have been well trained and evenly developed, and any specialisation in the secondary schools at the expense of this liberal training is to be deprecated.

Mr. J. T. Jackson considered that entrance examinations should be so framed as to be capable of being passed by students of good general education only. His own experience was that men who commenced engineering at an early age were less successful generally than those who commenced at a later age.

Prof. W. C. Unwin considered that some early specialisation was desirable. Boys of eighteen, without such specialisation, had to look forward to three years of study and three years of practical work before being in a position to take responsible posts. This would bring them to twenty-four years of age before they were capable of earning a living. Not much was asked for, but he thought that a boy of eighteen might be expected to be prepared in part for his future career. He did not agree, as the result of his experience at Coopers Hill and at the Central College, with Mr. Jackson's remarks regarding non-success being due to an early start in engineering. Prof. H. J. Spooner pointed out, in regard to mathematical training, that students were now generally given courses in practical mathematics. The result is that few students now took mathematics as a regular study, necessitating much special cramming for mathematical examinations.

In closing the discussion, Mr. B. Hall Blyth said that he thought it would bear some good fruit if it resulted only in the restoration of Euclid to its old place in the schools. He agreed with the many speakers on the dangers of cramming, and thought that the institution, whose own examination was open to the charge, might take these remarks to heart.

Mr. R. Elliott-Cooper, as chairman of the section on practical training, opened the proceedings in this section by saying that it was of importance that all should be agreed, if possible, as to what practical training really means. Real and useful practical training can be obtained only under the actual or commercial conditions which cannot be found in educational establishments. The knowledge which young engineers may obtain from, say, two years in the workshops of a contractor after his college course is completed, should be supplemented by the experience to be gained in an engineer's office. Such experience would include designing, drawing, specifying, and estimating. In a few branches of the profession pupilage does not occupy its former place, but, taking the profession as a whole, inquiry shows that it still holds an important position.

Mr. Alfred F. Yarrow, in dealing with the apportionment of training between practical work and scientific study, took up the social aspect of the question, and gave some suggestions applying to apprentices in or on works far distant from their homes. He was of opinion that a sandwich system of six months in the works and six months at college was desirable. The student living at home during his college term would be under good influences and affectionate surroundings such as were impossible during a lengthy apprenticeship away from home. London colleges especially should so arrange their courses as to render this system available for their students. Further, some member of the staff in the works should be looked upon as an adviser to the apprentices. He should be accessible at all times, and should make a point of interviewing each lad at least once in three months and ascertain if he could be of service to him. He should also keep in touch with the work of lads attending evening classes by personal visits to the local technical schools. This system is of advantage both to apprentices and employers, and would enable the latter to select wisely those apprentices whom it is desirable to retain after the termination of their apprenticeship. It is a notorious fact that employers have often lost the services of many capable men through being ignorant of the talent that has passed through their works.

Mr. William H. Allen, dealing with the case of a student determining to go both to college and to works, was of the opinion that the college course should be taken first. In his experience this is the order of procedure in which will be found the best chance of success. As regards how much study should be undertaken by a pupil during the period of his practical training, Mr. Allen thought that, if a young man does his duty conscientiously in the works from 6 a.m. to 6 p.m., he will find that as much as his health can stand, without burdening it by further serious study at night. A period of training in the workshops extending over three years is desirable, and should not be specialised too greatly.

Mr. F. E. Robertson referred to the deficiencies in the knowledge of elementary science in young men trained in locomotive works who present themselves to him for examination for posts in India. Prof. Arnold said that his experience had been quite different, and cited the case of the Midland Railway, who handle the training of their apprentices in an excellent manner. Prof. Arnold regretted the modern drifting apart of the engineering side and the metallurgical side in the training of engineers. Mr. E. R. Dolby thought that too much was being attempted in the training of an engineer, and that better results would be obtained by subdivision, as is done in the architectural profession.

Mr. E. Benedict thought that people who accepted premiums should also accept responsibility, and advocated the deputing of someone to look after apprentices.

Prof. E. G. Coker regretted the loss of engineering establishments in London, and favoured a six months' sandwich system. Prof. W. E. Dalby found that teaching was much easier to workshop-trained youths than to others. The sandwich system is best if it can be worked; he had asked works to take pupils on this system, but had not always met with success. The premium question presented a difficulty.

Mr. W. B. Worthington, dealing with the question of training in the engineer's office, said that engineering as an art and profession is based upon the matter of design. However good a man's training and experience on constructional works or in the shops may be, it will not, without experience in the engineer's office, make him a civil engineer, although it may make him a good mechanic or contractor.

Dr. W. C. Unwin presided over a joint meeting of the sections on scientific and practical training, and in opening the proceedings gave a carefully considered statement of the relations between the employers and college-trained youths. This is printed in full elsewhere in the present issue.

Dealing with the relation of engineering employers and colleges from the point of view of the practical training of college students, Prof. J. E. Petavel thought that the employer can cooperate in the educational work of the university by a frank and friendly criticism of the methods adopted, and by offering to take college graduates on a six months' trial.

Mr. James W. Horne directed attention to the cases of engineering firms who have developed a keen interest in the better education of those apprentices who start at sixteen years of age. Every encouragement is given to attend evening classes, and several give facilities for attending college on one or two afternoons a week. By these means many have reached a standard of education which enables them to proceed to a college for two or three years' courses. Such apprentices are a valuable asset to the nation.

Mr. Alexander Siemens did not consider it possible to allow apprentices off on one or two afternoons a week. Mr. Hall Blyth referred to the mistaken idea which some engineering professors have that their students are ready, on leaving college, immediately to take a responsible place in the works. Sir Wm. White directed attention to the many cases of swelled-head in college graduates, a matter which interferes with their employment in a good many instances. Reference was also made in the discussion to the plan followed at Bristol, whereby unsuitable students are advised, through their parents, to withdraw from college at the earliest possible moment. (It may be said here that other colleges also follow this procedure.)

Prof. Archibald Barr advocated a six months' sandwich system, and pleaded that engineering institutions should allow a wide latitude in the systems of training that they will recognise. Prof. Henry Louis did not think that the six months' sandwich system is satisfactory, and suggested the course followed by mining students of three years' college followed by three years' practical work.

Mr. John A. Brodie had little sympathy with those who think it necessary that young engineers should work a reduced number of hours in the workshop in order to enable a greater time being given to study. If the youths of our day cannot or will not stand the strain of the severe training which has given good results in the past, they will probably have to take a position behind those who are prepared to do so. There are other lines of life in which young men can acquire both money and position more easily and quickly than in the engineering profession.

Prof. Stephen M. Dixon opened the subject of the value of a university degree in engineering science in relation to professional competence. There is a feeling in some quarters that the university graduate is rather in the way in an engineer's office. Matters, however, in this respect are improving. When engineering firms recognise the advantage of having assistants thoroughly trained in the principles underlying practice, and whose training also specially fits them for adopting new ideas rapidly, they will be only too glad to cooperate with the universities in completing the education of the engineer.

In dealing with the same subject, Prof. Charles F. Jenkin said that an engineering degree may be looked at in three lights: as a guarantee that the holder has had the best theoretical training and has profited to some extent thereby; as the final step in that type of liberal education at the universities of which England is justly proud; as a broad basis on which State recognition of the engineering profession may be founded.

Prof. Fleming attacked vigorously the whole system of degree examinations as being wrong and as liable to pass candidates whose knowledge was of a very scrappy nature, a view which was promptly controverted by another speaker, who said that he had five degree men in his London office and was entirely satisfied with the results. The same speaker had a good word to say for men who had been trained in evening classes; he was of opinion that swelled-head accounted for some failures in graduates, but that these men were of use after they had been got in hand.

Profs. Dalby, Hopkinson, and Goodman dealt with the subject of the position and uses of engineering laboratories in relation to education at college. All agree that small units are of more service in the work of education than the very large pieces of apparatus—steam engines of large power and the like—which used to find favour.

In closing the conference, Mr. Alexander Siemens suggested that schoolmasters should try to agree on a common syllabus for leaving examinations. University teaching should be scientific and wide in its scope; the training of the mind is all-essential. He was of opinion that practical training should begin by a year in the workshops to be followed by the college course, and then back to the workshops again for completion of the practical training.

TOTAL SOLAR ECLIPSE, TASMANIA, MAY, 1910.[1]

IN a short introduction the author and leader of the expedition explains his motive in publishing in full this account of a solar eclipse expedition which was, unfortunately, unsuccessful in its main object. The site chosen for the camp was in very wild, mountainous country on the southern shores of Tasmania, and in these circumstances it is evident that, going as a private party, exceptional provision had to be made for the many details of equipment, transportation, and maintenance of the observers during the period of preparation for the observations. It was with the hope of giving useful information on these questions that the author decided to present the log of his journey at length.

In chapter i. a very interesting account is given of the general preparations for the eclipse, the prospecting journeys for selection of site, that finally occupied being on Hixson Point, Port Davey. After this was settled, some time was occupied in arranging for and purchasing tents, camp equipment, food and drink, and other details necessary for nine persons during a stay of one month. At the camp, nothing was available for food except fish, wallaby, wombats, and kangaroos. A complete list is given of the details of the equipment and stores; in the case of foodstuffs, &c., both the quantities taken out and the amount unused are given, from which future pioneers in this class of work may learn wisdom; for instance, lime juice was evidently not the beverage most sought after, as out of eighteen bottles taken, thirteen are recorded as unused. A very useful item is the actual cost of the expedition, reckoned from Hobart out to Port Davey, the stay there, and back to Melbourne—347*l*.

As a more or less detailed account of the instruments taken out for the work on the eclipse has already appeared in NATURE, it is only necessary here to say that, in spite of most trying and tempestuous weather, the whole apparatus was adjusted ready for the eclipse time. Details of the work involved for each section are given, with very clear photographic illustrations of the progress and methods adopted in transporting the heavy cases over the difficult ground from the shore to the camp site. Provision had been made for obtaining photographs of the corona with telescopes of various apertures and focal lengths, and for the spectrum of the chromosphere and corona with a powerful concave grating spectrograph.

