THURSDAY, FEBRUARY 25, 1892.

THE SCIENCE MUSEUM AND THE TATE GALLERY.

WE are informed that various Government Departments are now considering the many questions raised by the Tate Gallery controversy in the real spirit of scientific inquiry—that is, they are at last attempting to deal with the facts, and they are consulting some people who ought to know something about them.

As the President and Officers of the Royal Society, and many other representative men throughout the country, have stated their opinion on the matter to Lord Salisbury in a memorial last year; and as they were pretty certain to have known the facts before they committed themselves to any statement involving them in great responsibility if they were wrong; the friends of science can only rejoice that at last the Government Departments are dealing with the matter in a scientific way.

The facts are as follows. In our Museum arrangements, while books and pictures—or, in other words, literature and art—are thoroughly represented, from the Museum point of view, on a scale worthy of the nation, only three branches of science—natural history, geology, and mineralogy—have so far been provided for, and by the Natural History Museum. This was pointed out about twenty years ago by the Duke of Devonshire's Commission; and the many advantages which would be certain to accrue to an industrial nation like ours, from collections representing the various physical, chemical, mechanical, and metallurgical sciences and their applications to industry, were very clearly referred to.

The Government, one may almost say, of course, took no notice of this important recommendation, or next to none. In 1876 the Royal Commissioners for the Exhibition of 1851—a body thoroughly well capable of judging of the importance of the Duke of Devonshire's recommendation—showed their opinion of its importance by offering a plot of ground stretching from Queen's Gate to Prince's Gate, and £100,000 to build this Science Museum. This offer, the Government of the day—most of the Governments of the day knowing and caring little about science, although they are so glad to receive donations for art—declined. So matters went on, Committee after Committee being appointed, reporting, and having their deliberations ignored, until at last, as a result of a Committee appointed by the Treasury itself, the plot of ground which the Government might years ago have had for nothing, was purchased, and purchased, of course, in strict relation to the discussions which, as we have shown, had been going on for the last eighteen years. At least, if that be not so, why was it purchased at all?

We now come to the plot of ground. It contains 350,000 square feet. What facts have we before us to aid us in answering the question as to whether this plot of ground is sufficient to do for all other sciences what the Natural History Museum does for three? Here, of course, if possible, we must appeal to facts, if facts exist. Let us assume that the new Museum is to be built on the precedent afforded by the Natural History Museum. The exhibiting space in the Natural History Museum consists of 152,000 square feet. To provide this exhibiting space in a building in which, of course, many other things besides exhibiting space have to be considered, the ground plan of the existing building contains an area of 162,000 square feet. The building itself, however, stands in a plot of ground covering about 500,000 square feet, and in this way ample possibilities of future extension are provided.

We are first, then, face to face with the fact that, assuming the Museum which is to do for all the other sciences what the Natural History Museum does for the three named, can, in a few years, be restricted to the same exhibiting area; we begin with a space of, roughly, 150,000 square feet *less* for that which must eventually be the *larger* Museum. Anyone will see that, in the nature of things, it is unlikely that the Museum which has to look after the interests of all the inorganic sciences, and their infinite applications to the arts (by which, of course, the industrial arts are meant), will always remain smaller than that Museum which has to provide for biology chiefly. But the matter, unfortunately, does not end here. It is practically certain that the Royal College of Science will require considerable additions to meet the demands made upon it for science teachers. It is perfectly well known already to all educationists that it has already entirely overflowed the small and ill-contrived building in which it is located; and the Professors of Physics and Geology are already camping out in temporary buildings of the most inconvenient description. We believe it is thoroughly conceded by the Government that new laboratories for physics, astronomical physics, and chemistry must be at once erected. Now, it is perfectly certain that, at this time of day, such laboratories as those, with their various appendages, cannot be erected on a space much less than 100,000 square feet; so that it comes to this, a site already too small for the purposes of a Science Museum and its future extension—if it is to be treated like the Natural History Museum—must have filched from it, for other scientific needs, another slice of 100,000 square feet.

This may be, perhaps, permitted, because, although the Science Museum may lose in area, it would gain enormously in advantage, for the reason that the teaching in the future may be given in close relation with the instruments which are used in it.

The suggestion that Mr. Tate should be allowed still another 100,000 square feet from this plot—from a plot containing less than 300,000 square feet available for building purposes—of course was preposterous from the beginning, and would have been at once shown to be impossible if the problem had been considered at all from the region of hard, dry fact. Unfortunately, it was not so considered, and hence an apparent antagonism between the interests of science and art, which has been going on now nearly a year. We have attempted to show that the question is not a question of sympathy—it is a question of fact; and now that the Chancellor of the Exchequer is dealing with the facts, as they ought to have been dealt with in the first instance, there can be no longer any doubt as to which way the issue will be decided.

We believe it is contended by some who are not acquainted with the exact terms of reference to the last

Treasury Committee that their very indefinite suggestion as to the space to be provided to meet present needs really referred to their view as to the maximum amount of space which a Science Museum should be allowed to cover. We are certain that the scientific members of that Committee would repudiate any such opinion, and they would quote, as we have quoted above, the actual facts concerning the Natural History Museum as negativing any such idea.

Finally, it is advisable to point out the terrible want of any sense of perspective on the part of the Legislature on matters relating to science and art. We do not object to any expenditure the Government may choose to make upon art, but it is our clear duty to point out that the interests of science must not be neglected in order that art may be encouraged. The £70,000 paid for the land which has given rise to all this discussion is the same sum as that given not long ago for a single picture. The "British Luxembourg"—whatever that may be—which the Government is supposed to be now fostering, was intended to contain most of the pictures now in the South Kensington Museum; so that the edifying spectacle was to have been, or indeed may yet be, seen, of emptying all the picture galleries at one end of the Museum while £400,000 of public money is being expended—this has been agreed to by the Government with alacrity—in building new picture galleries at the other. Nor is this all. The Tate Gallery, if built, is to be maintained by the Government: we are informed this will cost at least £4000 annually. Here, then, is another endowment of, say, £160,000 for art. We do not object to this if the nation so wills it; but is it wise that all this time the training of our science teachers—our great requisite just now—is to be carried on in sheds, and that the only concern that the Government shows in relation to the Science Museum, which is to include the Patent Museum, is to still absorb year after year the patent fees, which ought in justice to be used for the improvement of our national industries?

CHEMICAL TECHNOLOGY.

Manual of Chemical Technology. By Rudolf von Wagner. Translated and edited by William Crookes, F.R.S., from the thirteenth enlarged German edition, as remodelled by Dr. Ferdinand Fischer. With 596 Illustrations, 968 pages. (London: J. and A. Churchill, 1892.)

WHEN a book has reached a thirteenth edition, it may usually be assumed that its form and contents are so familiar to all who are likely to be interested in the subject of which it treats as to require no description, and that it has attained so substantial a reputation as to be independent alike of praise or blame flowing from the pen of the critic. In the case of the book before us, however, the author is no longer living; and having completed in 1880 the eleventh edition of his work, the care of future issues devolves upon others. There is probably no subject which changes so rapidly, owing to the advances of science and the incessant activity of inventors, as chemical technology, or the application of chemistry to useful purposes in the industrial arts; and a glance at the table of contents of such a book as this is sufficient to remind one of the vast range of subjects with which chemistry has to do. It is the extent and diversity of these subjects which suggests at once the idea of the difficulties which must attend the compilation of such a treatise even when the work of so distinguished a technologist as von Wagner is carried forward by editors so eminent as Dr. F. Fischer and Mr. Crookes. That they have discharged their duty in no perfunctory spirit is evident from the fact that many sections are entirely new, and that, as compared with the eleventh German edition, more than half the text and the illustrations have been replaced by new matter. And further, as pointed out by the English editor, a manual such as this must be in many respects adapted to the conditions of the country where it is written, and, if translated for use elsewhere, it requires modification. The prices of raw materials, of fuel, and of labour, and the laws in different countries have in each case their influence upon the conditions under which chemical industries are carried on. As already remarked, a book in its thirteenth edition must have been found useful by a good many people, but the first thought that passes through the mind in turning over the pages of Wagner's "Technology" is—For whom can this book have been intended? It treats of everything: of fuel and metallurgy, of water, acids, and alkalies, of pigments and dyes, of glass and cements, of food and fibres, of leather, soap, wood, matches, and many smaller subjects. In the limits of 950 pages it is not possible to provide all the details essential to each process which would be sought by a manufacturer, and an examination of the volume shows that the treatment of the successive subjects is very unequal. The impression derived from its perusal is that, on the whole, it is most likely to be useful to senior students or to chemists who wish for general information relating to chemical manufactures, but that the efforts which have evidently been made to incorporate into this new edition an account of modern processes are spasmodic, and not always successful.

The articles on soda and on chlorine, for example, are among the best in the book. The several processes for the recovery of sulphur from alkali waste, including the Schaffner-Helbig and the Chance patent processes, are described with some detail; and the Weldon-Péchiney process for obtaining chlorine from magnesium chloride is described at length with the aid of numerous illustrations. But even in this latter case, in view of the importance of the process, which is still on its trial, information more recent than December 1887, might have been expected. There is no reference to more recent processes for electrolyzing alkaline chlorides; but this, perhaps, is expecting too much in the way of the "latest intelligence"; and doubtless the same remark applies to the absence of any description of liquid chlorine as a commercial article. This, however, does not account for the curious error by which the number formerly given as the density of liquid chlorine, 1·33, is assigned to the gas. The admirable work of Knietsch (*Annalen*, 259) on the properties of liquid chlorine deserves to be had in remembrance, more especially as it was done in the laboratory of an alkali-works, with industrial objects in view.

A really unsatisfactory article is that which relates to iron and steel. The ores are stated to be roasted in heaps and in special kilns, but no description is given of the process. The blast-furnace to which the longest description is devoted is an open-top furnace of forty years ago. No figure is given of a modern English blast-furnace; the form shown in the cut, and dismissed in eight lines of text, is not to be seen in this country. The cup and cone charger, general in Great Britain, is not even mentioned. There is no straightforward account of the successive chemical changes which are supposed to take place as the charge descends in the furnace, but, without introduction, the reader is plunged into a disproportionately long discussion of the "heat conditions of the blast-furnace." There is no description of the *tuyères*, though it is mentioned that they are cooled by water, nor of the sand casting-bed, nor of the resulting pigs. In the account of "crude" iron, as it is called, it is surely not enough to say that, "according to the nature of the carbon, it is distinguished as white and grey crude iron," and then to give just six lines to each of these two varieties, without so much as mentioning the various grades distinguished by the numbers familiar in every ironworks, or the purposes in the forge or the foundry to which they are severally applicable. Following the description of "crude" iron is a section occupying about three-quarters of a page on the examination of iron and steel, which might as well have been omitted, as it is quite useless. The same inadequate treatment is noticeable throughout the article, and to an English reader it must appear strange that English metallurgists, with the exception of Bessemer, Thomas and Gilchrist, and Bell, whose name appears once, are generally ignored. This seems to be one of the articles in which the English editor has not been sufficiently careful to supply the modifications indicated in the preface as requisite in order to adapt the book to the conditions prevailing in this country.

Turning now to subjects of a different character, there is the vast field of coal-tar and the colours derived from the tar hydrocarbons. The colours are dealt with in sixty-seven pages. In comparison, therefore, with other subjects in the same volume, this important and interesting department of applied chemistry receives perhaps its fair share of space; but, recalling the fact that in another book on applied chemistry recently published nearly the same number of pages is devoted to naphthalene and its derivatives alone, it is obvious that considerable condensation must have been effected in the work before us. In connection with this article, every chemist who has any feeling for consistency in chemical nomenclature must protest against the playful variety of spelling which the editor permits in the names of the hydrocarbons. On the same pages we have paraff*ine*, benz*ol*, naphthal*ine*, anthrac*ene*, triphenyl meth*an*. This is really too bad!

Referring to the thirty pages devoted to dyeing and tissue-printing, we find a practical acknowledgment of the inadequacy of the treatment which many subjects receive by the insertion of a list of books to which the reader is referred for further information. And this recalls the fact that elsewhere throughout the volume references to authorities in the shape of original memoirs, books, or patent specifications are rarely given. This is an omission which might with great advantage be repaired in a future edition, not only because further information is often essential, but because there are, necessarily, scattered up and down the pages of such a comprehensive work as this a good many statements which require some qualification, or, at any rate, some positive evidence to make them completely acceptable, as well as others which are manifestly antiquated and obsolete. For example, the statements (p. 223) as to the influence of the addition of aluminium to iron and steel ought not to be put forward without proof. Certainly the fact that the manufacture of pure aluminium is now practically abandoned tends to show that it has not been found so valuable in connection with iron and steel as was at one time asserted.

There is no very pressing necessity for introducing photometry into a book on chemical technology; but if there is a reason for its introduction, there must be a still stronger reason for making the description practical. It is not so in this case, for while Bunsen's photometer is referred to, rather than described, in eight lines, Violle's attempt to utilize the light emitted from melted platinum as a photometric unit is described at length, with a figure, and the article concludes with the statement that the amyl acetate lamp will probably in time supersede the other units. We think not.

Manufacturers who have managed to keep the details of their plant and their processes practically secret must derive some amusement from books which profess to describe their manufactures. The successful English monopolists of phosphorus-making (Messrs. Albright and Wilson) are represented, no doubt correctly, as making more phosphorus than anybody else; but we can imagine the quiet smile with which they would regard the pictures of apparatus said to have been invented and employed by themselves, as well as such ridiculous statements as (p. 410) that "bone ash is now the only material used by phosphorus-makers," &c. If it were not for the lofty air of knowledge with which such things are described, it would not matter greatly; but it would be more candid towards the reader if the writer of such an article as that on phosphorus would begin by declaring roundly that he does not *know*, but only *imagines*, that the following is the process likely to be employed.

The difficulty of purifying the pages of successive editions of so large and complex a book of phraseology which is obsolete or inappropriate is illustrated on p. 398, where a brief exposition is given of the views entertained by "the late Dr. Gerhardt" concerning the constitution of fulminating mercury. Not a word is mentioned of more recent chemical discussions on the fulminates, though they have been renewed more than once in the five-and-thirty years which have elapsed since the death of the distinguished French chemist.

The editors have produced a volume which contains much entertaining and instructive reading. From what has been said, however, it is obvious that the reader must not expect to find the whole truth, and nothing but the truth, set forth in any such cyclopædic production. It is not possible for any one man, or any three men, to array, without mistake, the accumulated stores of human knowledge and experience in such a subject as applied chemistry. W. A. T.

AN AGRICULTURAL TEXT-BOOK.

Elements of Agriculture. A Text-book prepared under the authority of the Royal Agricultural Society of England. By W. Fream, LL.D. Pp. 450, 200 Figures. (London: John Murray, 1892.)

DR. FREAM'S new book, the first edition of which was exhausted on the day of publication, is a distinct gain to agricultural literature. We naturally expect such a work from Dr. Fream's pen to have the botanical portions of the subject dealt with at considerable length, and in this we are not mistaken, half the book being taken up by the discussion of the various plants with which the farmer has to deal, either as crops or weeds. This division has to be made, to some extent, at the expense of other branches of the subject, which is perhaps not quite a desirable thing to do in a general text-book; though it might be argued that it is far better to do one thing well than to do several things indifferently, and Dr. Fream has certainly done one thing well in his treatment of "The Plant." The style is good, and the descriptions very lucid, concise, and sound, the book containing a vast store of information in small compass. There is not, however, a single reference to any original paper, an omission especially to be regretted when the author is discussing the Rothamsted experiments; in fact, we think that the addition of a list of the papers published by Lawes and Gilbert would have considerably enhanced the value of the book.

The author divides his work into three parts. Part I., which occupies the first 77 pages, treats of the origin and properties of the different kinds of soils, their improvement, cultivation, and manuring. Part II., "The Plant," occupies a full half of the book (pp. 78–302). The first two chapters of this part treat of seeds and their germination, and the structure and functions of plants, these subjects being discussed in popular, but very instructive fashion.

Chapter xi. treats of the cultivated plants of the farm, grouped under the natural orders to which they belong. This is the longest chapter in the book, and includes 30 pages given to the consideration of the natural order Gramineæ. Then follow a short chapter on weeds, and chapters upon harvest machinery and the management of grass land. In chapter xvi., the theory of rotations is ably dealt with; the various crops of the farm, with their cultivation, are considered in turn; this chapter also contains a handy table of the characters of the seeds of the common farm crops. In the second column of this table, opposite *mangel*, is the statement, "Should germinate at least 120 per cent. (see p. 141)." On turning to p. 141 we find the following:—

"The seed of commerce (mangel) consists of the ovary with its seeds, embedded in the swollen base of the perianth, which thickens and hardens as it ripens, becoming angular and somewhat woody. Hence, when a mangel or beet *seed* is set to germinate, it is not unusual for two or three shoots to appear from a single seed."

On p. 97 are some remarks on this very matter, with definitions of fruit and seed: "The *fruit* is the ripened ovary, the *seed* is the fertilized and ripened ovule." Is it not better to adhere to these definitions, and thus avoid speaking of the germination of a sample of seed as *over 120 per cent.*?

On p. 282, Dr. Fream speaks positively upon the much-debated practice of burning diseased potato-haulms. Further research upon the liability of old haulms to propagate the fungus (*Phytophthora infestans*) is much needed. The writer's own observations by no means lead him to consider them so dangerous in spreading disease as some people think they are. The question of the utility of sulphate of copper as a remedy for potato disease is dismissed in very few words. It is to be hoped that the subject will receive fuller treatment in succeeding editions, as many trials have recently been made; and although these have not always been attended with success, many of the failures reported turn out upon investigation to be due to the fact that the Bouillie Bordelaise (or other mixture) was improperly prepared or improperly applied. It would appear that impurities in the mixture exert considerable influence upon its efficacy as a check on the growth of the fungus. The use of a mixture of sulphates of iron and copper with lime generally gives negative results; and even the presence of ferrous sulphate in any quantity in commercial bluestone appears at times to exert a deleterious effect.

