THURSDAY, MAY 14, 1891.

PRACTICAL GEOLOGY.

Aids in Practical Geology. By Grenville A. J. Cole, F.G.S., Professor of Geology in the Royal College of Science for Ireland. (London: C. Griffin and Co., 1891.)

An Introduction to the Study of Petrology: the Igneous Rocks. By Frederick H. Hatch, Ph.D., F.G.S. (London: Sonnenschein and Co., 1891.)

HOWEVER prophetic may have been the far-seeing premonitions of men in advance of their age in the dim past, and however invaluable may have been the additions made to the superstructure since, it can scarcely be doubted that the foundation-stones of geology were laid by Scotchmen and Englishmen towards the end of the last, and during the earlier part of the present century. And what a charm is there about the story of these sturdy pioneers, not perhaps quite the men whom one would have picked out as most fitted or most likely to become the fathers of a new science. It has about it the elements of a genuine romance. For the early training of few of these men was such as to give a scientific bent to their mind; they did not have what we are pleased to call "the advantage of a scientific education"; it is probable that they never spoke, perhaps never dreamed of, such a phrase as "the scientific method," which we are so fond of formularizing, and on which we plume ourselves somewhat. But in spite of these seeming drawbacks, rather perhaps because with these men genius was allowed to run its spontaneous untrammelled course, they opened out to mankind a domain of knowledge the very outskirts of which had been barely touched upon before. Of shrewd mother-wit were they; too keen of eye to be wrong about their facts; not a few were ardent sportsmen, and the same instinct which led them to ride straight to hounds or patiently and warily to stalk the deer, led them also, as they brushed away minor details, to go direct to main issues, and carried them on, without rest but without haste, through the toils of many a year's steady field-work. With what awe and reverence do we look up to these giants when we pass their achievements in review!

Nor does it one whit impair this feeling of respectful admiration to turn to the other side, and cast a glance at what were their unavoidable shortcomings. They were too hard-headed to be illogical in the matter of straightforward inferences, but it was hardly to be expected that they would escape going astray sometimes when they ventured on recondite speculation. Rough is not the word for their method; incomplete would be nearer the mark, but even that can scarcely be applied when the means at their disposal are taken into account. No one had yet taught the value of the microscope and balance to the geologist; and, when these and other instruments of precision were introduced, there was just a tendency to gird at appliances that had a finnicking look about them to Titans who had so long and so successfully relied on their hammers and their wits.

But by degrees it became clear in Germany, and later on in England, that, though the great main roads of the newly-discovered territory had been tracked out with such brilliant success, methods more refined than had sufficed for pioneering work must be introduced if all the intricacies of its lanes and by-ways were to be explored. Then the swing of the pendulum rather tended to bring about a disposition to exalt the new means of investigation, and there was just a risk that the sound basis of field-work might come to be undervalued if not neglected; and that Mineralogy and Petrology, instead of being the handmaids of Geology, might be thought to constitute the whole of that science. But the mischief never went far. The mantle which had fallen from the shoulders of the great fathers was not to be lightly cast aside; and, while every new aid was cordially welcomed, the conviction grew stronger and stronger that honest work in the field must for ever be the starting-point of geological inquiry.

How thoroughly this truth has become engrained in the minds of geologists is seen directly we open Prof. Cole's "Aids in Practical Geology." A large part of the book is taken up with minute and precise directions for carrying out the various kinds of microscopical, optical, and chemical examination of minerals and rocks. But on the first page we read—

"Such aids in determinative geology as are given in the following pages may be applied in any halting-place, or in cities after the return from an expedition; but, in any case, observations made on specimens are of slight importance if uncoupled with knowledge of their true position in the field."

And again—

"After a study of a number of type specimens, the student is recommended to go out to some well-described district, and to endeavour to recognize the varieties of igneous and sedimentary rocks by careful observation in the field. In this way alone can he appreciate the various modes of weathering, the massive or minuter structures due to jointing, the smooth or rugged outlines that characterize the masses of which his hand-specimens form a part. . . . Nothing short of striking the rock-mass *in situ* with the hammer, and taking in with the eye its position and surroundings, even to the broader features of the landscape, should content the geologist who would follow worthily the founders and masters of the science."

Again and again the author reiterates the lesson—

"Just as no mountain mass can be described by a stranger from a number of hand-specimens, however beautiful, so no rock can be adequately described from isolated microscopic sections. Again and again the observer will pass from his section to the solid specimen, and from this, in memory at any rate, to the great mass of which it formed a part."

And in dealing with the nomenclature of igneous rocks, the chaotic state of which is so largely due to the ignoring of their field-relations, it is insisted that—

"The following out of an igneous rock in the field is a most important lesson, and will soon determine what is valuable and what is valueless in any proposed scheme of classification."

That the author, in these and similar passages, is not speaking from hearsay, not merely re-echoing what is now a truism, is shown by the admirable practical directions which he gives in the first chapter for the outfit and procedure of the field-geologist. Here, and indeed throughout the book, the instructions are detailed and precise.

The author has not forgotten the time when he was a beginner, his early failures, and the disappointments of his student-days, when, from the neglect of some slight precaution, he failed to obtain the results he had been led to expect; and he has used every means in his power, by minute and specific instruction, to shield those who use his book from similar mishaps. As an instance, take what he says about the effect of acids on minerals. How often has the self-taught man turned wearily to one book after another on mineralogy, in the hope of getting some definite information on this point, and all he arrived at was the curt statement, "Soluble in acids," which each apparently had copied from its predecessor, or all had borrowed from some common source. What acid? Concentrated or dilute? Cold or hot? Quickly, or perhaps only after a fortnight's boiling? All these points he was left to make out for himself as best he could. The happier pupil of Prof. Cole is treated far more liberally, and will not have to weary himself by feeling about in the dark if he attend to the cautions and instructions of the book now before us. The directions for blowpipe-work are equally precise. Only one who has been himself an actual worker would have told the observer to wait "till the first red glow has gone off" before noting the colour of a borax-bead. Of course, anyone would, sooner or later, find this out for himself; but, till he had found it out, he would probably blunder not a little; and anything that economizes time nowadays is not to be despised. There is no need to multiply instances; everyone who uses the book will find that it eminently deserves the epithet of "practical," which the author has assigned to it.

But are there no weak points on which the critic may exercise his function? Attention may perhaps be called to the following:—On p. 6, a graphical method, due to Mr. Dalton, is given for determining the full dip of a bed from the dips on two oblique sections. The writer may perhaps be pardoned for preferring a method of his own, given first in the *Geological Magazine* for 1876, p. 377. But, independently of any personal predilection, it may be said that the diagram in the case of this method is simpler than in that of Mr. Dalton. This makes it easier to recollect, and, besides, the fewer lines there are in a graphical construction the less is the chance of error. In dealing with "streak," it would be well to notice that the true streak of some hard minerals, Iron-glance for instance, is not obtained till they have been rubbed down in an agate mortar.

Doubt is thrown on the value of Turner's test for the detection of boron (p. 41): there is an article by Dr. C. Le Neve Foster in the *Mineralogical Magazine* (vol. i. p. 77) which should be consulted in this connection.

It is hardly worth while criticizing the nomenclature and classification of the crystalline rocks. No two petrographers are in agreement here, and probably the existing schemes of arrangement are all of about equal value. There is fortunately no multiplication of species or introduction of new names. It might be possible to take objection to the description of Quartz-felsite as a compact form of Granite, for the part played by the quartz in the two rocks is totally different, and must be correlated with a difference in their mode of consolidating. Quartz-felsites are specially common as dykes, and there may have been facilities for the escape of water in their case, up the fissures which they fill, that were not present in the case of the more thoroughly buried magma of Granite. It was doubtless the presence of water in the granite-magma which kept the quartz fluid or plastic after the other minerals had crystallized; its escape in the case of Quartz-felsite may have led to the early crystallization of the quartz. In dealing with the foliated rocks, the author touches on the debated point of the "true schists." We are pretty well used to this phrase, and have waited long in the hopes of being told what constitutes a "true schist," but our patience has not yet met with the reward it merits. The author is of opinion that "the alleged distinction between schist-like rocks and schists of pre-Cambrian age requires great delicacy of definition." This is delicately put, and will command the assent of most geologists.

The palæontological section will perhaps be looked upon somewhat derisively by those well versed in biology. But it will serve its end, which is to enable those who cannot pretend to any large amount of biological knowledge to know the commoner fossils when they see them, and determine the genus to which they belong. The method may have a large element of "rule-of-thumb" about it, it may be called empirical, but in a large number of cases it is not practicable to attain to anything better. And it has a certain educational value, for it makes a student use his eyes even if it but slightly disciplines his reason.

That the work deserves its title, that it is full of "aids" and in the highest degree "practical" will be the verdict of all who use it.

Nor will Dr. Hatch's handy volume be any less welcome. Those who wish to have in a compact form the prominent characters of the rock-forming minerals and the igneous rocks, will find all the information needed by a student concisely and lucidly put forth. Some slight acquaintance with crystallography and the optical properties of minerals is assumed. A short section on these subjects would have made the book more self-contained, and need not have increased its size very materially.

The igneous rocks are defined to be "those that have been formed by the consolidation of molten material." There is a spice of danger in the word "molten," for it may lead to the belief that the fluidity of the material was the result of "dry heat." In the case of a Laccolite the view so generally held is taken, that the overlying beds have been bent up by the intrusion of a molten mass. It is, to say the least, quite as likely that earth-movement caused a differential amount of bending in two adjoining beds, and that, as an empty space was thus gradually formed between the two, the molten matter was driven into it.

On the subject of the classification of the igneous rocks we find the following healthy expression of opinion: "The various types are so intimately related, that any attempt at rigid and systematic classification is not likely to meet with any great measure of success." Certainly not till some sounder basis of classification than any yet suggested is hit upon. In the meantime Dr. Hatch's grouping is one that from its clearness and simplicity will be a real boon to the student.

A most useful feature in the book is the list of localities where each rock occurs. The illustrations are very well executed. Though the book has appeared only recently, one teacher at least can already bear testimony, founded on actual experience, as to its value to students.

A. H. GREEN.

BACTERIOLOGY.

Les Virus. Par Dr. S. Arloing. (Paris: Ancienne Librairie, Germer, Baillière, et Cie., 1891.)

THE name of Dr. S. Arloing as the author of a work on bacteriology is a sufficient guarantee that the book is worth reading, nor are we disappointed. "Les Virus" is one of the best volumes on this science yet produced. It is not a mere compilation of other men's work, giving a categorical account of the numerous pathogenic and non-pathogenic bacteria now recognized, but is a thorough scientific investigation into the principles of one of the most important branches of medical science, and might perhaps be better called a manual of "microbiology."

The work is divided into six parts, under the following heads:—

(1) General considerations as to the nature of the bacterial poison.

(2) Form and mode of life of the microbes (biology).

(3) The part taken by the microbes in the propagation and spread of infectious diseases.

(4) Struggle of the host against the poison. Natural extinction and artificial destruction of its effects.

(5) Immunity enjoyed by the body against certain microbes.

(6) Attenuation and reproduction of the bacterial poison.

It will be seen by the above list that this work covers a large field, and one not exactly dealt with by any previous author.

In the first part, which is subdivided into six chapters, Dr. Arloing commences with an historical survey of the science of bacteriology, pointing out the gradual extension of ideas from the time of Rhazes, who, in the ninth century, attributed small-pox to a process of fermentation "comparable to that which takes place in the juice of the grape when made into wine"; touching then on the works of Rayer, Davaine, Chaveau, and others, the author traces the development of the science until present times and the discoveries of Koch and Pasteur. An interesting comparison of the "virulent" parasites with simple parasites, such as *Trichina spiralis*, then follows; and, next, the formulation of two statements which form the basis of the modern science: (1) the active agents of the virulent process are organisms; (2) these organisms are living, and possess specific properties.

The second part of the work deals with the biology of bacteria. The methods of cultivating them are fully described, and, what we do not remember to have seen in any other work on bacteriology, there is a full account of the effect on micro-organisms of nourishment, temperature, light, atmospheric conditions, and electricity. In this part, also, are two most important chapters—namely, the effects on the microbes of the nature of the cultivating medium. This is only just beginning to be properly understood, and its investigation has already been productive of valuable results.

The chapter on the products of the growth of micro-organisms is hardly up to the general excellence of the work. It has not been sufficiently brought up to date, so that the researches of Dr. Hankin, and the more complete investigations of Dr. Sidney Martin in reference to the albuminoses and alkaloids, do not appear in it. The diastases and ptomaines are, however, fully discussed, and much may be learnt from a perusal of this chapter.

The third division of the book is devoted to the *rôle* which the microbes play in the propagation and causation of disease. The chapter on contagion is one of the best in the book, and would alone form a most valuable *brochure.* After a consideration of the general modes in which contagion is carried, a most exhaustive account is given of air, water, soil, food, and artificial inoculation (vaccination) as carriers of disease. As a natural sequence, the modes of entry of the germs into the body are then described, auto-infection being included; and next we have a consideration of what may become of the organisms after their entry, and the changes which take place in the host. The descriptions here given are exceedingly precise, and, although rather condensed, convey all that can be desired.

Passing now to the fourth part, we find four chapters devoted to the strife between the host and the microbes, and the natural extinction and artificial destruction of the poison. In the third chapter the subject of disinfection is noticed, both by heat and antiseptics, special attention being drawn to the necessity of the careful disinfection of sputum, linen, bedding, &c.,—points which cannot be too strongly insisted upon in all hospitals, and not merely in those devoted to fevers or diseases of the chest.

The fifth part deals with the very difficult, and, at present, vague subject of "immunity." Dr. Arloing divides immunity into two classes—"acquired" and "natural." On this subject no one is more qualified to speak than the author of this work, for he has made it almost a special study for years, and it is treated of in his usual masterly way.

The sixth and last part contains some of the more recent researches (especially those of Pasteur) on the attenuation of the virus.

Taking the work as a whole, we cannot speak too highly of it. We heartily congratulate the author on the success of his labours. The book is well illustrated, and we cordially recommend it to all those who wish to study a subject so replete with interest and of such vital importance to mankind.

F. J. W.

OUR BOOK SHELF.

Anleitung zur Bearbeitung meteorologischer Beobachtungen für die Klimatologie. Von Dr. Hugo Meyer. (Berlin: Julius Springer, 1891.)

WERE this little book less severely technical in form, it might be commended to the notice of that large class of observers whose sole aim and object in meteorological registration is to ascertain the characteristics of the local climate and to compare them in detail with those shown by the similar records of other places. It teaches how the results of observation may be tabulated or graphically represented in the forms most approved by climatologists, and discusses with much precision the meaning of different kinds of mean values; though, indeed, it omits all mention of the geometric mean, the application of which in climatology was lately under discussion in the Royal Meteorological Society. But it is, we fear, hardly elementary enough to meet the requirements of beginners and amateurs, especially such as regard a formula of any complexity with something of that distant respect that they accord to holy mysteries; and on the other hand it aims at nothing beyond the formal and statistical presentment of facts, and never deviates into the seductive, if sometimes illusive, field of physical causation. It is what its title proclaims it to be, a guide to the working out of meteorological observations for the purposes of climatology—the climatology, that is to say, of the temperate zone. For those who work in a more extended field, some of the author's methods and dictates may be found to need modification. His schedule of the usual hours of observation makes no mention of those most frequently observed in the tropics, and his uncompromising condemnation of the use of Lambert's formulæ in reducing wind-registers, however justifiable in the case of the variable winds of these latitudes, ignores that of countries where trade-winds or monsoons blow steadily for weeks or months together with but little deviation from the normal quarter, and where the direction undergoes a regular oscillation daily. In working out this daily oscillation at such places, the use of Lambert's formula is not only justified but almost indispensable.

Within the somewhat narrow limits that Dr. Meyer has prescribed to himself, he has executed his task carefully and conscientiously, but in this country, at least, his merits are likely to be appreciated by only a small class; chiefly, indeed, by that estimable few who find in plodding labour its own sufficient reward. The student who is endowed with some share of scientific imagination, who loves to trace the inner workings of Nature, and sees in diagrams and tabulated statistics only means to this end, will find Dr. Meyer's work a somewhat dry study; and when he shall have mastered its contents, should he ever be challenged by Arthur Clough's "Questioning Spirit," and asked,

"What will avail the knowledge thou hast sought?"

he must answer as he best may from his own mental resources. His author, at least, will not help him to a reply.

Intensity Coils: how made and how used. By "Dyer." Sixteenth Edition. (London: Perken, Son, and Rayment.)

In this book a simple and interesting account is given of galvanic batteries, induced electricity, and the methods of making and using intensity coils, which include numerous experiments that may be described briefly as "popular." In the present edition many other branches of the subject have been touched upon, including electric lighting, electric bells, electric telegraph, electric motors; and a few words are said on the telephone, microphone, and phonograph. Although the book is not presented as a scientific treatise, but simply as a guide containing the necessary instructions for making and using the above-named instruments, yet by its means many may be led to make a more advanced study of the subject, which to-day is of such high importance.

General Physiology. By Camilo Calleja, M.D. (London: Kegan Paul, Trench, Trübner and Co., Limited, 1890.)