Included is a report on the observations made by J. Brooks for the determination of the astronomical position of the site, and a description of the corona photographs obtained by another party at Queenstown. After examination of these plates, on which the diameter of the moon's image is 4·4 mm., Mr. W. H. Wesley reports that the extensions of the corona were very small, in no part reaching beyond one quarter the moon's diameter from the limb. On a plate submitted by another observer, with a smaller image still, 1·5 mm., the extensions reach about one diameter. The most striking feature was a wide rift, fairly symmetrical with the South Pole, extending for nearly 50° along the limb. There was also a long ray of synclinal character on the east of this southern rift.

The form of the corona appears to be of the type associated with the period intermediate between the maximum and minimum of solar activity.

The volume is illustrated by thirty-five excellent photographic reproductions showing the interior of Port Davey Harbour, incidents in the transportation and erection of the various instruments, a scrub fire which very nearly destroyed the camp, and the photographs of the corona obtained at Queenstown by the Rev. L. S. Macdougall and Mr. J. Booton.

CHARLES P. BUTLER.

[1] Report of the Solar Eclipse Expedition to Port Davey, Tasmania, May, 1910. By F. K. McClean and others. Pp. 42+35 plates. (London: Printed by R. Clay and Sons, Ltd. Plates reproduced by A. E. Dent and Co., Ltd., 1910.)

THE CAMBRIDGE ANTHROPOLOGICAL EXPEDITION TO WESTERN AUSTRALIA.

IN the early part of last year plans were made for an anthropological expedition to Western Australia, and Mr. A. R. Brown, Fellow of Trinity College, Cambridge, who had been re-elected to the Anthony Wilkin studentship, was appointed leader, the main object of the expedition being to study the social organisation and magico-religious beliefs and observances of the natives. Mr. Brown left England at the end of July, 1910. Soon after his arrival in Perth, mainly through the instrumentality of Mrs. D. M. Bates, a donation of 1000*l*. was made to the expedition by Mr. Samuel P. Mackay of that State. This munificent gift is a notable example of public spirit, and it proves that, despite the manifold claims of a very new State on its citizens, there are individuals who have the advancement of pure science at heart. The benefaction was particularly acceptable, as Mr. Brown's modest resources consisted only of the studentship, a gift of 200*l.* from Sir John Murray, and a grant from the Royal Society of 100*l.* Field investigation is very expensive in Australia, partly owing to the great distances that have to be traversed in order to come into contact with the natives, who even then may be in very small bands.

On his first excursion Mr. Brown was accompanied by Mr. E. L. Grant Watson, of Trinity College, Cambridge, who assisted in taking photographs and measurements of natives, and by Mrs. Bates, who for some years has been employed by the Government of Western Australia to collect information concerning the aborigines of the State. Mrs. Bates has a very considerable knowledge of the natives, and her valuable MS. notes have been placed at Mr. Brown's disposal.

In the south-west corner of the State the natives are extinct; the greater portion of the western half of the State is unopened, and mainly desert country; the natives are quite wild, and it is at present almost impossible to get into touch with them. There remains a broad band of country to the east and the Kimberley gold-field district which have been opened up for pastoral purposes or for gold mining. The gold-field blacks are for the most part beggars, and suspicious and treacherous; they are constantly moving from place to place.

In the eastern part of the region, between the Fortescue and Gascoyne Rivers, there are eleven tribes which belong to one type of social organisation. They never practise circumcision or subincision; they have the usual four-class system with the kinship system commonly associated therewith, but cross-cousin marriage is prohibited, and there are specific kinship terms to distinguish own mother's brother's children from tribal mother's brother's children. The members of a clan may eat their totem, which is inherited in the male line, and ceremonies of the *Intichiuma* type are performed for the increase of the totem. Each tribe is divided into definitely circumscribed local groups, the descent of the local group being in the male line. A local group consists mainly, and perhaps in some cases entirely, of persons of the same totem. In most cases the spot at which the ceremonies for the increase of a totem are performed is within the territory of the local group of the men of that totem, but in a few cases a local group contains no totem centre, and the men of the group must journey to some neighbouring group to perform these ceremonies. The totemic groups are united into larger social divisions, for which Mr. Brown has not yet found a suitable name. During the last two or three generations many irregular marriages have taken place, which have resulted in the distribution of totems through the four classes. In such cases the children take the totem of the father, but enter the class to which they would have belonged if the mother had taken her proper husband. To the south the tribes are, on the whole, very similar to the above; to the east, north-east, and south-east circumcision and subincision are not usually practised; they have the four-class system, and in some tribes every person has several totems (as many as twelve or more), which are inherited in the male line; these may be eaten, and *Intichiuma* ceremonies are performed. Inland between Fortescue and De Gray Rivers, cross-cousin marriage is not permitted, and no distinction is made between own mother's brother's children (and own father's sister's children) and tribal mother's brother's children.

Scattered sporadically all over the area investigated are found the beliefs that children are the result of food eaten, or that they may be projected by magic into a woman. In all cases these beliefs exist side by side with ordinary totemism and entirely independent of it, and also with a perfectly clear recognition of the normal method of procreation.

Mr. Brown is at present on a six months' visit to uninvestigated tribes, mainly in the north-east of the State.

A. C. HADDON.

EXPLORATIONS IN DUTCH NEW GUINEA.

ON Monday, July 3, Captain C. G. Rawling lectured before the Royal Geographical Society on the geographical results of the British expedition in Dutch New Guinea, which was organised by the British Ornithologists' Union, and was led by Mr. Goodfellow until illness compelled his return. The dense tropical jungle of the low plain between the mountains and the coast, the heavy rainfall, and the sickness which incapacitated their carriers, prevented the travellers from reaching the higher portions of the range, but the scientific results, zoological, ethnographical, and geographical, are most valuable. Captain Rawling and Dr. Marshall stayed for some time with the pygmy tribes of the lower hill ranges, and obtained much information concerning their customs, habits, and general character. Subjoined are some extracts from Captain Rawling's paper.

From inquiries made before leaving England, it was decided that the Oetakwa, the mouth of which river was known to lie due south of Carstenz peak, the greatest of the snow-peaks, should be utilised as our line of communication. Prior to our advent but two rivers on the whole of the south-eastern coast of New Guinea had been visited, the Oetakwa and the Mimika, and these had only been explored for a few miles from their mouth. Further inquiries made in Batavia induced Mr. Goodfellow to change his objective from the Oetakwa to Mimika, and this, little as we suspected it at the time, sealed our fate as to all possibility of ever reaching the Snows.

Almost coal-black in colour, and rather exceeding the average European in height, the Mimika coast native, with his splendidly developed muscles, is physically an almost perfect man. But the brutal features of his face, accentuated by the closely cropped head, makes him anything but an attractive creature. The mass of fuzzy curly hair, in which the natives of other districts take so much pride, is here cut off by means of sharpened shells, split bamboo, or an old piece of hoop iron. What remains is closely plaited in ridges. With the loss of hair nearly all love of decoration or ornament seems to have vanished, their dress consisting of a large white shell worn on the stomach, a hollow carved bamboo, or a narrow strip of tree bark beaten soft and pliable. Round the neck a few beads may be strung, while below the knees and around the biceps a narrow band of plaited grass may often be seen. As a further attempt at ornament, a few white feathers of the hornbill may be stuck into the hair, or if a fierce expression is desired, the split beak of the same bird is pushed through a hole in the septum nasi. The women are even less given to trinkets, for, besides the narrow strip of bark hanging down in front and behind, they are as bare as nature made them; poor creatures, they have little time to think of anything but work. Widows are rather favoured in this respect, for their weeds consist of a great poke bonnet, in addition to a bodice and skirt, all of grass. The instinct of self-adornment is, however, very strong, for trade articles, such as beads and cloth, were subsequently not only eagerly sought for but worn on all important occasions. Absolute nakedness was rare amongst the Wakatimi people, for girls wore some form of dress from an early age, and boys took on the garb of manhood at the age of fifteen or sixteen.

The front teeth of the men, but not the women, are in many instances sharpened to a point, a painful process, for the operation is carried out, not by filing, but by chipping the sides away with a piece of iron, or, if this is not available, a hard shell used after the manner of a chisel. The custom of sharpening the teeth is often put down as a sign of cannibalistic practices, but we have no reason to believe that the habit of eating human flesh is ever here put into practice. Certainly during our sojourn amongst these tribes we saw no signs of cannibalism, nor when examining the human bones preserved in every house, did we find any evidence pointing to such a custom. When questioned on this subject, some natives showed abhorrence, whilst others exhibited, at any rate, no great disgust at the suggestion.

Wakatimi, as we afterwards found, was but a sample of other coast villages, consisting of a long row of huts made of pandanas and palm leaves held up by poles cut from the forest. Each new-comer added on his hut to the last, at the same time removing the partition, by this means turning the village into one endless habitation, broken by their respective doorways. The floors were of sand brought from the seashore, and, with the exception of the crudest of fireplaces, other furniture there was none. Skulls and the bones of departed relatives dangled in grass bags from the roof, blackened by the smoke of the fires. Now and again a wood pillow might be seen, otherwise the interior was bare. Outside there stood, ready for instant use, a stone club, a few spears—the heads fashioned from the leg-bones of pig or human beings—or a bow and a sheaf of arrows, to which weapons they flew on the slightest provocation.

The natives of this portion of New Guinea are divided into three classes or tribes. First come the people inhabiting the low-lying ground near the coast and extending inland for about twelve miles, and known to us as the coast natives. Then comes a strip of land practically uninhabited, and above this again, on a level with the headwaters of the Mimika, a race known to us as the up-river natives. Still further north, and inhabiting the foothills, are the pygmies. These three tribes are entirely distinct from one another, having no communication in the north and south direction—even though living on the same river—but passing freely to the east and west. The dividing line is hard and fast, and is not crossed except for occasional purposes of trade.