The third part of the book contains a short account of the structure and functions of farm animals, their various breeds, and also chapters upon feeding. The subject of dairying is dismissed in a chapter of 20 pages, and is the least satisfactory portion of the book. Under "Cheddar Cheese making" is given a process which is rather indefinite, and seems a sort of mixture of several methods; the instructions for this process are vague, the temperatures (where given) are rather uncertain, and nothing whatever is said as to the proper condition of the curd at the end of each operation. On p. 429 this remark is made:—

"Real Stilton, for example, is a double cream cheese, the cream of the evening's milk being added to the morning's milk."

"Real Stilton" of this nature is a myth; the so-called "cream Stiltons" are made from two curds, and do not contain added cream, the term being used (rather ambiguously we confess) to signify that no cream was abstracted from the milk used in making the cheeses.

A few errors in the text may be mentioned here. On p. 65 and elsewhere, ammonium carbonate is spoken of as a volatile gas; it would be more accurately described as a volatile solid. The average percentages of albuminoids in certain foods given in Table xviii. (p. 343) are most of them too low. In Table xx. (p. 344) the percentage of albuminoids in cotton cake is put down at 23·17, whilst it is given as 19 in Table xviii. on the previous page; the former value is more nearly an average one. The percentages of soluble carbohydrates in some feeding stuffs given in Table xxi. are not quite satisfactory; those for wheat, barley, and bean straws are much too high, especially that of 35 for bean straw; about one-fifth that figure would be nearer the truth. We think it would have saved space, and much trouble and confusion to the student, if Tables xviii., xix., xxi., xxiii., xxiv., and xxvi. had been combined, and the full analyses of the feeding stuffs given in a single table.

Table xxxi. (p. 408) is headed, "Percentage of nitrogen and minerals in the fasted live weights of cattle, sheep, and pigs." This heading should be, "Amounts of nitrogen and minerals in 1000 pounds fasted live weights, &c."

The illustrations in chapters xxv. and xxvi. are very poor indeed, especially Fig. 190, which is perhaps intended as a puzzle print. Also Figs. 171-3 are very bad attempts to convey an idea of what a cow is like. Figs. 160-4 are equally poor.

Apart from these details, we cannot but say that the book as a whole is an admirable work, and superior to anything of its kind which we have yet seen. It will prove a boon, alike to students and to educated farmers.

W. T.

HYLO-IDEALISM.

Further Reliques of Constance Naden. Edited by George M. McCrie. (London: Bickers and Son, 1891.)

WE have already in these pages expressed our opinion, in a notice of her "Induction and Deduction," that, had the hand of Death been withheld, Miss Naden would have made valuable contributions to philosophic thought. The volume of "Further Reliques" now before us serves to justify this opinion. It is questionable, however, whether her friends have been well-advised in including the "Geology of the Birmingham District," admirable as it is as a student's prize essay. In any case, since it was included, it would have been only just to Miss Naden to have requested someone acquainted with geological nomenclature to revise the proofs. On a single page we have *Trianic* for *Triassic*, *Keupes* for *Keuper*, *Llandeils* for *Llandeilo*, and *Paradoyidian* for *Paradoxidian*. This essay is dotted over with such misprints (the genus *Orthis* being printed, on p. 25, both *Orttiis* and *Orttus*). Nor are other parts of the book entirely free from errors due to careless editing. On p. 4, *destruction* is printed where the sense requires *distinction;* and on p. 160, *evidence*, where the author clearly meant to write *eloquence*.

As we before pointed out, for Miss Naden the fundamental principle in philosophy is the famous Protagorean formula of relativity, that "Man is the measure of all things, of things that are that they are, and of things that are not that they are not." In a kind of parable she describes the creation of the external world :—

"A myriad etherial waves, of inconceivable minuteness, enter the tiny window of the eye, and beat against the delicate lining of its darkened chamber. The pulsations are taken up, and transmitted along the optic nerve to the base of the brain, and thence to the gray thought-cells of the cerebral hemispheres. And in these gray thought-cells lives the God who says, 'Let there be light,' and there is light. If the optic nerve be an inefficient messenger; if, maimed or paralyzed, it fail to convey the vibrations received from without, the creative fiat will never be issued, and the world will remain, for the God of that one cerebrum, without form, and void. He is not a First Cause, since a stimulus is needed to set him in action; but he is certainly the only authentic Creator of the world as yet discovered by science, philosophy, or religion."

Whether this way of putting the matter is in the best possible taste, we do not pretend to decide. It is with Miss Naden's philosophical position, not with any other aspect of her views, that we can deal here. Philosophically, her view is, that the gray thought-cells are for her, and for each and all of us, the creators of the world. "But here," she continues—in her article on "Hylo-idealism," prefixed to certain letters addressed to her by Dr. Lewins, of the Army Medical Department—

"But here comes the most critical point of the inquiry. If the universe be simply a more or less coherent vision; if its very solidity and extension be but parts of the 'realistic' drama, how are we to know that there is any such thing as matter? . . . How are we to be sure that the brain itself really exists, and that the all-generating cells are not mere illusory appearances?"

How does Miss Naden answer this question, by which she is by no means the first to be puzzled?

"The puzzle," she says, "is not so hard as it looks. The uttermost sceptic tacitly assumes the possibility of argument; that is, of a course of reasoning, in which every step is dependent on the preceding step, while the origin of the whole is some group of observed facts. If this be a delusion, and the last step stand in no kind of causal connection with the first, evidently argument is impossible, and the sceptic's lucubration shares the general invalidity. A succession of mere mental phenomena, of mere inert pictures, cannot constitute reasoning, because one inert picture cannot produce or condition another. If a mental state possess no property except the property of being perceptible, it is obviously purely passive, and exerts no real influence upon subsequent mental states. Now, as this position is utterly unthinkable, and is not less destructive to scepticism than to materialism, we are obliged to assume the existence of some active basis of thought—that is, of something which thinks. What we assume of the individual self we extend analogically to other men, who are to us other selves. And having seen that sensation and motion follow upon excitation of the brain, and are suspended or destroyed by paralysis of the brain, we are justified in restoring our thought-cells to their proud creative eminence, and in proclaiming that they constitute this 'active basis of thought'; that they think, and therefore exist."

In an earlier paper on "Scientific Idealism," included in the "Further Reliques," Miss Naden says :—

"For the present I must be content to plagiarize from Descartes, and to say of the cerebrum, '*Cogitat ergo est.*' It can appear to us only phenomenally, and we cannot speak of it otherwise than in terms of phenomena; but here, at least, we are forced to assume an underlying *noumenon*, while renouncing the vain hope of penetrating to its essential nature by reason or intuition."

So, after all, the thought-cells which have been restored to their proud creative eminence and proclaimed as constituting the "active basis of thought," turn out to be phenomena like the rest of the "realistic drama," and "even the vibrations supposed to impinge on the surface of the body, and the molecular tumult propagated along the nerves, are merely convenient intellectual representations of the unknown"—to which Dr. Lewins adds in a footnote, "and nothing until asselfed," without, however, explaining how "nothing" in the process of asselfment becomes something.

Such, so far as we understand it, is hylo-idealism. In it one recognizes an old friend under a new name. It would seem that Miss Naden admitted with reluctance the phenomenal nature of the all-creative thought-cells.

As it is, her views are hardly consistent. She generally appears to regard the organism or the thought-cells as the only reality—a reality set over against, and in marked contradistinction to, the "realistic drama" of the surrounding universe. But, in the passage above indicated, she admits the phenomenal nature of the thought-cells, and confesses an unknown *noumenon*. We do not think that Miss Naden's philosophy had reached its final form when Death so untimely snatched her hence.

C. Ll. M.

PHYSIOGRAPHY.

The Realm of Nature. An Outline of Physiography. By Hugh Robert Mill, D.Sc. Edin., F.R.S.E. (London: John Murray, 1892.)

THE scope of physiography has perhaps been more misunderstood than that of any other subject. It had its birth in the Government system of science examinations, and the new name was applied to distinguish the subject from the older and narrower one of physical geography, which was so widely taught in elementary schools.

In the great majority of the text-books which have hitherto appeared, the authors have rigidly followed the lines laid down in the syllabus of the Science and Art Department, and have generally contented themselves with stating facts, more or less accurate, without properly considering their inter-relation. It is a great relief to turn from these to the book before us. It is written independently of all examinations, and in it the true place of physiography among the sciences is clearly defined.

The book is one of the University Extension manuals, of which the editor (Prof. Knight) says: "Their aim is rather to educate than to inform." No better subject could therefore have been selected as one of such a series, and Dr. Mill fully bears this aim in mind. Indeed, in this respect the book is worthy to be compared with the "Introductory" science primer written by Prof. Huxley some years ago. The opening chapter deals with such matters as "Nature," "Science," "Use of the Senses," "Reason," "Common Sense," "Cause and Effect, and 'Natural Laws,'" all of which are set forth very clearly, and made deeply interesting. For the benefit of those who may have doubts as to the meaning of physiography, we may quote the following from this chapter:—

"Physiography means literally the description of Nature (p. 1). . . . It describes the substance, form, arrangement, and changes of all the real things of Nature in their relations to each other, giving prominence to comprehensive principles rather than to isolated facts (p. 3). . . . In order to have a just conception of the universe, the results of specialized research must be fitted harmoniously together. This is the function of physiography, which has, consequently, a unique value in mental training, being at once an introduction to all the sciences and a summing up of their results. It enables a beginner to obtain a quicker insight into any of the special sciences and a fuller grasp of it, while, at the same time, a student versed in any one special science is enabled to appreciate far more fully than an unversed one its relation to all others and to the system of the universe" (p. 13).

Succeeding chapters deal with subjects somewhat after the order of the syllabus of the Science and Art Department. It is only necessary to say of these that they are excellent, and that they have been revised by well-known specialists, including such names as Prof. Tait, Dr. Copeland, and Dr. John Murray. In chapter xvi. there is an admirable account of "Life and Living Creatures," in which is given a good outline of the classification, distribution, and functions of animals and plants. The final chapter is reserved for "Man in Nature," and deals with the distribution of the various types of humanity, the effects of environment, migrations, and man's power of altering the course of Nature's works. A useful list of memoirs and books is appended to each chapter.

The book is fully illustrated, chiefly by new diagrams, and there are nineteen beautiful maps, which have been specially prepared by Mr. Bartholomew, whose competence for such work is well known. These maps form an important feature of the book, and illustrate, amongst other things, earthquake regions, isotherms, rainfall, and the evolution of continents.

The whole book shows signs of the greatest possible care in preparation, and it is not an easy matter to suggest improvements. It is a very valuable contribution to the literature of the subject, and we trust that it will meet with the appreciation it deserves. It is admirably adapted for all thoughtful persons desiring an insight into scientific methods; and although not intended as a book for use in schools, all teachers and students of the subject will do well to make themselves acquainted with its contents.

A. F.

OUR BOOK SHELF.

Grasses of the South-West. Plates and Descriptions of the Grasses of the Desert Region of Western Texas, New Mexico, Arizona, and Southern California. Part II. By Dr. George Vasey, Botanist, Department of Agriculture. 50 Plates, with descriptive Letterpress. (Washington: Government Printing Office, 1891.)

THE first part of this work was issued in October 1890, and duly noticed in our columns. It also contained 50 plates. It is a pity that in this second part the numbering from 1 to 50 is begun over again. It would have been much more easy to cite the plates continuously from 1 to 100. The present part goes all through the series of tribes again, and includes 4 species of Paspalum, 3 of Stipa, 4 of Muhlenbergia, 5 of Sporobolus, 9 of Triodia, 5 of Diplachne, and 4 of Eragostis. Most of the species are endemic, and very few of them have been figured before. None of them are British species, but one of the Stipas is a variety of the common European feather-grass (*Stipa pennata*). A good general handbook of grasses is one of the books most urgently wanted both by systematic botanists and agriculturists; and the United States Department of Agriculture is doing a very useful work in bringing out these bulletins. The next two bulletins are to be devoted to the grasses of the Pacific slope, and the four numbers will bind up into one volume, containing 200 figures.

Sporting Sketches in South America. By Admiral Kennedy. (London: R. H. Porter, 1892.)

THE sketches presented in this volume appeared originally in *Land and Water*, and are now reprinted with only such slight changes as "time and circumstances have rendered necessary." They are very pleasant reading, and should be of considerable service to naval officers, yachtsmen, and sportsmen who may visit South America. The commission of the *Ruby* in South American waters extended over three and a half years, and during that time the officers seem to have missed no opportunity of indulging their taste for sport. The game killed amounted to 13,349 head. What was killed was never wasted, for there were on board 250 persons to be fed. Although Admiral Kennedy has much to say that will be especially interesting to sportsmen, they are by no means the only class of readers to whom he appeals. He visited many districts which are well worthy of being described, and the impressions they produced upon him are invariably recorded in a fresh, simple, and straightforward style. The text is illustrated with copies of some pen-and-ink sketches by the author.

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.*]

Cirques.

PERMIT me, while thanking Mr. I. Russell for his friendly criticism of my views as to the origin of *cirques* (p. 317), and for a copy of his memoir on the Mono Valley, in which his own are developed, to indicate the reasons why I remain unconvinced.

(1) The first part of his criticism appears to me to be wide of the mark. I never said that the three conditions, quoted by him, were *necessary* for the formation of *cirques*, only that they were "the most favourable." I have described two *cirques* in granite (*Geol. Mag.*, 1871, p. 535), and seen them in other crystalline rocks.

(2) I never said that other topographical features might not be associated with those dispositions of rocks, which are often found in *cirques*. I stated that for the production of *cirques*, not only certain materials, but also a particular machinery, were required. Lines of cliffs, ravines, alcoves, corries, are numerous in the Alps: *cirques* are comparatively rare.

(3) The instances described in my original paper (Quart. Journ. Geol. Soc., 1871, p. 312) were called *cirques* at that date, and, I believe, still retain the name. They are in all essential respects identical with Gavarnie and similar places in the Pyrenees, which are always called *cirques*. Hence, it appears to me, Mr. Russell cannot elude my arguments by proposing a new terminology. Moreover, to call these places alcoves, is to misuse the word. An alcove holds a bed, or screens two lovers in earnest conference from over-curious eyes—it is a small affair. The diameters of the Alpine *cirques* are measured by hundreds of yards; their walls by hundreds of feet. Mr. Russell's studies in the Appalachians (unless I misunderstand him through ignorance of the locality) appear to me to indicate no more than that (as I have repeatedly affirmed) no real demarcation can be drawn between *cirques*, corries, and bowl-like heads of valleys. Of each I have seen examples, little as well as big. The formation of an alcove, as described by him, is only a special case of an action, identical in kind (but different in degree and environment) with that by which the above-named are produced. As it seems to me, he proves, not that *cirques* and alcoves are genetically distinct, but that they are genetically identical. One stream will make an alcove, many a *cirque*.

(4) I have never denied that under certain conditions a basin (generally quite shallow) may be scooped out by ice-action on the floor of a corrie, but the peculiar "tooth-drawing" action of the base of the *névé*, postulated by Mr. Russell, appears to me only an hypothesis. I stated, when it was propounded by Mr. Helland, that I knew of no evidence in its favour, and much in opposition (*Geol. Mag.*, 1877, p. 273); I repeat the same now. Wherever I have seen the bottom of a corrie uncovered, the rock has been smooth and ice-worn, not rough with the sockets of extracted "plugs" of rock.

(5) As a *bergschrund* usually narrows in descending, the tension of the ice should be at a minimum at the base of the *névé*, and I do not believe that atmospheric cold is appreciably more potent there than at the bed of a glacier. Moreover, as the ice must move more slowly in the *névé* basin than in the glacier, the former standing to the latter in the relation of a pond to its effluent, the friction there should be least.

(6) Some of the Alpine *cirques* lie rather low, and are *not* in a position where glaciers would first appear, as is shown by the fact that they are now full 3000 feet below the snow-line. Perhaps, however, Mr. Russell supposes rock wrenched away to a depth of quite a thousand yards. If so, I have nothing to add to what I wrote in 1871 and 1877. All that I know of the Alps and of other mountains is opposed to any such notion.

(7) Above three at least of the *cirques*, described by me, the cliffs rose in inaccessible steeps to the very summit of the range. From the highest peak of the Diablerets one could almost throw a stone on to the floor of the Creux de Champ, a full mile below; the two *cirques* near the Surenen Pass are immediately under the crest of the range (as shown in my diagram). Yet *there are the streams* fed by snow-ledges, which, though comparatively narrow, keep them still at work. Doubtless, in some cases the recession of the walls may bring to an end the work of excavation. But even in Egypt, in a *cirque*-like glen beneath a narrow crest, Mr. Jukes-Browne found evidence of streamlets (*Geol. Mag.*, 1877, p. 477); and I may cite Herr J. Walther (*Abhand k. Sächs. Gesellsch.*, xvi. 345) for the occurrence of "amphitheater oder circusthäler" in that country, though he attributes more to the action of wind than I should venture to do.

I might continue, but it may suffice to say that the result of twenty years' experience has been increased confidence in the general accuracy of the views expressed by me in 1871. These, it appears to me, Mr. Russell combats only by attempting a distinction, which I believe to be non-existent, and devising a method of glacier-erosion, which I believe to be not only mechanically impossible, but also contrary to the facts of Nature.

T. G. BONNEY.

Bedford College and the Gresham University.

YOU have given publicity to various communications on the subject of the Albert or Gresham University; may we therefore request that you will extend a like favour to us, and insert a brief statement from one of the Colleges for Women, deeply interested in everything affecting higher education in and for London?

The following are some of the grounds on which the Bedford College, London, opposes the Draft Charter:—

(1) That the Bedford College, London, for Women, provides a complete academical course in the two faculties of arts and science, and therefore feels that from the very outset it is entitled to be included in any proposed University for London, on an equal footing with University and King's Colleges, with due representation on the Council. For such immediate affiliation the Draft Charter does not make any provision.