The author of this book means by the word "physiology" "discourse of nature"; and his intention is to denote by it "the study of positive science in the abstract sense." The scheme he has set before himself is nothing less than "to comprehend under the fundamental principle of mechanism—conservation of energy—all the laws and theories concerning nature." In order to show the spirit in which he sets about the accomplishment of his task, it may perhaps be enough to say that he regards the planets as "bodies constituted of organic and inorganic matter," and that to him living organic matter seems "the proximate agent of planetary movements, for which non-living bodies are only the cosmic medium." The sun, we learn, is not "a body in combustion," but "principally a great reflecting mass, which, situated in the focus of the orbits of many planets, reflects their infra-luminous emissions, these producing light by their conglomeration." As for "natural light or daylight," it is "a photothermic radiation produced by transference, not only of the radiating motion of the planets, but also of the motion engendered by solar living beings." If anyone is attracted by writing of this kind, he will find plenty of it in Dr. Calleja's amusing volume.

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.*]

Co-adaptation.

It sometimes appears to me that the neo-Darwinians must speak a language of their own, because they are so fond of telling me, in a stereotyped phrase, that, "if words have any meaning," such and such words have expressed some meaning which no ordinary grammatical construction can extract. The present is a good case in point. Prof. Meldola says that he finds "a remarkable discrepancy" between my two previous letters on the above subject, and seeks to reveal it by quoting from the first letter, thus:—

"'I do not . . . hold myself responsible for enunciating Mr. Herbert Spencer's argument, which the quotation sets forth. I merely reproduced it from him as an argument which appeared to me valid on the side of "use-inheritance." For not only did Darwin himself invoke the aid of such inheritance in regard to this identical case, . . .' &c. If words have any meaning, this implies that Dr. Romanes agrees with Darwin in regarding this case as one in which 'use-inheritance' played a part."

Does it? When a man says that in his opinion a certain argument in favour of a certain conclusion is valid, is this equivalent to his saying that he accepts the conclusion? And when he adds, twice over, that he purposely abstains from expressing any opinion of his own with regard to the conclusion, is this equivalent to his saying the precise opposite?

The state of the case is simply as follows. Prof. Meldola reproduced Mr. Wallace's argument against Mr. Spencer's defence of "use-inheritance." I wrote to show that this particular argument was invalid; but that there was another argument on the same side, which, if adduced, would be valid, *supposing that it could be sustained by facts.* Now, in his reply, Prof. Meldola abandoned the invalid argument, and adopted the one which I had stated. Accordingly I wrote a second time, in order to show that we were then agreed upon this being the only argument which could be logically brought against Mr. Spencer's position. But I again added that I would express no opinion as to whether this argument could be successful in subverting Mr. Spencer's position. In point of fact, with regard to *this* question I have no fully-formed opinion to express. But, unless the neo-Darwinians have eventually become unable to comprehend the attitude of "suspended judgment," one would suppose that they might still appreciate the difference between sifting arguments as good or bad on both sides of a question, and finally deciding with regard to the question itself.

Christ Church, Oxford, May 8. George J. Romanes.

I wrote in good faith when in my last brief communication I expressed the intention of allowing the subject to drop, because I considered that the discussion had arrived at a stage when those who were interested in the matter would be able to form their own opinion as to the value of the arguments adduced on either side of the question. I very much regret to find, however, that Dr. Romanes—whose amount of spare time appears to be most enviably inexhaustible—still finds it necessary to prolong the correspondence. I am compelled, therefore, to enter the field once more, if only for the purpose of presenting my own case in its true light. What Dr. Romanes's position may now be I must confess is becoming distinctly less clear with each of his contributions to the subject, but I am not the first who has lost his way in attempting to thread the mazes of this writer's productions. As far as I am concerned it will suffice to say that the case is not "simply" as he presents it in the foregoing communication. In the review of Mr. Pascoe's book, from which this discussion originated, I did not merely reproduce "Mr. Wallace's argument against Mr. Spencer's defence of 'use-inheritance.'" I accepted that argument as valid, but I extended it by emphasizing the importance of the factor of superimposed useful characters accumulated during successive periods of the phylogeny. I pointed out that large numbers of cases of co-adaptation might be thus accounted for, and I used Mr. Spencer's own illustration by way of example. In summing up his own conclusion, Mr. Wallace says: "The difficulty as to co-adaptation of parts by variation and natural selection appears to me, therefore, to be a wholly imaginary difficulty which has no place whatever in the operations of Nature" ("Darwinism," p. 418). Not only, therefore, has Dr. Romanes misrepresented my view, but he has gone further. The other "argument on the same side" referred to in the above communication is this very denial of co-adaptation as a fact in Nature. This, with most amazing *sangfroid*, is now claimed by my correspondent, who speaks of it as "the one which I had stated"! I must leave it to others to decide what value can be attached to the statements of a writer who adopts the principle of appropriating an argument, and putting it forward in a manner which would lead most readers to consider that he had been the first to elaborate it simply because he has expressed the same idea in abstract symbols instead of in concrete terms.

The next phase in the discussion is the admission by Dr. Romanes that Mr. Wallace's conclusion is correct, *i.e.* that co-adaptation is non-existent: "As it appears to me, from his reply, that Prof. Meldola's views on the subject of 'co-adaptation' are really the same as my own, I write once more in order to point out the identity" (NATURE, vol. xliii. p. 582). Mr. Romanes did more, therefore, than simply point out that we were agreed that this was "the only argument which could be properly brought against Mr. Spencer's position." He said that our views were "really the same," and this after I had accepted Mr. Wallace's conclusion as to the non-existence of co-adaptation. To crown all, he now tells us that he has no fully-formed opinion to express, but that he is in a condition of "suspended judgment"! I must really leave the case as it stands. If "neo-Darwinians" have a language of their own, at any rate it appears to be intelligible among themselves, if only from the circumstance that they have been enabled to stereotype a phrase which conveys their views with respect to the difficulty of following my correspondent's reasoning. I have been no more fortunate than other "neo-Darwinians" in this attempt, but in the endeavour to carry on the discussion of a biological question with a writer who stops short as soon as the subject assumes a truly biological aspect (see NATURE, vol. xliii. p. 582), I have become keenly impressed with the utter sterility of Dr. Romanes's method, which not only fails to advance our knowledge of the origin of species by any substantial contribution of fact, but which degrades the theoretical side of the subject into mere verbiage. If this is "palæo-Darwinism," I am rejoiced to think that I am grouped with those who are outside the pale.

In conclusion, to prevent further misunderstanding, let me add that, in admitting that the chances are "infinity to one" against a number of independent *useful* variations occurring when required in the same individual, I merely quoted the expression as given by Mr. Herbert Spencer and repeated by Dr. Romanes. I do not for a moment suppose that Mr. Spencer used the words in any more than a colloquial sense as indicating that there were "heavy odds" against such a combination, and in this sense only is my admission made. That the phrase has no exact mathematical significance is, I imagine, sufficiently obvious, but I have thought it desirable to make this qualification.

R. MELDOLA.

Physiological Selection and the Different Meanings given to the Term "Infertility."

IN the discussion concerning the segregation of varieties occupying the same region, and the influence of physiological selection in securing this result, it is necessary that we consider the different meanings given to some of the terms by different writers. The general fact on which Dr. Romanes insisted, in his paper on "Physiological Selection," was compatibility in the reproductive system of some, and incompatibility in that of others belonging to the same species. On p. 360 of his paper we read that "racial incompatibility," "however produced," "is the primary condition required for the development of varieties into species." Infertility and sterility are also used by him as equivalents for incompatibility in the reproductive system. Thus, on p. 400 we find the statement that "All natural varieties which have not been otherwise prevented from intercrossing, and which have been allowed to survive long enough to develop any differences worth mentioning, are now found to be protected from intercrossing by the bar of sterility—that is, by a previous change in the reproductive system of the kind which my theory requires."

Dr. Romanes did not attempt to catalogue the different forms of discriminative incompatibility that are included in the incompatibilities of the reproductive systems of different races, but reference was made to three forms: (1) to compatibility in the time of flowering in those of the same race, as contrasted with incompatibility in those of different races, as on pp. 352 and 356; (2) to greater numerical fertility when the male and female elements of the same race unite, than when those of different races unite, as in the note on p. 354; and (3) to numerical infertility through deficient production by hybrids, as on p. 369, and p. 357 in the note, and in the suggested experiments on p. 405, in which the pure and hybrid seed are both to be sown, and the comparative "degrees of fertility" to be noted. To these forms which were mentioned, we may add, as coming under the category of physiological incompatibilities, (4) lack of vigour in hybrids; (5) lack of adaptation in hybrids; (6) lack of escape from competition with kindred in hybrids; and (7) the superior energy and promptness with which the male and female elements unite in pure unions, as contrasted with cross unions. Dr. Romanes probably refers to this principle when he speaks of sterility as "failure to blend" (p. 365).

This last, when associated with the free distribution of the fertilizing elements, ensures the segregation (that is, the discriminative isolation) of two or more varieties occupying the same area and propagating during the same season, and therefore seems to me the most important of the forms of physiological segregation. This segregative principle, which I call potential or prepotential segregation, must, in almost every case, be operative between species and varieties that continue distinct while indiscriminately mingled on the same area and while fertilized by elements freely and indiscriminately distributed during the same season, for no other principle is able to secure free propagation and at the same time to prevent crossing under such conditions. Seasonal segregation is here excluded, and the other forms of physiological segregation when acting under such conditions are of little avail in preventing swamping unless carried to the extreme, and they then involve a waste of from one-half to the whole of the germs of the less numerous variety; for the most favourable case possible is when two varieties occupy the area in equal numbers, and such cases rarely exist, especially in the initial history of species.

Though numerical infertility and tardy potency are readily distinguished, complete impotence and complete numerical sterility are more likely to be confounded; for the complete incapacity of the male and female elements of different varieties for uniting involves failure to produce hybrids, as complete as when the elements unite without producing living offspring or germinating seed. The great difference is that in the case of cross impotence the germ remains unaffected by the alien fertilizing element, and therefore ready to be fertilized by any fertilizing element of its own kind that may reach it; while in the case of simple numerical cross sterility (if there be any such case) the alien elements promptly unite, and therefore leave no opportunity for subsequent fertilization by the coming of the kindred fertilizing elements. Cross impotence, with prepotence of pure unions when associated with the free distribution of the fertilizing elements, produces positive segregation; for, when characterizing varieties occupying the same area, it ensures the propagation of each with its own kind while preventing crossing; but numerical infertility of first crosses produces what I call negative segregation, for, though it is unable to secure segregate breeding, it lessens or obliterates the swamping effects of any crossing that takes place, and is therefore of great importance in the preservation of distinct varieties and species when the positive forms of segregation only partially prevent crossing. The four forms of hybrid inferiority mentioned above are also forms of negative segregation, and, though of the highest importance when co-operating with prepotential segregation or any other principle that partially prevents cross unions, are, it seems to me, incapable of preserving distinct varieties or species, when unassisted by any degree of positive segregation.

We are now prepared to see how the different meanings of infertility have occasioned more or less misunderstanding in the discussion of physiological selection and its effects. With Dr. Romanes, the seven forms of segregation above-mentioned are all forms of infertility between races, and therefore are all causes of physiological selection; while in my nomenclature, all but the first are included under impregnational segregation, and only the second and third are considered forms of cross infertility (or, in other words, of segregate fecundity). Using the term in this restricted sense, I have elsewhere maintained that it is very improbable that cross infertility is, in any case, the only isolative principle securing the continuance of distinct varieties and species indiscriminately commingled on the same area, even when the elements are freely distributed; and as this statement is liable to be taken as equally applicable to physiological selection, I wish to have it clearly understood that, in my usage, the two terms are not equivalent, and what I have said of cross infertility is not in the same sense true of physiological selection.

In Dr. Wallace's criticism of physiological selection, he seems to limit the meaning of infertility between races to numerical infertility of first crosses, and then assumes that this is the only incompatibility that is included under physiological selection. This limitation, if correct, would of course limit the effects that could properly be attributed to this principle.

Before closing I wish to raise the question whether a high degree of selective numerical fertility between races is not always associated with some degree of selective potential fertility. Or, using infertility in the more restricted meaning given in my nomenclature, is not a high degree of segregate fecundity and cross infertility always associated with some degree of segregate prepotence and cross impotence? As we know that these two forms of incompatibility are usually, if not always, associated in the segregation of species, is it not probable that they are similarly associated in the segregation of varieties? Again, as we know that segregate prepotence, when associated with the free distribution of the fertilizing elements, will produce prepotential segregation, effectually preventing crossing, without impairing powers of survival, and as there are many cases in which the continued segregation of varieties occupying the same area is due entirely to this principle, and many other cases in which it is due to weakened forms of this principle associated with other forms of incompatibility in the reproductive system, and still other numerous cases in which partial isolation (produced by a slight diversity of habits, or by the occupation of adjoining districts) would be speedily broken down except for these physiological incompatibilities, are we not fully warranted in the assertion that physiological selection is an essential factor in the evolution of many species?

The importance of this form of segregation having been recognized, the question naturally arises as to what have been the causes through which the incompatibility has ceased to be sporadic, and has become racial. As Dr. Romanes has not entered on the discussion of this point, I have given the more attention to it. I think I have succeeded in showing: (1) that any portion of a species subject to temporary isolation, through occupying a new station or district, is more or less liable to become incompatible with the rest of the species, owing to the cessation of reflex selection, by which the mutual fertility and other compatibilities of an inter-generating stock are kept in force (see NATURE, vol. xlii. pp. 28 and 369); (2) that partially segregative endowments are, through the very laws of propagation, cumulative (see "Divergent Evolution," Linn. Soc. Journn. —Zool., vol. xx. pp. 246–260); (3) that all the transformations that arise in forms thus segregated are inevitably divergent, and not parallel (see "Intensive Segregation," Linn. Soc. Journ.—Zool., vol. xxiii. pp. 312–322). JOHN T. GULICK.

26 Concession, Osaka, Japan.

Propulsion of Silk by Spiders.

THE author ("O. P. C.") of the article on "Arachnida" in the "Encyclopædia Britannica," says:—"The emission of silk matter appears to be a voluntary act on the part of the spider; but it is a disputed question among arachnologists whether spiders have the power forcibly to expel it, or whether it is merely drawn from the spinnerets by some external force or other. Mr. Blackwall, author of the 'History of Spiders in Great Britain and Ireland,' is of the latter opinion. Mr. R. H. Meade (Yorkshire) in Report of the British Association, 1858, thinks, that (from microscopic anatomical investigations which he has himself made) there is good evidence of spiders having the power to expel it; for he finds a certain muscular arrangement which would apparently suffice to give this power, and observers have actually seen the lines propelled."

Owing to the doubt herein expressed, may I ask your insertion of a chance observation lately made by me upon a spider, which has convinced me of the truth of the theory that spiders do expel their lines at will, and this, too, as secondary to one still remaining attached to the spinnerets?

She was hanging from the ceiling about 3 feet from a mullioned window, against which I was able to observe her movements most accurately. I was first led to observe her closely, by finding myself attached to her within one minute of my approach. On my breaking this line, she attempted to regain the ceiling; a breath of air from me stopped and brought her down again, when I saw her draw her legs together, pull her head up higher than the spinnerets of her abdomen by means of her ceiling-line, and, *following upon no visible effort of hers whatever*, I was the next moment conscious of the presence of another line stretching out from her spinners to a distance short of 3 feet, and at an angle of about 75° with the first. This line failing to find an attachment floated upwards and lay alongside of the other, and the spider again made for the ceiling. Nine times during the space of one hour, I got her to repeat this attempt to make a horizontal connection. Between two of the intervals of her attempts, I called in two naturalist friends who both witnessed with me, *and at the same instant of time*, the sudden appearance of the new line.

With each successive trial, I was able to substantiate and improve my observation; at first the appearance of the line seemed instantaneous, as to its whole length; next I was able to detect its elongation of itself after about 2 feet of its length was visible; then I could see it leaving the spinnerets; and finally, during the last moment of its travel, I could perceive very distinctly that it drew the spider slightly forward.

From these premises I can but infer that the viscid matter contained by the silk-glands, which, at the ordinary slow rate of emission, turns to gossamer immediately upon its exposure to the air, when expelled as now, violently, remains viscid sufficiently long to reach a certain distance.

These secondary threads, carried towards the ceiling by the spider, were never brought down again when she fell to the length of the main line, but were each time left, disconnected from her, at the spot from whence she fell when I blew her. Their loose end invariably floating upwards until alongside of the spider's main line, was, I think, noteworthy.

In conclusion, I would say that *sight* seemed to play no part in her choice of a direction for the connecting line; though I was close to her all the time, and indeed the only object apparently which was close enough, she only hit me the first time, when perhaps she had *heard* my approach; this may strengthen the remarks made by Mr. C. V. Boys in your number for November 13, 1890, where he says: ". . . . sight, as we understand the term, in spite of their numerous eyes, seems to be absent." S. J.

St. Beuno's College, St. Asaph, N.W., April 27.

The Crowing of the Jungle Cock.

I THINK there can be little doubt that Mr. H. O. Forbes has fallen into the same mistake as I had, in regard to Mr. Bartlett's statement that "none of the known wild species are ever heard to utter the fine loud crow of our domestic cock."

At first I took this to mean that the jungle cock did not crow *at all*, and was collecting notes from sporting men here, to supplement my own 26 years' experience, when yours of February 5 arrived, and by it I see that Mr. Bartlett implies that the crow is not so full, loud, and long, as that of our barn-door cock.

Mr. Forbes exactly gives the difference, as thinner, more wiry, and high pitched; it is also shorter, at least in the wild *G. ferrugina.* These I have often heard crowing, and shot in the extreme east of Asam, where for a very large area, on the Upper Diking River, and across Patkai, there are no inhabitants.

This same *G. ferrugina* is, however, to be found wild all over Asam, and the countries around; eggs found in the jungles are often hatched under domestic fowls, and hence these are frequently crossed, and the crow of the cock varies much in consequence.