With the latitudes of Atabo at the coast and Parimau known, and the azimuth obtained, it was easy to fix all the points in the range from Carstenz in the east to Mount Darwin in the west. The height of Carstenz, formerly assumed to be about 18,000 feet, was found to have an altitude of rather more than 16,000 feet, while to the west three more great snow-peaks were discovered, with a height of about 15,500 feet, and these we named Mount Idenburg. Beyond and between these two mountains two other great snow-peaks were visible, evidence that the ground to the north does not fall abruptly away. But more interesting than all was the discovery that the great range, stretching from Carstenz in the east to the Charles Louis mountains in the west, a distance of eighty miles, formed one immense unbroken precipice, culminating in its greater sheer height at Mount Darwin. We were never in a position to measure with the theodolite a greater sheer height of more than 6500 feet, but from many views obtained while climbing, I have no hesitation in stating that the greatest perpendicular height is, at this spot, not less than 10,500 feet, or two miles.

The snow-line is at about 14,500 feet, the glaciers on Carstenz descending lower and falling over the precipice to the south. From a letter received from Lieut. Postema, the naval officer in charge of the survey of the Dutch Expedition on the Oetakwa river, I understand that he is of like opinion that Carstenz mountain is not climbable. The extreme wetness of this district is, without doubt, due to the great altitude and proximity of these mountains, the rainfall being in excess of any other portion of New Guinea.

To sum up the final results of the expedition: large and valuable collections of birds, mammals, reptiles, butterflies, and moths had been formed, together with botanical and ethnographical specimens; a new and unknown race of pygmies discovered, studied, measured, and photographed; a range of mountains, containing the greatest precipice in the world, together with 3000 square miles of country, surveyed and mapped, new snow-mountains found, and many great rivers explored; and a long stretch of coast-line surveyed. We had accomplished the longest cross-country journey ever undertaken in Dutch New Guinea, *i.e.* eleven marches from the up-river camp, had proved the impossibility of the Mimika river as a line of advance to the Snows, and, on the other hand, the value of the great rivers to the east if the same goal is intended. From experience, and our heavy death-roll will bear me out, I have no hesitation in saying, first, that the land is an impossible one to any but a Papuan; and secondly, that, unless most carefully picked, no natives of the East Indies, with the exception of Dyaks from Borneo, are of the slightest value as carriers in South Dutch New Guinea.

WORKSHOP AND COLLEGE.[1]

IT is now agreed that engineering training should include scientific instruction and practical and commercial experience. But very divergent views are held as to the relative importance of the different components of such a training and as to the order and duration of each. We must recognise at once that the field of engineering employment is a very wide one, and that different capacities are required in different parts of that field. It is therefore in no way surprising that persons, whose opportunities of observation give them every right to express a definite opinion as to what is best for one special branch of engineering, have arrived at conclusions very different from those held by others, whose field of work has been in a different branch. No system of engineering training can be arranged to meet all special demands, and it is the object of such a conference as this to find out what is most essential in all courses of training and what modifications are practically possible to meet different cases.

First of all, I think it must be assumed that the engineering education we are specially charged to consider is that of young men whose aim it is to arrive ultimately at a professional status, such a status as membership of this institution implies. Their hope is to be employed ultimately in the design and control and direction of engineering work. Of course, some of them may fail in capacity or may lack opportunity, and may drift to one of the many more commercial occupations allied to engineering; and even there the knowledge they have acquired will be a valuable asset. But at the outset a professional career is aimed at, and the system of engineering education must be arranged to meet that condition. It is probably only by educating many that the few can be found who have the capacity and character necessary to achieve considerable advances and to do work of national importance.

Now, very few young men at eighteen can foresee into what line of employment they may be driven, and consequently the first or undergraduate stage of education must be broad, so as to fit students for widely different spheres of work.

Perhaps the greatest defect of engineering education at present is the want of more provision for the higher and more specialised education of the few students of real capacity discovered in the sifting process of an undergraduate course.

In spite of the general acceptance of the view that a study of scientific principles and their application is a necessary preliminary to a practical course in field or workshop, a certain jealousy of college education is still obvious in some practical engineers. While conceding in words that some scientific education is necessary to an engineer, they would, in fact, confine it to very elementary matters. They would greatly restrict the time given to it, and they are disposed to depreciate the value of any higher teaching, and even to regard it as mischievous and likely to unfit a man for the strenuous life of the manufacturing workshop.

I believe the idea that a college course unfits a man for practical work is a wholly mistaken one. There may be students who prove unfit for practical work in spite of college education. They would equally fail if their education was purely practical. There may be college courses, I am afraid it must be said there have been college courses, badly arranged, and teachers in colleges less competent than is desirable. But such things are inevitable. Nothing is more certain than that there has been a great improvement of college teaching of applied science in the last twenty years.

An employer who takes into his works college students is, I think, often disposed to expect from them an immediate availability which is unreasonable. It is not the main object of a college course to make a student acquainted with the details of any particular business; that is the proper object of the first year or two of practical work. It is not the main object of a college course to fit students specially for such work as will fall to them

[1] Opening remarks at a Conference on Education and Training of Engineers (joint meeting of Sections II. and III., Scientific and Practical Training), held at the Institution of Civil Engineers, by Dr. W. C. Unwin, F.R.S., chairman of Section II.

while in the lowest rank of the profession. The college course must contemplate the fitting of a student for his whole career, and provide him with an intellectual equipment which will only gradually become useful as he rises to higher rank in his profession. The view of the employer who looks only to the immediate usefulness of the student is a short-sighted one.

Nevertheless, a college course is an unpractical and badly designed one if, at the end of it, a student is not more capable and useful from the first, in any type of workshop, than a lad without such a training. The college discipline is bad if he has not much more character and energy than the raw lad. His training has been a failure if he does not pick up the specialised details of any business to which he may be put far more rapidly than an untrained lad. I should like to suggest to those practical engineers who are implicitly, if not openly, hostile to college training, who would, at any rate, greatly restrict it, and who advocate lengthened periods of workshop apprenticeship, that they exaggerate the value of such workshop experience as an ordinary apprentice gets. I am not, of course, considering artisan apprentices, but young men expecting to rise to positions of trust. Here and there are works where special trouble is taken with apprentices; but in general apprentices are left to pick up what knowledge they can with very little help, and in many cases, I think, a good deal of their time is absolutely wasted. Some skill of handicraft is no doubt acquired. But the engineer works with his head, not with his hands, and manual skill is of use to him only in very exceptional cases.

I hope I do not in any way underrate the value, to mechanical engineers especially, of that kind of knowledge of materials, of tools, of processes, and of cost which can only be learned in the workshop. But I think practical engineers forget how little of this valuable knowledge really comes to the works apprentice. The engineer of higher rank who discusses matters with foremen and draughtsmen, who has responsibility for design and cost, and is, moreover, in a position to know the reason of all decisions, is learning in the workshop all his life, and naturally sets a high value on the knowledge slowly acquired by years of constant and close observation. So high a value that perhaps he ignores the importance of the scientific knowledge which was, somewhere and by someone, applied in bringing his business to a state in which it can be carried on successfully as a mere manufacture. But the knowledge which comes to those in responsible positions is not open to the ordinary apprentice, and he learns slowly, if at all, unless he brings to the works such a knowledge of principles and methods that he can interpret for himself what he sees. No system of workshop apprenticeship can, I think, be considered satisfactory unless someone is specially charged with care of the apprentices, whose duty it should be to make sure that they have opportunity of seeing a great variety of work and of helping them over their difficulties.

I believe employers will find—some of them have found already—that they owe a debt to the colleges, and that the college-trained student will prove, with a minimum of special experience, a valuable assistant, and in some cases the originator of a real advance in practice. I should like to plead that in return the employer might be a little more ready to give college students a year or two years' run of the works, either without remuneration or with a small remuneration just enough for disciplinary purposes. I do not think there would be any loss in the case of a properly trained student, and the employer would in many cases find an assistant worth keeping and promoting.

GOLD MINING IN THE TRANSVAAL.[1]

THE discovery of gold on the Witwatersrand was made in the year 1885. The growth of the field was at first slow. Some of the earliest workers believed that the auriferous gravel, exposed in shallow open workings, was a superficial deposit of the nature of the alluvial "placers" of California and Australia. The true character of the conglomerate beds was, however, soon realised by those who were fortunate enough to possess some geological knowledge, and by 1887 stamp-mills were in operation, the output from the Witwatersrand mines for that year being 81,045*l*. From 1887 onward the progress has been rapid.

[1] Abridged from the nineteenth "James Forrest" lecture delivered on June 28 before the Institution of Civil Engineers by Dr. F. H. Hatch, vice-president of the Institution of Mining and Metallurgy.

Down to the permanent water-level, at a vertical depth varying from 200 to 300 feet, the conglomerate beds were "free-milling," that is to say, the iron pyrites, with which the gold is intimately associated, had been destroyed by oxidation, thus setting free the gold. Below the water-table the colour of the rock changes from red to blue: the ore becomes pyritic; and the gold is no longer so amenable to recovery by amalgamation, as is the case with the oxidised ore. This was the first difficulty that had to be overcome. Up to the year 1890 the treatment of the Rand ore had consisted of crushing in stamp-mills, and the recovery of 50 to 60 per cent. of the gold, by amalgamation on mercury-coated copper plates. The tailings received no further treatment; they were considered to be valueless, and, where the ground permitted it, were allowed to flow away.

The successful introduction of the cyanide process in 1890 inaugurated a new era in the history of Rand gold mining. It is no exaggeration to say that the great success of the Witwatersrand gold industry is a direct result of the introduction of the cyanide process. For the majority of the mines, the gold won by this process represents the difference between profit and loss, and without it the profitable working of the vast quantity of low-grade banket now being mined on the Rand would be impossible.