(2) That should the Draft Charter become law, a University would be created which, while exercising only some of the functions of a Teaching University, would do this in such a manner as to effectually prevent the foundation of any Teaching University which should be capable of expanding to the ever-growing demands of the Metropolis.

(3) That while the Act of 1871 abolished all ecclesiastical tests in the Universities of Oxford, Cambridge, Durham, and all Colleges therein to such an extent that all denominational Colleges are separate from the Universities, this Draft Charter of the Albert University allows one of its component Colleges to impose ecclesiastical restrictions. The Council, therefore, in its petition to both Houses of Parliament claims that the Bedford College be made an original constituent College in any new University for London, with due representation in the governing body, and prays that the present Charter be not granted unless so amended as to be far more comprehensive, more adequate to the present and future needs of the Metropolis, and free from all ecclesiastical restrictions.

W. J. RUSSELL, Chairman of Council.
LUCY J. RUSSELL, Honorary Secretary.

Bedford College, London, February 19.

The Implications of Science.

CAN you allow me space to reply to Miss Jones's courteous letter (p. 366), which I shall have the more pleasure in doing as I hope to be able to clear up the points in dispute between us?

I think it is clear—Miss Jones at least admits so much—that some part of the meaning of a term is determined arbitrarily; either by public opinion for general use, or by the individual reasoner, if he wishes to use the term for some special purpose of his own. For this does not imply that there ever was a formal contract as to the meanings of terms. The fact that terms do not mean the same to people of different nations is enough to show that the determination of the meaning by each nation is, in part at least, essentially arbitrary. It is also, I think, clear that there is a part of the meaning of some terms at least which is not arbitrary, when once the arbitrary part has been settled. There is a slight ambiguity about my use of the word "definition," which, however, when once noted, need cause no confusion, as the context will always show in which sense I am using it. Commonly a "definition" denotes an assertion which determines the arbitrary part of the meaning of a term; but when I speak of "the definition" as opposed to "the import" of a term, I mean not this assertion itself, but that part of the meaning of the term which it determines. This explains how it is that I do not regard "the definition" of a term (as opposed to its import) as a *thing*. Even if it is clearly conceived in the mind it is only an abstract idea; but the point is that it is not necessary that it should be conceived at all. Thus it is not necessary that a definition by connotation should be "of something," if that means that the term should have denotation. Even if the term has any denotation, *i.e.* if the mind grasps it as denoting a thing or idea, this denotation must, in a symbolic argument, be regarded as merely "accidental clothing."

Just as some part of the meaning of a term is arbitrary and some part not, so in any system of logic some part of the system is arbitrary and some part not. Now, either the denotation or the connotation of a term may be laid down arbitrarily, but *the connection between the two is not arbitrary*. One cannot arbitrarily lay it down that "such a thing possesses such attributes." If, therefore, a definition is to be an arbitrary assertion, it must only lay down either the denotation or the connotation of a term, and not parts of each. It would, of course, be logically permissible to use the word "definition" in a different sense, but then definitions would not be arbitrary assertions. This is exactly what is done if the assertion "Two straight lines cannot inclose a space" is called a definition by denotation; and therefore, in my system of logic, I cannot admit it to *be* a definition. This is one of the arbitrary features of my system. But it is not arbitrary when I say that in such assertions as "Nothing can both be and not be," and "Twice two is four," there is nothing absolute at all. The truth of these assertions is determined entirely by the arbitrary parts of the meanings of their terms. Miss Jones says: "If definitions were purely arbitrary, as Mr. Dixon holds, what would prevent my saying that *Four* $(1+1+1)$ *means twice two* $(1+1)+(1+1)$?" Precisely; this is exactly the point. *There would be nothing at all to prevent it.* Boole actually did this very thing, as his law of indices in his mathematical analysis of logic. According to his law $a^{1+1+1} = a^{(1+1)+(1+1)}$.

There remain one or two minor points to be answered. As to induction, I think Miss Jones's view is substantially the same as mine. Of course it is possible to set out an inductive generalization in a syllogistic form as Miss Jones does; only the whole of the induction is then contained in the assumption of the major premiss—the syllogism itself is in no sense inductive.

If Miss Jones thinks the truth of the formula $(a+b)^2 = a^2+2ab+b^2$ is deduced by generalization from a single concrete instance, how does she explain the fact that sometimes $(a+b)^2=0$, as in Grassmann's "Aeussere Multiplication," and sometimes $(a+b)^2=a+b$, as in Boole's logic?

I see nothing to differ from in the paragraph about S and P; except perhaps that if I define such a term as "metal" by denotation I do *not* say "This and all other things like it in certain respects" are metal. That would be mixing up denotation and connotation in one definition. I say only "This, and this, and this, . . . are metal."

The explanation of the point about the mathematical truths is simply that I do not consider the assertion "Two straight lines cannot inclose a space" as a mathematical truth at all, if "straight line" is defined by denotation. I certainly believe it to be true, but its truth is not of the same nature as that of the assertion "Twice two is four," or even "*Cogito, ergo sum*"; it is neither a truism nor a necessary truth, in my senses of those terms, but can only be established by induction.

EDWARD T. DIXON.

Trinity College, Cambridge, February 19.

The Value of Useless Studies.

IT is rather surprising that Prof. Ayrton should indulge in covert sneers at Universities for devoting themselves to useless studies. It certainly ill becomes one whose life is bound up with electrical science, which is of such recent growth that nobody can pretend to forget how it owes its origin to those who studied it while useless. If Universities do not study useless subjects, who will? Once a subject becomes useful, it may very well be left to schools and technical colleges, to patent-mongers, and the trade. Mr. Bury is, on the other hand, mistaken in two respects. That a subject is useless is hardly worth considering as a recommendation for its being made *compulsory* on students. There are too many useless subjects for that. The great objection to compulsory Greek is that it is the principal stumbling-stone in the way of any literature being studied by ordinary University students. The Bible produced very little effect until it was read in translations; and the danger of a pagan revival, if ancient literature were studied without the obstruction of difficult languages, is the best reason for insisting on these languages in a Christian University. The second mistake of Mr. Bury is that it is any part of the business of a Universtiy to *teach*. Universities should certainly give facilities for students to *learn*. It is the business of the students to learn. If they are so ill prepared that they have not acquired the art of learning, they should go to a college or school or private teachers, and get taught; for teaching is the business of these institutions and persons. The business of Universities is to advance culture and knowledge, and to afford students an opportunity of learning how to do this. Prof. Ayrton, by omission rather than by commission, seems utterly unable to appreciate the value of literature for its own sake. How can all this fierce toil he extols so justly advance a lot of savages?

GEO. FRAS. FITZGERALD.

Trinity College, Dublin, February 13.

The Nickel Heat Engine.

IN trying Mr. Smith's experiment on the rotation of nickel (NATURE, January 28, p. 294), I find my disk has a more complex action.

It will be remembered that the nickel is mounted on a vertical axis, and has the poles of an electro-magnet put near two points, A and B, 90° apart. Let C be any point in the larger arc, and suppose heat is applied to A.

I find that at first the disk moves in the direction ACB with a hesitating, uneven motion; it revolves several times, and almost develops into a steady rotation, when it beats back, oscillates for a while, and goes off with a brisk even spin in the direction BCA. After about two minutes this fails, and a new start is attempted the opposite way, but there is little more movement, until time has been given for cooling.

It makes no difference at which pole the heat is applied. The disk is 50 mm. diameter, rather less than 1 mm. thick, and is covered with lamp-black.

W. B. CROFT.

Winchester College, February 12.

THE UNIVERSITY OF LONDON.

PROBABLY before this letter is in type the Charter of the Albert, or, as it is now to be called, the Gresham, University, will have been presented to Parliament. What will be its ultimate fate remains to be seen. But I have sat through a long night's debate in the House of Commons to see a Government turned out in the small hours of the morning on a University question. I do not suppose that the proposed Charter will raise an issue of that importance. But I think that the Government may not be unmindful of past history, and, in what is probably the last session of a moribund Parliament, may not be willing to push very vigorously what is perhaps the crudest scheme of University organization which has ever been proposed in this country.

Before Parliament takes so serious a step as founding a new educational institution, the question may fairly be asked, On what ground is it necessary? Everyone knows that we have a University in London which is a State institution; and one may not unreasonably inquire, In what respects is it inadequate to its duties; and, if so, why cannot its defects be remedied without beginning the work all over again?

To neither inquiry has, it seems to me, any very satisfactory reply at present been given. The reason perhaps is that beside the ostensible reasons for the Charter which are put forward, there are others in the background which are less conveniently avowable, and which therefore it is anything but easy to meet.

Of the former, the most important is that the existing University does not teach. This is an undoubted fact. If, however, it were provided with a body of Professors, as has often been suggested, it might teach just as much as Oxford and Cambridge do. That defect is clearly, then, not irremediable. But, on a little further investigation, it turns out that something more than teaching is meant. What is desired is that the teachers shall also have the control of the examinations. What is wanted is to introduce into London the teacher-examination system, which is said to be the distinctive feature of Universities in Germany. To quote an able article in the *Standard* of January 19:—

"There must be a Teaching University in order that the teachers might control the studies, and the degrees be a recognition, not of mere knowledge (which might or might not be superficial), but of sound intellectual training."

Now, I have discussed this position at some length in the pages of NATURE, and I need not repeat what I have already said. But it may be pointed out that, though the teacher-examiner system once obtained to some extent in the British Isles, it has almost entirely been abandoned. As Lord Sherbrooke used to remark, "Teachers no longer sample their own goods." Their doing so led, I think it cannot be doubted, to serious abuses, the possible nature of which is sufficiently obvious. Even in the English Universities, some of the Colleges practically had the entire control of the education of their undergraduates; but they deliberately abandoned this exclusive privilege, and handed over the business of testing the performance of their pupils to their respective Universities; in other words, they intrusted the business of sampling their goods to those who had nothing to do with their manufacture. The Scotch Universities most closely approximated to the German system; but here, again, external examiners have been introduced, who undoubtedly were intended to act as a check upon the Professors and their teaching.

It can thus, I think, be shown to demonstration that the system which obtains in Burlington Gardens is not really so anomalous as it is made to appear. The teacher-examiner cry is, then, of the nature of a reaction; and though to a certain extent I am not without sympathy with it, I am not sure that, on the whole, it is not a mischievous reaction.

Now, when a great institution like the University of London is attacked, one attaches naturally some importance to the quarter from which the attack comes. I quite admit that the University, perhaps from an excessive sense of its own dignity, has rather allowed the case against it to go by default, and has done little in its own defence. But the world outside may judge for itself. I am prepared to contend that there is no examining body in the world which does its work in a more efficient and conscientious way. Its examination rooms are crowded; it commands the services as examiners of the most distinguished teachers of the country; and whether in science, law, or medicine, amongst those who have successfully submitted to its ordeals are to be found the names of some of the most distinguished men of the time. It seems clear, then, that the attack on the University would not come from either the students or the public. It can hardly be doubted that the latter is more than satisfied. When parents allow their sons to embark in University studies, they like to get something tangible as a result; and in a degree at Burlington Gardens they undoubtedly do get something the value of which is generally accepted.

The complaint comes, however, from the teachers; and this not in the provinces, but in London alone. Their case was put before the Commission by Prof. Lankester in a very striking and able way. I do not doubt that his evidence has produced a very powerful effect. But having studied it, I am bound to say that, having been a graduate, an examiner, and a member of the Senate of the University, I cannot accept all he says as giving a fair account of the inner working of the system. The burden of his complaint is that the teacher is unreasonably fettered in giving the best and most modern kind of teaching by the restrictions imposed upon it by the University. In so far as there is ground for the complaint, I think it admits of remedy. But I cannot refrain from remarking that the provincial teachers who send candidates to the London examinations—and who, I suppose, are not, on the average, less competent and enthusiastic than their London colleagues—do not, as far as I can ascertain, in any way sympathize with their complaints. On the other hand, Prof. Lankester was for ten years an examiner in the University, and when he last left office I am much mistaken if he had not succeeded in moulding the curriculum in his subject practically into entire accordance with his own views. And even under the system as it exists I observe in his evidence that he admitted to Sir William Thomson, that "a teacher may, with judiciousness, of course, and common-sense in his teaching, teach the best that he knows."

I cannot keep feeling, then, that in the outcry raised by some of the London teachers there is something a little unreal. The existing system neither works so badly, nor is it in itself so bad, as it is represented to be.

The Privy Council, however, appears to have thought differently, and has set itself to the task of creating a Teaching University for London. And this is what Parliament is asked to sanction. The fatal objection to the scheme is that the title is a misnomer; the proposed University is, as I have already pointed out in NATURE, in effect, no more a Teaching University than the existing institution.

It is perfectly clear that by a Teaching University is meant, by those who understand what they are talking about, a University of the German type, governed and administered throughout by the professorial body. If I understand his views, this is the solution of the difficulty which Prof. Karl Pearson desires. I am far from saying that I am opposed to the establishment of such an institution. It would certainly be an interesting experiment, and it might be a great success. It might be created out of University College standing alone; and for my own part I have always wondered that that distinguished place of study has never sought such a transformation. It would, of course, require a profound reconstruction of its constitution, and the total elimination of what may be called the lay element in its government. Or it might be effected, though with more difficulty, if University and King's Colleges could be induced to fuse. Prof. Lankester contemplated such a possibility; but it is probably as hopeless as to try to get anything but an emulsion out of oil and water.

The organization of such a Teaching University is not without its drawbacks, and I am not at all sure that London would be the best seat for one. The system is only a success in Germany because Universities are very numerous, and the number of students in each for the most part not very large. The essence of the system, as I understand it, is the close personal contact of teacher and student. Each gets saturated, so to speak, with the other; at the end of the curriculum the Professor knows, or thinks he does, all that the pupil has acquired, and gives him his degree accordingly. I paint the picture in its most attractive colours. And for pupils of a certain type I believe the system to be excellent. But for the large majority I am not so sure; and I think that under such a system a little independent examination would often act as a considerable surprise both to teacher and taught.

It seems to me that the fact is often lost sight of that education in all its forms is rather a tedious business. What the teacher has to deal with are not merely the enthusiastic few who march with joy alongside their instructor, and scarcely see the obstacles that bestrew the path, but the large mass, headed at the best with stolid industry in front, to tail off with incorrigible straggling behind. Here the discipline of the examination-room, backed by parental wrath at failure, seems to me well-nigh indispensable. Even in such cases as these, no doubt personal contact with the teacher can do much; but then the classes must be small. The limit is soon reached when the individual powers of the teacher, however energetic, are reduced to a vanishing quantity. I have found myself that in a class of forty I reached the extreme limit on which I could hope to produce any effective direct impression. Frankly, I do not believe that for large classes, such as might be expected in London, the teacher-examiner system has any merit at all. I think myself that, if the teacher directs and organizes the teaching to the best of his powers, he may, with distinct advantage, leave the testing of the results to other hands. I know that this is what I should prefer myself; and I speak with the experience of a man who has spent, and not wholly without success, some of the happiest years of his life in the work of teaching.

But I certainly agree that the teacher should have something to say in the business of examining. He does, in London, already have a good deal to say as a matter of fact, but very indirectly. I would remedy this by giving all teachers of University rank a definite status in the University.

The bare statement of the fact that the University of London is merely an examining body does very inadequate justice to the work it has accomplished. It has undoubtedly stimulated and disciplined the studies of vast numbers of persons whom the older Universities would never have touched. And it has reacted on the teaching given in London and elsewhere by insisting on a progressively high standard. The educational influence has been consequently far-reaching, and in my judgment not small. This is effected by the schedules or syllabuses which are prescribed by the Senate and from time to time are varied as the progress of instruction in each subject makes change desirable. It is notorious that by this means the University of London has compelled the teaching bodies which send it candidates to develop the efficiency of their methods of instruction.

I am quite prepared to admit that in theory the machinery by which this result is brought about is not perfect. But in practice, like so much else in this country, it works tolerably well. In some of the evidence given before the Commission it is almost implied that the Senate draws up the schedules itself. But, having been both an examiner and a member of the Senate, I know something of the process. Generally speaking, it is something like this. An examiner finds that an existing schedule is scarcely in touch with the best current teaching. He communicates his opinion to the Senate, and suggests alterations. The Senate generally calls to its assistance one or more of the past examiners in the subject, and also any persons of acknowledged competence whose judgment will probably lead them to a right decision. The result is that, if the alterations proposed by the examiner are found to meet with general approval, they are adopted. In time the process is repeated; and probably no year ever passes without something of the kind being done. Practically, the work is well done, and the general esteem in which the degrees given at Burlington Gardens are held appears to me a tolerable proof of the fact.

It is evident that what I have described is accomplished by what may be called semi-official means. I should prefer myself to have it done by a more definite organization. If Faculties were constituted in the University, the most experienced teachers might meet to settle the schedules in the best possible way. Even then some would probably not be wholly satisfied; but they would have at least the opportunity of explaining their views to their fellow-teachers, and, if reasonable, it is improbable that they would not meet with some recognition.

Another very important result would flow from the organization of the Faculties. One of the absurdest things about the proposed Charter is that, apart from medicine, except University and King's Colleges, it ignores the existence of any other educational institutions which either at present exist, or are certain hereafter to be created, for the educational needs of so vast a city as London. A great evil at the present time is the isolation of such institutions. They do not recognize each other's existence or work in any way in conjunction. If their teachers, under the *ægis* of the central University, could be brought together to confer on educational matters, it can hardly be doubted that instruction in London would be better organized, gaps filled up, and wasteful overlapping obviated. All this might be done without any interference with the autonomy of the separate institutions, and by men exchanging ideas in conference and discussion.