But the difference between the wild *G. ferrugina* and our "barn-door" cock, in this particular, is so well marked that it could *invariably* be detected.

I may perhaps mention a curious sight I saw last year, within 100 yards of my bungalow, in the evening. A cloud of white ants were rising on the air, in the main road, and a jackal and jungle cock were busy eating the "neuters" swarming all over the ground; presently another jackal joined and the cock was between them: all were so busy feeding that they took no notice of each other, the jackals often lying on their bellies, while the cock moved about between them, at 2 or 3 yards only. By this time 15 or 20 people were looking on and laughing. Suddenly a third, younger jackal, joined the group, and after eating the ants a short time, and walking about like the others, dropped into the ditch and *stalked* the cock, crouching close to him. The latter at once flew, and made a bee line for the forest 400 yards off. The total area of the ants was about 20 feet by 8 only. S. E. PEAL.

Sibsagar, Asam, March 27.

Antipathy [?] of Birds for Colour.

WITH regard to the destruction of the yellow crocus by the sparrow, mentioned by your correspondent "M. H. M." in NATURE, vol. xliii. p. 558, this bird appears to have a predilection for yellow. In an article on "Birds' Nests and Nest-building," in the *Animal World*, present number, an instance is given of sparrows using the flowers of the laburnum for their nest. Only lately I have been watching them picking out the yellow centres of the daisy, but in this case *it was for food*, and I am inclined to believe that some portion of the crocus is also eaten. At this time of the year they are well known to be partial to buds and flowers of different kinds—for instance, the blossoms of the gooseberry bushes.

Doubtless, the bright yellow colour attracts the attention of this now much censured bird, so omnivorous in his tastes and such a general scavenger, and therefore not wholly to be condemned.

Clevedon, April 28. T. B. J.

The Destruction of Fish by Frost.

REFERRING to Prof. Bonney's letter in NATURE, vol. xliii. p. 295, regarding the destruction of fish by frost, and in which he asks for information from more northern latitudes, I may say that during the winter of 1885–86, at Cape Prince of Wales, Hudson's Strait, when the thickness of ice in a small lake was being measured, live fish were often seen; and upon the last occasion, when the ice measured six feet and half an inch, several were thrown up with the water that, upon our cutting through, immediately overflowed. These fish were about an inch and a half in length and were extremely lively. I may add that during the summer both feeder and outlet of the lake averaged about eight inches in depth and the lake nine feet in its deepest part. The former ceased to flow on November 8, when, too, ice, fourteen inches in thickness, covered the lake. F. F. PAYNE.

Meteorological Service of Canada,
Toronto, April 16.

The Flying to Pieces of a Whirling Ring.

WITH reference to the recent discussion in your columns on the whirling of steel bands, the following results will be of interest.

A weldless steel flask, with spherical body 12 inches in diameter and $\frac{3}{8}$ inch thick, constructed for use in a centrifugal milk separator, to revolve about its axis of symmetry at a normal speed of 7000 revolutions per minute, was whirled at a gradually increasing speed, with a view to ascertaining the "bursting" velocity.

At 16,000 revolutions per minute the body of the flask had bulged 2 inches in diameter: this is equivalent to an extension of 17 per cent. of the circumference; the peripheral speed being 840 feet per second, and the tension 31·5 tons per square inch.

The experiment was not continued, as it was considered sufficiently satisfactory, and the bulged flask is kept as a curiosity.

CHAS. A. CARUS-WILSON.

McGill University, Montreal.

HERTZ'S EXPERIMENTS.[1]

III.

IN the last article the principles upon which a rapidly vibrating electric oscillator should be constructed were considered, and how the sudden break-down of the air gap enabled these rapid vibrations to be started. It is probable that this break-down occurs in a time smaller than the thousand millionth of a second. How very rapid interatomic motions must be!

Consider now the principles on which an apparatus is to be constructed to receive the vibrations produced by this oscillator. We may observe in the first place that as we are dealing with a succession of impulses at equal intervals of time we can utilize resonance to accumulate the effect of a single impulse. Resonance is used in an immense variety of circumstances to accumulate the effect of a series of impulses, and is avoided in another immense variety of circumstances to prevent accumulating the effect of a series of impulses. We see, we hear, we photograph by using it; we use it to make musical sounds, to keep clocks and watches going, to work telegraphs. By avoiding it carriages drive safely over rough roads, ships navigate the seas, the tides do not now overwhelm the land, the earth and planets preserve their courses round the sun, and the solar system is saved from destruction. Resonance may be thus described:—If a system is able to vibrate by itself in any way, and if we give it a series of impulses, each tending to increase the vibration, the effect will be cumulative, and the vibration will increase. To do this the impulses must be well timed, at intervals the same as the period of vibration of the system itself. Otherwise some of the impulses will tend to stop the vibration, and only some to increase it, and on the whole the effect will be small. In order to use resonance in the construction of the detector of waves of electric force, we must make our detector so as to be capable of an electric vibration of the same period as the generator of the waves. If we do this we may expect the currents produced in it to be increased by each wave, and thus the electrification at its ends to increase, and so increase the chance of our being able to produce a visible spark. Two ways of using a detector have been mentioned. One is to observe the heating of a conductor by the current in it, and the other to observe a spark due to the electrification at the end of the conductor. The latter is the most sensitive and has been most frequently employed, and is the method first employed by Hertz. Two forms of detector may be used for observing sparks. One form consists of a single conductor bent into a circle with its two extremities very close together. An electric charge can oscillate from one end of this to the other round the circle and back again. If the circle be the proper size, about 70 cm. in diameter for the large sized oscillator and about 8 cm. in diameter for the smaller sized one described in the last article, the period of oscillation of this charge will be the same as that of the charge on the generator of the waves, and its oscillation will be increased by resonance until, if the ends of the circular wire be close enough together, the opposite electrification of the ends will become great enough to cause a spark across the gap. The other form of detector depends on using two conductors, each of which has the same period of electric oscillation as the oscillations we wish to detect. These

[1] Continued from p. 14.

are placed in such a position that an end of one is near that end of the other which will at any time be oppositely electrified. For example, if the electric force in our waves be in vertical lines, then if we place two elongated conductors, one vertically above the other and separated by a very small air space, the electric force alternating up and down will cause currents to run up and down the conductors simultaneously, and the upper ends of both will be similarly electrified at any instant, while the lower end of the upper one will always be oppositely electrified to the upper end of the lower conductor, and if these two points, or two short wires connected with them, be close enough together, a spark will pass from one to the other whenever the electric force sets up these electric oscillations in the conductor. Thus this apparatus is a detector of the electric force. Whenever there is a spark we may be sure that there is electric force, and whenever we cannot get a spark we may be sure that there is either no electric force or anyway too little to produce sparks. The apparatus will be more sensitive for electric forces that oscillate at the same rate as the natural vibration of the electric charge on the conductor, because the effect of each impulse will then add to that of the last; resonance will help to make the electrifications great, and so there will be a better chance of our being able to produce a spark. We may weaken the strength of this air gap by reducing the pressure of the air in it. To do this the ends of the conductors, or wires connected with them, must lead into an exhausted air vessel, such as a Geissler's tube. There is no doubt that much longer sparks may thus be produced, but they are so dim and diffused that when dealing with very minute quantities of electricity those sparks in a vacuum are not more easily seen than the smaller and intenser sparks in air at atmospheric pressure. The additional complication and difficulty of manipulation from having the terminals in a vacuum are not compensated for by any advantages. This whole detecting apparatus works on somewhat the same principle as a resonator of definite size connected with one's ear when used to detect a feeble note of the same pitch as the resonator. Such a resonator might very well be used to find out where this note existed and where it did not. It would detect where there were compressions and rarefactions of the air producing currents of air into and out of your ear. In the same way the conductor sparking tells where there are alternating electric forces making currents alternately up and down the conductor, and ultimately electrifying the end enough to make it spark. In the sound resonator there is nothing exactly like this last phenomenon. We have much more delicate ways of detecting the currents of air than by making them break anything. If anybody would allow the electric currents from a Hertzian detector to be led directly into the retina of his eye, it would probably be a very delicate way of observing, though even in this direct application of the current to an organ of sense it is possible that these very rapidly alternating currents might fail to produce any sensible effect, for they are not rapid enough to produce the photochemical effects by which we see.

To recapitulate the arrangements proposed in order to detect whether electric force is propagated with a finite velocity, and if possible to measure it if finite. It is proposed to create electric oscillations of very great rapidity, oscillating some four or five hundred million times per second, and it is expected thereby to produce waves of electric force whose length will be less than a metre if they are propagated with the velocity of light. It is proposed to do this by causing an electric charge to oscillate backwards and forwards between two conductors, and across an air gap between them. This oscillating charge is to be started by charging the conductors, one positively and the other negatively, until they discharge by a spark across this air gap. By making the conductors small, and the distance the charge has to go from one to the other small, the rate of oscillation of the charge can be made as great as we require. If waves are produced by this arrangement, we can reflect them at the surface of a large conducting sheet, and then loops and nodes will be produced where the incident and reflected waves co-exist. The loops will be places where the alternating electric forces are great, while at the nodes there will be no electric forces at all. In order to detect where there are these alternating electric forces and where there are none, it is proposed to use either a single wire bent nearly into a circle, with a very minute air gap between its ends, or else two conductors placed end to end, with a minute air gap between their ends. In either case, if the natural period of vibration of a charge on the single conductor, or on each of the conductors in the second arrangement, is the same as the rate of alternation of the electric force we wish to detect, there may be sufficient electrification of the neighbouring ends to cause a spark across the minute air gap. We are thus in possession of a complete apparatus for determining whether electric waves are produced, and what their wave-length is.

The experiment is conducted as follows:—

The two conductors which are to generate the waves are placed, say, one above the other, so that the electric charge will run up and down in a vertical line across the spark gap between them. They might be placed horizontally or in any other line, but for definiteness of description it is well to suppose some definite position. We may call them A and B. They are terminated in polished knobs, between which the spark passes. A and B are connected with the terminals of a Ruhmkorff coil, or a Wimshurst or other apparatus by which a succession of sparks may be conveniently made to pass from A to B. Before the spark passes, A and B are being electrified, and when the spark occurs the electricity on A rushes over to B, and part of it charges B, while the electricity on B rushes across the spark, and partly charges A, this taking place alternately up and down. Each time there is less electricity, for some is neutralized during each oscillation by the opposite charge; for energy is being spent, some in overcoming the resistance of the spark gap, *i.e.* in producing the heat developed there, and some in producing electric waves in the surrounding medium. Thus the electric energy of the two oppositely charged bodies A and B is gradually dissipated, and one way of describing this is to say that the two opposite electric charges combine and neutralize one another. This whole language of talking of electric charges on bodies, and electric currents from one to the other, of electric charges neutralizing one another, and so forth, is not in accordance with the most recent developments of electro-magnetic theory. At the same time, those for whom these articles are written are familiar with this language, and with the view of the subject that it is framed to suit, while they are unfamiliar with ether electrically and magnetically strained and thereby the seat of electric and magnetic energy, and consequently it would have added very much to their difficulty in grasping the details of a complicated question if it had been described in unfamiliar terms, and from an unfamiliar point of view.

The electric force in the neighbourhood of the vertical generator will lie in vertical planes through it, and as A and B are alternately positive and negative, the electric force will alternately be from above downwards, and from below upwards. If, then, this force is propagated outwards in a series of waves, we may expect that all round our generator waves of electric force will be diverging; waves in which the force will be alternately down and up. The state of affairs might be roughly illustrated by elastic strings stretched out in every direction from our generator. If their ends at the generator be moved alternately down and up, waves will be propagated along the strings, waves of alternate motion down and up.

In order to reflect these waves, we require a metallic sheet of considerable area some two or three wave-lengths away from the generator; so far away in order that we may have room for our detector to find the loops and nodes formed every half wave-length where the outgoing waves meet those reflected from the screen. Not too far away, or our waves will be too feeble even at the loops to affect our detector. The waves are thrown off all round, but are most intense in the horizontal plane through the spark, so that our detector had better be placed as near to this plane as possible. The detector may be either a very nearly closed circle of wire, or two conductors, each somewhat longer and thinner than the combined lengths of the generating conductors, and placed vertically over one another, and separated by a minute air gap. As the theory of this latter form of detector is simpler than that of the circle, it will simplify matters to consider it alone. The two conductors should each have a period of electrical oscillation up and down it, the same as that of the charges on the generator. The generator consists of two conductors certainly, but then during the time the spark lasts they are virtually one conductor, being connected by the spark across which the electric charges are rushing alternately up and down. Hence the period of oscillation of the charges on the generator corresponds to that on a single conductor of the same size as its two parts combined. Various experiments have been made as to the best form for these conductors that form the detector. They might be made identical with the generator, only that the spark gap in the generator should be represented by a connecting wire. They may be longer and thinner. If longer, they should be thinner, or they will not have the same period of vibration. On the whole, the best results have been got with conductors somewhat longer and thinner than the generator. It is not generally convenient that the spark between the two conductors that form the detector should take place directly from one to the other. It is not easy to make arrangements by which distance apart of these conductors can be regulated sufficiently accurately. The most convenient way is to connect the lower end of the upper conductor and the upper end of the lower one each with a short thin wire leading, one to a fixed small knob, and the other to a very fine screw impinging on the knob. The screw may then be used to adjust the spark gap between it and the small knob with great accuracy. This spark gap must be very small indeed, if delicate work be desired. A thousandth of a centimetre would be a fair-sized spark gap. The minute sparks that are formed in these gaps when doing delicate work are too faint to be seen, except in a darkened room. Having placed the detector in position between the generator and the screen, the difficult part of the observation begins. It is heartrending work at first. A bright spark now and then arouses hope, and long periods of darkness crush it again. The knobs of the generator require repolishing; the spark gap of the detector gets closed up; dust destroys all working; and not without much patience can the art be attained of making sure of getting sparks whenever the conditions are favourable, though it is easy enough not to get sparks when the conditions are unfavourable. Before making any measurements, all this practice must be gone through. It is hard enough with the success of others before us to encourage us, with their advice to lead us, with a clear knowledge of what is to be expected to guide us. How much credit, then, is due to Hertz, who groped his way to these wonderful experiments from step to step, without the success of others to encourage him, without the advice of others to lead him, without any certainty as to what was to be expected to guide him. Patiently, carefully, through many by-paths, with constant watchfulness, and checking every advance by repeated and varied experiments, Hertz worked up to the grand simplicity of the fundamental experiment in electricity that is engaging our attention.