At first the pulp from the stamp-mills was run into retaining dams, from which the sand was afterwards dug out and conveyed in Scotch carts or in mine-trucks for treatment in the cyanide vats. On account of the slime-content, only 30 per cent. of the gold left in the pulp from the amalgamating tables was recovered, the remainder being in the untreated slimes and in the residues.

The next step was the introduction of hydraulic classifiers, by means of which a considerable proportion of the slime was eliminated and a sand product obtained, which could be run direct into leaching tanks.

A process was then evolved for the treatment of the slimes. It consisted in causing the slime, overflowing from the sand-collectors, to settle, by the addition of lime, the bulk of the water being subsequently removed by decantation. The concentrated slime so obtained was then agitated with cyanide solution, which was ultimately drawn off by decantation.

The separation of sand from slime by the old-fashioned inverted pyramidal form of hydraulic classifier, and the decantation method for the removal of water or cyanide solution from sand or slime, are now giving place to the use of diaphragm cones and vacuum filters. In the most modern plants the separation of sand and slime in a mill product is effected by feeding the mill-pulp into a cone-shaped collector or diaphragm cone; the sand is drawn off as a thickened pulp from the bottom, while the slime flows over at the periphery, and after passing through a secondary washing cone is freed from most of its remain ing water on a Caldecott filter-table, which is a slowly rotating horizontal vacuum filter.

The treatment of slime has been much facilitated by the recent introduction of air-agitation tanks and vacuum-filters, which enable the enriched cyanide solution to be rapidly drawn off from the slime-residue and sent as a clear liquid to the extractor boxes. The precipitation of the gold was effected in the original MacArthur Forrest process by zinc shavings, and this method is still preferred for the rich solutions; but for weak solutions, such as are obtained in the treatment of slimes, zinc dust is employed as a precipitant.

One important result of the perfection of the slimes-treatment process has been the introduction of fine grinding in tube-mills, with consequent increased extraction and shortened treatment period. Further, the adoption of tube-mills has modified the function of the stamp-mill. Stamps are no longer employed for fine crushing, and amalgamation in the mortar-boxes has been completely abolished; and even such plate-amalgamation as took place in front of the stamp-mills has in many cases been done away with. Concurrently with the limitation of the effective range of the stamp-mill, the weight of individual stamps has been increased by lengthening the heads, until, with the 2000-lb. stamp of the new mill of the City Deep mine, the economic limit of the cam-lifted gravitation stamp appears to have been reached.

Underground, efforts have been made to solve the dust problem. With few exceptions, the Witwatersrand mines are dry mines, and the processes of machine-drilling, blasting, and shovelling consequently create and distribute through the air great quantities of fine dust. The inhalation of the dust-laden air causes a peculiar disease, known as miners' phthisis, a deadly complaint which is responsible for a high mortality among the white miners. By the proper application of water at the point of origin, the formation of dust can, to a large extent, be prevented; and several ingenious contrivances have been invented for catching the dust from the upward holes, into which water cannot be poured from a can, in the manner usually adopted with downward holes. The chief difficulty, however, appears to be to get the men to use the dust-arresters and to water-down the stopes and other working places after blasting. By better supervision and a stricter enforcement of regulations, such difficulties will doubtless be overcome.

In the early days of the Rand, and, indeed, up to quite recently, it has not been found necessary to employ any artificial system of ventilation, the numerous shafts and outlets to the surface of the outcrop mines having sufficed to maintain an ample supply of fresh air. But with deeper levels, fewer communications with the surface, and an increased rock-temperature, artificial ventilation is destined to play an ever more and more important part in the future. It will be impossible to work the deep levels economically without carefully thought-out schemes of ventilation, and for the success of these it will be necessary to have shafts with small frictional resistance and large air space, and to carry the air-current through special ventilating roads.

The ventilation problem has been seriously attacked on the Rand, and already ventilating fans, varying in capacity from 50,000 cubic feet per minute at 1 inch water gauge to 250,000 cubic feet at 4 inches water gauge have been installed at many of the mines. In splitting the air current, the numerous dykes of igneous rock that traverse the Witwatersrand mines in a north and south direction (*i.e.* across the strike) can be made to serve as natural brattices, since they cut up the mines into air-tight compartments. The levels which penetrate these dykes must be permanently closed, or, if used for tramming purposes, closed by double swinging-doors.

Under the changed conditions now prevailing on the Rand, due to the enormously increased size of the properties brought about by recent amalgamations, and the consequent possibility of concentrating a large output on fewer main hoisting shafts than heretofore, the evolution of an entirely new system of underground transport is being accomplished. It is becoming recognised that the rock, broken in the stopes, can only be economically dealt with by handling it on a few main haulage-levels, situated at great intervals apart and driven straight, from point to point, in the footwall of the reef. These main haulage-roads, which are intended to serve also as the intake of the fresh-air current, can, on account of their economic importance, be constructed of large dimensions. They can also be carefully graded and equipped with heavy rails.

The handling of the broken rock in the stopes is, from an economic point of view, scarcely less important than its haulage on the levels. Everything depends on the angle of dip of the reef. In many of the outcrop mines of the Central Rand the high dip of the reef permitted the rock, broken in the stopes, to find its way by gravitation to the tramming-level, where it was drawn off as required from the stope-boxes; but with the dips of from 25° to 30° obtaining in most of the deep-level mines, the broken rock requires to be assisted down the stope-floors by shovelling. Only in the extreme East Rand, where the reef lies very flat (dipping at from 8° to 10°) is it possible to fill the trucks at the stope-faces and to run them thence direct to the tramming-levels. Hand-shovelling is uneconomical, and, moreover, is detrimental to health, on account of the dust it raises. Consequently, several attempts have been made to substitute for it some conveyer system of handling the broken rock. Stope-conveyers are shaking chutes consisting of iron plates. They are suspended by short chains from the roof next to the working face of the stope, and are kept in motion by ropes attached to the upper ends of the conveyers. Many of these shaking chutes are now in use on the Rand.

In the past, the use of machine-drills for stoping has not been looked upon with much favour. For this there are several reasons: first, the machine used was the heavy drill employed for development work, and with this type it was impossible to work in narrow stopes without breaking a large amount of waste; secondly, the bands of barren quartzite, with which the payable conglomerate is often interstratified, suffered such pulverisation by reason of the large blasting charges used that often it could not afterwards be eliminated by sorting; and, thirdly, the large blasting charges were found to weaken the roof, so that a greater number of pillars had to be left for its support than was the case with hand-drilling. Only in wide stopes, on a large homogeneous reef with good walls, could these drills be used to economic advantage. The necessity for a good stoping drill for narrow reefs, however, has become more and more pressing with the extension of the mining industry, with which the supply of native labour for hand-drilling has not kept pace.

By a series of competitive trials carried out under Government supervision, it has been established that machine-drilling in moderately narrow stopes costs no more, and perhaps even less, than hand-drilling by natives. Hundreds of small drills (drilling a hole to take a ⅞-inch explosive) are already employed for stoping on the Rand, and their average duty is three-quarters of a fathom per shift. To stope very narrow and low-grade reefs hand-labour has still to be used; but it is hoped that a drill capable of doing even this class of work will eventually be evolved.

Another important problem which has recently forced itself on the notice of those responsible for the mining operations on the Rand is the support of the hanging wall. The removal of the gold-bearing conglomerate bed, which, except for its somewhat steeper dip, may be compared to a coal-seam, leaves an open space, which is not allowed to fall in, as in a coal mine worked on the long-wall retreating system, but is supported over enormous areas by pillars of unworked conglomerate in the stopes, by ribs left above and below drives, and by pillars left to ensure the safety of the shafts, supplemented in some cases by the stowing of waste rock. These methods have sufficed in the past to keep open the stopes and drives and to protect the shafts; but owing to the robbing of the stope pillars in the outcrop mines, and more especially to the increased pressure of the superincumbent rock mass in the deep levels, serious movements of the hanging wall have lately been making themselves felt, crushing the pillars in the stopes, destroying the ribs above and below the drives, and in some cases even affecting the shaft pillars.

To arrest this untoward movement, which at one time threatened the loss of the main thoroughfares of some of the mines, a system of sand-filling has been adopted. By this system the abandoned stopes and other working places in the mines are filled with sand taken from the residue dumps. The sand is mixed with sufficient water to cause it to flow down pipes in the shafts and to be discharged in the stopes prepared for its reception. Underground, the pulp is conducted by wooden launders to the stope to be filled. Barricades are used to keep the sand in place; but it drains well, and soon packs solid enough to bear the weight of a man. The effluent water is pumped back to the surface. At first it was feared that the cyanide remaining in the sand would be dangerous to the mine-workers; but a little research has eliminated this source of danger. The effluent water from the sand-packs underground shows no trace of cyanide, and no hydrocyanic acid has been found in the air of the stopes which are being filled. The filling of the worked-out stopes will also assist ventilation, since it will prevent the dissipation of the fresh-air current. The system is already in use at many mines, and there is little doubt that it will be universally adopted.

One of the most remarkable economic changes on the Rand is now being brought about by the concentration of the steaming plant at two or three centres, from which power is distributed to the mines by electric transmission or in the form of compressed air. This has largely been the work of the Victoria Falls and Transvaal Power Company. Electrically transmitted power is rapidly supplanting independent steam power for mills, winders, sinking engines, underground hoists, pumps, &c., owing to the favourable rate at which it can be purchased from the power company. The price has been fixed by agreement at 0·561 pence per unit until October, 1912, and thereafter at 0·525 pence. Transmission is effected both by overhead lines (at 40,000 volts along the Rand, and at 80,000 volts from Vereeniging, a distance of 30 miles) and by underground cables (at 20,000 volts). The length of the overhead lines is 150 miles, that of the underground cables 35 miles. A portion of the power supplied by the company is in the form of compressed air for rock-drills.