There is one point on which the new Charter has been attacked with which I do not in any way sympathize. The inclusion of King's College in the proposed University is objected to on the ground of its being a denominational institution. But in Australia and Canada the association of denominational Colleges with the Universities is found a simple and effective solution of difficulties which for a long time will probably be insuperable in any other way. One of the most interesting things in the history of English Universities in modern times has been the removal to them of Nonconformist Colleges. For my part, I can see no possible objection to their being admitted ultimately to full University privileges. If the new University is to be accepted, it is idle to object to the admission of King's College, or to demand that it should abandon its characteristic features as the price of admission.

To sum up the position. The present University is denounced because it fetters the teaching and is not in touch with it: this turns out to be rather a matter of form than of fact. To remedy this a new Examining University is to be created, on much the same lines as that already existing, which in its turn is to be allowed to go on exactly as before. Surely, in the whole history of the reform of institutions, nothing so futile was ever proposed.

The real academic need of London is left perfectly untouched. This is the organization of the higher University teaching. I am more and more convinced that a distinction ought to be drawn between what may be called ante-graduate and post-graduate study. The former, leading up to the ordinary Bachelor's degree, may very properly be left to the Colleges. More than this, without a vast increase of endowment and staff, I do not see how they can accomplish. I regard a student who has taken his Bachelor's degree as having learnt the technical language of his study. He may then, in a considerable number of cases, devote himself to original inquiry. And this the University of London encourages him to do, as examination is no longer compulsory for the Doctorate. It may be, and in fact is, obtained by original work embodied in a thesis. I confess I should like to see the University of

London provided with a body of superior Professors, who would not merely add to its distinction by their own labours and public instruction, but would guide the studies and researches of the young graduates. With the right men for Professors, and such picked students as the vast area of London would supply, I do not doubt that the reputation of the University of London would at no distant date rival that of any in the world. I hope Prof. Karl Pearson will forgive me if I appropriate in this connection a portion of his letter in the *Academy* of December 19:—

"The professorships ought to be the best in England, and the chief posts, at any rate, might remain in the gift of the Crown; the laboratories and libraries ought to be the best equipped in the Kingdom, and the University ought to draw students and investigators, not only from the five millions of London, but from the Greater Britain over the seas. Such a University would not only be able to retain in London men like Burdon-Sanderson, Seeley, Gardiner, Sylvester, and Lankester, but it would bring others there."

With the main idea expressed in these words with so much enthusiasm I most heartily agree. But I think those understand their countrymen and the possibilities of things best who would use, as I would, the existing University as a foundation. It has deserved well of the State; it has done its appointed work in the past well: instead of abusing it, we should strive to remove its defects, and give it higher work to do.

Two possible objections to my proposition may be considered. It may be said that such professorships are not wanted in London because such professorial teaching is supplied by Oxford and Cambridge. My reply is, that only a small portion of the population pass through those Universities, and London would draw from a much larger field. Again, it may be objected that the Professors of the existing teaching institutions in London would object to the creation of posts of a superior grade. On the other hand, such posts might stimulate ambition; they would be the prizes of the academic career. To object to them seems to me as reasonable as for a stuff-gownsman to object to the existence of judges, or for a curate to that of bishops.

The depletion of London of its most distinguished teachers, which continually goes on, is a real loss to its intellectual life. Dr. Dollinger says:—

"The force which moves the world, that which brings on the important crises in the history of mankind, is not to be found in material interests and passions, but in the great ideas which it is the business of Universities to work out."

Why should London, of all places in the world, dismiss from its midst, as it has long continued to do, those whose gift it is to open up most successfully new territory in the unknown world of knowledge? Yet the depletion goes on. Not one of the five officers of the Royal Society is at the moment resident in London. The President attends its meetings from Glasgow, and the Senior Secretary from Cambridge.

I must say a word about Gresham College. The promoters of the Albert University, whose untiring energy would be invaluable in a better cause, have, no doubt, done a clever thing in securing the adhesion of this obsolete institution to their scheme. Years ago I attended one of the prelections with two friends. The Dean of Manchester favoured, very unwillingly, an audience composed of ourselves and a few casual passers-by, whipped in apparently by the beadle, with a demonstration of Euclid I. 47, in the Latin tongue. Yet the institution which had descended to this mere husk of formality had once, if tradition is to be believed, been one of the most famous seats of learning in the world, and Francis I. is said, in emulation of it, to have founded the Collège de France in Paris. I believe the present Gresham Professors are not quite so sleepy as they were; but the contrast between the two rival institutions is more than melancholy. That Gresham College can nowadays clothe its dry bones and live is more than doubtful. It is, no doubt, only too glad to undertake the "teaching" department of the new University, which proposes to carry on its examining work in the empty building in Basinghall Street.

Kew, February 20. W. T. THISELTON-DYER.

A PRELIMINARY STATEMENT OF AN INVESTIGATION OF THE DATES OF SOME OF THE GREEK TEMPLES AS DERIVED FROM THEIR ORIENTATION.[1]

MR. LOCKYER has made out I think quite satisfactorily that the Egyptian temples were so oriented that the rising or setting of some conspicuous star on or near the axis of the temple, and visible from the adytum, would give warning of sunrise; and he applied to me for particulars of Greek temples for the purpose of seeing if there was any analogy, and the comparison appeared to promise a favourable result. Mr. Lockyer had found, before he had proceeded far in these studies, that he had been anticipated to a considerable extent by Herr Nissen, of Bonn, who has published several articles on the subject in the *Rheinisches Museum of Philologie*, and has brought within his scope both the Egyptian and the Greek temples. There is room, however, in the inquiry for a distinct work on the Greek temples, and especially with the help of more exact measurements of the orientation angles than Herr Nissen has made use of; as he appears to have contented himself with magnetic bearings—which are liable to considerable local variations, which are sufficient in an inquiry like the present to vitiate many of the conclusions that may be founded on such measurements—and there is a want of recognition of the influence of an elevated horizon. I had taken, in several instances, astronomic observations with a view to the more exact orientation of different temples, but something more is wanted even in the case of most of these—namely, the apparent altitudes of the mountains in the directions of the axes of the temples. I wish also to add that, but for Mr. Lockyer's suggestion, I should probably not have carried the inqury further than I already had done.

The great value of the inquiry lies in this: that it offers a means of determining, within tolerably close limits, the date of the foundation of a temple—not, perhaps, in most cases (although in some I believe it does) of the very structure which we now see, but of an earlier foundation on the same site. The key to the chronology lies in the movement of the stars with reference to the local horizon, owing to what is called the precession of the equinoxes. The object the ancients had in using the stars was to employ their rising and setting as a clock to give warning of the sunrise, so that on the special feast days the priests should have timely notice for preparing the sacrifice or ceremonial, whatever it may have been:

> "Spectans orientia solis
> Lumina *rite* cavis undam de flumine palmis
> Sustulit," &c.

The inquiry, even in its present state, is sufficient to establish a very high probability that the principle is a true one. There is nothing vague about it. It has to be kept within very severe limits, and it holds good nevertheless.

No stars can be accepted except from among the brightest, unless conspicuous star groups may have been used instead. Again, of single stars, only such can have been used for orientation in Greek temples which during

[1] Being the substance of a paper read to the Society of Antiquaries on February 18, 1892.

a period not incompatible with reasonable archæology rose or set very near the line of the sun's course at some period of the year; and a further restriction is this, that the rising or setting must be just so far in advance of sunrise as to enable the star to be seen from the adytum of the temple, and, at the same time, not preceding it by any longer interval than is necessary.

If, in addition to this, we find, as is frequently the case in the Egyptian temples, and is not without parallel in Greece, that as the star to whose point of rising or setting the axis of a temple was first aimed worked away from its then position by *precession*, either the doorway of the temple was altered, or a new temple founded alongside, so as to retain the desired observation; and in every case of such new temple being so built it is found to have followed the same cult as the original; if, in addition to this, in different provinces temples are found of which the cult is known, and which are so planned as to be able to use the same star—with such decided differences of orientation, however, as were prescribed by latitude and the local circumstances of the surrounding heights—we obtain a further strong corroboration, and one that will in many cases be sufficient to determine the cult, where this has not been otherwise pointed out.

One further step requires to be taken to occupy the ground with perfect confidence—viz. to inquire what analogy is there between the days of the month when the sun would rise ushered in, as it may be said, by the temple's peculiar star, and the days of the festivals as derived from historical sources. In this comparison we must not expect a coincidence on every point.

The date of the temple foundation in many cases is pre-Homeric, whilst the basis of the historical account of the date of the feast is probably post-Persic. There may have been an interval of nearly 1000 years between the two, so that there is room for changes. Again, owing to their double system of reckoning months and years, considerable variation in the dates given by Mommsen, whose authority I mainly follow, is quite possible; and besides this, in some of the cases given below, the orientation day, if I may so call it, may be in fault one or possibly two days for want of the exact particulars of the site to which I have made allusion.

Firstly, speaking of Attic feasts, the great temple at Eleusis is an example very much to the point. The star which seems to have determined the orientation is Sirius, shining as it rose at midnight along the axis of the temple on September 14. The Eleusinian mysteries are stated to have commenced on the 16th of that month. In this case the sun was not looked for; the weird light of the star reflected from some combination of jewels was more likely to have been suited to the mysteries. It is perhaps less likely that this ceremony would have been changed than in most of the other cases.

The axis of the older Erechtheum had the central star of the fine constellation Aquarius setting heliacally on August 9.	The lesser Panathenaia, dated August 13–14.
Warning of the sunrise at Sunium was given by the setting of the Pleiades on October 20.	A feast to Minerva and Vulcan is dated October 30.
The star α Arietis rose heliacally to the older Olympieium at Athens, April 2, more than 1000 years B.C.	The feast Olympia is recorded for April 19 in later times.
The temple of Diana Brauronia on the Acropolis of Athens agreed with the rising of Aquarius (the central star ζ Aquarii in particular) on February 21 at the presumed date of its foundation.	The Little Mysteries were celebrated February 21.—N.B. A temple of Diana was in close connection with the great temple at Eleusis.

In the earliest times, as already explained, the stars were used as the only available clocks, but probably by the end of the sixth century, whether by the discovery of the behaviour of the stars or by the invention of the water-clock (which is recorded to have been used to some extent in the fifth century), or other approximate means of measuring time, the dependence on the stars alone had ceased; and the later temples, in Greece at any rate, appear to have been built without any reference to these.

At Athens this applies to the existing or new Erechtheum, the Theseum and the temple of Nike Apteros, which temples do not seem to have been built parallel to any old foundations. The old foundations under the Erechtheum have a very different angle. The sunrise, however, was considered in these just as much as before, for owing to the artistic instinct of the Greeks, they seem invariably to have secured for their principal festivals the fine effect of the first sunbeam on the statue; but as all the temples, whether old or new, admitted of two axial coincidences with the sunrise—one which might have a clock star (as it may be called) to announce the dawn; the other, except by rare accident, having none, the desired effect would have been attained on both occasions when the sun had the same declination.

It will be obvious that if the axis of a temple in any latitude had been directed due east (the horizon also being level), the rising sun would coincide with it at both the spring and autumn equinoxes. Similarly every other amplitude would have two solar coincidences (provided that is, in accordance with what has just been stated, the axis fell within the solstitial limits). When it had been found that the precessional movement had taken away the star from the direction of the axis, there would have been no reason for preferring one of these solar coincidences to the other, and the feast could have been shifted to a different date if it had been thought more convenient. It would appear that something of this sort may have taken place at Athens, for we find on the Acropolis the Archaic temple, which seems to have been intended originally for a vernal festival, offering its axis to the autumnal sunrise on the very day of the great Panathenaia in August.

The Chryselephantine statue of the Parthenon, which temple followed on the same lines as the earlier Hecatompedon (originally founded to follow the rising of the Pleiades after that constellation had deserted the Archaic temple alongside), was lighted up by the sunrise on the feast to the same goddess in August, the Synæcia, instead of some spring festival, for which both these temples seem at first to have been founded.

The temple at Sunium, already quoted for its October star-heralded festival to Minerva, was oriented also axially to the sun on February 21, the feast of the lesser mysteries.

Of temples of later foundation we have the following for which no suitable stars can be found:—

The Erechtheum, its sun axis days are March 2 and September 4, the latter being the date of the *Niceteria*, the special festival of that temple, supposed to record the celebrated contest between Minerva and Neptune, considered to have been on September 3. Another instance is the Theseum.

The *Thesea* are put down for October 8–9. The sunrise theory points out either March 7 or October 7. Does not this fact restore the disputed title of Theseus to this temple?

There cannot be so much known respecting the feasts in the other provinces.

The Olympic games were held, according to most authorities, soon after midsummer, but by others in the autumn. I quote on Nissen's authority the following:—

420 B.C., Sept. 14.	412 B.C., Sept. 12.
416 B.C., Aug. 31.	408 B.C., Sept. 2.

The heliacal rising of the star Spica (α Virginis) seems to belong to the Heræum, and indicates September 15.

The Isthmian games took place either in May or

August, according to the Olympiad. The axis of the temple at Corinth coincides with the sunrise on both those months, but to only one of them (that of May) is a star applicable.

Further confirmation of the truth of this general theory—namely, that the amplitudes of stars determined the orientations of temples—lies in the fact that in the majority of instances, at any rate, the same star belongs to the same cult. I am satisfied that this can be established for the Egyptian temples. In Greece we find the following.

The star α Arietis is the brightest star of the *first sign of the Zodiac*, and would therefore be peculiarly appropriate to a temple of Jupiter. The heliacal rising of this star agrees both with the Olympieium at Athens and that at Olympia. There is a considerable difference in the deviation of the axes of these two temples from the true east; but this is exactly accounted for by greater apparent altitude of Hymettus over the distant Mount Pholœ.

The Pleiades are common to the following temples of Minerva, viz. the Archaic temple on the Acropolis, the Hecatompedon, and Sunium. In the two former it is the rising, the latter the setting star.

α Virginis or Spica must have been supposed to be sacred to Juno. The Heræum at Olympia agrees exactly with this view, and the Argive Heræum can be referred to no other; but, as the foundations of the earlier Heræum are not now visible, the exactness of the coincidence cannot be thoroughly established without re-excavating part of the site. There is nothing, however, inconsistent to this view in what is known about it. The nomenclature of the temple of Juno at Girgenti rests on a rather weak historical basis; but Spica entirely supports it.

There must have been something in common between the temples at Corinth, Ægina, and Nemea. The two last, at any rate, are reputed temples of Jupiter, and I have reason to think that also the Temple of Jupiter at Girgenti agrees with the same star—namely, Antares.

Approximate dates derived from the orientation of Greek temples.

Star.	Name of temple.	Place.	Month and day.	Year of foundation. B.C.	Star rising or setting.	No.
Pleiades (η Tauri)	Archaic temple of Minerva	Athens	April 20	1495	R	1
	Hecatompedon temple of Minerva	Athens	April 25	1120	R	2
	Temple of Minerva, Sunium	Sunium	October 20	1125	S	3
Sirius	Temple of Ceres, Eleusis (for midnight mysteries)	Eleusis	September 14	1380	R	4
Fomalhaut (α Piscis Australis)	The same for sunrise	Eleusis	November 18	1350	S	5
Spica, *i.e.* α Virginis	The Heræum, Olympia	Olympia	September 15	1300	R	6
	The Heræum, Argos	Argos	February	about the same time	S	7
	The Heræum, Girgenti	Sicily	September 15	1180	R	8
α Arietis	Jupiter Olympius, Athens (temple attributed to Deucalion	Athens	April 1	1135	R	9
	Jupiter Olympius, Olympia	Olympia	April 3	760	R	10
Antares (α Scorpii)	Temple at Corinth	Corinth	May 1	700	S	11
	Jupiter Panhellenius, Ægina	Ægina	May 6	670	S	12
	Nemea, Temple of Jupiter	Nemea	similar to	two last	S	13
Aquarius (ζ Aquarii)	Oldest temple at Epidaurus (the Hiero)	Epidaurus	July 29	1270	S	14
	Older Erechtheum, Athens	Athens	August 9	920	S	15
	Diana Brauronia	Athens	February 21	750	R	16
δ Corvi (?)	Temple of Themis, Rhamnus	Rhamnus	September	about 1150	R	17
	Temple of Nemesis, Rhamnus	Rhamnus	September	780	R	18

Temples of later foundation for which no heliacal star has been found.

Star.	Name of temple.	Place.	Month and day.	Year of foundation. B.C.	Star rising or setting.	No.
	The Theseum	Athens				
	The new Erechtheum	Athens				
	The temple of Wingless Victory	Athens				

The above table of approximate results, which I have put together, must for the most part be considered preliminary, and subject to amendment when further particulars have been ascertained, which I am in hopes of being able to obtain in the course of the present season. Nevertheless, I do not think that as respects the examples mentioned in Nos. 1, 2, 9, 12, and 16, there will be much need of alteration, as of these I am already in possession, though not of all, yet of the most important measurements. Of the remainder I do not feel so confident, but there is still a good deal that can be pointed out in respect to some of them which is consistent with historical and architectural archæology.

Olympia must have been a sacred spot long before the Olympiads began to be dated, and the Heræum there appears to be the most archaic temple structure that exists in Greece. The date suggested by the orientation, 1300 B.C., does not seem unreasonable.

Then we come to the establishment of the Olympiads, which began 776 years B.C. Compare the date of the great Temple of Jupiter derived from its orientation, 760.

The temple at Corinth was thought by archæologists of the past generation to date from about the middle of the seventh century B.C. The date I get from its orientation is 700.

The temple at Ægina, it is evident from its architecture, is somewhat later than that at Corinth or the Olympian Jupiter. That is also the orientation view of the case. At the same time, I think that the interval between Nos. 11 and 12 ought to be more than thirty years. I rather expect that more exact measurements at Corinth will throw back somewhat the date of that example. Want of clear weather obliged me to be content with magnetic bearings at Corinth, and these may easily be at fault as much as 1°

F. C. PENROSE.

VOLCANIC ACTION IN THE BRITISH ISLES.