Having gained command over the apparatus, we may look about for places where sparks occur easily, and for others where they cannot be produced. Two or three places may be found where no sparks can be observed. These places will be found to be nearly equidistant. They are the nodes we are in search of. The distance between any pair is half the distance an electric wave is propagated during the period of an oscillation. Their presence proves that the electric force is not propagated instantaneously, but takes time to get from place to place. If the electric force were propagated instantaneously, there might be *one* place where the action of the currents induced in our reflecting sheet neutralized the direct action of our generator; but there could not be a series of two or more such places between the generator and the reflecting sheet. That there are more than one proves that electric force is propagated from place to place, and does not occur simultaneously everywhere. It sets the crowning stone on Maxwell's theory that electric force is due to a medium. Without a medium there can be no propagation from place to place in time. It only remains to confirm by calculation that the rate of propagation is the same as that of light. This is a complicated matter. It involves the question of how fast should, on any theory, the charge oscillate up and down a conductor. The problem has only been accurately solved in a few special cases, such as that of a sphere by itself. The conductors that have been employed are not this shape, are not by themselves, and so only rough approximations are possible as to the rate at which these oscillations occur. Knowing the wave-length will not determine the velocity of propagation unless we know the period of vibration; and consequently this direct measure of the velocity has only been roughly made; but it agrees as accurately as could be expected with Maxwell's theory that it must be the same as the velocity of light if electrical phenomena are due to the same medium as light. The conviction that more accurate determinations will confirm this agreement is founded upon safe ground. It was pointed out that the ether that transmits light and is set in vibration by the molecules of matter can hardly avoid moving them itself. This ether can hardly help having other properties than merely transmitting a comparatively small range of vibrations. It can hardly help producing other phenomena. When it has been shown that, if there is a medium concerned in conveying electric and magnetic actions, it must possess properties which would enable it to transmit waves like light; and when it has been shown that there *is* a medium concerned in conveying electric and magnetic actions, and that the rate at which they are conveyed is approximately the same as the rate at which light is propagated; the conclusion is almost unavoidable that we are dealing with the same medium in both cases, and that future experiments, capable of accurate calculation and observation, will confirm the conclusion that electric force is propagated through, and by means of, the luminiferous ether with the velocity of light. We really know very little about the nature of a wave of light. We know a great deal more about electric and magnetic forces, and much may be learnt as to the nature of a wave of light by studying it under the form of a wave of electric force. The waves produced by the Hertzian generator may be a metre long or more. The difficulty is to get them short enough. We know a good deal about how they are produced, and from this, and also by means of suitable detectors, we can study a great deal about their structure. They are truly very long waves of light. Atoms are Hertzian generators whose period of vibration is hundreds of millions of millions per second. A Hertzian generator may vibrate rapidly, but it is miserably slow compared with atoms. And yet the wonder is that atoms vibrate so slowly. If a Hertzian generator were, say, 10^{-7} cm. long, about the size of a good big atom, its period of vibration would be some hundreds of times too rapid to produce ordinary light. Atoms are probably complicated Hertzian generators. By making a complicated shape, as, for example, a Leyden jar, a small object may have a slow period of vibration. All that is required is that the capacity and self-induction may be large in comparison with the size of the conductor. We saw that these rapidly vibrating generators have but little energy in them: they rapidly give out their energy to the ether near them. This is also the case with atoms. These, when free to radiate, give up their energy with wonderful rapidity. How short a time a flash of lightning lasts! It is hardly there but it is gone: the heated air molecules have so suddenly radiated off their energy. The reason why atoms in the air, for instance, do not radiate away their energy like this is because all their neighbours are sending them waves. Each molecule is a generator, but it is a detector as well. It is kept vibrating by its neighbours: it occupies a part of the ether that is in continual vibration, and so the atom itself vibrates. As each atom can radiate so rapidly, it must be a good detector: its own vibrations must be very much controlled by the neighbourhood it finds itself in; and as the waves of light are very long compared with the distances apart of molecules, those in any neighbourhood are probably, independently of their motions to and fro, each vibrating in the same way. It is interesting to calculate how much of the energy in the air is in the form of vibrations of the ether between the molecules of air. A rough calculation shows that in air at the ordinary density and temperature only a minute fraction of the total energy in a cubic centimetre is in the ether; but when we deal with high temperatures, such as exist in lightning-flashes and near the sun, and with very small densities, there may be more energy in the ether than in the matter within each cubic centimetre. All this shows how wide-reaching are the results of Hertz's experiments. They teach us the nature of waves of light. We can learn much by considering how the waves are generated. Let us consider what goes on near the generator, consisting of two conductors, A and B, sparking into one another. Before each spark, and while A and B are being comparatively slowly what is called charged with electricity, the ether around and between them is being strained. The lines of strain are the familiar tubes of electric force. If A be positive, these tubes diverge from all points of A, and most from the knob between it and B, and converge on B. Where they are narrow, the ether is much strained; where wide, the ether is but little strained. Each tube must be looked upon as a tube of unit strain. The nature of the strain of the ether is not known; it is, most probably, some increased motion in a perfect liquid. We must not be surprised at the nature of the strain being unknown. We do not know the nature of the change in a piece of India-rubber when it is strained, nor indeed in any solid, and though the ether is much simpler in structure than india-rubber, it can hardly be wondered at that we have not yet discovered its structure, for it is only within the present century that the existence of the ether was demonstrated, while men have known solids and studied their properties and structure for thousands of years. Any way, there is no doubt that the ether is strained in these tubes of force when A and B are oppositely charged, and that the energy per cubic centimetre of unstrained ether is less than that of strained ether, and that the work done in what is called charging A and B is really done in straining the ether all round them. When the air gap breaks down, and an electric spark takes its place, there is quite a new series of phenomena produced. Suddenly, the strained ether relieves itself, and, in doing so, sets up new motions in itself. The strained state was probably a peculiar state of motion, and in changing back to ordinary ether a new and quite distinct state of motion is set up. This new state of motion all round the conductors is most intense near the spark, and is usually described as an electric current in the conductors and across the spark, or as a rushing of the electric charge from one conductor to the other. The electric current is accompanied by magnetic force in circles round it, and the tubes of magnetic force define the nature of the new movement in the ether as far as we know it. Hitherto, for the sake of simplicity, the existence of this magnetic force has been unnoticed. It is due to a peculiar motion in the ether all round what are called electric currents. The current in fact consists of little else than a line, all round which this movement is going on; like the movement surrounding an electrified body, but also unlike it. Whenever electric forces are changing, or electrified bodies moving, or electric currents running, there this other peculiar motion exists. We have every reason for thinking that this, which may be called the magnetic strain in the ether, as the movement all round electrified bodies was called the electric strain—that this magnetic strain only exists in these three cases: (1) when the electric strain is changing; (2) when electrified bodies are moving; and (3) when electric currents are running. These three may be all cases of one action: certainly the magnetic strain that accompanies each is the same, and it seems most likely that the electric change is only another aspect of the magnetic strain. There are analogies to this in the motion of matter that partly help and partly annoy, because they partly agree and partly will not agree with the etherial phenomena. Take the case described in a former article of a chain transmitting waves. Attention was drawn to the displacement of a link and to its rotation. Now for the analogy: to seem at all satisfactory the first thing that would strike one would be to pay attention to two *motions*, to the velocity of displacement of the link and to its rotation. This would lead to interminable difficulties in carrying out the analogy. We cannot liken electric strain to a velocity in this direct and simple way, because, what are we to do with a change in the strain which produces the same effects as a continuous current? A change in the strain is all very well, it would be like a change in the velocity, but what about a continuous change in the velocity: we can hardly suppose a velocity continually increasing for ever: we are evidently landed in immediate difficulties. It is better therefore to be content to liken the electric strain to a displacement of the chain link. It seems most likely that it really is a peculiar motion in the ether, but we must be content for the present with the analogy. If we want to drive it further, we must suppose stress in the chain that draws the link back to be due to a motion in the chain or of things fastened to it, and then the changed motions produced by a displacement of the chain might be analogous to the peculiar motions accompanying electric strain. It would lead us too far to work out this analogy. Returning to the simpler case of the displacement of the link representing electric strain, and the velocity of its rotation representing magnetic strain, see how the actions near a Hertzian generator may be likened to what takes place when a wave is being sent along a chain. While the conductors are being slowly charged we must suppose electric strain to be produced in all the surrounding space. This is a comparatively slow action, and as the rate of propagation is very rapid, the electric strain will rise practically simultaneously in the whole neighbourhood, and that it does so is a most important fact to be taken account of in all our deductions from these experiments. This slow charging must be represented by a slow raising of one end of the chain, which raises the rest of it to a great distance apparently simultaneously if the raising be done slowly. Suddenly the air gap breaks. This might be represented by lifting the chain with a weak thread, and by having the end of the chain fastened to a pretty strong spring. When the thread broke the spring would pull the chain back quickly, would pass its position of equilibrium, and thus commence a series of rapid vibrations on each side of this position; the vibrations would gradually die away owing to the energy of the spring being gradually spent, partly on friction in itself, and partly in sending waves along the chain. In actually performing the experiment, an india-rubber tube or limp thin rope is better than a chain when hung horizontally, as the chain is so heavy; when it can be hung vertically, a chain does very well. In the description it simplifies matters to describe a chain, because it is easier to talk of a link than of a bit of the rope: a link has an individuality that identifies it, while a bit of the rope is so indefinite that it is not so easy to keep in mind any particular bit. Consider now what these waves are, what sort of motion originates them. When the spring first starts, the near parts of the chain move first. What happens to any link? One end of it moves down before the other. What sort of motion, then, has the link? It must be rotating. Thus it is that change in the displacement is generally accompanied by rotation of the links. Thus it is that change in electric strain is accompanied by magnetic strain. The analogy goes farther than this. Each wave thrown off may be described as a wave of displaced or as a wave of rotating links, and the most displaced are at any time the most rapidly rotating links. Just in the same way, what have hitherto been called waves of electric force may also be looked upon as waves of magnetic force. Because there are two aspects in which the motion of the chain may be viewed does not diminish from the essential unity of character of the wave-motion in its waves; and similarly the fact that these Hertzian waves have an electric and a magnetic aspect does not diminish from the essential unity of character of the wave-motion in them. At the same time the two elements, the displacement of a link and the rotation of a link, are quite distinct things; either might exist without the other; it is only in wave propagation that they essentially co-exist. In the same way electric strain and magnetic strain are quite different things; though in wave-motion, and indeed whenever energy is transmitted from one place to another by means of the ether, they essentially co-exist.

FIVE YEARS' PULSE CURVES.

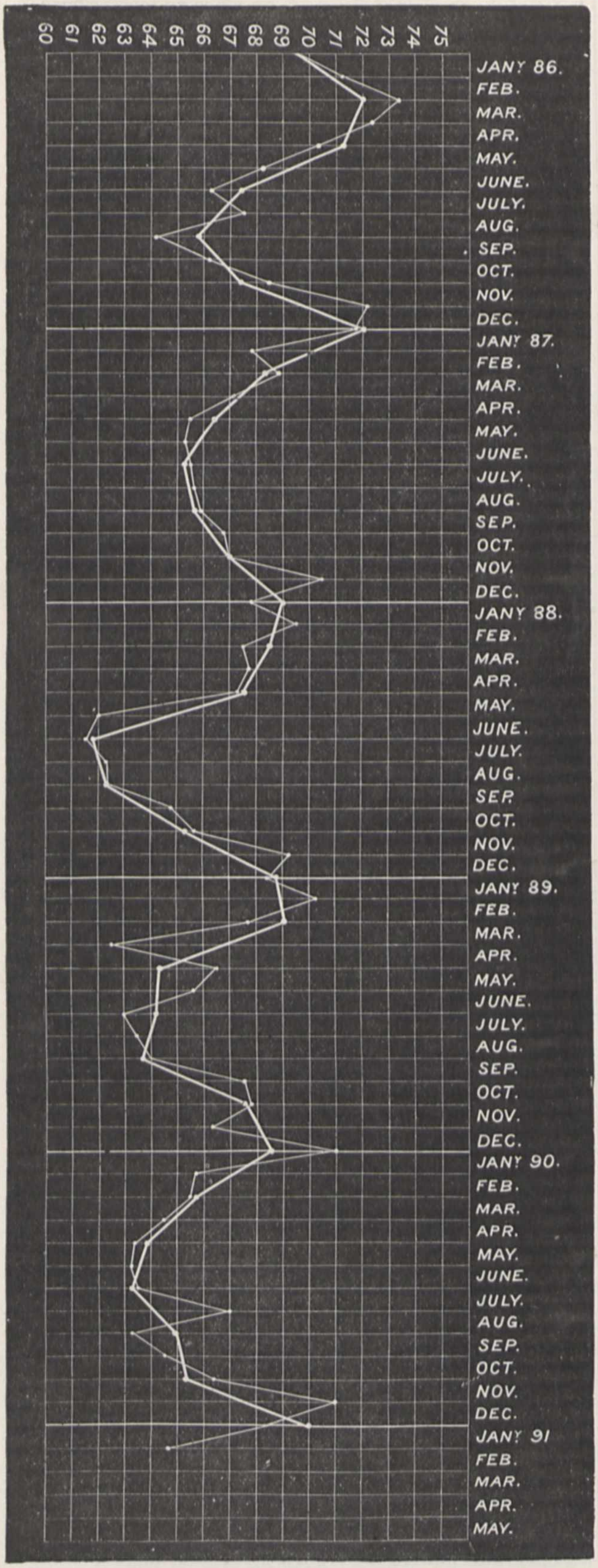
OVER five years ago it occurred to me that there would be considerable interest in keeping a systematic record for some time of the rate of pulsation, *i.e.* of the number of beats (per minute) of the pulse. I therefore commenced the practice by taking, every night, an observation of my own pulse; these observations, originally undertaken solely for my own personal interest, have been continued without intermission up to the present time; and, on throwing the results into a graphic form, I found so close a symmetry and concord between the curves for these five years, that I thought it might be interesting to readers of NATURE to have these results put before them.

First, then, as to the method adopted in these observations. I count the pulse beats for one minute[1] every night[2] before retiring to bed, and invariably while in a standing posture. From the records thus obtained the average for each month is deduced in the usual way, viz. by adding together all the numbers for the month, and dividing by the number of days on which observations were taken. With regard to this important consideration —*i.e.* of the number of observations, since an insufficiency thereof would, of course, greatly vitiate the value of my curves—I may state that during the first four years I omitted to take an observation on only seventeen nights altogether. During the fifth year, I find twenty-one observations missed. Nevertheless, the *net* data from which the curves are deduced are not quite so abundant as this statement would imply; for, in calculating the monthly average, I have invariably struck out altogether *all readings above* 79. My reason for this procedure was simply that I wished to obtain a curve showing the normal pulsations; now, anything much above 75 is abnormally high (especially in my own individual instance, for it will be noticed that my pulse is below the usual average of 70), and I can nearly always assign a distinct cause, such as the feverishness caused by a cold, or excitement, or recent exercise; it therefore appeared to me fairest to knock out altogether the results of such disturbing causes, and since for this purpose an arbitrary line must be drawn somewhere, I decided to draw it at 79. On the other hand, however, I have retained all the other readings, no matter how low they might be, although the "fifties" are very common, and occasionally even the "forties" have been touched. It might, perhaps, be thought that these very low readings should be neglected equally with the very high, but such a course appeared to me altogether illegimate, both because such low readings seemed, judging from their occurrence, to be, so to speak, *normally* caused, and unassignable to any distinct extraordinary cause known to myself, and also because I should hardly have known where to draw a minimum line. However, I now regret that the readings below 50, at any rate, were not rejected; but such readings are so extremely rare that they cannot have much influenced the curves. In order that the reader may judge for himself on what data these curves are founded, I have appended a table showing the *net* number of readings from which each monthly average was drawn, and have also stated (in brackets) the number of readings below 60 included in each month.

Turning now to the curves themselves, this monthly average is shown by the *thin* line. It is necessary to explain that these curves were drawn by marking the monthly average by a dot on the *extreme right* of each space representing a month. I was undecided for some time whether to adopt this plan or to mark this dot in the *middle* of each monthly space; but after trying both plans I concluded that now adopted to be the simpler. The actual curves were, of course, obtained by connecting all these dots by straight lines.

On examining this monthly curve, it is at once obvious that there is a strong similarity between the five years; clearly every year the curve falls through the spring, until about midsummer, and then rises wonderfully steadily and regularly in every case (except in 1889) through the autumn to November or December. On the whole, two maxima seem to be indicated—namely, one in November, followed by a fall, and then by a rise to another maximum in February or January. But it will be noticed that in the winter 1889-90 there is the unusual phenomenon of a fall through November, and then the two maxima are replaced by an intermediate maximum reached in December. So that here, in spite of the broad concord and regularity, there was rather too much local irregularity to be altogether satisfactory. In the lower portions of the curves, again, there is even more irregularity. Those of 1887 and 1888 (but emphatically the former) are indeed remarkably free from aberration; but in 1886 there is an extraordinarily abrupt and irregular rise through July, followed by a compensating fall through August. In 1890 there is an almost identical irregularity in the same two months; while in 1889 we have a remarkable irregularity in the spring. Now these irregularities puzzled me a good deal; still, in each case

[1] Thus avoiding the considerable error that is introduced by counting for, say, fifteen seconds only, and multiplying.
[2] The time has varied from 23.0 o'clock to 1 o'clock.



(except that of the winter irregularity, 1889–90), I could assign a fairly plausible explanation. For instance, during the summer of 1886 I was under medical treatment: in July of 1890 I was touring among the Swiss mountains: while at the end of February 1887 I had removed from a low-lying northern suburb, to a rather higher southern one; this change might with some plausibility be considered as the possible disturbing cause in the 1889 spring curve.

Nevertheless, looking at the results as a whole, I was not satisfied with the curves: it appeared to me as by no means improbable that the monthly average was calculated on a rather too short period, thus allowing temporary disturbing causes to manifest themselves unduly. I therefore determined to try the effect of calculating the averages on a *two-monthly* period, throwing into one total January and February, March and April, May and June, July and August, September and October, November and December respectively. On drawing the curves corresponding to these averages (*thick*-lined curve), I was delighted to find order and symmetry completely regnant: all the aberrations have of course disappeared, and order is supreme. This two-monthly curve clearly shows a single maximum in winter, followed by a fall to the minimum at midsummer, and then by a rise to the winter maximum.

It is evident that the curves for all five years are very closely similar, though by no means identical[1] in nature; but I am especially anxious to point out the extraordinary symmetry displayed by the *curves on either side of a maximum or minimum* point. For instance, the curves for the following periods,

1886 July–October,	1888 November–May 1889,
,, April–December,	1889 The whole year,
,, November–February 1887,	

are wonderfully symmetrical, in some cases even being almost geometrically exact.

What, however, may be the exact interpretation of these curves I must leave it to those better acquainted than myself with physiology to decide; but it is worth noting that these curves are exactly contrary to the statement in Michael Foster's text-book, that the pulse *is said* to rise in summer.[2]

The following is the table above referred to as showing the *net* data for each month, and also (in brackets) the number of readings below 60 included in each case:—

1886.		1888.	
January	28	January	25 (1)
February	19	February	23 (1)
March	21	March	24 (3)
April	25	April	30 (4)
May	30 (1)	May	30 (11)
June	29	June	30 (11)
July	30 (1)	July	31 (10)
August	28 (8)	August	28 (9)
September	29 (2)	September	28 (5)
October	29 (1)	October	31 (5)
November	25	November	26 (2)
December	23	December	28 (3)
1887.		1889.	
January	28 (2)	January	25
February	26 (1)	February	23 (2)
March	30 (3)	March	31 (9)
April	27 (4)	April	28 (8)
May	27 (3)	May	28 (3)
June	25 (5)	June	29 (10)
July	31 (3)	July	28 (4)
August	30 (6)	August	28 (7)
September	28 (2)	September	27 (4)
October	30 (2)	October	30 (2)
November	23	November	26 (4)
December	25 (2)	December	25 (1)

[1] Seeing how very many causes must co-operate in producing the one resultant—of pulsation-rate—it would be very strange if the curves for different years *were* identical.

[2] I quote from the third edition.

1890.

January	27 (5))	July	24 (2)
February	24 (5)	August	30 (8)
March	28 (4)	September	29 (7)
April	28 (7)	October	24 (5)
May	27 (7)	November	22 (1)
June	26 (8)	December	30

If these numbers be compared with the curves, it will be found that in a rough way they agree with them; the diminishing number of these low readings every autumn, no less than their increase towards the summer, being obviously correlated with the rise and fall of the curves.