A considerable economy will be effected by this centralisation of power generation, and the consequent reduction in the number of independent steaming plants. From the price per unit at which the Victoria Falls and Transvaal Company are supplying power, the cost of a horse-power per annum, utilised continuously day and night, can be calculated: it works out at 14*l.* It is not so easy to arrive at the average cost of a horse-power year on the mines prior to electrification, but it is stated to have been 28*l.* In any case, the saving due to the substitution of electric motive-power for steam-power is undoubted, and there is, moreover, the indirect advantage of greater flexibility and more perfect control.

The present position of the industry and the progress to be expected in the future may be illustrated by a few statistics.

Since the discovery of the Field in 1886 to the end of 1910 the Rand has milled 155 million tons of ore, and produced gold to the value of 276,000,000*l.*, this being an average of 35·6*s.*, or 8·4 dwt. of fine gold to the ton milled. During the same period dividends amounting to 72,416,550*l.* have been distributed, equivalent to 9·3*s.* per ton milled.

During 1910 gold to the value of close on 31,000,000*l.* was produced by crushing 21,500,000 tons of ore; this is equivalent to an average yield of 28*s.* 6*d.*, or 6·7 dwt. of fine gold per ton milled. The working costs averaged (from the returns of fifty-six companies) was 17*s.* 7*d.* per ton, giving an average profit of 10*s.* 9*d.* per ton milled.

Seven of the largest companies, crushing close on a quarter of the whole tonnage, are working at an average cost of 13·8*s.* This very remarkable result has been brought about by increasing, to their economic limit, the size of the units used in the various operations, such as trucks, stamps, tube-mills, vats, pumps, &c.; by the simplification of the methods of handling ore; and by replacement, so far as it is economy to do so, of hand-labour by mechanical appliances. Larger units of development and the centralisation of power plant have also contributed to this result; while the amalgamation of the properties into larger units has helped to lower working costs, by permitting a reorganisation of the transport and hoisting arrangements, and by reducing the standing charges.

Future of the Goldfield.—Working at a cost of 13·8*s.* per ton means that the cost of development, extraction, and reduction, including administration, is covered by a recovery of 3¼ dwt. of fine gold per ton. On 5 dwt. ore, therefore, this would allow of a profit of nearly 7*s.* 6*d.* per ton; and over a considerable area of the Rand the average grade of the ore-bodies is not much above 5 dwt. The inclusion of large tonnages of relatively poor reef, which formerly were considered outside the range of practical mining, has been made possible by lower operating costs. The grade of the ore crushed has fallen in consequence. This does not necessarily imply that the increased depth of the mines has (*per se*) caused a falling-off in the actual value of the ore-deposit considered as a whole.

The effect of this increased tonnage and diminished grade on the life of the Rand goldfield as a whole is an interesting subject for speculation. From the data available the production of gold to be expected from the Main Reef series, if worked down to a vertical depth of 6000 feet, may be estimated.

It figures out at 1,046,000,000*l.*, which, on the basis of an average output of 30,000,000*l.* per annum, is equivalent to a life of thirty-five years, *i.e.* down to a vertical depth of 6,000 feet. But, if at still greater depths the banket should contain sufficient gold to yield a profit, after deducting the cost of working, we may rest assured that it will be worked. What, then, are the limiting factors? They are generally considered to be (1) the mechanical difficulty of raising the ore to the surface from such great depths, and (2) the effect of the temperature gradient. With regard to the mechanical question, the electrical transmission of power applied to stage-winding has so modified the mining engineer's conception of the depth from which deep hoisting is practicable, that it is now generally assumed that there are no mechanical difficulties that cannot be overcome if it pays to do so. As to the temperature question, figures based on Mr. Marriott's careful experiments, which showed that the rise is only 1° F. for every 208 feet of depth, indicate that the rock-temperature at 7000 feet would not exceed 97·5° F., and with efficient ventilation the air temperature would of course be considerably lower. It follows, therefore, that for all practical purposes the whole question turns solely on the gold content, and what that may be at a vertical depth of 7000 or 8000 feet, no one can tell. This much, however, may be said: the geological structure of the country clearly points to the continuance of the conglomerate or banket beds to still greater depths than even 7000 or 8000 feet, before the bottom of the great synclinal basin of the Witwatersrand is reached; and, beyond that point, the beds must still continue until they rise to form the southern lip of the basin known to exist beyond the Vaal River.

THE FUNDAMENTAL PROPERTIES OF THE ELEMENTS.[1]

THE mystery that enshrouds the ultimate nature of the physical universe has always stimulated the curiosity of thinking man. Of old, philosophers sought to solve the cosmic problem by abstract reasoning, but to-day we agree that the only hope of penetrating into the closely guarded secret lies in the precise estimation of that which is tangible and visible. Knowledge of the actual behaviour of material and of energy provides the only safe basis for logical inference as to the real essence of things. Faraday was deeply imbued with this conviction; and it is widely recognised as the basis of all modern experimental science. The subject of my lecture to-night concerns the methods and general results of several extended series of investigations, planned with the hope of adding a little to the foundations of human knowledge by means of careful experiment.

At the outset let me remind you of an old saying of Plato's, for it sounds the keynote of the lecture:—"If arithmetic, mensuration, and weighing be taken away from any art, that which remains will not be much."[2] In other words, the soundness of all important conclusions of mankind depends on the definiteness of the data on which they are based.

Lord Kelvin said:—"Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than looking for something new. But nearly all the grandest discoveries of science have been the rewards of accurate measurement and patient, long-continued labour in the minute sifting of numerical results."[3] The more subtle and complicated the conclusions to be drawn, the more exactly quantitative must be the knowledge of the facts.

Measurement is a means, not an end. Through measurement we obtain data full of precise significance, about which to reason; but indiscriminate measurement will lead nowhere. We must choose wisely the quantities to be measured, or else our time may be wasted.

Among all quantities worthy of exact measurement, the properties of the chemical elements are surely some of the most fundamental, because the elements are the vehicles of

[1] Abridged from the Faraday lecture delivered before the Chemical Society by Prof. T. W. Richards on June 14.
[2] Plato, "Philebus" (trans. Jowett), 1875, vol. iv., p. 104.
[3] Sir W. Thomson (Lord Kelvin), address to British Association, August, 1871, "Life," ii., 600.

all the manifold phenomena within the range of our perception.

Weight is clearly one of the most significant of these properties. The eighty or more individual numbers which we call the atomic weights are perhaps the most striking of the physical records nature has given us concerning the earliest stages of the evolution of the universe. They are mute witnesses of the first beginnings of the cosmos out of the chaos, and their significance is one of the first concerns of the chemical philosopher.

Mankind is not yet in a position to predict any single atomic weight with exactness. Therefore the exact determination of atomic weights rests upon precise laboratory work; and in order to arrive at the real values of these fundamental constants, chemical methods must be improved and revised so as to free them from systematic or accidental errors.

What, now, are the most important precautions to be taken in such work? These are worthy of brief notice, because the value of the results inevitably depends upon them. Obvious although they may be, they are often disregarded.

In the first place, each portion of substance to be weighed must be free from the suspicion of containing unheeded impurities; otherwise its weight will mean little. This is an end not easily attained, for liquids often attack their containing vessels and absorb gases, crystals include and occlude solvents, precipitates carry down polluting impurities, dried substances cling to water, and solids, even at high temperatures, often fail to discharge their imprisoned contaminations.

In the next place, after an analysis has once begun, stance into the balance case.[1] Every substance must be collected and find its way in due course to the scale-pan. The trouble here lies in the difficulty in estimating, or even detecting, minute traces of substances remaining in solution, or minute losses by vaporisation at high temperatures.

In brief, "the whole truth and nothing but the truth" is the aim. The chemical side of the question is far more intricate and uncertain than the physical operation of weighing. For this reason it is neither necessary nor advisable to use extraordinarily large amounts of material; from 5 to 20 grams in each experiment is usually enough. The exclamation, "What wonderfully fine scales you must have to weigh atoms," simply indicates ignorance; the real difficulties precede the introduction of the substance into the balance case.[1] Every substance must be assumed to be impure, every reaction must be assumed to be incomplete, every measurement must be assumed to contain error, until proof to the contrary can be obtained. Only by means of the utmost care, applied with ever-watchful judgment, may the unexpected snares which always lurk in complicated processes be detected and rendered powerless for evil.

That the atomic weights may be connected by precise mathematical equations seems highly probable; but although many interesting attempts have been made to solve the problem,[2] the exact nature of such relationships has not yet been discovered. No attempt which takes liberties with the more certain of the observed values is worthy of much respect. It seems to me that the discovery of the ultimate generalisation is not likely to occur until many atomic weights have been determined with the greatest accuracy. No trouble being too great to attain this end, the Harvard work will be continued indefinitely, and attempts will be made to improve its quality, for the discovery of an exact mathematical relationship between atomic weights would afford us an immeasurably precious insight into the ultimate nature of things.

But weight is only one of the fundamental properties of an element. Volume is almost, if not quite, as important in its own way, although far more variable and confusing. All gases, indeed, approach closely to a simple relationship of volumes, defined by the law of Gay Lussac and the rule of Avogadro, and well known to you all. In the liquid and solid state, however, great irregularities are manifest, and very little system as regards volume is generally recognised.

About twelve years ago, the study of such small irregularities as exist among gases led me to the suspicion of a possible cause for the greater irregularities in liquids and solids.[1] On applying van der Waals's well-known equation to several gases, in some tentative and unpublished computations, it seemed clear that the quantity *b* is not really a constant quantity, but is subject to change under the influence of both pressure and temperature. This conclusion has also been reached independently by van der Waals himself.[2] But if the quantity *b* (supposed to be dependent upon the space actually occupied by the molecules) is changeable, are not the molecules themselves compressible?[3]

The next step in the train of thought is perhaps equally obvious. If changes in the bulk of molecules are to be inferred even from gases, may not the expansion and contraction of solids and liquids afford a much better clue to the relative expansion and contraction of these molecules?