AT the anniversary of the Geological Society, held on the 19th inst., the retiring President, Sir Archibald Geikie, gave the annual address, which was devoted to a continuation of the subject treated of by him last year. He now dealt with the history of volcanic action in this country from the close of the Silurian period up to older Tertiary time. The remarkable volcanic outbursts that took place in the great lakes of the Lower Old Red Sandstone were first described. From different vents over central Scotland, piles of lava and tuff, much thicker than the height of Vesuvius, were accumulated, and their remains now form the most conspicuous hill-ranges of that district. It was shown how the subterranean activity gradually lessened and died out, with only a slight revival in the far north during the time of the Upper Old Red Sandstone, and how it broke out again with great vigour at the beginning of the Carboniferous period. Sir Archibald pointed out that the Carboniferous volcanoes belonged to two distinct types and two separate epochs of eruption. The earlier series produced extensive submarine lava-sheets, the remains of which now rise as broad terraced plateaux over parts of the lowlands of Scotland. The later series manifested itself chiefly in the formation of numerous cones of ashes, like the *puys* of Auvergne, which were dotted over the lagoons and shallow seas in central Scotland, Derbyshire, Devonshire, and the south-west of Ireland. After a long quiescence, volcanic action once more reappeared in the Permian period; and numerous small vents were opened in Fife and Ayrshire, and far to the south in Devonshire. With these eruptions the long record of Palæozoic volcanic activity closed. No trace has yet been discovered of any volcanic rocks intercalated among the Secondary formations of this country, so that the whole of the vast interval of the Mesozoic period was a prolonged time of quiescence. At last, when the soft clays and sands of the Lower Tertiary deposits of the south-east of England began to be laid down, a stupendous series of fissures was opened across the greater part of Scotland, the north of England, and the north of Ireland. Into these fissures lava rose, forming a notable system of parallel dykes. Along the great hollow from Antrim northwards between the outer Hebrides and the mainland of Scotland, the lava flowed out at the surface and formed the well-known basaltic plateaux of that region.

The address concluded with a summary of the more important facts in British volcanic history bearing on the investigation of the nature of volcanic action. Among these Sir Archibald laid special stress on the evidence for volcanic periods, during each of which there was a gradual change of the internal magma from a basic to an acid condition, and he pointed out how this cycle had been repeated again and again even within the same limited area of eruption. In conclusion, he dwelt on the segregation of minerals in large eruptive masses, and indicated the importance of this fact in the investigation, not only of the constitution and changes of the volcanic magma, but also of the ancient gneisses where what appear to be original structures have not yet been effaced.

THE CENTENARY OF MURCHISON.

ON February 19, 1792, Roderick Impey Murchison was born at Tarradale, in Ross-shire. By a curious and appropriate coincidence, the anniversary of the Geological Society, the date of which is fixed by statute, fell this year on the 19th of the present month, the hundredth anniversary of the birth of the illustrious author of the "Silurian System." It was a further remarkable conjuncture that the President of the Society, who had to give the annual address, and take notice of the centenary, was Murchison's literary executor, who was designated by him as the first Professor of Geology in the chair which he founded in the University of Edinburgh, and who now fills the office which he held for so many years—that of Director-General of the Geological Survey. In referring to the doubly interesting features of this anniversary, Sir Archibald Geikie spoke of his great chief with warm admiration. The twenty years which have passed since Murchison's death enable geologists to make a truer estimate of Murchison's real achievements than was possible at the time when his commanding presence filled so prominent a place in the scientific world of his day. They have been able to correct some of his observations and discard some of his generalizations, yet the solid mass of original work done by him remains as a lasting memorial of his genius and industry. In the broad basis of facts, and in the skilful marshalling of these facts in their ordered relations, which distinguished his work among the Silurian rocks, the hand of a consummate master of geological investigation is to be traced. His name has become a household word in geology, and will go down to future ages as that of one of the great pioneers of the science.

Murchison, during all his scientific career, was closely associated with the Geological Society, and took a keen personal interest in its welfare. By his will he left a sum of money to found a medal and fund to be given annually for the reward and encouragement of geological research. This year the medal was awarded to Prof. A. H. Green, of Oxford, and the balance of the fund to Mr. Beeby Thomson. An interesting proof of the affectionate regard entertained for Murchison's memory was afforded by an announcement made by the President. He stated that, a few days before the meeting, an old friend of Murchison, who desired to remain unknown, had come to him and asked to be allowed to offer a slight tribute in remembrance of the man and his work, on his centenary, at the anniversary meeting of the Society. The President was requested to select two geologists (by preference Scotsmen) who were carrying on geological work in Murchison's spirit, and seeking to advance the special branches of research to which he devoted himself, and to present to each of them a cheque for £50, with a framed portrait of the author of the "Silurian System." Sir Archibald Geikie said that the task assigned to him was made comparatively easy by the terms of the generous gift. He had no doubt that the Society would agree with him that there were pre-eminently two Scottish geologists marked out as recipients of this benefaction, who were disciples of Murchison, and were carrying on his work, but with no slavish obedience to the opinions of their master, and who, by their conjoint work, alike with hammer and pen, well deserved this unexpected and appropriate reward—Mr. B. N. Peach and Mr. John Horne. As a touching addition to this pleasing incident, we have since learnt that while the anniversary was being held at Burlington House, the faithful friend who had made this offering to Murchison's memory was engaged in the cemetery at Brompton carefully brushing and washing his tomb. Driving snow was falling at the time from a gloomy sky, in strange contrast with the glow of affection that was piously renovating the inscription that records the name and resting-place of one of the great leaders of modern geology.

H. W. BATES, THE NATURALIST OF THE AMAZONS.

HENRY WALTER BATES was a native of Leicester, and was engaged in his father's warehouse when, about the year 1845, he made the acquaintance of Alfred Russel Wallace, then English master in the Collegiate

School of that town. Bates was at that time an ardent entomologist, while Wallace was chiefly interested in botany; but the latter at once took up beetle-collecting, and after he left Leicester the following year kept up an entomological correspondence with his friend. Two years later Wallace proposed a joint expedition to Para in order to collect insects and other natural objects, attracted to this locality by the charming account of the country in Mr. W. H. Edwards's "Voyage up the Amazon," a choice confirmed by the late Edward Doubleday, who had just received some new and very beautiful butterflies collected near the city of Para. The two explorers sailed from Liverpool in April 1848, in a barque of 192 tons burthen, one of the very few vessels then trading to Para, and the results of their journey are well known to naturalists. They made joint collections for nearly a year while staying at or near Para, but afterwards found it more convenient to take separate districts and collect independently. Bates spent eleven years in the country, divided pretty equally between the lower and the upper Amazon, and he amassed a wonderful collection of insects. Returning home in 1859, he devoted himself to the study of his collections, and in 1861 read before the Linnean Society his remarkable and epoch-making paper on the Heliconidæ of the Amazon Valley. In this paper, besides making important corrections in the received classification of this group and its allies, he discussed and illustrated in the most careful manner the wonderful facts of "mimicry," and for the first time gave a clear and intelligible explanation of the phenomena, their origin and use, founded on the accepted principles of variation and natural selection. In spite of countless attacks—usually by persons who are more or less ignorant of the facts to be explained—this theory still holds its ground, and notwithstanding the constant accumulation of new facts, and its discussion by new writers, it has never been more clearly or more fully explained than by its original discoverer.

So early as March 1860, Mr. Bates commenced a series of papers for the Entomological Society, under the title of "Contributions to an Insect Fauna of the Amazon Valley." These were at first devoted to the Diurnal Lepidoptera, and in one of them he gave a new classification of the whole group, founded chiefly on the structure of the legs, and leading to the conclusion that the Papilionidæ formed one of the lowest families, while the Nymphalidæ were the highest. This classification has been very generally adopted by entomologists, though there are a few dissentients, who hold that the principle adopted to determine the rank or grade of the respective families is an unsound one. Later on he wrote many papers on the various groups of Longicorn beetles; and finding that his circumstances and the time at his disposal did not allow him to keep up and study two such extensive groups as the Coleoptera and Lepidoptera, he parted with his fine collection of South American butterflies to Messrs. Salvin and Godman, and thereafter devoted himself exclusively to the study of Coleoptera. Later still, he almost confined his attention to the Carabidæ, on which important group he became a recognized authority. His largest works in this direction were his contributions to the "Biologia Centrali-Americana": Vol. I., Part 1 (Geodephaga); Vol. II., Part 2 (Pectinicornia and Lamellicornia); Vol. V. (Longicornia). A supplement to the Geodephaga has since been published in the Transactions of the Entomological Society of London for 1890 and 1891; and a supplement to the Longicornia was in course of preparation, but not finished at the time of his death.

In 1864, he was appointed Assistant Secretary to the Royal Geographical Society, an appointment he held till his death. Besides editing the Journal and Proceedings, and carrying on an immense correspondence with travellers and others in every part of the world, he had practically the entire management of the large establishment of the Society, and the chief burden of the arrangements for the various meetings, as well as those for the Geographical Section of the British Association. There can be little doubt that it was the confinement and constant strain of this work that weakened his constitution and shortened a valuable life.

When we consider the originality and clearness of exposition in his first great paper on "Mimicry," the accuracy and fulness of knowledge displayed in his systematic and descriptive work, and the power of observation and felicity of style which characterizes "The Naturalist on the Amazons," we cannot but regret that circumstances should have compelled him to devote so much of his time and strength to the mere drudgery of office work, and be thereby to a great extent debarred from devoting himself to those more congenial pursuits in which he had shown himself so well fitted to excel.

His high reputation, both as a hard-working entomologist and philosophic naturalist, led to his being twice chosen President of the Entomological Society of London, first in 1869, and again in 1878; while he was elected a Fellow of the Royal Society in 1881. His somewhat rugged features, quiet, unassuming manners, and thoughtful utterance, must be familiar to all who have attended the evening meetings of the Royal Geographical Society during the last twenty-seven years. Rarely has any Society had a more efficient secretary, who not only carried on its work with accuracy and judgment, but also gained the respect and esteem of all who came in contact with him. He died on Febuary 16, at the age of sixty-seven. A. R. W.

THOMAS ARCHER HIRST.

WE regret to have to record the death of Dr. Hirst, the well-known mathematician. He was the youngest of the three sons of Mr. Thomas Hirst, a wool-stapler, and was born at Heckmondwike, in Yorkshire, on April 22, 1830. In 1844 he became an articled pupil of Mr. Richard Carter, land agent and surveyor at Halifax; but afterwards he went to Germany, and studied at several Universities, taking the degree of Doctor of Philosophy at Marburg in 1852. His intercourse with Steiner, at Berlin, gave a strong impulse to his studies, and ultimately determined their character. Dr. Hirst on his return to England filled the vacancy at Queenwood College caused by Tyndall's appointment to the Professorship of Natural Philosophy in the Royal Institution. The work at Queenwood occupied most of his time, so that during the three years for which he held the post his only original paper was a note "On the Existence of a Magnetic Medium" (R.S. Proc., vii., 1854).

Towards the close of 1854 he married, and in consequence of his wife's delicate health he passed the winter of 1856-57 in the south of France. During this period he wrote two papers "On Equally Attracting Bodies" (*Phil. Mag.*, xiii., xvi.).[1] On the return journey Mrs. Hirst died (1857) in Paris. After this sad event Dr. Hirst spent six weeks with Prof. Tyndall on the *mer de glace* (cf. "Glaciers of the Alps"): he then returned to Paris, and attended the lectures of Chasles, Liouville, Lamé, and Bertrand. At this time he translated Poinsot's famous memoir "On the Percussion of Bodies," and contributed a paper, "Sur le Potentiel d'une Couche infiniment mince comprise entre deux Paraboloides Elliptiques" (Liouville, *J. de M.*, ii., 1859).[2] The winter of 1857-58 was spent in Rome. Here was written for Tortolini's *Annali* the memoir "Sur la Courbure d'une Série de Surfaces et de Lignes" (vol. ii., 1859), an abstract of which was subsequently published in the *Quarterly Journal of Mathematics*. In these stirring times Dr.

1 Cf. Chasles, "Rapport sur les Progrès de la Géométrie," p. 144.
2 Chasles, "Rapport," p. 303.

Hirst received a cordial welcome from the mathematicians of Southern Italy, and then going north he followed the victorious armies as far as San Martino and Solferino. After the Peace of Villafranca he visited the town of Cremona, and here commenced an acquaintance of life-long duration with Prof. Luigi Cremona.

In 1860, Dr. Hirst took up his residence in London, and for a short time took the advanced mathematical classes in University College School, in consequence of Mr. Cook's illness, and on that gentleman's death he became his successor. This office Dr. Hirst held for five years, and here, with Prof. Key's full concurrence (see Dr. Hirst's preface to Wright's "Elements of Plane Geometry," 1868), he taught geometry to classes of beginners without the use of "Euclid." Subsequently, in 1870, at the request of the Ladies' Educational Association, he gave a course of twenty-four lectures on the subject of geometry to a class of sixty ladies at St. George's Hall. The syllabus of these lectures was printed at the time. He was so well satisfied with the results of his attempt that when, in 1871, the Association for the Improvement of Geometrical Teaching was started, though he had taken no part, directly, in its formation,[1] he at once gave in his adhesion to the movement, and contributed very materially to its success, by his accepting the office of President, and by his doing yeoman's service during his tenure of the office (1871–78). Previous to this Dr. Hirst had, in 1865, been elected Professor of Mathematical Physics in University College. This post he vacated in 1867, when he succeeded Prof. de Morgan in the Chair of Pure Mathematics. It was on January 16, 1865,[2] that the London Mathematical Society was started. Of this Society Dr. Hirst was one of the pillars, and it was in a great measure through his fostering care that it has made the mark it has. He served on the Council from 1865 to November 1885, and for the session 1890–91. He vacated the office of Treasurer when he was elected President for the years 1872–73, 1873–74.

In 1870, Dr. Hirst was appointed to the new office of Assistant Registrar to the University of London, and thereupon resigned his Professorship, and the General Secretaryship of the British Association, which he had held from 1866. In 1873, when the Royal Naval College was founded, he became the Director of Studies, and held the office for ten years, when the precarious state of his health necessitated his retirement, and the passing of several winters abroad. He died on February 16.

In 1861, Dr. Hirst was elected a Fellow of the Royal Society. He was three times a member of the Council of the Society, and twice one of its Vice-Presidents. In 1883 one of the Royal Medals was awarded to him for "his investigations in pure geometry; and, more particularly for his researches into the correlation of two planes and into the complexes generated by them." He was a Fellow of the Royal Astronomical Society, a member of the Physical Society, and of several Continental Societies. He served for some years on the Council of University College, London, and was also a member of the Senate of the University of London.

Dr. Hirst revised the mathematical articles in Brande's "Dictionary of Arts and Sciences," and contributed new ones; and published a translation of Clausius's treatise on "The Mechanical Theory of Heat" (1867).

The following titles of papers may be mentioned:— "On the Volumes of Pedal Surfaces" (Phil. Trans., 1863; *Crelle*, lxii., 1863; and Tortolini, *Annali*, v., 1863). "On the Quadric Inversion of Plane Curves" (R.S. Proc., 1865; cf. Chasles, "Rapport," p. 167, "Ce mémoire est un travail fort complet"). This was his first *purely* geometrical paper. It was translated by Cremona in the *Annali di Matematica* (vii., 1865), and a form of it is published in the *Nouvelles Annales* (v., 1866). His remaining papers, mainly contributed to the London Mathematical Society's Proceedings, are:—"On Correlation in Space" (abstract of Presidential Address, 1874, Proc., vi.). "Note on the Correlation of Two Planes" (Proc., viii.). "On Cremonian Congruences" (Proc., xiv.). "On Congruences of the Third Order and Class" (Proc., xvi.). "On Cremonian Congruences contained in Linear Complexes" (Proc., xvii.). "On the Correlation of Two Spaces, each of Three Dimensions" (Proc., xxi.). "On the Complexes generated by Two Correlative Planes" (*Chelini* Memorial Volume, 1881). "Sur la Congruence Roccella" (*Circolo Matematico*, 1886).

[1] Opening remarks in the Presidential Address, A.I.G.T., First Report, January 17, 1871 (cf. also Nature, vol. ii. pp. 65, 141, 164).
[2] Memoir of Augustus de Morgan, pp. 280–86.

DR. THOMAS STERRY HUNT.

Dr. T. Sterry Hunt, who died at New York on the 12th of this month, in his sixty-sixth year, was widely known from his geological works, especially those relating to chemical geology. For some years past he had been in feeble health, suffering much from heart-disease. Early in this year he was attacked with influenza, from which he seemed to be recovering, but a relapse occurred, from which he failed to rally. Born on September 6, 1826, at Norwich, in Connecticut, he was educated for the medical profession, but in 1845 became assistant to Prof. B. Silliman at Yale College, and was also chemist to the Geological Survey of Vermont. In 1847 he joined the Geological Survey of Canada, under Sir W. Logan, as chemist and mineralogist. From 1856 to 1862 he was Professor of Chemistry at Laval University in Quebec, giving his lectures in French. From 1872 to 1878 he was Professor of Geology at the Massachusetts Institute of Technology. He was elected a Fellow of the Royal Society in 1859, and in 1881 received the honorary degree of LL.D. at Cambridge. Dr. Hunt was one of the founders of the International Geological Congress at Philadelphia, in 1876; he attended the meetings of the Congress at Paris in 1878, Bologna in 1881, Berlin in 1885, and London in 1888, taking an active part in the proceedings of each.

Although by birth a citizen of the United States, he is best known as a Canadian geologist, and, after retiring from the Canadian Survey, he lived for some years in Montreal. But latterly he preferred to consider himself once more as belonging to the United States, and for a few years before his death was a resident in New York.