F. H. PERRY COSTE.

THE SCIENCE MUSEUM AND GALLERY OF BRITISH ART AT SOUTH KENSINGTON.

VIGOROUS protests continue to be made against the appropriation, for the new Gallery of British Art, of the site which ought to be used, as originally intended, for the Science Museum. Several letters on the subject by men of high authority have been printed in the *Times;* and on Tuesday a deputation, which could not but command attention and respect, waited upon Lord Cranbrook and Mr. Goschen to represent to them the opinions held by all who are in a position to form a trustworthy judgment on the question. The Government are still engaged in considering the matter, and it is to be hoped that they are receiving and giving heed to the counsel of their natural advisers, although, unfortunately, this is *a priori* extremely doubtful.

We print the letters addressed to the *Times* by Sir F. Bramwell, Mr. Poynter, and Sir J. Coode, and an account of the proceedings of the deputation on Tuesday.

IT has for many years been recognized that the science collections at South Kensington are housed in a manner which largely diminishes their value for their principal use—viz. that in connection with the Royal Normal School of Science.

This school, as every one knows, is, as regards its main building, situated on the east side of Exhibition Road, while the collections are scattered about in the South Gallery and in the West Gallery adjacent to Queen's Gate.

In 1885 the Government appointed an inter-departmental committee to consider the subject and to report, and they nominated me, as being unconnected with any department, chairman of the committee. The committee (with one dissentient) reported in the sense that on the land lying west of Exhibition Road, and between that road and Queen's Gate, suitable buildings should be erected according to a complete design, but that they should be carried out in successive portions. Nothing was done on this report.

In 1889 another committee was appointed; this committee made very similar recommendations, and last year the Government acquired further land.

There are now on the west side of Exhibition Road, and immediately opposite the science schools, the observatories used by Mr. Norman Lockyer, and also a newly-erected physical laboratory.

Everything seemed to be, after all these years of waiting, in train for affording the needed accommodation, when, incredible as it must appear, the Chancellor of the Exchequer announced that the whole of this well-considered and satisfactory arrangement is to be given up. He stated it had been determined to sweep away the observatories and the physical laboratory, already on the west side of the road, and close to the science schools, and to devote this particular plot of ground to a picture gallery. I look upon this as a most disastrous proceeding, and one that, in the interest of the great National Department at South Kensington, should not be entertained for one moment.

Any one who will take the pains to visit the ground, or even to look at an accurate plan of it, will see that there is plenty of good space available for the picture gallery without interfering with the needs of the science collection, and that the notion of building it where proposed is so thoroughly preposterous that, as our American friends say, it must have originated in "pure cussedness."

FREDERICK BRAMWELL.

No. 5 Great George Street, Westminster, May 9.

SIR FREDERICK BRAMWELL in his letter of this morning points out the disastrous effect on the interests of the national Department of Science at South Kensington which will result from the intrusion of the new Gallery of British Art, to be planted precisely on the spot where it will cause the greatest amount of inconvenience. To an artist a still more flagrant instance of "pure cussedness" in this matter would appear to be that the building should be placed where it can have no connection with the existing galleries, when there is a piece of ground higher up the road in immediate connection with them.

The galleries on the east and west of the Horticultural Gardens, which were built for pictures at the time when there was a scheme for holding annual international exhibitions, are, whether by a happy "fluke" or by careful calculation on the part of their constructor, General Scott, without doubt the best lighted and the best proportioned picture galleries that have ever been constructed in England. Sir Frederick Leighton has, I know, expressed this opinion, and every artist who exhibited in these galleries during the three or four years that the exhibitions were held there will, I believe, agree in it: "We never saw our pictures look so well." These galleries are even now being connected by a building crossing the intervening space, the lower half of which will belong to the Imperial Institute, while the upper part is to be available for purposes of exhibition, thus making a connected group, and what would appear to be an unrivalled building for the purposes of a Gallery of British Art.

Why these buildings, acknowledged to be as good as they can be, and actually ready on the spot, should not be used for this purpose, according to what I understand was the original and nearly accepted scheme, it is somewhat difficult to understand. If the building for which £80,000 has been so liberally offered were placed higher up the road, above the Technical Institute, where there is a piece of ground available, it would back immediately on the Eastern Gallery, in which the Indian collection is now housed, thus affording provision for the extension of the collection, which is growing annually by the addition of the pictures purchased under the Chantrey bequest, and to which it is certain that further considerable additions will constantly be made by gift and bequest as soon as there is a place in which they can be properly and permanently exhibited.

Also, there is for once, if advantage be taken of it, an opportunity for carrying out a reasonable and consistent scheme for both science and art.

EDWARD J. POYNTER, R.A.

28 Albert Gate, S.W., May 11.

HAVING served on the Committee on Machinery and Inventions in connection with the Science and Art Department of the Committee of Council on Education, I desire most emphatically to endorse the protest of Sir Frederick Bramwell which appears in your columns of this day's date.

Although the fees received from patentees up to the end of 1885 exceeded the expenditure of the Patent Office by upwards of $2\frac{1}{2}$ millions sterling, nothing practically has been done to put the Patent Museum and Museum of Machinery and Inventions in an efficient condition.

Year after year the Committee, of which I am a member, has urged that more space should be given to the authorities at South Kensington, and now, when it was thought the recommendations were about to be realized, it is asserted that the promised site is to be devoted to a picture gallery.

I sincerely trust that this intention may not be carried out, but that the site in question, which exactly faces the Royal College of Science, will be appropriated for the science collections, to which purpose it has long been assigned.

JNO. COODE, President.

The Institution of Civil Engineers, 25 Great George Street, Westminster, May 11.

The deputation which waited upon Lord Cranbrook, the Lord President of the Council, and Mr. Goschen was large and representative. Mr. Plunket, M.P., First Commissioner of Works, was also present. Among the deputation were: Sir William Thomson (President of the Royal Society), Sir Bernhard Samuelson, M.P., Sir George Gabriel Stokes (Past President of the Royal Society), Mr. C. Acland, M.P., Sir Frederick Bramwell, F.R.S., Prof. Story-Maskelyne, M.P., Sir Douglas Galton, C.B., Mr. Poynter, R.A., Prof. Unwin, Mr. Francis

Galton, Prof. Ayrton, Prof. Flower, C.B., Prof. Armstrong (Secretary of the Chemical Society), and Mr. Fletcher and Mr. Woodward, of the British Museum.

Prof. Story-Maskelyne, in introducing the deputation, in the absence of Sir Henry Roscoe (who is laid up with influenza), said it embraced a body of gentlemen distinguished not so much by their numbers as by their character, representing as they did the Royal Society and the scientific men of England. They had come there to ask that the question of the site of the new National Gallery for British Art should be reconsidered. Those who were deeply concerned in what he might call the new University which had risen for science at South Kensington felt that the proposed building would be a wedge put in between the place now oocupied by it and the place dedicated to science. Scientific men would have to go across the road to get to another and interesting branch of the National Science Collection in a portion of the ground which would then be considerably remote from where they at present were. They understood it to be very much a question of money, and it was believed that the Government would have to ask Parliament to supplement the grant of £80,000 given by the anonymous donor. What he asked was that they should not be told offhand that the scheme could not be altered, but that they should be allowed to take the sense of Parliament as to whether the site was to be occupied in the way proposed or not. They objected to the money being simply asked from Parliament and the control taken out of its hands.

The Chancellor of the Exchequer.—You may entirely exclude that. That will not be done. We shall take such precautions by trustees and by contracts that such a contingency will not occur.

Prof. Story-Maskelyne said he was very glad to hear that.

Sir William Thomson, on behalf of the Royal Society, said they respectfully protested against the proposal to take the site now occupied by the physical laboratory of the Royal College of Science for any other purpose. Sixty pupils were now actively engaged. There was also a mining school in the same locality. No other sites could be as convenient as the site which those departments at the present occupied. It would be most fatal to the science work if the present arrangements were interfered with or the scientific collections, so conveniently arranged, were disturbed. Nor would the proposed site be the most convenient one for the pictures. A far better one would be that at present occupied by the School of Cookery, which, while affording ample room for the present proposal, would also be perfectly convenient for subsequent expansion in a direction that would result in the most admirable collection of picture galleries in the world.

The Chancellor of the Exchequer.—Can you tell us—for I have not yet been able to make it out—in what way the proposed arrangement would be fatal to the work of the College of Science?

Sir William Thomson.—By cutting the school in two—by separating the school from the place in which the instruments are kept.

The Chancellor of the Exchequer.—You mean that it is too far to walk?

Sir William Thomson.—It would be dreadfully risky to have to carry about delicate instruments.

Sir Bernhard Samuelson, as a member of a departmental committee which considered the question of housing the College of Science, supported Sir William Thomson's views, and pointed out that already there had been an encroachment upon the land which had been acquired for the purposes of the Science Museum.

The Chancellor of the Exchequer.—It was purchased for science and art. You do not contend that the whole of it should be devoted to science?

Sir Bernhard Samuelson said he did. He would like to ask the Chancellor of the Exchequer, after the assurance which he had just given that there would be no occasion to go to the House of Commons for a vote in aid of this work, whether he meant that, if there should be an expansion of the art gallery, some one would be ready to extend the munificence of the present donor.

The Chancellor of the Exchequer.—I think that that is rather a matter for our grandchildren. I think there is plenty of space to fill with worthy pictures for a very long time to come.

Prof. Story-Maskelyne.—But £80,000 will not do it.

Sir B. Samuelson said he hoped the question of the site would be reconsidered, and that those representing science should have the assurance that ample space would be given them not only for their present requirements but also for the extension which appeared to be looming in the future.

The Chancellor of the Exchequer.—I am anxious to provide well for science. We hope to bring science into one centre fronting the Imperial Institute.

Sir B. Samuelson said that if they were given an area equal to the amount purchased last year for the purpose of science alone they ought, in his opinion, to be content. But already there had been a small encroachment, and the fact of their having no actual claim to the ground would lead to further encroachment, which would, in the end, make it impossible for science to be efficiently provided for.

The Chancellor of the Exchequer.—I am anxious to show you that, quite irrespective of my interposition, we have not been blind to the interests of science, and that one of our plans has been to satisfy science in the most ample manner for the future.

Sir B. Samuelson said the art gallery was looked upon with a great deal of jealousy, and in the next place they feared that the full area of 200,000 feet, which they considered to be absolutely necessary for the future requirements of science, would be encroached upon.

Sir Frederick Bramwell, who was chairman of the departmental committee which considered the question in 1888, said there was a site to the north of the City and Guilds Institute, and from the east to the west there were galleries, and a cross gallery was being made by the Imperial Institute which would give communication one with the other, and which would be in immediate connection with the site he suggested. That would be an admirable art gallery. He would be glad to see the Science School and everything belonging to it moved so that there might not be a road dividing it. He trusted that the anonymous donor might be induced to see that his gift would prove more graceful if he did not impose a condition that would have so prejudicial an effect as would be the case if the recommendations of the two committees he had referred to were disregarded.

Lord Cranbrook.—The question, of course, so far as it can be considered will be considered, and I quite agree with Sir Frederick Bramwell that nobody can predict what may be done hereafter. You may have a scheme which, in itself, is a good one, but which may possibly have to wait. But in the meantime I can assure you that the interests of science will be most carefully considered, and that we will do what we can in order to further them.

Prof. Story-Maskelyne, having thanked Lord Cranbrook and Mr. Goschen for the hearing that had been given to their views,

The deputation withdrew.

We have received the following communication on this subject:—

Sir,—The curious admissions made by Mr. Goschen to the deputation which waited upon him and the Lord President indicate very clearly that we have, in the present muddle touching the site of the Art Gallery, another of those instances in which we suffer from the system, or, rather, want of system, which is characteristic of the relation of Government to science, and from the absence of scientific knowledge in those branches of the public service by which matters of the highest scientific moment are settled. A reference to some of the facts will, I think, show this very clearly.

The particular site which has been allocated by the Government in this way for the purposes of an art gallery forms part of a piece of land which, as is well known, only last year was deliberately purchased by the same Government for scientific purposes—to be quite accurate for "science and the arts"—that is, science and its manifold applications. *The space of ground thus purchased was less than half the space allotted to the Natural History Museum.* I say deliberately, because the purchase of the land in 1890 had for its object the carrying out of one of the recommendations of the Duke of Devonshire's Commission, which dates from the year 1874—namely, the erection of a Science Museum.

This object so warmly commended itself to the Royal Commissioners of the 1851 Exhibition that in 1876 they offered the land on which the Imperial Institute is now being erected and a sum of £100,000 towards its realization. Few acquainted with the manners and customs of our Government Departments in relation to science will be surprised to hear that this magnificent offer was refused; and it is to prevent a like disastrous mistake being now made that the strong memorial was presented to Lord Salisbury.

The ideal arrangement for a great national collection of scientific apparatus which is to do for the sciences of experiment and observation what the British Museum does for literature and antiquities, the Natural History Museum for biology, and the National Gallery for art, is that it shall be in close connection with laboratories where the apparatus can be used, presided over by experts who are familiarly acquainted with its construction and uses.

This was the ideal recommended to the Government by the Duke of Devonshire's Commission in 1874, and such is the ideal now being carried out by several of our provincial Colleges.

As all Londoners know, at present the Science Schools and the collection of scientific apparatus, which are both necessary for the realization of this scheme, are placed one on the east side of Exhibition Road, and the other chiefly in the Western Galleries. If the apparatus is employed in teaching, it must necessarily be transported about a quarter of a mile and back from the one to the other. And this accounts for the strange processions occasionally met in the neighbourhood of the Museum carrying delicate apparatus along the street alike in wet and dusty weather.

When the new piece of land was purchased last year on the recommendation of a very strong Treasury Committee, it was naturally expected that, as the overcrowded state of the existing school buildings rendered immediate action imperative, plans would be at once drawn up for an extension in the closest possible contiguity with the present building—that is to say, on the part of the newly-acquired plot immediately fronting it.

It was also believed that the Science Museum would be built in close and organic relation with the new laboratories, and that a scheme would be initiated which would supply pressing needs, and could, in course of time, be developed into the ideal institution which has been sketched.

These plans, to the carrying out of which the friends of science confidently looked forward, would be rendered absolutely futile by the grant for art purposes of the particular plot the alienation of which from the use for which it was purchased will render the objects of its purchase nugatory.

All hope of a compact site, therefore, for the future worthy representation of physical science would disappear as the result of this action of the Government.

The public have a right to know who is responsible for this, and how far the scientific officers of the Science and Art Department have been consulted. If they have in any way been consenting parties, it seems probable that they will have a *mauvais quart d'heure* with their scientific brethren who have signed the memorial and who attended the deputation; if they have not been consulted, the whole transaction is a disgrace to our administrative system.

An idea of the *impasse* in which this decision has landed matters scientific at South Kensington was to be gathered from one of Mr. Goschen's replies as to the makeshift arrangements at first proposed:—

(1) The second half of the Science Schools is to be built somewhere at the back of the new Art Gallery. This at once prevents all close relationship between the two halves of the same institution.

(2) The scientific apparatus is to be distributed in galleries which, although built for artistic purposes, are not considered good enough for art.

These, I presume, are the Western Gallery, the present *terminus a quo* of the processions to which reference has been made, a corresponding Eastern Gallery, now occupied by the Indian Museum, and the upper part of a new gallery, also designed for art, situated between the Imperial Institute and the Royal College of Music. All these galleries are as far removed as the limits of the Government estate will permit from the Science Schools, with which they are supposed to be in organic connection.

It appears, therefore, that the provision to be made for the Science Museum, which ought to rank, and in the future must rank, with the British Museum, the National Gallery, and other like institutions, is that the two halves of the Science Schools are to be widely sundered, while any organic connection with the Science Museum is to be rendered impossible.

I do not think, Sir, I need occupy any more of your space with recent history; the whole question stands thus:—

(1) In our museum system Art, Antiquities, Literature, and Natural History are magnificently provided for.

(2) Science is not provided for at all in any permanent manner.

(3) During the last twenty years Royal Commissions, Treasury and Departmental Committees without number, and deputations, have pointed out this gap.

(4) Last year the Government bought, and the Royal Commissioners for the Exhibition of 1851 sold cheap, a plot of land to be used for this purpose, and for this purpose alone.

(5) The plot is less than half of that on which the Natural History Museum stands.

(6) The Government now barter away a large portion of this small site for a mess of pottage.

I am, Sir,
Your obedient servant,
F.R.S.

NOTES.

THE ladies' *soirée* of the Royal Society will take place on Wednesday, June 17.

ON Tuesday the Convocation of the University of London considered the Draft Charter drawn up by the Senate. A resolution to the effect that the scheme should be approved was moved by Lord Herschell, seconded by Sir Richard Quain, and supported by Dr. Pye Smith. Mr. Bompas, Q.C., Mr. R. H. Hutton, and others spoke on the other side. In the end the scheme was rejected, 461 voting against it, and only 197 recording their votes in its favour. The whole subject needs to be thoroughly reconsidered, as the question of the higher teaching, one of the points first insisted on, seems to be dropping out of view. To educationists this is, of course, the really important element of the subject; and it cannot be for ever tolerated that the existence of an Imperial Examining Board, because it has been wrongly named, should prevent the largest city in the world from securing educational advantages which have for centuries been possessed by many a small German town.

THE Government of New South Wales have granted for the purposes of the Sydney Biological Station a plot of land of two acres on the north shore of Port Jackson at a part where the littoral fauna is particularly rich, and where the conditions are in other respects highly favourable. The Royal Society have made a grant of £50 towards the cost of the proposed new station.