Most physical chemists refer all changes in volume to changes in the extent of the ***empty space*** between the molecules. But are there, after all, any such empty spaces in solids and liquids? Solids do not behave as if the atoms were far apart within them; porosity is often conspicuous by its absence. Take, for instance, the case of glass; the careful experiments of Landolt on the conservation of weight[4] show that glass is highly impermeable to oxygen, nitrogen, and water for long periods. Such porosity as occurs in rigid, compact solids usually permits the passage only of substances which enter into the chemical structure of the solids themselves. Thus nitrogen cannot free itself from imprisonment within hot cupric oxide, although oxygen can escape;[5] again, water cannot evaporate into even the driest of atmospheres from accidental incarceration in crystals lacking water of crystallisation.[6] Palladium, on occluding hydrogen, is obliged to expand its bulk in order to make room for even this small addition to its substance. The behaviour of platinum, nickel, and iron is probably analogous, although less marked.[7] Fused quartz, impermeable when cold, allows of the passage of helium and hydrogen at high temperatures;[8] but most other gases seem to be refused admission, and very many solid substances appear to act as effective barriers to the passage of even hydrogen and helium, especially when cold. In these cases, as in so many others, the so-called "sphere of influence" of the atom is the actual boundary by which we know the atom and measure its behaviour.[9] Why not call this the actual bulk of the atom?

From another point of view, the ordinary conception of a solid has always seemed to me little short of an absurdity. A gas may very properly be imagined with moving particles far apart; but what could give the rigidity of steel to such an unstable structure? The most reasonable conclusion, from all the evidence taken together, seems to be that the interstices between atoms in solids and liquids must usually be small even in proportion to the size of the atoms themselves, if, indeed, there are any interstices at all.

Very direct and convincing evidence of another sort is at hand. The idea that atoms may be compressible

[1] Richards, "Methods Used in Precise Chemical Investigation," published by the Carnegie Inst. of Washington, 1910, No. 125, p. 97.

[2] See especially Rydberg, *Zeitsch. anorg. Chem.*, 1897, xiv., 66.

[1] Richards, "The Significance of Changing Atomic Volume," Proc. Amer. Acad., 1901, xxxvii., 1; 1902, xxxvii., 300; 1902, xxxviii., 293; 1904, xxxix., 581; *Zeitsch. physikal. Chem.*, 1902, xl., 169, 597; 1903, xlii., 129; 1904, xlix., 15.

[2] Van der Waals, *Zeitsch. physikal. Chem.*, 1903, xxviii., 257. His earlier publication on this topic (Proc. R. Akad. Wetensch. Amsterdam, 1898, xxix., 138) was unknown to me at that time. See also Lewis, Proc. Amer. Acad., 1899, xxxv., 21.

[3] Van der Waals speaks cautiously, but with some conviction, as to the probable compressibility of the molecules on p. 283 of the paper cited above.

[4] H. Landolt, "Über die Erhaltung der Masse bei Chem. Umwandlungen," *Abhandlung der königl preuss. Akad. der Wissenschaften*, 1910.

[5] Richards, *Zeitsch. anorg. Chem.*, 1892, i., 196; Proc. Amer. Acad., 1893, xxviii., 200.

[6] Baker and Adlam, Trans., 1911, xcix., 507.

[7] Richards and Behr, Publ. Carnegie Inst., 1906, lxi.

[8] Jacquerod and Perrot, *Compt. rend.*, 1907, cxliv., 135.

[9] Since these ideas were first advanced, Barlow and Pope have brought forward much interesting evidence concerning the significance of the volumes of solids and liquids, which supports the idea that the atoms are closely in contact with one another (Trans., 1906, lxxxix., 1675; 1907, xci., 1150; 1908, xciii., 1528; 1910, xcvii., 2308).

receives striking confirmation from a recent interesting investigation of Grüneisen[1] concerning the small effect of low temperatures on the compressibility of metals. The average compressibility of aluminium, iron, copper, silver, and platinum falls off only 7 per cent. between the temperature of the room and that of liquid air. Extrapolation of the curves indicates that at the absolute zero very little further diminution should occur. So far as we can guess, therefore, the hard metals are almost as compressible at the absolute zero as at room temperatures. But at the absolute zero all heat-vibration is supposed to stop; hence this remaining compressibility must needs be ascribed to the atoms themselves.

If the atoms are compressible, all mathematical reasoning which assumes them to be incompressible rests upon a false basis. The kinetic theory of *gases* remains unmolested by these considerations, except as they indicate the changeability of b in the equation of van der Waals, but the new views affect seriously the application of this equation to solids and liquids.

Let us proceed to trace a few of the outcomes of our hypothesis. If atoms may really be packed closely together, the volumes of solids and liquids should afford valuable knowledge concerning the relative spaces occupied by the atoms themselves under varying conditions. The densities of solids and liquids then assume a significance far more interesting to the chemical philosopher than before, because they have a more definite connection with the fundamental nature of things.

An apparent objection at once suggests itself; if the particles in condensed material are really touching one another, how can we account for heat within the material? Would such closely packed atoms be able to vibrate?

The theory of compressible atoms supplies as one of its own corollaries the immediate answer to this question. If atoms are compressible throughout their whole substance, they may contract and expand, or vibrate within themselves, even when their surfaces are prevented from moving by being closely packed together. It is thus possible to conceive of a vibrational effect, even in contiguous atoms, provided we can conceive of these atoms as being elastic throughout all their substance. Agitation sufficient to produce even the Brownian movement might easily exist in such a system.

Clearly there is nothing impossible or obviously contradictory to experimental knowledge in the notion that atoms are compressible; indeed, the old idea of small, hard particles far apart is really more arbitrary and hypothetical than the new conception. The obvious simplicity of the latter is rather in its favour than otherwise, as in Dalton's atomic theory. In general, the more simply a hypothesis interprets the phenomena of nature, the more useful the hypothesis is likely to be, provided, of course, that the interpretation is adequate. The modern philosophy of pragmatism is a good guide in such matters; a theory not obviously illogical should be judged by its usefulness. Let us, then, test the new hypothesis by applying it to other aspects of physical chemistry.

If pressure produces a change in the sizes of the atoms and molecules themselves, may not the actual volumes of liquids and solids be used as a guide to the unknown internal pressures within them? Cannot we thus discover whether or not chemical affinity exerts pressure in its action? To follow this clue, the simplest possible case was chosen at first, namely, the comparison of the contractions taking place on combining several elements in succession with a single very compressible one. The changes of volume occurring during the formation of oxides were first computed; later, chlorides and bromides were studied. According to the theory of compressible atoms, we should expect to find greater contraction in cases of greater affinity. A diagram depicting typical data concerning certain nearly related chlorides strongly supports this inference.[2] One line shows the total change of volume which occurs when a grain-molecule of chlorine combines with the equivalent weight of metal; the other gives the heat evolved during combination. The lines show distinct parallelism; that is to say, reactions evolving much heat manifest great contraction. In cases of this kind, the heat of reaction is usually not very different from the change of free energy; therefore we may infer that greater *affinity* is associated with greater contraction; and it is but a small leap in the dark to guess that the change of volume is *caused* by the pressure of affinity. Since chemical affinity holds two elements firmly together, why should it not exert pressure? And if it exerts pressure, why should not the volume of the system be diminished by this pressure?

Evidently the change of volume in any case must depend not only on the intensity of the pressure exerted by the affinity, but also, among other things, on the compressibility of the substances concerned. The greater the compressibility, the greater should be the change of volume caused by a given pressure of affinity. Before any definite conclusion can be drawn, the differences in compressibility must be taken into account.

These thoughts led to the measuring of the compressibilities of a large number of elements and simple compounds. The previously employed methods for solids and liquids being unsatisfactory, a new and highly satisfactory method was devised for the work done at Harvard. The compressibilities of thirty-five elements and many single compounds were studied by this method with sufficient care to leave no doubt as to their relative values. It became at once manifest that the formation of a compound of a compressible element was attended with greater decrease of volume than the formation of a similar compound of a less compressible element, other things being equal.[1] This is just what the theory leads us to expect, and is a fact inexplicable by any other hypothesis as yet known to me.

Another essential aspect of the theory of compressible atoms is that which concerns cohesion.[2] If the pressure of chemical affinity causes atomic compression, may not the pressure of cohesive affinity also have the same effect? Traube suggested this possibility, but looked at the whole question from a different point of view.[3] The affinity which prevents solids and liquids from vaporising is generally admitted to produce great internal pressure; must it not tend to compress the molecules into smaller space? Molecules with high cohesive affinity (those of substances hard to volatilise) should be much compressed and possess small volume, whereas molecules with a slight cohesive affinity should be more bulky. Moreover, those molecules already much compressed by their own self-affinity would naturally be but little affected by additional pressure. Thus, as regards two substances otherwise similar, the less volatile one would be less compressible, denser, and possess greater surface tension.[4] These outcomes of the theory correspond with the facts in a majority of cases thus far studied; for example, *o*-xylene is denser, less volatile, less compressible, and possesses a greater surface tension than either *m*-xylene or *p*-xylene. Differences of structure and differences of chemical nature sometimes conceal these relations; the parallelism appears most strikingly among isomeric compounds. In brief, the bulk of evidence strongly indicates that cohesiveness as well as chemical affinity exerts pressure in its action, and hence that each plays a part in determining the volumes occupied by molecules.

Thus the computation of the space occupied by either a solid or a liquid becomes a very complex matter. Not only must the various chemical affinities at work be taken into account, but also the cohesive attraction of both factors and products, and the compressibilities over a very wide range of all the substances concerned. Discoverable parallelism in volume changes is to be expected only when one alone of these forces is the chief variable.

The exact mathematical working out of the consequences

[1] E. Grüneisen, *Ann. Physik.* 1910, [iv], xxxiii., 1239. The relative values for the compressibilities recorded in this investigation are doubtless trustworthy, although the absolute magnitudes are somewhat uncertain because they depend on the rather inadequate theory of elasticity.