Dr. Hunt's most important geological work was done in connection with the Geological Survey of Canada, with and under Logan. They led the way in the study of the Archæan rocks of that area, and Hunt gave to them many of the names which have since become well known, and too widely used, in the Archæan controversy. His work on the geology of petroleum was of high value, and he long ago clearly stated generalizations as to its occurrence which later investigations, over wider areas in North America and in other districts, have fully verified. Other important researches, published in the official Reports of the Canadian Survey and elsewhere, related to limestone, dolomite, and gypsum; salt; the chemistry of natural waters; the porosities of rocks; rock-weathering, &c. The well-known "Geology of Canada," issued by Logan in 1863 as Director of the Survey, was in large part written by Hunt, the parts on lithology and on economic geology being almost entirely his; he likewise read the proofs of the whole. He also wrote much on Alpine and Italian geology, and on the classification of the older Palæozoic rocks; in the Cambro-Silurian controversy he was a warm advocate of Sedgwick. The origin of serpentine was also a favourite subject, he stoutly maintaining its aqueous origin. As regards the ancient crystalline rocks generally, he to a large extent reverted to the Wernerian view, but with some important modifications; these he explained in his "crenitic hypothesis."

Dr. Hunt's earlier papers (1846–49) were wholly on chemistry and mineralogy, and to these subjects he always gave much attention. Some of his latest writings are purely chemical, dealing mainly with the more speculative aspects of that science. Perhaps in these questions, as is certainly the case with many of his theoretical views on geology, Dr. Hunt failed to carry conviction to the minds of his fellow-workers; and it may well be doubted if some of his views on these matters will ultimately add to his scientific reputation. But it would be unjust on this account to ignore the mass of solid work which he accomplished, and the suggestive hints which are scattered throughout his writings.

Dr. Hunt was a man of wide reading and general culture; he possessed a marvellous memory, and great conversational powers. In his company one might for hours forget that science was his special study, so well informed was he in history, literature, and philosophy. His conversation on such subjects possessed an additional interest from his personal acquaintance with many American authors. He was thus an excellent travelling companion, and the writer will not soon forget with what thrilling effect he recited Macaulay's "Horatius," within sight of Cortona and its Etruscan walls.

W. TOPLEY.

NOTES.

THE date of the Bakerian Lecture to be delivered before the Royal Society has been altered to March 10. Prof. James Thomson has chosen as his subject "The Trade Winds."

THE general arrangements for the Edinburgh meeting of the British Association have now been completed. The first general meeting will be held on Wednesday, August 3, at 8 p.m., when Dr. William Huggins, F.R.S., will resign the chair, and Sir Archibald Geikie, For.Sec.R.S., Director-General of the Geological Survey of the United Kingdom, President-Elect, will assume the Presidency, and deliver an address. On Thursday evening, August 4, at 8 p.m., there will be a *soirée*; on Friday evening, August 5, at 8.30 p.m., a discourse will be delivered by Prof. A. Milnes Marshall, F.R.S.; on Monday evening, August 8, at 8.30 p.m., a discourse on magnetic induction will be delivered by Prof. J. A. Ewing, F.R.S.; on Tuesday evening, August 9, at 8 p.m., there will be another *soirée*; and on Wednesday, August 10, the concluding general meeting will be held at 2.30 p.m. The different Sections will assemble for the reading and discussion of Reports and other communications on Thursday, August 4, and on the following Friday, Saturday, Monday, and Tuesday. The delegates of Corresponding Societies will meet on Thursday, August 4, and Tuesday, August 9, at 3.30 p.m. Excursions to places of interest in the neighbourhood of Edinburgh will be made on the afternoon of Saturday, August 6, and on Thursday, August 11.

IT is proposed that Englishmen shall celebrate the fourth centenary of the discovery of the New World, and do honour to the memory of Columbus, by establishing in Jamaica a marine biological station on the lines of the marine laboratories at Naples and Plymouth. The institution would be called "the Columbus Marine Biological Station." An excellent letter on the subject by Lady Blake appeared in the *Times* on Wednesday. The scheme has been laid before Prof. Huxley, Prof. Ray Lankester, Prof. Flower, Dr. Günther, Dr. Ball, Sir John Lubbock, Mr. Scott, Mr. Sclater, and numerous other scientific men, all of whom warmly approve of it. For the establishment of the laboratory on a sound basis an outlay of £15,000 will be required. The following have consented to receive subscriptions:—Prof. Ray Lankester, Oxford; Dr. Günther, British Museum (Natural History), Cromwell Road; Dr. Ball, Science and Art Museum, Dublin; the Duchess of St. Albans, Bestwood Lodge, Arnold, Notts.; and Messrs. Coutts and Co., bankers, 59 Strand. The Hon. Walter Rothschild, 148 Piccadilly, has undertaken the duties of honorary secretary.

ON Saturday last a meeting was held in the Combination Room of St. John's College, Cambridge, to discuss a proposal for the provision of a national monument to the late Prof. Adams. The Rev. Dr. Taylor, the Master of the College, presided; and among those present were Dr Peile (Master of Christ's, and Vice-Chancellor), Dr. Ferris (Master of Caius), Dr. Porter (Master of Peterhouse), Mr. Aldis Wright (Vice-Master of Trinity), Dr. Forsyth, Prof. Hughes, Dr. Hobson, Prof. Thomson, Dr. Glaisher, Dr. Frost, Dr. Sandys, Prof. Mayor, and Sir George G. Stokes, M.P. The Master said that Prof. Adams had memorials in Cambridge in the Adams Prize, and his portraits at that College and at Pembroke. His own work was his monument in the annals of science. They wished to commemorate his name and personality in the eyes of the world in that central sanctuary where, age after age, they commemorated their national types of various kinds of supreme excellence which were the glory of the world. The first suggestion of that came to him from Archdeacon Farrar. The suggestion had been mentioned at a College meeting and by it adopted, and they were met that day to carry it out. He thought the better method would be to form a large and influential committee, containing the most prominent names in mathematics and science, which would enable them to show there was a general feeling in favour of it. Then he thought the request might be made to the Dean and Chapter, on behalf of the Committee, by the Chancellor, the Duke of Devonshire, and in a letter which he had received from the Duke he stated that he should be very glad to give any assistance in his power to carry out the wishes of the Committee. Among those who had agreed to join the Committee were the Astronomer-Royal, the Master of Trinity, Dr. Salmon (Provost of Trinity College, Dublin), the Master of Corpus, Mr. Justice Romer, Prof. Jebb, Mr. Courtney, Lord Rayleigh, Prof. Newton, the Gresham Professor of Astronomy, Prof. Cayley, and Sir Donald Smith (Chancellor of Montreal University), who asked to be allowed to subscribe £100. The following motion, proposed by the Master, seconded by Sir G. G. Stokes, and supported by Dr. Glaisher and Prof. Liveing, was carried unanimously: "That the late Prof. John Couch Adams, by his discovery of the planet Neptune, and other masterly work, published or unpublished, is entitled to be named with the great astronomers of the world; and that this meeting pledges itself (so far as in it lies) to promote and carry out the scheme for placing a memorial to the late Professor in Westminster Abbey." The following resolutions were also carried:—"That the memorial consist of a bust, with tablet and inscription." "That a Committee be formed (with power to add to their number) to carry out the scheme; that the Master of Pembroke College and Prof. Liveing be the Treasurers, and the Master of Peterhouse, Dr. D. MacAlister, and Dr. Glaisher the Secretaries, and that such and such persons be the Executive Committee." "That any surplus from subscriptions after payment of the necessary expenses to be used in the first instance to defray the cost of presenting copies of the collected papers of Prof. Adams to learned Societies and libraries at home and abroad, and that the remainder (which, if of sufficient amount, shall be constituted a permanent memorial fund) be offered to the Master and Fellows of St. John's College to form an Exhibition or Scholarship fund for the encouragement of the study of mathematics or physics by the undergraduate students of the

College, such fund to be administered in such a manner as the Masters and Fellows may from time to time determine."

AT a meeting of the electors to the Lowndsean Professorship of Astronomy at Cambridge, held on February 20, Sir Robert S. Ball, Astronomer-Royal for Ireland, was elected to succeed the late Prof. Couch Adams. Sir Robert Ball is fifty-one years of age. He is a native of Dublin, and was educated at Trinity College. When twenty-five years old, he was appointed Lord Rosse's astronomer at Parsonstown. He became Professor of Applied Mathematics and Mechanism at the Royal College of Science of Ireland in 1867, and Professor of Astronomy at the Dublin University, and Astronomer-Royal for Ireland, in 1874. In 1873, he had been made a Fellow of the Royal Society. He has done much by his writings and lectures to create and foster a popular interest in astronomical study. In relation to this appointment we give the following extract from the *Cambridge University Reporter* of February 23:—The Council of the Senate beg leave to report to the Senate as follows: "The arrangement by which 'the superintendence and management of the Observatory' were intrusted to the late Lowndsean Professor (Grace, May 2, 1861, Ordinances, p. 239) has now terminated, and as no provision has been made for the future direction of the Observatory, the Council think it desirable that a special Syndicate should be appointed to consider the question." The Council therefore recommend: "That a Syndicate be appointed to consider what provision should be made for the future superintendence and management of the Observatory, and to report to the Senate before the end of the present Lent Term." "That the Vice-Chancellor, Dr. Ferrers, Master of Gonville and Caius College, Prof. Sir G. G. Stokes, Dr. Glaisher, Prof. Liveing, Prof. Thomson, and F. Whitting, M.A., of King's College, be appointed a Syndicate to consider what provision should be made for the future superintendence and management of the Observatory, and to report to the Senate before the end of the present Lent Term."

THE Queen has approved the appointment of Dr. Thomas Clifford Allbutt, F.R.S., to be Regius Professor of Physic in the University of Cambridge, in the room of the late Sir George Paget.

MR. J. SCOTT KELTIE has been appointed to succeed the late Mr. H. W. Bates, F.R.S., as Assistant Secretary of the Royal Geographical Society.

AT the meeting of the Royal Geographical Society on Monday, Mr. Theodore Bent read before a large audience a paper on his recent exploration among the Zimbabwe and other ruins. The paper was one of great interest. Mr. Bent said that, with his wife and Mr. Robert Swan, he went to Mashonaland primarily to examine the ruins of the Great Zimbabwe. These ruins, so named to distinguish them from the numerous minor Zimbabwes scattered over the country, were situated in south latitude 20° 16′ 30″, and east longitude 31° 10′ 10″, at an elevation of 3300 feet above the sea-level, and formed the capital of a long series of such ruins stretching up the whole length of the west side of the Sabæ River. They covered a vast area of ground, and consisted of the large circular building on a gentle rise with a network of inferior buildings extending into the valley below, and the labyrinthine fortress on the hill, about 400 feet above, naturally protected by huge granite boulders and a precipice running round a considerable portion of it. Mr. Bent gave a minute description of the ruins, drawing attention to evidence that their ancient inhabitants must have been given to the grosser forms of native worship. Perhaps the most interesting of their finds in one portion were those in connection with the manufacture of gold. Mr. Bent held that the ruins and the things in them were not in any way connected with any known African race; the objects of art and the special cult were foreign to the country altogether, where the only recognized form of religion was, and had been since the days when the early Portuguese explorers penetrated into it and El Masoudi wrote, that of ancestor worship. It was also obvious that the ruins formed a garrison for the protection of a gold-producing race in remote antiquity. So we must look around for such a race outside the limits of Africa, and it was in Arabia that we found the object of our search. All ancient authorities speak of Arabian gold in terms of extravagant praise. Little, if any, gold came from Arabia itself; and here in Africa gold was produced in large quantities, both from alluvial and from quartz, from the remotest ages. A cult practised in Arabia in early times was also practised here; hence there was little room for doubt that the builders and workers of the Great Zimbabwe came from the Arabian peninsula. He had no hesitation in assigning this enterprise to Arabian origin, and to a pre-Mahomedan period.

AT the anniversary meeting of the Geological Society, held at Burlington House on Friday last, the following officers were elected:—President: W. H. Hudleston, F.R.S. Vice-Presidents: Prof. T. G. Bonney, F.R.S., L. Fletcher, F.R.S., G. J. Hinde, Prof. J. W. Judd, F.R.S. Secretaries: Dr. H. Hicks, F.R.S., J. E. Marr, F.R.S. Foreign Secretary: J. W. Hulke, F.R.S. Treasurer: Prof. T. Wiltshire. The following are the members of the Council: Prof. J. F. Blake, Prof. T. G. Bonney, F.R.S., James W. Davis, R. Etheridge, F.R.S., L. Fletcher, F.R.S., Prof. C. Le Neve Foster, Sir A. Geikie, F.R.S., A. Harker, H. Hicks, F.R.S., G. J. Hinde, W. H. Hudleston, F.R.S., Prof. T. McKenny Hughes, F.R.S., J. W. Hulke, F.R.S., Prof. J. W. Judd, F.R.S., J. E. Marr, F.R.S., H. W. Monckton, Clement Reid, J. J. H. Teall, F.R.S., W. Topley, F.R.S., Prof. T. Wiltshire, Rev. H. H. Winwood, H. Woodward, F.R.S., H. B. Woodward.

IN the February number of the *Kew Bulletin* much useful information on sisal hemp (*Agave rigida*, Mill.) is presented. The cultivation of sisal hemp has lately been developed to so remarkable an extent in the Bahamas that hemp-growing has become, for the moment, one of the most prominent of the new industries of the tropics. The *Bulletin* mentions most of the localities where plants of sisal hemp are now found, and the material it has collected will be of great service to all who may think of embarking in a fibre industry at the present time.

AMONG the other contents of the *Kew Bulletin* is an interesting correspondence between Mr. Thiselton-Dyer and the Vice-Chairman of the Middlesex County Council on the question of instruction in horticulture. There is so much vague talk nowadays about technical education that all who wish the words to be used in the right sense will read with pleasure Mr. Thiselton-Dyer's remarks on the proper way of learning the art of cultivating plants. "The cultivation of plants," he says, "is an art which can only be acquired by practice, and therefore, it appears to me, cannot be taught in the lecture-room any more than painting or shoe-making. I know of no royal or theoretical road to the acquisition of a competent or even useful knowledge of the gardener's art except by beginning at the bottom and going through every operation, from the most elementary to the most difficult and refined. If an intelligent young man does that, and keeps his eyes open, he may become a successful gardener. But the mere reading of books and attendance on lectures will never, in my judgment, make anyone even a moderately competent gardener."

A REPORT on the botanical collections made by Dr. Brown Lester, Medical Officer to the Gambia Delimitation Commission, was published in the *Kew Bulletin* for October and November 1891. A translation of the botanical section of the reports made by the French members of the Commission is given in the February number of the *Bulletin* for the purpose of supplementing Dr. Brown Lester's notes.

APPENDIX II., 1892, of the *Kew Bulletin* contains a list of the new garden plants of the year 1891. The list includes, besides the plants brought into cultivation for the first time in 1891, the most noteworthy of those which have been re-introduced after being lost from cultivation. Other plants in the list have been in gardens for several years, but either were not described or their names had not been authenticated till recently.

IN their Irish Education Bill the Government propose that a large proportion of the funds at their disposal for the improvement of national education in Ireland shall be spent for the benefit of the teachers, who as a class have hitherto been too much neglected. The rest of the amount will be devoted to a capitation grant, and to the freeing of all schools in which the fees do not exceed six shillings a year per child. Attendance at elementary schools, if the Bill becomes law, will be compulsory in Irish towns, but in rural disiricts it will be open to the people to accept or reject compulsion as they may think fit.

THE National Association for the Promotion of Technical and Secondary Education has issued an appeal to the electors of the London County Council on the subject of technical instruction. As everyone interested in technical education knows, London has devoted to the relief of the rates the whole of its share of the grant obtained from the proceeds of the beer and spirits duties. This has been done in direct opposition to the wish of Parliament; and the Association has no difficulty in showing that the grant will be continued only if it is used for the purposes to which the House of Commons intended it to be applied. It may be hoped that the appeal will be widely read, and that voters will perceive that it deals with a matter by which their interests must sooner or later be vitally affected.

THE principal article of interest to meteorologists in the *American Meteorological Journal* for January is by A. L. Rotch, on the mountain meteorological stations of the United States. At the present time the only stations in operation throughout the year are the Lick Observatory, in California, and the Blue Hill Observatory, in Massachusetts. That on Mount Washington (6280 feet above the sea) was established in 1870, and partially closed in 1887; during the three following years it was opened during the summer months only. At no other station in the world was such severe weather experienced, as the highest wind velocity often occurred with the lowest temperature. During a storm in February 1876, when the temperature fell to $-50°$, a wind velocity of 184 miles an hour was recorded. In foggy weather the frost formed upon the anemometer cups in such quantity as to break off the arms. The observations at this station have been much lessened in value, owing to their not being published in detail, and to the want of a low-level station for comparison. The Blue Hill Observatory is only 640 feet above the sea, and was opened in 1885. The hourly values for five years have been printed in the Harvard College Observatory. For several years hourly observations of clouds have been made, with a view to benefit weather predictions. The Observatory on Pike's Peak, Colorado (14,134 feet), was built in 1873, and for fifteen years was maintained by the Signal Service. It was closed in 1888, and the observations have been published in the Annals of the Harvard College Observatory. The average annual temperature was 19°, and the extremes 64° and $-39°$. Pike's Peak is remarkable for its electrical storms. When the air is moist, and generally when snow is falling, sparks emanate from the fingers of the outstretched hands; but the station was only once struck by lightning. The Lick Observatory is on Mount Hamilton, 4300 feet above the Pacific Ocean, which is plainly visible from the summit. Fragmentary observations have been made at various other stations, the most important of which were those by Prof. Langley, on Mount Whitney, California, in 1881, which have served to change the theory of the nature of the heat received from the sun, and to show that the sun is much hotter than had been supposed. The article is accompanied by photographic illustrations of several of the stations.

ELECTRICITY is being applied to a novel use in the U.S. Navy. Four electric fans have been placed by the Crocker Wheeler Company in the turrets of the powerful iron vessel *Miantonomah*, the intention being that they shall blow away the smoke from the guns.