THE annual meeting of the German Ornithological Society is being held this year at Frankfort, and the attendance is somewhat larger than usual, as several ornithologists have stopped at Frankfort on their way to the Congress at Budapest. The subject of zoological nomenclature was considered on Tuesday, when a discussion on the rules proposed by Dr. Reichenow and Graf von Berlepsch ensued. The question will be further considered at the forthcoming Ornithological Congress at Budapest, where Dr. Reichenow will be the exponent in the systematic section.

THE *conversazione* of the Society of Arts will be held at the South Kensington Museum on Wednesday evening, June 17.

M. EDMOND BECQUEREL, son, and successor as Professor, of Antoine César Becquerel, died on Monday, in Paris, at the age of 71. He was the author of treatises on the solar spectrum, the electric light, magnetic phenomena, and other scientific subjects.

PROF. JAMES GEIKIE, of the University of Edinburgh, has been delivering a course of lectures at the Lowell Institute, Boston, on Europe during and after the Ice Age. The course began on March 13 and ended on April 10.

A shock of earthquake was felt at Athens on Monday evening.

The fourth summer meeting of University Extension and other students, to be held at Oxford in August, will be divided into two parts. The first part of the meeting will begin with an inaugural lecture by Mr. Frederic Harrison on Friday evening, July 31, and will end on Tuesday evening, August 11. The second part of the meeting will begin on Wednesday morning, August 12, and end on Monday evening, August 31. In natural science fifty-nine lectures will be delivered, and there will be classes for practical work in the University laboratory and observatory, &c. Among the scientific lecturers will be Mr. E. B. Poulton, Prof. A. H. Green, Mr. W. E. Plummer, and Mr. C. Carus-Wilson. Scholarships to the value of £120 have been offered by various gentlemen for the purpose of enabling University Extension students, who would not otherwise be enabled to afford it, to study for a short time at Oxford.

A glass case just placed in the Mammal Gallery of the British Museum contains a series of specimens of two of the largest species of Asiatic Wild Sheep, collected and presented to the nation by Mr. St. George Littledale, the well-known sportsman. Three of these represent Marco Polo's Sheep (*Ovis poli*) from the Pamir Range, and three of them the Ammon (*Ovis ammon*) of the Altai. These are, we believe, the first perfect specimens of *Ovis poli*, the finest and largest of all the Asiatic Sheep, that have yet been brought to England, the species been generally known only by its horns, which are remarkable for their enormous size and width.

The Australasian Association for the Advancement of Science has published the Report of its second meeting, held at Melbourne in January 1890. The volume is edited by Prof. W. Baldwin Spencer. No one who glances over the volume can fail to recognize that the Association is likely to exercise a most important influence on the development of scientific research and thought among our kinsfolk in the Australasian colonies.

The Ealing Microscopical and Natural History Society, of which the Rev. G. Henslow is President, has issued its Report and Proceedings for 1890. The Committee are able to record that the work of the Society proceeded quietly but steadily on the lines laid down in previous years; the evening meetings, the excursions, and the *conversazione* having all been held in their appointed seasons, and having had a full measure of success. Among the subjects brought before the evening meetings were "Adventures in Siberia," by Mr. H. Seebohm; "The Natural History of Malta," by the Rev. G. Henslow; "Diatoms," by Mr. E. M. Nelson; and "A Gossip on Mushrooms and Toadstools," by Dr. M. C. Cooke.

During the last fortnight, according to the Cairo correspondent of the *Times*, there have been in Upper and Lower Egypt large swarms of locusts, which have caused much alarm, as it is believed that they originate from eggs laid in the country last year. The damage done to the young maize, sugar, and cotton is as yet insignificant, though some individual growers have had to re-sow cotton patches which had been devastated. The provincial Mudirs have received orders to do everything in their power to secure the extermination of the locusts. The correspondent says that this is the most serious reappearance of an old Egyptian plague that has been recorded for about forty years.

A circular relating to certain alterations in the Science and Art Directory for the session 1891-92 has been issued to managers of schools of science and art by the Lords of the Committee of Council on Education. The following is an outline of the alterations, so far as they refer to science, or to science and art together:—(1) Subject 6—Theoretical Mechanics—will be treated in two subdivisions: (*a*) the mechanics of solids, and (*b*) the mechanics of fluids—liquids and gases—payments being made on each subdivision as a separate subject. Subject 8—Sound, Light, and Heat—will be treated in three subdivisions in the advanced and honours stages, which may be taken, and will be paid upon, separately. The elementary stage will still include all three subjects, but the syllabus will be curtailed and rendered easier, especially in "Sound." (2) These subdivisions will not be considered as separate subjects in the interpretation of the rule which limits the number of subjects on which payments may be made on a student in any one year. (3) The number of National Scholarships in science to be competed for each year will be increased from 14 to 22. (6) In both science and art, the prizes of books, as distinguished from certificates, will be largely reduced in number, and only given in competition; those prizes which are now awarded simply on the student attaining a certain standard of excellence in the examinations being abolished. The time has passed when such prizes from a central authority, which entail a disproportionate cost and delay in administration, were justified by the necessity for stimulating science and art schools; and the Lords of the Committee of Council on Education are of opinion that the scholarships which will be substituted for them will be more useful. They trust that those interested in education in the several localities will themselves provide prizes of books for deserving students which may be useful to them in their studies.

According to the Indian papers, a persistent effort is being made by the Geological Department of the Government of India, in association with the Burmah Government, to explore the tin resources of Tenasserim. The flourishing condition of the almost adjacent Malay States of Perak and Selangor, which are under British protection, is mainly due to the income derived from tin royalties. A year ago an expert was borrowed from the Straits Settlements and placed in Tenasserim under Mr. Hughes, of the Geological Department. The party has this year been joined by Dr. Warth, the officer who did very good work for the Government in the Punjab salt mines; and Dr. King, the Director of the Department, has left Calcutta for an inspection of the survey operations which have been conducted during the last twelve months. It is now two years since the Chief Commissioner of Burmah sent a special officer to report on the tin mines of the Straits Settlements, and the present explorations are being conducted in pursuance of the recommendations then made.

A passage in the correspondence of Leibnitz and John Bernoulli, to which Prof. Hellmann has recently called attention in the *Meteorologische Zeitschrift*, indicates that Leibnitz conceived the idea of the aneroid barometer, which was first practically realized by Vidi in 1847. Bernoulli, early in the eighteenth century, was considering the phosphorescence of mercury in the barometer, and the possibility of making a new instrument which would give the variations of air-pressure on a larger scale; also the idea of a barometer for travellers; and Leibnitz tells him he had thought of a portable barometer, without mercury, in which a metallic case should be compressed by the weight of the air. A bladder, or leather case, which he also suggested, Bernoulli considered would be too hygroscopic.

Messrs. Macmillan and Co. have just published "Natural Selection and Tropical Nature—Essays on Descriptive and Theoretical Biology," by Mr. Alfred Russel Wallace. The volume consists mainly of a reprint of two well-known volumes of essays—"Contributions to the Theory of Natural Selection," and "Tropical Nature and other Essays." Several essays have been either wholly or in part omitted. On the other hand, the author has included essays on the antiquity of man in North America, and on the debt of science to Darwin, which have hitherto been accessible only in the periodicals where they originally appeared. The text has been carefully corrected, and some important additions have been made.

A SUPPLEMENT to Dr. T. Lauder Brunton's "Text-book of Pharmacology, Therapeutics, and Materia Medica" has been issued by Messrs. Macmillan and Co. It presents the additions made in 1890 to the British Pharmacopœia of 1885. Although the medicinal substances contained in the British Pharmacopœia of 1885 are considered in the body of the work under the natural divisions of the mineral, vegetable, and animal kingdoms to which they belong, the author thinks it is easier to remember the additions by grouping them together according to their uses. A complete alphabetical list of them is also given.

A "BOTANICAL ADDRESS-BOOK" has been issued by the well-known Leipzig publisher, Wilhelm Engelmann. It contains a list of living botanists, and of botanical institutions, societies, and periodicals.

F. A. BROCKHAUS, of Leipzig, has issued a catalogue of scientific works which are offered for sale at his establishment. It includes, besides books, a large number of scientific periodicals and the publications of many learned societies.

THE 92nd and 93rd Parts of the "Länderkunde von Europa," edited by Alfred Kirchhoff, have been published. They present an excellent account of various parts of the Balkan Peninsula.

WILLING'S (late May's) useful "British and Irish Press Guide" for 1891 has been published. This is the eighteenth annual issue.

THE first number of a monthly journal for civil, mechanical, and electrical engineers, was published last week. The new journal is called the *Engineering Review*, and is edited by Mr. H. C. E. Andrée and Mr. Edward Walker.

AT the meeting of the Linnean Society of New South Wales on March 25, the Rev. Dr. W. Woolls read a paper on the classification of Eucalypts. After critically reviewing the characters of Eucalypts which have, from time to time, been made use of for classificatory purposes, more particularly those of the anthers and of the bark as set forth in the anthereal and cortical systems of Bentham and Mueller, the author suggested the probable value of a classification based on the characters of the fruit—such as shape, position of the capsules, the number of cells, and the appearance of the valves, &c.

CAPTAIN PETERSEN, of the Swedish barque *Eleanora*, noted a submarine earthquake in the volcanic region of the Atlantic west of St. Paul Rocks on March 13 between 7 and 8 p.m. According to a statement in the printed matter prepared for publication on the Pilot Chart of the North Atlantic Ocean for the present month, the ship was heading north-west, going about 3 knots, with a light easterly wind and calm sea, when a noise was heard on the port side, like a heavy surf, and almost immediately the sea began to bubble and boil like a huge kettle, the broken water reaching as high as the poop-deck. No distinct shock was felt, but after the disturbance struck the ship she continued to tremble as long as it lasted. After about an hour it ceased for an hour, and was then followed by another similar disturbance. A bubbling sound was all that could be heard, and the water appeared foamy, but it was impossible, on account of the darkness, to say whether it was muddy. The next day weather and sea were as usual. Position at 8 p.m., lat. 3° 47′ N., long. 42° 03′ W. The region from St. Paul Rocks to and including the Windward Islands is especially subject to earthquakes, and reports similar to the above are often received.

AT the ordinary meeting of the Institution of Civil Engineers on May 5, Mr. William Langdon read an interesting paper on railway-train lighting. He pointed out that the main questions to be determined were whether electricity was safe, trustworthy, and less costly than other illuminants. The fact that electrically-lighted trains had now been running for a considerable period without accident appeared to him conclusive evidence of its safety, and experience had shown that there was no reason to doubt its trustworthiness where efficient provision had been made; and he believed that when the cost of applying any of the illuminants, whether oil, gas, or electricity, to a complete railway system was taken into account the latter would be found the most economical. Regarding electricity as the illuminant which would, at no distant date, be universally employed for train lighting, Mr. Langdon suggested the desirability of arriving at a common basis with regard to the following fundamental points: (1) electrical system; (2) form and position of the electrical couplings; (3) pressure of current. Unless this was effected it was to be feared that unnecessary difficulties might be created by the diversity of the plans adopted.

MR. C. J. HANSSEN, a civil engineer of Copenhagen, has proposed a new international system of measures and weights, to which he invites our attention. He hopes that England will adopt his system, and that then the United States and Russia will follow, and thus the new system would become entirely international. Mr. Hanssen proposes that the English foot should be increased in length by about 1/2500th part of its present length (from 1·00000 to 1·000403); the pound avoirdupois, the ounce, and the imperial gallon, remaining unaltered. The cubic foot, as Mr. Hanssen states, would then contain exactly 1000 ounces of distilled water at 4° C.; and its intercomparison with the metric units of weight, length, and volume, would become apparently easy. We fear, however, that there is little hope in this country of introducing any such new system. As Mr. Chaney has indicated in his report on the Metric Conference, there are only two things possible in the metrology of this country: either to adhere to the present Imperial system, or to introduce the metric system. No half-way or modified Imperial system, such as Mr. Hanssen would propose, appears to be possible.

THE Deutsche Seewarte has published, in vol. xiii. of its *Aus dem Archiv*, a paper by Captain C. H. Seemann, one of the assistants in that establishment, entitled "Weather Lexicon: an Index to the European Weather Charts from 1876–1885." The author considers that the principles we at present possess for forecasting the weather—*e.g.* Buys-Ballot's law, the relation of the tracks of depressions to the distribution of pressure and temperature, or the dependence of the lower air-currents upon the upper currents—are not sufficient for the purpose, and he has made an index of the various similar types of weather-charts. He has calculated the barometrical differences which occur each day in three directions: (1) from Hamburg towards the north-west (Stornoway); (2) from Hamburg to the south-west (Biarritz); and (3) from Hamburg to the north-east (Helsingfors); and, by knowing the difference for any day, a reference to a table of such differences shows the dates of other charts with similar conditions, so that, by selecting one which appears most suitable to the present conditions, we may judge of the probable weather from that which actually followed that particular type. In the paper in question, only barometer and wind have been taken into account; the distribution of temperature would, of course, have great influence upon the changes of weather, but the author preferred to postpone the consideration of that element in this primary classification.

IN the new number of the Journal of the Bombay Natural History Society, Lieutenant H. E. Barnes continues his interesting papers on nesting in Western India. Speaking of house-sparrows, he says that no amount of persecution seems to deter them from building in a place when they have once made up their minds to it. At Deesa, he found that a pair had built a large nest in the antlers of a sambur in the veranda. Another pair made a nest in the soap-box in the bath-room, and although the nest was destroyed several times, they would not desist, and at last, "from sheer pity," he had to leave them alone. The most peculiar case was when a pair had a nest in a bird-cage hanging against the wall, just above where the "durzi" sat all day working, and close to a door through which people were passing in and out continually. The door of the cage had been left open, the previous occupant having been transferred elsewhere. Not only were four eggs laid, but the nestlings were reared, although the cage was frequently taken down to be shown to visitors. Once the eggs were nearly lost, a boy having taken them out. The fuss made by the birds led to the recovery of the eggs. The author has a curious note on another peculiarity of sparrows. "I have often," he says, "had to turn the face of a looking-glass to the wall to prevent them from injuring themselves, for immediately one of them catches a glimpse of himself in it, he commences a furious onslaught on what he imagines must be a rival, and, if not prevented, will continue fighting the whole day, only leaving off when darkness sets in, recommencing the battle at dawn the next day. I once tried to see how long it would be before the bird gave in, but after two days, seeing no likelihood of his retiring from the unequal contest, I took pity on him and had the glass covered up. The bird did not seem in any way exhausted, although I do not think that he had a morsel of food for two days."

SOME remarkable electrical phenomena accompanying the production upon the large scale of solid carbon dioxide are described by Dr. Haussknecht, of Berlin, in the current number of the *Berichte* of the German Chemical Society. In order to obtain large quantities of solid carbonic acid it is found most convenient in practice to allow the liquid stored in the usual form of iron cylinder to escape into a stout canvas bag, best constructed of sail-cloth or some such strong fabric, instead of the usual lecture-room receiving apparatus, the cylinder being inclined from the vertical so as to permit of a ready and uniform exit from the opened valve. The liquid under these circumstances issues at pressures varying from 60–80 atmospheres, and a compact snow-like mass of solid carbon dioxide is formed in the canvas receiver, owing, as is well known, to the extreme lowering of the temperature of the liquid due to its sudden expansion and the accompanying absorption of heat. When the experiment is performed in the dark, the canvas receiver is seen to be illuminated within by a pale greenish-violet light, and Dr. Haussknecht states that electric sparks 10–20 cm. long dart out from the pores of the cloth. If the hand is held in these sparks the usual pricking sensation is felt, similar to that perceived on touching the conductor of an electric machine at work. Dr. Haussknecht further states that the phenomenon is very noticeable in the dark whenever there is a leakage in any portion of the compressing apparatus or the manometers connected therewith. The reason assigned for this development of statical electricity is similar in principle to that usually accepted in explanation of the hydro-electric machine of Sir William Armstrong. As the liquid carbonic acid is issuing from the valve it becomes partly converted into gas which is violently forced through every pore of the canvas. Moreover, carried along with this stream of gas are great quantities of minute globules of liquid, which are brought in forcible contact with the solid particles already deposited. Dr. Haussknecht therefore considers that the electrical excitation is due mainly to the violent friction between these liquid globules and the solid snow. It is very essential for the successful reproduction of these electrical phenomena that the carbon dioxide should be absolutely free from admixed air; that prepared artificially yielding much finer results than that obtained from natural waters, which latter contains considerable quantities of air. The luminosity is not generally developed in the interior of the receiver until a crust of solid carbonic acid 0·5–1 cm. thick has been deposited, which renders the probability of the correctness of the above theory all the greater. Dr. Haussknecht has constructed a special form of apparatus, with which he is now experimenting, with the view of being able to determine the sign, nature, and quantity of the generated electricity.

THE additions to the Zoological Society's Gardens during the past week include two Brown Capuchins (*Cebus fatuellus* ♂ ♂), an Ocelot (*Felis pardalis*), a Coypu (*Myopotamus coypus*), two Ring-tailed Coatis (*Nasua rufa*), two Cayenne Lapwings (*Vanellus cayennensis*), seven Burrowing Owls (*Speotyto cunicularia*) from South America, presented by Mr. James Meldrum; a Pig-tailed Monkey (*Macacus nemestrinus* ♀) from Java, presented by Mr. C. Powell; a Common Hare (*Lepus europæus*), British, presented by Mr. H. T. Bowes; three Pintails (*Dafila acuta* ♂ ♂ ♀), European, a Mandarin Duck (*Æx galericulata* ♀) from China, presented by Mr. G. F. Mathews, R.N., F Z.S.; a Common Boa (*Boa constrictor*) from South America, presented by the Directors of the Museum, Demerara; two Cheer Pheasants (*Phasianus wallachii* ♂ ♀) from Northern India, twelve Common Teal (*Querquedula crecca*, 4 ♂, 8 ♀), European, purchased; a Viscacha (*Lagostomus trichodactylus*), a Red Kangaroo (*Macropus rufus*), born in the Gardens.