[2] Richards, Proc. Amer. Acad., 1902, xxxvii., 399; also especially J. Amer. Chem. Soc., 1909, xxxi., 188.

[1] Richards, Proc. Amer. Acad., 1904, xxxix., 581. [2] *Ibid.*

[3] See especially Traube, *Ann. Physik.*, 1897, [iii], lxi., 383; 1901, [iv], v., 548; 1902, viii., 267; 1907, xxii., 519; *Zeitsch. physikal. Chem.*, 1910, lxviii., 289; also Walden, *Zeitsch physikal. Chem.*, 1909, lxvi., 385 Their interpretation depends largely on the application of van der Waals's equation and the complicating assumption of a *co-volume*; however, Walden's very recent paper presents a number of interesting and important relations concerning internal pressure, which seem to demand the assumption of atomic compressibility for their explanation.

[4] Richards and Mathews, *Zeitsch. physikal. Chem.*, 1908, lxi., 449.

is very far in the distance, if, indeed, it can ever be attained. This fact does not, however, militate in the least against the plausibility of the idea. Although mankind has not yet been able to devise a method of mathematical analysis which will solve at one stroke the gravitational relations of three bodies, nature is not on that account prevented from causing three or more bodies to act on one another with the force of gravity, or astronomers from calculating as nearly as may be the consequences by a process of approximation.

Carried through to its logical conclusion, the idea that atoms are compressible gives one quite a new conception of the molecular mechanics of the universe. The influence of atomic compressibilities may be perceived everywhere, and in most cases each fact seems to fit easily and without constraint into its place in the hypothesis. Even apparent exceptions, such as the abnormal bulk of ice, may be ascribed in a reasonable fashion to superposed effects. A detailed discussion of many applications of the theory is impossible here, but a few may be suggested in order to make clearer its possibilities.

The satisfying of each valence of an atom would cause a depression on the atomic surface, owing to the pressure exerted by the affinity in that spot. The stronger the affinity, the greater should be this distortion. Evidently this conception gives a new picture of the asymmetric carbon atom, which, combined with four other different atoms, would have upon its surface depressions of four unequal magnitudes, and be twisted into an unsymmetrical tetrahedron. The combining atoms would be held on the *faces* of the tetrahedron thus formed, instead of impossibly perching upon the several peaks. According to this hypothesis, the carbon atom need not be imagined as a tetrahedron in the first place; it would assume the tetrahedral shape when combined with the other four atoms. One can easily imagine that the development of each new valence would change the affinities previously exercised, somewhat as a second depression in the side of a rubber ball will modify a forcibly caused dimple in some other part. Thus a part of the effect which each new atom has on the affinities of the other atoms already present may be explained.

Many other physico-chemical phenomena assume a new aspect when viewed from the point of view of this idea. New notions of the mechanism of the critical phenomena, surface tension, ductility, malleability, tenacity, and coefficient of expansion are gained. The peculiar relations of material and light, such as magnetic rotation, fluorescence, partial absorption, and so forth, may be referred to the modified vibrations of distorted atoms. The deviations from the exact fulfilment of many older generalisations concerning volume (such as the equation of van der Waals already cited, the comparative volumes of aqueous solutions, especially of electrolytically dissociated substances,[1] and the variations in the crystal forms of isomorphous substances) are seen to be a foregone conclusion. Moreover, the theory, although not necessarily dependent on the modern belief that atoms are built up of numbers of much smaller corpuscles, is consistent with that belief; for would not such an entity be compressible?

The more closely the actual data are studied, the more plausible the hypothesis of compressible atoms appears. Ten years' experience with its interpretations leads me to feel that the idea is highly suggestive and helpful in stimulating new search after truth and in correlating and codifying diverse facts. By such fruit are hypotheses justified.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The following is the text of the speech delivered by Prof. Love in presenting Sir W. T. Thiselton-Dyer for the degree of D.Sc., *honoris causa*, at the Encaenia on June 28:—

Adest nobis ornandus Willelmus Turner Thiselton-Dyer, vir magnam in Botanica laudem adeptus, huius Academiae olim alumnus. Qui cum Dublinii, Corinii, Londonii hanc scientiam profiteretur, docendi rationes ita novavit ut florentissimis totius Europae scholis schola Britannica par fieret. Idem postea regalibus hortis Kewensibus praefectus varia negotiorum genera ad Botanicam pertinentia promovit. Testis est India, Cinchonae Peruvianae, in medendo potentissimae, nunc ferax: testes etiam Taprobane insula et Chryse Chersonesus Heveae Brasiliensis cultrices, cuius arboris virtutes omnibus notissimae sunt. Adeo non solum saluti, sed etiam rei familiari civium hic noster sua opera inserviit.

MR. J. H. PRIESTLEY, lecturer in botany at the University of Bristol, has been appointed professor of botany at the University of Leeds.

MR. JOHN BLAKEMAN, head of the mathematical department of the Leicester Technical School, has been appointed as principal of the Northampton Technical School.

AN endowment fund of 200,000*l.* has been presented to University College, Reading, with a view to enable it to apply for a charter as an independent university. Of the amount mentioned, Lady Wantage has given 50,000*l.*, Mr. and Mrs. George W. Palmer 100,000*l.*, and Mr. Alfred Palmer 50,000*l.* Mr. Alfred Palmer has, in addition, presented to the college the freehold of four acres of land contiguous to the college site, at present held and used for horticultural purposes by the college under lease from him.

WE learn from *Science* that a gift of 4000*l.* to aid general research in the study of diseases at the Yale Medical School has been announced from an old student. Further gifts of 2000*l.* toward the endowment of the University clinic, and to the Peruvian exploration fund, for the Yale expedition under Prof. Hiram Bingham, have also been announced. From the same source we find that gifts of 20,000*l.* in lands by Messrs. James B. and Benjamin N. Duke, of 10,000*l.* for a new building by Mr. James B. Duke, and of 2000*l.* by Mr. B. N. Duke for improvements, were announced recently at Trinity College, Durham, N.C.

THE General Assembly of the State of Illinois has granted to the University of Illinois for the next two years the sum of 703,860*l.* *Science* says that this is the largest grant ever made by a State legislature to a State educational institution. The General Assembly has not only recognised the immediate needs of the University, but has looked ahead and made provision for the future by levying a one mill tax for the continued support of the University. It is estimated that this tax will yield an income to the University, two years hence, of about 450,000*l.* a year. In addition, the University will receive from the Federal Government and other sources funds that will bring its income to about 400,000*l.* per annum for the next biennium.

SOCIETIES AND ACADEMIES.

LONDON.

Mineralogical Society, June 13.—Prof. W. J. Lewis, F.R.S., president, in the chair.—G. S. **Blake**: Zirkelite from Ceylon. The results of five analyses made on fragments grouped together according to their specific gravity, which ranged from 5·2 to 4·4, showed remarkable variation in the percentage composition, the densest containing about 20 per cent. thoria and little uranium, and the lightest 14 per cent. U_3O_8 and little thorium; the precise formula is uncertain. A few crystals, some simple and some twinned, were met with; they apparently belong to the hexagonal system ($cr = 53° 22'$), the observed forms being $c(0001)$, $m(10\bar{1}0)$, $r(10\bar{1}1)$, $s(20\bar{2}1)$, $d(10\bar{1}2)$, $e(20\bar{2}3)$, and r the plane of twinning; they were opaque in mass, but translucent and isotropic in splinters.—Rev. Mark **Fletcher**: Note on some crystals of artificial gypsum. The crystals, which were formed in the condensing plant of a distillery at Burton-on-Trent, were twinned about 101, and the forms 100, 110, 230, 111 were observed.—L. J.

[1] Baxter has very recently discussed this matter from the point of view of the theory of compressible atoms (J. Amer. Chem. Soc., June, 1911).

Spencer: The larger diamonds of South Africa. Historical notes relative to the "Excelsior," "Jubilee," and "Imperial" diamonds were given, together with a tabular statement of the weights of the rough and cut stones in carats and grams, and the percentage yield of the cut brilliants from the rough.—F. H. **Butler**: Brecciation in mineral veins. In vein-breccias due to fracture *in situ* (crush-breccias) replacement of country-rock is a characteristic feature. Where the coarse fragments in a brecciated fissure-vein indicate erosion, removal of fine rock débris may be inferred. Fragments that are angular and uneroded and completely isolated by encrusting material often indicate by shape and position their former existence as a single mass. The quiet removal of such fragments into a vein-cavity after reunion, and also the banding, with concomitant contortion of adjoining soft country-rock, by their cement-substance, may be ascribed to the hydrostatic pressure and the solvent and mineralising properties of the waters which furnished that substance. The coarse constituents of breccia may have been crushed *in situ*, or forced from fissure-walls by earth movements, or detached therefrom by aqueous pressure and solution.—Arthur **Russell**: Prehnite from the Lizard district. Two distinct types of crystals, tabular and prismatic, were recently found by the author on hornblende-schist at Parc Bean Cove, Mullion, Cornwall, the former showing the forms 001, 302, 061, and the latter 100, 001, 110, 061, and the rare form 301.