AN interesting compound of carbon with the metal barium, possessing the composition C_2Ba, is described by M. Maquenne in the current number of the *Comptes rendus*. It may be considered, perhaps, as an acetylide of barium—that is, a compound formed by the replacement of the hydrogen of acetylene, C_2H_2, by metallic barium. For immediately it is brought in contact with water pure acetylene gas is evolved with great rapidity. M. Maquenne has obtained the new substance by the direct action of metallic barium, employed in the form of an amalgam consisting of one part barium and four parts mercury, upon powdered retort-charcoal. Upon distilling such a mixture in a current of hydrogen, when the mercury had been expelled and the temperature attained redness, an energetic reaction was found to occur between the barium and the carbon, with production of the new carbide or acetylide. The hydrogen took no part in the reaction, and M. Maquenne has subsequently found that it may be replaced by nitrogen; the latter, however, being less advantageous, inasmuch as the carbide produced is then admixed with more or less cyanide. The new substance, as obtained when hydrogen is employed to furnish the atmosphere, consists of a grey, friable mass, which remains quite unaltered when heated to bright redness. The moment, however, it is thrown into cold water it is decomposed, with a rapid effervescence of a gas which possesses the odour of acetylene, burns in the air with a luminous flame, precipitates a red substance resembling acetylide of copper from an ammoniacal solution of cuprous chloride, and, in short, possesses all the properties of acetylene. M. Maquenne adds that the acetylene thus obtained is remarkably pure. The reaction with water may be expressed by the equation—

$$C_2Ba + 2H_2O = C_2H_2 + Ba(OH)_2.$$

Barium acetylide would appear to be analogous to the compounds obtained by M. Berthelot by heating the metals of the alkalies in a current of acetylene, and also to the acetylide of calcium prepared by Wöhler. The direct formation of this substance from barium and carbon, together with its reaction with water, afford another mode of synthesizing acetylene, which M. Maquenne considers to be of interest from the point of view of the formation of the natural hydrocarbons. He considers it probable that other metals possess this same property of forming acetylides under the influence of high temperatures. If, therefore, as M. Berthelot has attempted to show, it is a fact that acetylene forms the primary material, or starting-point, for the formation of other hydrocarbons, it is quite possible that such compounds of metals with carbon, upon coming in contact with water under conditions of more or less pressure, may give rise to the production of the immense stores of natural hydrocarbons, such as those which exist in the petroleum wells of Russia and the New World.

THE additions to the Zoological Society's Gardens during the past week include a Sykes's Monkey (*Cercopithecus albigularis* ♀) from East Africa, presented by Mr. G. N. Wylie; a Beatrix Antelope (*Oryx beatrix* ♀), an Indian Gazelle (*Gazella bennetti*) from Arabia, presented by Lieut.-Colonel Talbot; a Goshawk (*Astur palumbarius*), European, presented by Captain Noble; a Common Quail (*Coturnix communis*), European, presented by W. K. Purnell; a Hybrid Goose (between *Anser cinereus* and *A. brachyrhynchus*), captured in Holland, presented by Mr. F. E. Blaauw, C.M.Z.S.; a Gould's Monitor (*Varanus gouldi*), a Stump-tailed Lizard (*Trachydosaurus rugosus*) from New South Wales, presented by Mr. T. Hellberg; a Chub (*Leuciscus cephalus*), British fresh waters, presented by Mr. H. E. Young; two Yaks (*Poëphagus grunniens* ♂ ♀) from Tibet, three Gigantic Salamanders (*Megalobatrachus maximus*) from Japan, deposited; an Azara's Agouti (*Dasyprocta azaræ*), a Pucheran's Hawk (*Asturina pucherani*), a Sulphury Tyrant (*Pitangus sulphuratus*), two Short-winged Tyrants (*Machetornis rixosa*) a Brown Milvago (*Milvago chimango*), an Orange-billed Coot (*Fulica leucoptera*), a Cayenne Lapwing (*Vanellus cayennensis*), six Rosy-billed Ducks (*Metopiana peposaca* 3 ♂ 3 ♀) from South America, purchased; an American Bison (*Bison americanus* ♂) from North America, received in exchange; a Gayal (*Bibos frontalis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE SOLAR DISTURBANCE OF 1891, JUNE 17.—In the October number of the *Observatory* Mr. H. H. Turner publishes an article on the luminous outburst on the sun observed by M. Trouvelot on June 17, and recorded in these columns on July 9. The disturbance was of such an unusual character that M. Trouvelot hazarded the suggestion that it was possibly accompanied by perturbations of the magnetic elements. Mr. Whipple was good enough to look over the Kew curves to see if they showed any such variations, and a negative result was obtained. Mr. Turner, however, after an examination of the Greenwich records has succeeded in finding "a very minute, though unmistakable, disturbance at almost precisely the time noted by Trouvelot. . . . The disturbance is smaller than many others on the same day, although the day itself was very quiet: but it differs from others in its abruptness, which is clearly shown in all three curves. The change in declination is only about 1', and in H.F. 0·0005 of the whole H.F." Diagrams illustrating these fluctuations accompanied Mr. Turner's paper. It seemed strange that the Kew and the Greenwich records should differ in their indications, so a further enquiry was sent to Mr. Whipple, who replied as follows:—"I have again referred to the curves of June 17, 1891, and fail to find any trace of what can by any means be termed to be a magnetic disturbance at the time in question—accepting Sabine's interpretation of a magnetic disturbance (see Phil. Trans, vol. cliii., p. 274), and so avoiding loose expressions. According to the *Observatory*, October 1891, Father Sidgreaves is quite of our opinion as to the case in point." The evidence in favour of a magnetic disturbance simultaneously with Trouvelot's observation is thus not very strong.

PHOTOGRAPHY OF SOLAR PROMINENCES.—In a communication to the Paris Academy on February 8, M. Deslandres described some new results obtained by him in the photography of solar prominences. The object of the research was to photograph the spectra of prominences further into the ultra-violet than had previously been done. In July of last year, M. Deslandres, following Prof. Hale, succeeded in photographing the spectra to λ 380. He has now been able to obtain negatives upon which the spectrum extends from λ 410 to λ 350. In order to obtain this result, a siderostat with a mirror 8 inches in diameter has been employed to project the sun's image, a Rowland grating has been used to produce the spectra, and the lenses of the observing telescope have been made of quartz. The photographs show eight bright lines of the ultra-violet hydrogen series, and it is believed that observations made from an elevated station would lead to the detection of the remaining two. The line a little more refrangible than hydrogen α (λ 388), is also recorded upon the plates. Photographs have been taken of the spectra of spots and faculæ. The calcium lines at H and K often appear bright upon them, and are always stronger than the hydrogen lines. But no new facts appear to have been discovered in this direction of work.

ON THE VARIATION OF LATITUDE.—Dr. S. C. Chandler has published a series of papers on the variation of latitude, in the *Astronomical Journal* from No. 248 to No. 251. The general result of a wide discussion indicates a revolution of the earth's axis of inertia about that of rotation from west to east, with a radius of 30 feet measured at the earth's surface, in a period of 427 days.

NON-EUCLIDIAN GEOMETRY.[1]

EVERY conclusion supposes premisses; these premisses themselves are either self-evident and have no need of demonstration, or can only be established by assuming other propositions; and as we cannot continue this process to infinity, every deductive science, and especially geometry, must rest on a certain number of axioms which cannot be demonstrated. All treatises on geometry therefore commence with the enunciation of these axioms. But a distinction must be made between them: some—such as this for example, "Two quantities that are equal to a third quantity are equal to one another"—are not geometrical propositions, but are analytical ones. I regard them as analytical *a priori* judgments, and as such I will not discuss them. But I must insist on other axioms which are special to geometry. Text-books for the most part state them very explicitly:—

(1) Only one straight line can be drawn between two points.
(2) A straight line is the shortest distance between two points.
(3) Only one straight line can be drawn through a point parallel to a given straight line.

Although the demonstration of the second of these axioms is generally dispensed with, it would be possible to deduce it from the other two, and from those, of which the number is more considerable, that we admit explicitly without stating them, as I shall explain in the sequel.

Efforts have also for a long time been made without success to demonstrate the third axiom, known under the name of the *postulatum d'Euclide*. The amount of trouble that has been taken in that chimerical hope is truly beyond imagination. Finally, at the commencement of the century, and almost simultaneously, Lowatchewski and Bolyai, two men of science, a Russian and Hungarian respectively, established, in an irrefutable manner, that such a demonstration was impossible; they have very nearly rid us of the inventors of geometries without postulates: since their time the Academy of Sciences only receives annually one or two new demonstrations.

The question was still not settled; soon a great step was made by the publication of the celebrated memoir of Riemann, entitled "Ueber die Hypothesen welche der Geometrie zum Grunde liegen." This small treatise has inspired the majority of recent works, of which I will make mention subsequently, and among which must be mentioned those of Beltrami and Helmholtz.

The Geometry of Lowatchewski.—If it were possible to deduce the *postulatum d'Euclide* from the other axioms, it would evidently happen that in denying the postulate and admitting the axioms, we should be led to contradictory results; it would then be impossible to base a coherent geometry on such premisses.

But this is precisely what Lowatchewski has done. He supposes in the first place that—

"*Several straight lines can be drawn through a point parallel to a given straight line.*"

And he moreover retains all the other axioms of Euclid. From these hypotheses he deduces a series of theorems among which it is impossible to detect any contradiction, and he constructs a geometry the faultless logic of which is not inferior to that of the Euclidian geometry.

The theorems are, certainly, very different from those to which we are accustomed, and they disconcert us a little at first.

Thus, the sum of the angles of a triangle is always less than two right angles; and the difference between this sum and two right angles is proportional to the surface of the triangle.

[1] Translation of an article that appeared in the *Revue Générale des Sciences*, No. 23, by M. H. Poincaré.

It is impossible to construct a figure similar to a given figure, but of different dimensions.

If a circle be divided into n equal parts, and tangents be drawn to the points of division, these n tangents will meet and form a polygon, provided that the radius of the circle be small enough; but if this radius is sufficiently large, they will not meet. It is useless to multiply these examples; the propositions of Lowatchewski have no longer any connection with those of Euclid, but they are not less logically connected together.

The Geometry of Riemann.—Let us imagine a world peopled only with beings deprived of thickness; and let us suppose that these animals, "infinitely flat," are all in one plane, and are not able to get out of it. Let us admit, further, that this world is removed sufficiently from others to be free from their influence. As we are making these assumptions, we may as well endow these beings both with reasoning powers and the capacity of founding a geometry. In this case they would certainly attribute to space only two dimensions.

But let us suppose, however, that these imaginary animals, all still devoid of thickness, have the form of a portion of a spherical figure, and not of a plane one, and are all on one and the same sphere without being able to leave it. What geometry would they construct? It is clear at once that they would only attribute to space two dimensions: that which will play for them the part of the straight line will be the shortest distance between two points on the sphere—that is to say, an arc of a great circle; in a word, their geometry would be spherical geometry.

What they will call space will be this sphere which they cannot leave, and on which occur all the phenomena of which they can have any knowledge. Their space then will be *without limits*, since on a sphere one can always go forward, without ever coming to an end, and nevertheless it will be *finite*—one will never find the limit, but one can make the circuit of it.

In fact, the geometry of Riemann is spherical geometry extended to three dimensions. To construct it, the German mathematician had to throw overboard not only the postulates of Euclid, but even the first axiom: *Only one straight line can be drawn between two points.*

On a sphere only one great circle in general can be drawn through two given points (which, as we have just seen, would play the part of the straight line to our imaginary beings); but to this there is an exception; for, if the two given points are diametrically opposed, an infinite number of great circles can be made to pass through them.

In the same way, in the geometry of Riemann, only one straight line in general can be drawn between two points; but there are exceptional cases where an infinite number of straight lines can be drawn between them.

There is a kind of opposition between the geometry of Riemann and that of Lowatchewski.

Thus, the sum of the angles of a triangle is—

Equal to two right angles in Euclid's geometry.
Less than two right angles in that of Lowatchewski.
Greater than two right angles in that of Riemann.

The number of parallels that can be drawn to a given straight line through a given point is equal—

To one in the geometry of Euclid.
To zero in that of Riemann.
To infinity in that of Lowatchewski.

Let us add that the space of Riemann is finite although without limit, in the sense already given to these two words.

Surfaces of Constant Curvature.—There was, however, one possible objection. The theorems of Lowatchewski and of Riemann present no contradiction, but, however numerous the consequences which these two geometers have drawn from their hypotheses, they were compelled to stop before they had exhausted all of them, for the number would be infinite: who can say, therefore, that, if they had carried their deductions further, they would not finally have found such contradictions?

This difficulty does not exist for the geometry of Riemann, provided that it is limited to two dimensions; for, in fact, the geometry of Riemann for two dimensions does not differ, as we have seen, from spherical geometry, which is only a branch of ordinary geometry, and consequently outside all discussion.

M. Beltrami, in considering in the same way the two-dimensional geometry of Lowatchewski to be only a branch of ordinary geometry, has equally refuted the objection in this case.

This he has done this in the following manner:—Consider on a surface any figure. Imagine this figure, traced on a flexible and inextensible cloth, to be laid on this surface, in such a way that when the cloth is moved and changes its shape, the various lines of this figure can change form without altering their length. In general this flexible and inextensible figure cannot leave its place without quitting the surface; but there are certain particular surfaces for which a similar movement would be possible; these are the surfaces with constant curvature.

If we resume the comparison that we previously made, and imagine beings without thickness living on one of these surfaces, they will regard the movement of a figure all of whose lines preserve a constant length as possible. A like movement, on the other hand, would appear absurd to animals without thickness living on a surface whose curvature was variable.

These surfaces of constant curvature are of two kinds:—

Some are of *positive curvature*, and can be so deformed as to be laid on a sphere. The geometry of these surfaces becomes then spherical geometry, which is that of Riemann.

Others are of *negative curvature*. M. Beltrami has shown that the geometry of these surfaces is none other than that of Lowatchewski. The two-dimensional geometries of Riemann and Lowatchewski are thus found to be re-attached to Euclidian geometry.

Interpretation of Non-Euclidian Geometries.—Thus the objection disappears as regards geometries of two dimensions.

It would be easy to extend M. Beltrami's reasoning to geometries of three dimensions. The minds which space of four dimensions does not repel will see here no difficulty; but they are few. I prefer, then, to proceed otherwise.

Let us consider a particular plane that we will call fundamental, and construct a kind of dictionary, making a double series of words, written in the two columns, correspond each to each, in the same way that the words of two languages, having the same signification correspond in ordinary dictionaries:—

Space	Portion of space situated above the fundamental plane.
Plane	Sphere cutting orthogonally the fundamental plane.
Right line	Circle cutting orthogonally the fundamental plane.
Sphere	Sphere.
Circle	Circle.
Angle	Angle.
Distance between two points ...	Logarithm of the anharmonic ratio of these two points and the intersections of the fundamental plane with a circle passing through these two points and cutting it orthogonally.
&c.,	&c.

Let us take, then, the theorems of Lowatchewski, and translate them by means of this dictionary, as we should translate a German text with the aid of a German-French dictionary. *We shall obtain then the theorems of ordinary geometry.*

For example, this theorem of Lowatchewski—"The sum of the angles of a triangle is less than two right angles"—is translated thus: "If a curvilinear triangle has for its sides the arcs of a circle which if prolonged would cut orthogonally the fundamental plane, the sum of the angles of this curvilinear triangle will be less than two right angles." Thus, however far one pushes the results of the hypotheses of Lowatchewski, one will never be led to a contradiction. Indeed, if two of Lowatchewski's theorems were contradictory, the translations of these two theorems, made with the help of our dictionary, would also be contradictory; but these translations are theorems of ordinary geometry, and everyone agrees that ordinary geometry is free from contradictions. Whence comes this certainty, and is it justified? This is a question that I cannot treat here, but which is very interesting, and, as I believe, soluble. The objection that I have formulated above no longer then exists.

But this is not all. The geometry of Lowatchewski, susceptible of a concrete interpretation, ceases to be a frivolous logical exercise, and is capable of application: I have not the time to mention here either these applications or the use that M. Klein and myself had made of them for the integration of linear equations.

This interpretation, moreover, is not unique, and one could construct several dictionaries analogous to that given above, and by which we could by a simple "translation" transform the theorems of Lowatchewski into theorems of ordinary geometry.

Implicit Axioms.—Are then the axioms explicitly enunciated in treatises the only foundations of geometry? One can be assured to the contrary when one sees that, after having successively abandoned them, there still remain some propositions common to theorems of Euclid, Lowatchewski, and Riemann. These propositions ought to rest on some premisses, as geometers admit, although they do not state them. It is interesting to try to liberate them from classical demonstrations.

Stuart Mill has made the assertion that every definition contains an axiom, since, in defining it, the existence of the object defined is implicitly affirmed. This is going too far: it is seldom that one gives a definition in mathematics without following it by the demonstration of the existence of the object defined, and when it is omitted, it is generally because the reader can easily supply it. It must not be forgotten that the word existence has not the same sense when it is the question of a mathematical creation as when we have to do with a material object. A mathematical creation exists, provided that its definition involves no contradiction either in itself or with the properties previously admitted.

But if Stuart Mill's remark cannot be applied to all definitions, it is none the less true for some of them.

A plane is sometimes defined in the following manner:—The plane is a surface such that the straight line which joins any two points in it lies altogether in the surface.

This definition manifestly hides a new axiom: we could, it is true, alter it, and that would be better, but then it would be necessary to enunciate the axiom more explicitly.

Other definitions give place to reflections no less important.

Such is, for example, that of the equality of two figures: two figures are equal when they can be superposed; to superpose them it is necessary to displace one until it coincides with the other; but how must it be displaced? If we ask, we should be answered that it ought to be done without changing its shape, and in the manner of an invariable solid. The "reasoning in a circle" would then be evident.