THE IRON AND STEEL INSTITUTE.

ON Wednesday and Thursday of last week the annual spring meeting of the Iron and Steel Institute was held. The gathering was announced to extend over Friday also, but for some reason, best known to those who had the control of the meeting, the second day's proceedings were so hurried through that all the business was disposed of by half-past one o'clock on the second day; no less than six papers being taken at the one sitting. Naturally there was very little discussion; and indeed the second day of the meeting might almost as well have been dispensed with, and copies of the papers given to members to take home to read at their leisure. It is seldom that we have been present at a duller gathering than that which the meeting became towards its close, there not being a dozen members present to hear the Secretary hurry through the papers one after another, the President apparently being only anxious that there should be no discussion to prolong the proceedings.

The following is a list of the papers read:—On the manufacture of war material in the United States, by Mr. W. H. Jaques, of Bethlehem, U.S.A.; on tests for steel used in the manufacture of artillery, by Dr. Wm. Anderson, Director-General of Ordnance; on certain pyrometric measurements and the method of recording them, by Prof. Roberts-Austen, F.R.S.; on the changes in iron produced by thermal treatment, by Dr. E. J. Ball, London; on a graphic method of calculating the composition of furnace charges, by Mr. H. C. Jenkins; on economical puddling and puddling cinder, by Prof. Thomas Turner, Birmingham; on the micro-structure of steel, by M. Osmond, of Paris. There were three other papers which were not read.

Upon the members assembling in the theatre of the Institution of Civil Engineers, which was lent for the occasion by the Council of the latter Society, according to their hospitable custom, Sir James Kitson, the retiring President, occupied the chair. After the usual formal business had been transacted, the new President, Sir Frederick Abel, F.R.S., was duly installed, and at once proceeded to deliver his inaugural address. Sir Frederick is also this year President of the British Association, and should spend a busy autumn attending both the meeting of the Iron and Steel Institute in Birmingham, and of the Association in Cardiff. The address was of considerable length, embracing a wide range of subjects and a long span of time. The duration of Sir Frederick Abel's official life has been long, exceptionally long for the years he has lived, for he obtained employment in the Government service at an early age. It was shortly after the outbreak of the Russian War that he succeeded the illustrious Faraday in the Professorship of Chemistry at the Royal Military Academy, and since then he may be said to have seen almost the whole history of the birth and subsequent growth of applied science in connection with the industries of iron and steel making. At the beginning of his career, he tells us in his address, those who, in this country, appraised at their proper value the services which the analytical and scientific chemist could render to the iron-master and manufacturer of steel might be counted on the fingers. Systematic mineral analysis was just in process of application, volumetric analysis was altogether in its infancy, and spectroscopic analysis was not even dreamt of. The metallurgic operations in the Arsenal at Woolwich were limited to the production of small castings of brass for fittings of gun carriages, and to the casting of bronze ordnance for field service. Our supplies of cast-iron ordnance for siege and naval use were drawn from a very few of our most renowned iron-works, and our shot and shell were exclusively supplied from private works. What Woolwich has become since those days—and in spite of its faults of administration it is something of which the country may be proud—and how large a part Sir Frederick has borne in this development, most of our readers must be well aware. In those days our most powerful guns were 8-inch smooth bore 68-pounders of cast-iron, weighing 95 hundredweight, and fired with a charge of 18 pounds of powder. Now we have the 110-ton breech-loading rifled gun, built up of steel hoops and tubes, the calibre of which is 16¼ inches, and which throws a steel projectile weighing 1800 pounds with a powder charge of 960 pounds. Notwithstanding the fact that the 110-ton gun is in advance of its time—our mechanical skill and engineering knowledge not yet being sufficient to properly carry out the design—it would be difficult perhaps to find a more striking example of the application of scientific principles to the industrial arts; although we must not forget that the credit of the advance is due rather to Elswick than to Woolwich.

Leaving the region of historical retrospection, the address makes reference to the proposal of Prof. Langley, of Michigan University, made at the last Bath meeting of the British Association, that a series of samples of steel should be distributed between the metallurgical experts of different countries, in order that they might be analyzed and a part deposited as standards in each of the countries. The sets of samples supplied to each country were to be identical in composition, but each set would contain specimens varying in composition. The results of the analyses were to be compared, the object being to promote greater uniformity of procedure and a selection of the best methods. The Crescent Steel Works of Pittsburg have supplied the samples, and the English experts have almost completed their work. Should the Commission succeed in bringing about uniformity of practice in this respect, it will do much towards lightening the work of those who have to compare the results arrived at in different countries. Sir Frederick next referred to Dr. Sorby's method of examination of iron and steel by microscopic examination of carefully prepared samples, in which the structure has been developed by treatment with a weak acid. It will be remembered that Dr. Sorby gave a description of his process in a paper read before the Iron and Steel Institute two or three years ago; and since then Dr. Herman Wedding has followed the matter up with success. Many years previously Faraday had pursued an analogous course of investigation. It is satisfactory to learn that "the systematic application of Sorby's system of microscopic examination of prepared surfaces of steel and iron is continually extending at the German works, and that many series of experiments have demonstrated that by this system of examination characteristic features of grades of iron may be discovered, physical differences co-existing with identity of chemical composition explained, and evidences of the true grounds of disasters obtained." A very interesting subject next occupied a place in the address. This was the self-destruction, if one may use the term, of steel projectiles by the development of cracks. It is well known that steel projectiles may be received from the manufacturer to all appearance perfectly sound, and after a time cracks will develop themselves. In extreme cases the occurrence has been so sudden that a violent rupture, attended by a sharp report, has taken place. The cause doubtless is the surface treatment to which the shot is subjected in order to get the requisite hardness, and which leads to internal strains being set up. In one case mentioned in the address the head of the projectile had been thrown to a distance of many feet by the violent spontaneous rupture of the metal. The importance of rest in bringing about a diminution, if not entire disappearance, of internal strains in masses of metal is illustrated by the behaviour of chrome steel projectiles, which had to be stored for several months before their transport to a distance could be ventured upon. In connection with this subject Sir Frederick referred to a previous report in which he dwelt upon the effect of time in establishing chemical equilibrium in masses of metal. He also quoted a letter written to him by Thomas Graham, when Master of the Mint, in which was discussed the tendency to the development of cracks in tempered steel dies, and stating that in the Mint it was generally considered that if such dies were kept in store for a year or two, they became less apt to crack when in use, and coined more pieces than dies newly tempered. The same phenomena have to be considered in the manufacture of steel ordnance; and an instance was given by the lecturer of the tube of a large gun which had fired three proof rounds. A circumferential crack was found to have become developed in the front threads of the breech screw, and, upon removing the jacket from the tube the crack extended forward along the chamber and into the rifling. When the tube was placed in the lathe, with a view to cutting off the injured portion, the crack suddenly developed itself with a loud report, and ran along to within eight feet of the muzzle; a spiral crack at the same time ran completely round the tube, which fell in two upon removal from the lathe. This instance will strengthen the hands of those who are opposed to oil-hardening the parts of a steel gun; and Sir Frederick's own words in connection with this vexed question are worth quoting. "One effect which the oil-hardening treatment has occasionally exercised in the case of particular qualities of steel is that of developing minute fissures or cracks in the metal, either superficially or in the interior of the mass. This cannot, of course, be rectified by any annealing process, and it is still a question, to be determined by the teachings of experience and the result of investigations, whether any definite or reliable modifications in the composition of steel used for guns, tending to secure the desired combination of hardness and tenacity may not be introduced, with the result that a method of treatment of the metal may be discarded which, however carefully applied, and however efficient the means adopted for reducing or neutralizing its possible prejudicial influence upon the physical stability of the parts of which a gun is built up, carries with it inherent elements of uncertainty and possible danger." Dr. Anderson's remarks on the subject of oil-hardening should also be read in connection with the observations contained in the President's report. On the whole, perhaps, it would not be rash to predict that the days of this process are numbered in connection with the manufacture of steel ordnance for Her Majesty's service. For a long time many of our best authorities have been opposed to it.

We have not space to follow the address into the subject of the effect of silicon in cast-iron. General interest in this matter was aroused a year or two ago by a paper read before the Iron and Steel Institute by Thomas Turner, of Mason's College; and since then the investigation has been followed up by German experimentalists, with a general result that, under certain conditions, it is concluded that silicon will contribute to the production of dense and homogeneous castings.

The following passage from the address speaks for itself. It would be well if it could be printed and distributed to every British iron or steel maker:—

"The absolute dependence of the development of new metallurgic processes upon the results of the labours of the analyst, the chemical investigator, the physicist, and the microscopist, and the thoroughness with which this all-important fact is appreciated by the German metallurgic establishments, afford new occasion for a regretful recognition of the distance which we are still behind our Continental brethren in availing ourselves of the advantages afforded by the constant pursuit of scientific research, and the thoroughly efficient, systematic, and direct application of the labours of the scientific investigator to the daily operations at works of all kinds, although it must be acknowledged that of late years we have made important progress in these directions. It has certainly been humiliating to have to admit that industries which the genius of individual

Englishmen, possessed of exceptional powers of applying to important practical purposes the results of research, have created and have developed to an extent foreshadowing their high importance, gradually passed out of our hands through the far-sightedness of the Germans, who have very long since recognized the absolute dependence of progress in such industries upon the constant pursuit of chemical research into the far-reaching and continually spreading ramifications of organic chemistry. Thus, in fields of work, where, in days past, and even of late, our industrial chemists have been content to pursue their attempts at progress with the co-operation of one or two young chemical assistants, small armies of highly-trained chemists, who have gained academic honours, and have won their spurs in original investigation, are in constant employment at the magnificent manufacturing establishments in Germany, systematically pursuing researches which constitute successive indispensable links in a great network of exhaustive inquiry, and which, while conferring large benefits on the science itself, are continuously productive of improvements in existing processes, or of the development of new methods, while, ever and anon, they result in some fresh discovery of great technical importance and high commercial value. Similarly elaborate and comprehensive arrangements now exist at important German iron and steel works for systematic investigation and comparison of materials of products and processes."

We must hurry over the remaining parts of Sir Frederick's address, and can only mention some of the chief subjects touched upon, referring our readers to the Proceedings of the Institute for fuller information. Thus we find the following matters occupying attention: the presence and effect of nitrogen in iron; the state in which carbon exists in steel; Osmond's study, by means of the Le Chatelier pyrometer, of the slow cooling of iron and steel, together with the phenomena of recalescence, and the existence of two allotropic forms of iron; the effect of aluminium in iron; Hadfield's researches in connection with manganese steel; the progress of nickel steel; and the interesting discovery of Langer, Quincke, and Ludwig Mond of the action of carbonic acid upon finely divided nickel at high temperature, in which it was found that the metal had the power of separating carbon from the gas, with production of carbonic acid in place of the oxide. These and other matters were dealt with at greater or less length, and constituted a most interesting and characteristic address.

Only one paper was read on the first day of the meeting. This was Dr. Anderson's contribution on tests for steel used in the manufacture of artillery. The announcement of a contribution on this subject by the Director-General of Ordnance Factories had caused a good deal of interest both among the scientific and manufacturing members of the Institute, more especially as it was known that the Government authorities had been overhauling the official test regulations. Unfortunately, however, the meeting was a little too early, so far as Dr. Anderson's paper was concerned, for the new regulations have not yet been officially published, and, until they are, it is against official etiquette, if not official rules, that they should be made known. The paper was therefore very like the play of "Hamlet" with the Prince of Denmark left out, and bore evidence of having been brought forward rather with a view of fulfilling a promise than because the author had anything new to advance. It was not Dr. Anderson's fault that his paper was robbed of its chief interest, and certainly the thanks of the Council were due to him for good-naturedly allowing it to stand on the programme. Notwithstanding what we have said, the paper was very interesting, but as we hope to hear Dr. Anderson again on the subject, when the official veto has been removed by publication of the new tests, we shall treat the matter briefly. It is first pointed out that the mechanical properties of steel, and of alloys generally, are affected in a remarkable manner by extremely minute quantities of substances, by the relative proportions, by the changes in some or all, produced by the more or less rapid changes of temperature, which influence dissociation and reveal their effects by recalescence; indicating, to a less degree, allotropic changes in some or all of the components. Chemical analysis sufficiently minute to detect even traces of every substance associated with iron would be tedious and costly. Years must pass away before chemical and physical science together will succeed in determining the laws which govern the mechanical properties of alloys. For these reasons, and others, the specifications of gun-steel used in Her Majesty's service exclude all definitions of chemical composition, so far, at any rate, as ordinary ingredients are concerned. The author thinks it is not sufficiently realized that metals are incapable of appreciable cubical compression under any stress that can in practice be brought to bear on them, whether fluid, pasty, or cold. Like ice and water, steel and cast-iron have a greater volume in a solid than in the liquid state, and, therefore, red-hot solid cast-iron or steel floats on the surface of the molten mass: although, it should be added, cold cast-iron will at first sink in a bath of liquid iron, but will rise to the surface and float when it has acquired a sufficient degree of heat to bring it about to a cherry red. This was shown by the well-known experiments of Mr. Wrightson, referred to at the meeting. The manner in which, during cooling, compressive stress is suddenly turned into tension high enough to cause rupture (due to the swelling during solidification) is dealt with; this being a subject also treated upon in the President's address. The bearing of these phenomena upon the process of hardening is also discussed. The relative influence of carbon in iron as a definite compound of carbon and iron dissolved in an excess of iron, and as a finely subdivided carbon diffused through the mass, is considered, and the author expresses an opinion that the "apparently capricious behaviour of steel" is due not only to the internal stresses engendered by oil-hardening, but also to the circumstance that the chemical condition of the steel and its molecular structure are greatly influenced by comparatively slight errors of judgment, or by carelessness in the adjustment of the temperatures at which the operations are performed.

A discussion followed the reading of the paper, in which the most interesting incident was Mr. Wrightson's description of his experiments to determine the volume of cast-iron at different temperatures. Mr. Edmunds, of Woolwich, defended the practice of oil-hardening for gun-steel; and Mr. Hadfield would attribute cracks in steel rather to contraction than expansion.

On the second day of the meeting the proceedings were opened by Prof. Roberts-Austen giving a verbal description of the Le Chatelier pyrometer, an instrument which is now well known to the scientific world. It may be of interest to state that Sir Lowthian Bell and other practical men spoke of the great assistance this pyrometer had been to them in the course of manufacturing operations.

M. Osmond's paper on the micro-structure of steel was no more than a note which accompanied the presentation of a series of micro-photographs. The paper of Dr. E. J. Ball, which followed, was supplemental to a previous paper contributed by him (see Journal Iron and Steel Inst., 1890, No. 1, p. 85); and, as the present paper will be supplemented by another, we will refer our readers to the Proceedings, merely giving the general conclusions arrived at by the author, which are as follows:—"(1) That in iron containing 0·1 per cent. of carbon, the tenacity of the metal increases by hardening, either in oil or in water, with the temperature at which the metal is quenched with a view to hardening, a maximum tensile strength being reached at a temperature of about 1300° C. This temperature once exceeded, however, the tenacity of the metal diminishes, although the extensibility increases. (2) By raising the percentage of carbon from 0·1 to 0·2, the maximum tenacity is attained, not at 1300° C., but at a much lower temperature—about 1000°—below the melting-point of iron oxide, which, moreover, was not present. (3) By further considerably increasing the percentage of carbon, this point of maximum tenacity apparently disappears almost entirely, the annealed metal having nearly as high a tensile strength as the same metal which has been quenched in oil from any temperature up to a bright red heat. Beyond this temperature, or when quenched in water, the hardened metal became so hard and brittle that it could not be gripped by the jaws of the testing machine." It will be remembered by those who attended the meeting when Dr. Ball's last paper was read that M. Osmond put forward the idea that the fourth point in change (in addition to Osmond's three points), which occurs, according to Dr. Ball, in very mild steel at a temperature approaching the melting-point, might be due to the fusion of iron oxide. The present paper is founded on this remark, but for the results, beyond the salient features given, we must, as we have said, refer our readers to the originals. Mr. Turner's paper, which was read next, does not require a detailed notice at our hands. It was an economic paper on a subject which is rapidly losing economic interest; and the author does not appear to have made himself well acquainted with the labours of previous investigators in this field. The paper of Mr. Jenkins does not admit of an abstract being made; whilst the last paper read, that of Lieutenant Jaques, U.S.N., was of such a voluminous nature that it might better be described as a treatise, and is far beyond our scope, as may be judged from its title.

The autumn meeting of the Institute will probably be held at Birmingham.

THE ROYAL SOCIETY SOIRÉE.

THE *soirées* given by the Royal Society become every year more pleasant. The one held on Wednesday, May 6, was in every sense most successful. We note some of the objects exhibited:—

Mr. J. Wimshurst exhibited an electrical influence machine (alternating and experimental).

The Trotter curve ranger was shown by Mr. A. P. Trotter. This portable instrument is intended to facilitate setting out large curves for railway and other work. It dispenses with tables of angles and with the use of chains and assistants. No cumulative error can arise as with theodolite work.