Royal Meteorological Society, June 14.—A. J. **Makower**, W. **Makower**, W. M. **Gregory**, and H. **Robinson**: Investigation of the electrical state of the upper atmosphere. The object of the experiments described was to measure the electrostatic potentials at various heights above the ground and the currents that flow down an earthed kite-wire. The method adopted was to send up kites or, in still weather, balloons attached to steel wires, provision being made for detaching sections of the wire from the winding drum so that the lower end might be anchored to a long rod of ebonite in order to insulate it from the ground. When this had been done the wire could either be earthed through a galvanometer to measure the current flowing down the wire, or else be connected to an electrostatic voltmeter having a range of 100,000 volts by means of a metallic line passing through glass tubing supported on long insulators to prevent brush discharges to the surrounding air or leakage to earth. Curves are given embodying the results of a series of flights made during the month of August, 1910, the potentials and currents that were measured being plotted as functions of the heights above the ground. The values obtained for the potential gradient near the ground lie between 0·5 and 1·5 volts per centimetre, and are in agreement with those deduced from the tests of previous experimenters using water-droppers or radium collectors, but it is found that the potential gradient diminishes rapidly as the height above the ground increases. Flights are recorded up to 4000 feet above the ground, at which height the potentials ranged between 40,000 and 60,000 volts, and the currents between 40 and 100 microamperes. Measurements were also made of the time taken by the kites and balloons to attain the full potential of the surrounding air from the moment at which the wire was disconnected from earth. This rate of charging is of interest in aëronautics in connection with the devising of suitable methods of preventing dangerous electric discharges from taking place between a balloon and the surrounding medium after a sudden change of height. The tests showed that the kites and balloons, the collecting area of which was about 150 square feet, charged up according to an exponential law, the exponential coefficient having values lying between 0·1 and 0·23, showing that a potential of half the full value was reached in about 5·5 to 7 seconds. It is argued that the rate of charging up is probably proportional to the radius of the balloon, and so the rate of charging up of large passenger balloons might be deduced from the rates determined with the small balloons used in these investigations. Attempts were made by the authors to discover a connection between the electrical state of the atmosphere and the prevailing temperature, barometric pressure, humidity, and wind velocity as registered on self-recording instruments sent up at each flight, but it was found that the amount of data collected was not sufficient to make such deductions possible. It seems that such conclusions will not be able to be drawn until continuous experiments extending over a considerable period of time have been made.

Geological Society, June 14.—Prof. W. W. Watts, F.R.S., president, in the chair.—Prof. W. S. **Boulton**: A monchiquite intrusion in the Old Red Sandstone of Monmouthshire. An unrecorded monchiquite, intruded into the Upper Old Red formation of Monmouthshire, is described. The manner of its intrusion is doubtful. The disturbance and metamorphism of the contact-rocks are dealt with, as also the rounded lumps of marl and subangular chips of sandstone incorporated in the igneous rock. The monchiquite contains large phenocrysts of augite and biotite, generally much corroded. Rounded "nodules" of olivine-augite rock with chromite are also included. A second generation of augite, biotite, and decomposed olivine occurs porphyritically in the groundmass. The ground-mass is a felt of minute elongated augite prisms, magnetite grains, and flakes of biotite. A complete analysis of the rock is given, which bears out the petrographical evidence that it is a very basic lamprophyre belonging to the monchiquite group. Its age and connection with the only other known intrusion into the Old Red Sandstone of the South Wales area are referred to.—Notes on the Culm of South Devon: Part i.—Exeter district, by F. G. **Collins**, with a report on the plant-remains by E. A. N. **Arber**, and notes on the Cephalopoda by G. C. **Crick**. The paper is to show that the fauna of the Culm Measures of South Devon proves these beds to be the equivalents of the Pendleside series of the Midlands. The actual fossiliferous localities are eighteen in number, but often the fossils are too poor for determination. It seems advisable to seek more evidence, and an attempt will be made by working due north from Waddon Barton, a point farther to the west.

British Psychological Society, June 24 (held at Manchester).—Dr. T. Graham **Brown**: Note on the perception of movement in the environment.—C. **Burt**: The experimental investigations of emotional dispositions.—Dr. H. **Watt**: A new classification of experiences.—Prof. C. S. **Sherrington**: A simple teaching apparatus for illustrating Listing's law.—Prof. J. Lorrain **Smith** and Dr. W. **Mair**: A chemical comparison of the brain substance of the child and the adult.

DUBLIN.

Royal Irish Academy, June 26.—Rev. Dr. Mahaffy, president, in the chair.—C. F. **Rousselet**: Clare Island Survey: rotifera. Mr. Rousselet collected all the rotifera except the Bdelloida, which are embodied in a special report by Mr. James Murray. The report shows that the rotifer fauna of Clare Island and the neighbouring parts of the mainland differs in no special features from that of many other parts of the British Islands. Mr. Rousselet gives a list of 103 species with their distribution. Some of these had not previously been recorded from the British Islands.—Eugène **Penard** and G. H. **Wailes**: The fresh-water rhizopoda obtained during the Clare Island Survey. The first collections of the fresh-water rhizopoda of Clare Island and the neighbouring districts of the mainland of Ireland were made by Dr. E. Penard, the work being continued later by Mr. G. H. Wailes. The present report has been drawn up under the joint authorship of these two investigators. The total number of species and varieties recorded is 140, of which *Cryptodifflugia eboracensis*, *Euglypha cirrata*, *E. rotunda*, and *E. armata*, as well as several varieties, are new to science. Seven other species are new to the British Islands.—D. J. **Scourfield**: Fresh-water entomostraca. In a preliminary report Mr. Scourfield states that 41 species of Cladocera, 23 species of Copepoda, and 13 species of Ostracoda have been observed on Clare Island and the neighbouring parts of the mainland. No new species have as yet been definitely identified, but a couple may eventually prove to be new to science. The great majority of the species recorded are common types in the British Islands; only a few are to be considered as rarities.—Miss A. Lorrain **Smith**: Lichens (Clare Island Survey). The lichen flora of the Clare Island district is extremely abundant, especially as regards rock and ground species. The rarity of trees renders bark species less widely distributed than usual. The present report deals with some 280 species and 40 subspecies, &c. Of these, between thirty and forty are hitherto unrecorded from Ireland, and several are new to the British Isles or only once previously found therein. A summary of previous work in the district shows that while the neighbouring county of Galway has been well explored by Larbalestier, Mayo was practically unworked until the present investigation.

PARIS.

Academy of Sciences, June 26.—M. Armand Gautier in the chair.—E. **Guyou**: Solution of problems of altitude. New tables of navigation.—J. **Boussinesq**: Calculation of the absorption in translucent crystals for plane waves, laterally undefined.—P. **Villard** and H. **Abraham**: A large electrostatic machine. A description of a specially constructed Wimshurst machine of twenty plates, capable of yielding 1 milliampere at 250,000 volts.—A. **Müntz** and E. **Lainé**: Considerations on the employment of sewage in agriculture. Analyses of the Paris sewage are given, showing its value when applied directly to various crops. It is deficient in phosphates, and to use it to the best advantage these should be added.—L. **Maquenne**: Concerning a recent communication by M. L. Cailletet (on the origin of the carbon assimilated by plants).—E. L. **Bouvier**: New observations on evolutionary mutations.—Édouard **Heckel**: The action of cold, of chloroform, and of ether on *Eupatorium triplinerve*. No odoriferous substance exists preformed in this plant, but such a substance is formed after desiccation for several hours, and much more rapidly after exposure to cold or to the action of anæsthetics.—J. Ph. **Lagrula**: A triple meteor observed at Nice.—Luigi **Giuganino**: Effect of the movement of the earth on light phenomena.—M. **Chanoz**: Images physically developed after fixing exposed gelatine-silver bromide plates.—J. **Gardner**: Apparatus for the telephonic reception of submarine signals. This consists of a microphone connected with a metal ring of carefully specified proportions, and attached to the armour of the ship. The signals can be perceived at increased distances, and their directions ascertained.—G. **Sagnac**: Movement of the earth and the optical phenomena in an entirely terrestrial system.—H. **Buisson** and Ch. **Fabry**: Measure of the intensities of the different radiations in a complex ray. The radiations from a quartz mercury vapour lamp were allowed to fall upon a thermopile after passing through various absorbing media, such as water, solution of potassium chromate, solution of quinine sulphate, solutions of oxalic acid, glass, &c., thus obtaining the amounts of energy carried by radiations of different groups of frequencies.—Georges **Meslin**: Circular polarisation.—L. **Bloch**: Some general theorems in mechanics and thermodynamics.—L. **Houllevigue**: Kathode rays produced in electric incandescent lamps. Conditions are described under which it is possible to obtain pencils of rays, easily deviable by a magnet, in the interior of incandescent lamps.—M. **Dussaud**: Economical incandescent lighting. Description of the great efficiency of a coiled filament of tungsten as compared with a carbon filament.—A. **Besson**: Action of the silent discharge on dry and damp ammonia. Small quantities of a substance which reduces copper salts are formed. This is probably hydroxylamine, produced according to the equation

$$NH_3+H_2O=H_2+NH_2OH.$$

—Paul **Pascal**: A method of optical control of magneto-chemical analyses.—J. B. **Senderens** and J. **Aboulenc**: Catalytic esterification of aromatic acids in the wet way. The yield of ethyl benzoate produced in presence of sulphuric acid was found to depend upon the amount of the latter added. Acids such as the toluic acids, and salicylic acid, resemble benzoic acid in this respect, and in having the carboxyl group attached directly to the benzene nucleus; whereas such acids as phenylacetic and phenylpropionic, in which this group is not directly attached to the nucleus, do not yield increasing amounts of ester with increase in the quantity of sulphuric acid used as catalyst. The effects of potassium bisulphate and of aluminium sulphate as catalysts were also examined.—G. **André**: The diffusion of saline substances through certain organs of plants.—H. **Astruc**, A. **Couvergne**, and J. **Mahoux**: The adherence of insecticides of arsenate of lead.—V. **Balthazard**: Identification by finger-prints.—Léon **Pigeon**: Measure of the degree of strabismus.—M. **Odier**: The part played by mercury and some of its salts in certain cancers.—M. **Foveau de Courmelles**: A cause of X-ray dermatitis.—MM. **Sollaud** and **Tilho**: The presence in Lake Chad of *Palaemon niloticus*.—E. **Kayser**: The influence of humous substances on micro-organisms.—M. **Lemoigne**: Denitrifying bacteria of filter-beds.—Jean **Bielecki**: The part played by mineral matters in the formation of the protease of anthrax.—Stanislas **Meunier**: Influence of the structure of certain fossil shells on the production of a new variety of fibrous silica.—M. **Lantenois**: The advance of geological knowledge concerning Indo-China.—Henry **Hubert**: The mechanism of rains and storms in the Soudan.

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