In truth, this definition implies nothing. It would have no meaning for a being who lived in a world where there were only fluids. If it seems clear to us, it is that we are accustomed to the properties of natural solids that do not differ greatly from those of ideal solids whose dimensions are all invariable.

Meanwhile, however imperfect it may be, this definition implies an axiom.

The possibility of the movement of an invariable figure is not a truth evident by itself, or at least it is only one in the same way as the *postulatum d'Euclide*, and not as an analytical *a priori* judgment would be.

Moreover, in studying the definitions and the demonstrations of geometry, we see that one is obliged to admit, without demonstrating it, not only the possibility of this movement, but even some of its properties.

This results, first of all, from the definition of the straight line. Many defective definitions have been given, but the true one is that which is understood in all the demonstrations where the straight line is in question:

"It may happen that the movement of a constant figure is such that all points of a line belonging to this figure remain immovable while all the points situated outside this line are displaced. Such a line will be called a straight line."

We have in this enunciation purposely separated the definition from the axiom that it implies.

Several proofs, such as those relating to the equality of triangles which depend on the possibility of letting fall a perpendicular from a point on a line, assume propositions that are not enunciated, since we must admit that it is possible to carry a figure from one place to another in a certain manner.

The Fourth Geometry.—Among these implicit axioms, there is one which seems to me worth mentioning, not only because it has given rise to a recent discussion,[1] but because in abandoning it, one can construct a fourth geometry, as coherent as those of Euclid, Lowatchewski, and Riemann.

To demonstrate that we can always raise from a point, A, a perpendicular to a straight line, AB, a straight line, AC, is considered movable round the point A, and in the first instance coinciding with the fixed line AB; and it is made to turn round the point A until it lies in the prolongation of AB.

[1] See MM. Renouvier, Léchalas, Calinon, *Revue Philosophique*, June 1889; *Critique Philosophique*, September 30 and November 30, 1889; *Revue Philosophique*, 1890, p. 158. See especially the discussion on the "postulate of perpendicularity."

We thus assume two propositions: first, that such a rotation is possible, and then that it can be continued until the two lines are in one straight line.

If the first point be admitted, and the second rejected, we are led to a series of theorems still more curious than those of Lowatchewski and Riemann, but equally free from contradiction.

I will quote only one of them, and that not the most singular: *A true straight line can be perpendicular to itself.*

The Theorem of Lie.—The number of implicit axioms introduced in classical demonstrations is greater than it need be, and it would be interesting to reduce them to a minimum. We can ask ourselves, in the first place, if this reduction is possible, if the number of necessary axioms, and imaginable geometries is not infinite.

M. Sophus Lie's theorem dominates all this discussion: it can be thus stated:—

Let us suppose that the following premisses are admitted:—

(1) Space has n dimensions.

(2) The movement of an invariable figure is possible.

(3) To determine the position of this figure in space, p conditions are necessary.

The number of geometries compatible with these premisses will be limited.

I can even add that, if n be given, a higher limit to p can be assigned.

If, then, the possibility of movement be admitted, only a finite number (and that a restricted one) of geometries can be invented.

The Geometries of Riemann.—However, this result seems to be contradicted by Riemann, because this investigator constructed an infinite number of different geometries, and the one which generally bears his name is only a particular case.

Everything depends, he says, on the way in which we define the length of a curve. But there are an infinite number of ways of defining this length, and each of these can become the starting point of a new geometry.

That is perfectly true; but most of these definitions are incompatible with the movement of an invariable figure, which is supposed possible in Lie's theorem. These geometries of Riemann, so interesting on many grounds, can only then remain purely analytical, and do not lend themselves to demonstrations analogous to those of Euclid.

The Nature of Axioms.—Most mathematicians regard the geometry of Lowatchewski only as a simple logical curiosity; some of them, however, have gone further. Since several geometries are possible, is it certain that ours is the true one? Experience, doubtless, teaches us that the sum of the angles of a triangle is equal to two right angles; but this is only because we operate on too small triangles; the difference, according to Lowatchewski, is proportional to the surface of the triangle; will it not become sensible if we work with larger triangles, or if our means of measurement grow more accurate? Euclidian geometry would only then be a provisional geometry.

To discuss this question, we ought in the first instance to inquire into the nature of geometrical axioms.

Are they synthetical conclusions *a priori*, as Kant used to say?

They would appeal to us then with such force, that we could not conceive the contrary proposition, nor construct on it a theoretical edifice. There could not be a non-Euclidian geometry.

To convince oneself of it, let us take a true synthetical *a priori* conclusion; for example, the following:—

If an infinite series of positive whole numbers be taken, all different from each other, there will always be one number that is smaller than all the others.

Or this other, which is equivalent to it:—

If a theorem be true for the number 1, *and if it has been shown to be true for* $n + 1$, *provided that it is true for* n, *then it will be true for all positive whole numbers.*

Let us next try to free ourselves from this conclusion, and, denying these propositions, to invent a false arithmetic analogous to the non-Euclidian geometry. We find that we cannot; we shall be even tempted in the first instance to regard these conclusions as the results of analysis.

Moreover, let us resume our idea of the indefinitely thin animals: surely we can scarcely admit that these beings, if they have minds like ours, would adopt Euclidian geometry, which would be contrary to all their experience.

Ought we, then, to conclude that the axioms of geometry are experimental truths? But we do not experiment on straight lines or ideal circles; only material objects can be dealt with. On what would depend, then, the experiments which would serve to found a geometry? The answer is easy.

We have seen above that one argues constantly as if geometrical figures behaved like solids. That which geometry would borrow from experience is therefore the properties of these bodies.

But a difficulty exists, and it cannot be overcome. If geometry were an experimental science, it would not be an exact science—it would be liable to a continual revision. What do I say? It would from to-day be convicted of error, since we know that a rigorously invariable solid does not exist.

Geometrical axioms, therefore, are neither synthetic a priori conclusions nor experimental facts.

They are *conventions*: our choice, amongst all possible conventions, is *guided* by experimental facts; but it remains *free*, and is only limited by the necessity of avoiding all contradiction. It is thus that the postulates can remain rigorously true, even when the experimental laws which have determined their adoption are only approximate.

In other words, *axioms of geometry* (I do not speak of those of arithmetic) are only definitions in disguise.

This being so, what ought one to think of this question: Is the Euclidian geometry true?

The question is nonsense.

One might just as well ask whether the metric system is true and the old measures false; whether Cartesian co-ordinates are true and polar co-ordinates false; whether one geometry cannot be more true than another—it can only be more convenient.

Now, Euclidian geometry is, and will remain, the most convenient:—

(1) Because it is the simplest; and it is not so simply on account of our habits of thought, or any kind of direct intuition which we may have of Euclidian space; it is the most simple in itself in the same way as a polynomial of the first order is simpler than one of the second.

(2) Because it agrees sufficiently well with the properties of natural solids, those bodies which come nearer to our members and our eye, and with which we make our instruments of measurement.

Geometry and Astronomy.—The above question has also been stated in another way. If the geometry of Lowatchewski is true, the parallax of a very distant star would be finite; if that of Riemann be true, it would be negative. Here we have results which seem subject to experience, and it has been hoped that astronomical observations would have been able to decide between the three geometries.

But what one calls a straight line in astronomy is simply the trajectory of a ray of light. If then, as is impossible, we had discovered negative parallaxes, or shown that all parallaxes are greater up to a certain limit, we should have the choice between two conclusions:—

We could renounce Euclidian geometry, or modify the laws of optics, and admit that light is not propagated strictly in straight lines.

It is useless to add that everyone would regard the latter solution as the more advantageous.

Euclidian geometry, then, has nothing to fear from new experiments.

Let me be pardoned for stating a little paradox in conclusion:—

The beings which had minds like ours, and who had the same senses as we have, but who had not received any previous education, might receive conventionally from an exterior world choices of impressions such that they would be led to construct a geometry different from that of Euclid, and to localize the phenomena of this exterior world in a non-Euclidian space, or even in a space of four dimensions.

For us, whose education has been formed by our real world, if we were suddenly transported in this new one, we should not have any difficulty in referring the phenomena to our Euclidian space.

Anyone who should dedicate his life to it could, perhaps, eventually imagine the fourth dimension.

I fear that in the last few lines I have not been very clear. I can only be so by introducing new developments; but I have already been too long, and those whom these explanations might interest have read their Helmholtz.

Desiring to be brief, I have affirmed more than I have proved: the reader must pardon me for this. So much has been written on this subject, so many different opinions have been put forward, that the discussion of them would fill a volume.

W. J. L.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 11.—"The *Rôle* played by Sugar in the Animal Economy: Preliminary Note on the Behaviour of Sugar in Blood." By Vaughan Harley, M.D.

This communication was to show that the causes why the whole amount of added sugar can seldom be recovered from blood are threefold. Firstly, the imperfections in the as yet known methods of analysis. Secondly, the different ways in which the albumens of the blood behave themselves while coagulating; some coagulating in the form of firm clots, which retain the saccharine matter in their interstices, rendering it impossible to extract all the sugar from them by washing; others separating as loose, flocculent curds, from which the sugar can be regained with comparative facility. While, thirdly, as bacteria were distinctly ascertained to have nothing to do in the matter, and yet the loss of the sugar added to the blood is in every instance distinctly progressive—according to the period of time the sugar is left in contact with the blood before the analysis is begun—Dr. Vaughan Harley considered himself justified in saying that there must exist in the normal blood itself a sugar-transforming agent. This he described as an enzyme; but refrained from going into any further particulars regarding it until his researches upon the subject are more advanced.

He gave tables of the results of his experiments, and compared them with those recently published by Schenk, Rohmann, and Seegen; showing that while the percentages of the sugar regained by the first observer ranged from 20 to 55 per cent., and those recovered by the two last experimenters fluctuated between 80 and 96 per cent., in his three different series of experiments, where different methods of analysis were employed, the percentages of the added sugar regained ranged respectively between 85 and 100; 92·9 and 99·3; and 94·7 and 99·9 per cent.

Mathematical Society, February 11.—Prof. Greenhill, F.R.S., President, in the chair.—The following communications were made:—On the logical foundations of applied mathematical sciences, by Mr. Dixon. He maintained the importance of distinguishing in all sciences between what is dependent on verbal conventions and what is not. He thus distinguished between that part of the meaning of a term which is laid down as its definition, and the part which remains to be discovered as a consequence of the definition. So also sciences might be divided into purely symbolic sciences, which being based on definitions alone conveyed no real information; subjective sciences, which deal with concepts and objective sciences, which deal with actual things. He then stated the conditions under which a set of assertions might be arbitrarily laid down as the definition of a term; and applied these conditions to show that Newton's three laws of motion could be regarded as a definition of the term force, that if this was done there could no longer be any discussion as to whether or not force alone is sufficient to account for the movements of matter. The anomaly that we are apparently able to determine directions absolutely, though we can determine positions only relatively, was explained, and a formal proof of all the elementary theorems of mechanics, including the principle of virtual work, might be deduced.—Note on the inadmissibility of the usual reasoning by which it appears that the limiting value of the ratio of two infinite functions is the same as the ratio of their first derived, with instances in which the result obtained by it is erroneous, by Mr. Culverwell.—On Saint Venant's theory of the torsion of prisms, by Mr. A. B. Basset, F.R.S.

DUBLIN

Royal Society, January 20.—Prof. W. N. Hartley, F.R.S., in the chair.—Reports on the zoological collections made by Prof. Haddon in Torres Straits, 1888–89: the Hydrocorallinæ, by S. J. Hickson. The specimens described are a female stock of *Stylaster gracilis*, *Distichopora violacea*, and *Millepora Murrayi*. Some of the smaller colonies of *Distichopora* are bright orange in colour, others vandyke brown, and the larger ones are deep purple with pale yellowish tips. The author believes that the differences in colour mean a difference in age and sexual condition. The smallest colonies are not sexually mature, the brownish have male ampullæ, and the oldest stocks are violet in colour, and are apparently female. In any case, the colour of *Distichopora* can no longer be regarded as the principal character for specific definition.—Sir Howard Grubb, F.R.S., exhibited and described a 4-inch equatorially-mounted refracting telescope of novel construction, in which the right ascension and declination circles were situated at the eye end of the telescope itself, instead of, as usual, on the polar and declination axes; thereby rendering the working of the instrument most convenient for the observer. The circles at the eye end of the telescope are connected by gearing to the polar and declination axes; but Sir H. Grubb described the method by which the "loss of time," and other errors, consequent on gearing, were almost totally eliminated, and the readings rendered quite sufficiently accurate for all ordinary purposes to which such a telescope would be put.—The first part of a memoir on the fossil fishes of the Coal-measures of the British Islands, by Mr. James W. Davis, was communicated by the Honorary Secretaries of the Society.

PARIS.

Academy of Sciences, February 15.—M. d'Abbadie in the chair.—On a new method of organic analysis, by M. Berthelot. The method consists in heating the compound in oxygen under a pressure of 25 atmospheres in a calorimetric bomb. Combustion is total and instantaneous, and therefore differs from that which appertains to the use of copper oxide.—On the employment of compressed oxygen in the calorimetric bomb, by the same author.—Action of alkaline metals on boric acid: critical study of the processes used in the preparation of amorphous boron, by M. Henri Moissan. The general result of the investigation is that when an alkaline metal acts on boric acid the reaction that occurs is accompanied with considerable heat, and, on account of the elevation of temperature, the greatest part of the boron set free combines with the excess of alkali, and with parts of the metal vessel used for the experiment. When this is afterwards washed out with water and hydrochloric acid, a mixture of boron, boride of sodium, boride of iron, boron hydride, nitride of boron, and hydrated boric acid is obtained after desiccation. This mixture is said to have the same composition as the substance which has hitherto been regarded as amorphous boron. M. Moissan will describe a method of preparing amorphous boron in a future paper.—Experimental researches on the transmissibility of cancer, by M. Simon Duplay.—The temporary star in Auriga, by M. G. Rayet. On February 10 and 11 the star appeared to M. Rayet to be orange-yellow or pale yellow. Its spectrum was examined by means of the 14-inch equatorial fitted with a spectroscope. It appeared to be continuous, the red and violet portions being comparatively bright. Four bright lines were seen in the green, and their wave-lengths were determined as 518, 501, 493, and 487.—Extension of Lagrange's equations to the case of sliding friction, by M. Paul Appell.—On the distribution of prime numbers, by M. Phragmen.—On the measure of high temperatures; reply to M. H. Becquerel, by M. H. Le Chatelier.—Remarks on the surface tension of liquid metals; a reply to a note by M. Pellat, by M. Gouy.—Variation, with temperature, of the dielectric constant of liquids, by M. D. Negreano. Experiments on benzine, toluene, and xylene, between 5° and 45° C., indicate that the dielectric constant diminishes with increase of temperature.—On the influence exercised on electro-magnetic resonance by an unsymmetrical arrangement of the long circuit along which the waves are propagated, by MM. Blondlot and M. Dufour. The experiments show that the wave-length, measured by means of a resonator, is independent of the dissymmetry of the two wires which transmit the electro-magnetic undulations.—The propagation of electric waves studied by a telephonic method, by M. R. Colson.—Magnetic perturbation of February 13 and 14, by M. Moureaux. The disturbance was first indicated on the magnetograph of the Parc Saint-Maur Observatory at 5h. 42m. on the morning of the 13th inst. The declination and horizontal force curves suffered a simultaneous rise, while the vertical component decreased. The most important phase of the pertarbation occurred between 11 p.m. and 2 a.m.; and about 5 p.m. of the 14th the elements had returned to their normal value. The disturbance in declination amounted to 1° 25′, and the horizontal and vertical components varied respectively more than $\frac{1}{37}$ and $\frac{1}{88}$ of their normal value.—Observations of atmospheric electricity, made by means of a captive balloon, by M. E. Semmola.—On the determination of the state of dissolved salts from a study of contraction, by M. Georges Charpy.—On some properties of bismuthic acid, by M. G. André.—On a carbide of barium, by M. Maquenne. (See Notes.)—Transformation of aromatic amines into chlorinated hydrocarbons, by MM. Prud'homme and C. Rabaut.—On the principles which accompany chlorophyll in leaves, by M. A. Etard.—Improvement of the culture of industrial and fodder potatoes in France: results of the season 1891, by M. Aimé Girard.—Contributions to the study of unplastered wines, by M. H. Quantin.—On the assimilation of carbohydrates, by M. Hanriot.—On the presence of numerous diatoms in the Cretaceous of the Paris basin, by M. Cayeux.—On the existence of zeolites in the calcareous Jurassic rocks of Ariège, and on the dissemination of these minerals in the Pyienees, by M. A. Lacroix.

AMSTERDAM.

Royal Academy of Sciences, January 30.—Prof. van de Sande Bakhuyzen in the chair.—Prof. Pekelharing spoke of the composition of the fibrin ferment. When oxalated blood-plasma is diluted with water and treated with acetic acid till moderate acid reaction, the precipitate consists chiefly of a substance which is soluble in alkali, in an excess of acid, and in neutral salt-solutions, and from which, by the action of pepsin-hydrochloric acid, is split off a nuclein—a substance that thus must be considered as a nucleo-albumin. This nucleo-albumin acquires, combined with lime, all the properties of fibrin ferment. It is very probable that this nucleo-albumin issues from the corpuscles of the blood.—Prof. Max Weber gave some results of his investigations of the fresh-water fauna of the islands of Sumatra, Java, Flores, Celebes, and Saleyer. Among the Crustacea, the Entomostraca are not essentially different from European forms. Isopods are only represented by marine species: Ichthyoxenus, Tachæa, Rocinela, and Bopyridæ. Amphipods are extremely rare, and only Orchestia was found. Nearly 70 species of Decapods were collected, out of which 33 are living also in brackish and sea water. It could be proved that immigration out of the sea into the rivers had taken place. An account was given of the life-history of *Ichthyoxenus Tellinghausii*.

CONTENTS.