Profs. Rücker and Thorpe, FF.R.S., exhibited a map showing the probable connection of lines towards which the magnet is attracted in England and France. Profs. Rücker and Thorpe found that the north pole of a magnet is attracted to a line which runs south from Reading, and enters the Channel near Chichester. M. Moureaux has traced a similar line from Fécamp to the south of Paris, but its southern termination has not yet been discovered. The directions of the two lines make it probable that they are parts of the same axis of disturbance.

The Director-General of the Geological Survey exhibited:—(1) Specimens illustrating the phosphatic chalks in England, France, and Belgium, arranged by Mr. A. Strahan, Geological Survey of England and Wales. Phosphatic band in the upper chalk of Taplow, containing about 30 per cent. of phosphate of lime. Taplow phosphatic chalk separated by washing into: (1) brown sand composed of phosphatised organisms, and containing about 50 per cent. of phosphate of lime; (2) chalky mud composed largely of rhabdoliths, coccoliths, and discoliths. Microscopic preparations of the phosphatised organisms of the Taplow chalk, showing Foraminifera, prisms of Inoceramus shell, fish-scales, fish-bones, and fish-pellets. Photographs of the Taplow phosphatised organisms, by Mr. J. J. H. Teall, F.R.S. Phosphatic chalk of Beauval (Somme), and microscopic preparation. Phosphatic chalk of Ciply (Belgium), and microscopic preparation.—(2) Illustrations of a former Arctic climate in the Lowlands of Scotland, determined by Mr. Clement Reid, Geological Survey. At Hailes, about three miles south-west of Edinburgh, in a thin seam of silt, resting immediately on boulder clay, Mr. J. Bennil, of the Geological Survey of Scotland, has lately found numerous remains of plants. These show a climate probably 15° or 20° colder than that of the Lowlands at the present day. In the following list the peculiarly Arctic species are marked with an asterisk. The only *tree* is an alder. The willows are all dwarf species; two of them (*Salix herbacea* and *S. reticulata*) still live on the higher mountains of Scotland, the third (*S. polaris*) is an Arctic form now extinct in Britain. At the same locality there is another deposit, probably of later date, which contains only plants still living in the neighbourhood, including several trees.

Thalictrum	**Salix herbacea*
Ranunculus aquatilis	**Salix polaris*
Viola	**Salix reticulata*
Stellaria media	*Alnus*
Oxalis acetosella	*Empetrum nigrum*
Hippuris vulgaris	*Potamogeton*
**Loiseleuria procumbens*	*Eleocharis palustris*
Menyanthes trifoliata	*Scirpus pauciflorus*
Stachys palustris	*Scirpus lacustris*
Ajuga reptans	*Scirpus?*
Chrysanthemum Leucanthemum	*Carex?*
Polygonum aviculare	*Isoetes lacustris*

The Executive Committee of the Silchester Excavation Fund exhibited (by permission of the Duke of Wellington):—(1) Iron tools and utensils of the Roman period, found together in a pit in the Romano-British city at Silchester, Hants, in September 1890.—(2) Bronze objects of the Roman period found at Silchester.

Prof. H. Carrington Bolton, Ph.D. (of New York), exhibited musical sand, from Arabia, United States of America, and the Hawaiian Islands, collected by the exhibitor.

Prof. H. G. Seeley, F.R.S., exhibited remains of Anomodont Reptiles from the Trias, Karoo, Cape Colony.

The Director of the Royal Gardens, Kew, exhibited a collection of views in the Royal Gardens, Kew, showing the development of the Gardens during the last fifty years. This series is a portion of a very extensive and valuable collection of prints, drawings, and photographs of the most interesting features in the Royal Gardens. It has been brought together during the past twenty years, and is now deposited for exhibition in Museum No. 3.

Messrs. J. E. H. Gordon and Co. exhibited Tomlinson regulator for electric light mains. The Tomlinson regulator is intended for use in transformer sub-stations. It is worked by a wire from the central station, but automatically corrects any error of the attendant at the central station. Ordinary automatic apparatus cannot be safely used for this purpose, as, though should such get out of order when taking out transformers, no harm is done except the waste of coal, yet if it gets out of order when putting in transformers it may burn up the sub-station. The peculiarity of the new apparatus is that if anything whatever goes wrong, all transformers are at once put in, thus ensuring absolute safety. By the courtesy of the Brush Company, and of the Metropolitan Electric Supply Company, who have lent the necessary machinery, the apparatus has been tried on a large scale in the Brush Company's works. A plant of 1950 lights capacity has been run for 24 hours with, and for 24 hours without, the new apparatus, with the result of a saving of $4\frac{1}{4}$ cwt. of coal, or, in other words, with the new apparatus there was a saving of 89 pounds of coal per 8 c.p. lamp per annum, or about 26 per cent. of the total coal bill.

Sir J. B. Lawes, Bart., F.R.S., and Dr. J. H. Gilbert, F.R.S., showed:—(1) Three enlarged photographs of Leguminous plants, grown in 1889, in experiments on the question of the fixation of free nitrogen. The plants were grown, in some cases with sterilization, and in others with microbe-seeding of the soil. With suitable microbe-infection of the soil, there was abundant formation of the so-called *leguminous nodules* on the roots of the plants, and there was, coincidently, very considerable fixation of free nitrogen. The evidence at command points to the conclusion that the free nitrogen is fixed in the course of the development of the organisms within the nodules, and that the resulting nitrogenous compounds are absorbed and utilized by the higher plant.—(2) Coloured drawing, by Lady Lawes, of the Rothamsted rain-gauges.—(3) Coloured drawing, by Lady Lawes, of the Rothamsted drain-gauges.

Old plan of the Mint in the Tower of London, exhibited by the Hon. Sir C. W. Fremantle, K.C.B. This document is described as an exact survey of "The Ground Plot or Plan of His Majesty's Office of Mint in the Tower of London." It bears the date $170\frac{1}{2}$, and must have been prepared by the order of Sir Isaac Newton, who was appointed Master of the Mint in 1699. The position of Newton's official residence is shown at A.

Mr. R. E. Crompton, M.Inst.C.E., exhibited:—(1) Section of armature winding, showing copper divided, twisted, and compressed, to avoid loss from eddy currents.—(2) Crompton's method of obtaining accurately sub-multiples of the ohm; for current-measuring purposes.

Prof. Oliver Lodge, D.Sc., F.R.S., exhibited:—(1) Revolving mirror. Rapid revolving mirror driven by clock-work, with detachable fan to give moderate speeds, with adjustable main spring to vary the speed, and with vacuum cover for highest speeds (the last not yet satisfactory). Slow moving index, to enable the speed to be determined; and electro-magnetic brake to regulate its going, or to stop it gradually. Mirror, 2·3 × 1 cm., silvered back and front; very light, but giving fair definition. It makes 5760 revolutions for 1 of the winding arbor. Used for analyzing sparks, and observing the speed of electric pulses along conductors of various kinds. Made by Mr. W. Groves.—(2) Clock for pointing out continually the direction of the earth's orbital motion. (Two home-made forms.) A disk, or dial, set on a polar axis with the obliquity of the ecliptic, is driven by a clock against the rotation of the earth. On the dial are recorded 365 days of the year. It is set once for all in the plane of the ecliptic, with the actual date pointing 90° from the sun. In the first instrument I devised, the direction of the right-dated radius of the dial henceforth points out the direction of the earth's motion at any instant, if the clock keeps sidereal time. A modified and improved instrument, devised by my assistant, Mr. Edward E. Robinson, adds a sighted pointer to the dial, this pointer being moved by hand to the right date; and the clock may then keep ordinary time. The dial is geared down 1 : 24, and driven by the minute hand, so as to be under the ordinary control of clock-regulation. In each instrument a one-day hand-shift is needed every 29th February.—(3) Resonant Leyden jars. A couple of independent but similar Leyden jar circuits arranged at a moderate distance from each other, the self-induction or capacity of one of them being adjustable, with an easy overflow path. On discharging one of the jars, the other resounds and overflows, being provided with an easy overflow path. The oscillations are much more numerous than with ordinary linear (Hertz) vibrators, and therefore some precision is demanded in the tuning.

Self-recording instruments, exhibited by MM. Richard Frères.

Method of recording pyrometric measurements at temperatures between 600° C. and 1200° C., exhibited by Prof. Roberts-Austen, C.B., F.R.S. The apparatus is that employed in a research undertaken for the Institution of Mechanical Engineers, and is used for automatically recording, by the aid of photography, the indications of a platinum and platinum-rhodium thermocouple. The experiments shown illustrate a method of recording the rate of cooling of heated masses of metal. Curves are shown to illustrate the kind of results which are obtained by the aid of the apparatus.

Length-measuring instrument, exhibited by Prof. W. C. Unwin, F.R.S. In ordinary screw or vernier micrometers the straining of the instrument alters the readings, and in using the instrument much depends on personal skill. In this instrument the contact is with fixed pressure, and independent of feeling. Delicate levels show when the instrument is adjusted.

Portraits of deceased astronomers and physicists, exhibited by Mr. W. B. Croft.

Mr. Killingworth Hedges exhibited:—(1) Electrical safety-valve.—(2) Exhausted bulbs, used to ascertain the space traversed by high tension alternating currents. The electrical safety-valve is designed for attachment to low pressure service lines, in order to prevent their being charged at a dangerous difference of potential from the earth. The glass bulbs were exhausted to different pressures, and fitted with electrodes of various forms, in order to ascertain if an arc could be started with an E.M.F. of 300 volts, which is the limit of potential fixed by the Board of Trade for currents of low pressure.

Focometer, exhibited by Prof. Silvanus P. Thompson. By this instrument can be determined the position of the two principal "focal planes" and of the two "principal planes" of Gauss, for any compound system of lenses, such as a microscopic objective or the lens of a photographic camera; thus giving the true focal length, and the positions and distance apart of the two virtual optical centres of the lens-system. The principle applied is that of finding directly the two principal foci, and then, by means of a right-and-left-handed screw, moving two micrometers placed at these foci to the two symmetric points where each micrometer coincides with the image of the other. The displacement so given by the screw is equal to the true focal length.

Mr. Shelford Bidwell, F.R.S., exhibited: (1) Selenium cells, the electrical conductivity of which is greater in the light than in the dark. (2) A selenium lamp-lighter, lighting an incandescent lamp automatically when darkness comes on. (3) A selenium alarm, for calling attention to the accidental extinction of a ship's light or railway signal lamp.—Mr. W. Crookes, F.R.S., exhibited electricity and high vacua.—Mr. G. J. Symons, F.R.S., exhibited photographs of damage produced by the tornado of August 18, 1890, at Dreux (Eure et Loire), France.—Prof. C. Piazzi Smyth exhibited examples of photographic enlargements of the solar spectrum, each magnified from the original negative from 25 to 27 times linear.—Mr. George Higgs exhibited photographs of the normal solar spectrum.

M. G. Lippmann exhibited colour photographs of the spectrum:—(1) Small spectrum, exposure about 3 minutes.—(2) Large spectrum, exposure about 6 minutes, without coloured screens. The colours seen on these plates are produced by the direct action of light; they are not due to any pigments, the substance of the films remaining colourless, but are of the same kind as the colours of soap-bubbles and mother-of-pearl, viz. interference phenomena; they are due to the structure imparted to the film by the stationary waves of incident light during exposure in the camera. These colours are perfectly permanent.

Prof. A. Schuster, F.R.S., exhibited some forms of Clark cells.

Prof. Emerson Reynolds, F.R.S., exhibited: (1) Specimens of tetrathiocarbamid-ammonium bromide, $(H_5N_2CS)_4NBr$, and related substances. (2) Series of photographs illustrating the application by Colonel Waterhouse of the above bromide to the reversal of the photographic image on gelatino-bromide of silver films.—Mr. W. Saville-Kent exhibited photographs of living corals, taken in Torres Straits.—Dr. W. Hunter exhibited a series of ptomaines—alkaloidal products formed by bacteria from animal tissues.—The Committee of the Camera Club exhibited allotropic forms of silver, prepared by Mr. Carey Lea, of Philadelphia, and described in *Amer. Journ. of Science* for 1889, and *Phil. Mag* for 1891.—Prof. G. F. Fitzgerald, F.R.S., exhibited crystals of platinum and palladium (prepared Mr. J Joly).—Prof. J. A. Ewing, F.R.S., exhibited Prof. Sekiya's model of a Japanese earthquake.—The Council of the Royal Society exhibited a cabinet containing medals struck in honour of Fellows of the Royal Society.—Mr. Edward Schunck, F.R.S., exhibited indigo-blue and allied substances and derivatives of chlorophyll.—Mr. Fred Enock exhibited microscopic preparations of the British Mymaridæ (egg parasites).—Dr. H. Woodward, F.R.S., exhibited skull and shoulder-girdle of *Procolophon trigoniceps* (Owen), collected by Dr. Exon in the Orange Free State (figured Phil. Trans., 1889, p. 267).—Mr. J. Howard Mummery exhibited specimens illustrating some points in the structure and development of dentine.—Mr. Allan Dick exhibited a new form of polarizing microscope.

Meteorological photographs, exhibited by Mr. Arthur W. Clayden. The photographs of clouds have been taken by reflection from a mirror of black glass, placed in front of the camera, so that the plane of its surface makes the polarizing angle with the axis of the lens. Those of hoar-frost show how the crystals attach themselves to the projecting portions of objects, such as the margins of leaves, the loose fibres of a string, and the thorns of a briar, and also their tendency to grow towards the direction from which the air has been moving.

THE BENUÉ AND THE KIBBÉ.

AT Monday's meeting of the Royal Geographical Society, Major Claude M. Macdonald, H.M. Commissioner to West Africa, gave an account of a journey up the Benué and its northern tributary the Kibbé, in the summer of 1889. The Benué, we need scarcely say, is the great tributary of the Niger. Major Macdonald referred to the previous explorations of Barth and others, and to the fact that it has been maintained that a connection existed between Lake Chad and the Benué, by the overflow of the Shari on one side and the Kibbé on the other. Major Macdonald has been the first to explore the Kibbé. After describing the ascent of the Benué, Major Macdonald went on to say that he and his party started on their journey up the Kibbé in the Royal Niger Company's stern-wheeler the *Benué*, on August 21.

The Kibbé at its mouth is some 250 yards wide, while the Benué is upwards of 600. The average depth of the Kibbé at this season of the year, nearly high water, is from 10 to 12 feet. On both banks for the first five miles the country is flat and well wooded, with patches of bright green grass, and looks very gamey, though owing to the high grass we saw no deer. A noticeable feature some five or six miles from the river is Mount Katie, a rounded hill, some 800 feet high, well wooded to its summit. This hill, from its isolated position, served as an excellent point on which to take angles for mapping purposes. Patches of cultivation were now to be seen on both banks, and after two hours' steaming the party passed the Fulbe village of Dinghi. The inhabitants, though they had never before seen a steamer or a white man, did not seem much disconcerted, and, when shouted to in their language, returned the salutations in a very friendly manner. On August 22 the *Benué* anchored off a large village on the left bank. "We very soon saw," Major Macdonald states, "that we had to deal with the purest-bred Fulbe we had seen so far. The crowd consisted almost entirely of women—by far the best-looking we had as yet seen on the Niger, and indeed the best-looking I have seen in either east or west Equatorial Africa. They wore the usual piece of cloth wound round their bodies, leaving their arms and shoulders bare, and reaching down below the knee. Their features, in most cases, approached the European, and their expression most gentle and modest, yet full of vivacity. They told us that the name of their village was Pamu, and that it was governed by an Emir, who was under the jurisdiction of the Emir of Yola. The men were armed with spear and bow and arrows, though they are said to be an agricultural people, and certainly it would seem so, for every yard of ground in the neighbourhood of Pamu was under cultivation. We asked them if they would bring us provisions in exchange for cloth; this they readily did, and we soon were hard at it, bartering pieces of cloth, salt, &c., for live stock, weapons, ornaments, and indeed anything. The whole time nothing but the greatest good temper prevailed, and I was much struck by their gentleness and courtesy; albeit the ladies were very good at a bargain, and I noticed that when it came to bartering their ornaments, members of the fair sex, who were not so young or so fair as their more fortunate sisters in this respect, surreptitiously handed their ornaments to the latter to dispose of, hoping thereby to get better value, and I am bound to confess they did."

Shortly after this the steamer came to a deserted strip of country, some fifteen miles in length, which was evidently the barrier between the Mahommedan and Pagan tribes; it was of an undulating character, with isolated hills, and well wooded. The river was still about 100 yards wide, but commenced to be dotted with grassy islands, and was in parts very shallow with a sandy bottom.

Next day, as the steamer advanced, the river narrowed again and made a sharp bend to the eastward, and approached a grassy range of mountains, leaving a higher range to the north. Half an hour after starting the party arrived at the foot of the grassy slopes of the former; a pathway, which could be traced for a considerable distance, wound up the face of the mountain and disappeared over one of its grassy ridges. Patches of cultivation could be seen dotted here and there; the main valley stretched back some three or four miles, but we could see no signs of a village.

"We were, however," Major Macdonald stated, "not left long in doubt as to whether the country was inhabited or not, nor as to the character of the inhabitants, for down the winding path, which was distant some 600 yards from where we were, came a line of warriors, some 200 in number; the majority of them were quite naked, though some few had a small cloth round their waists. They were all armed, mostly with spears, the almost invariable number being three. Leaving the pathway, they advanced in excellent order across the boulder-covered grassy piece of ground which lay between the river and the mountain side. We accordingly moved into mid-stream, which was only some 15 yards from the bank, and dropped anchor in about 4 feet of water. Our friends advanced straight at us, not a word being spoken, but an excellent line being maintained, when suddenly they all took cover behind boulders and tufts of grass, nothing being visible but the gleaming points of their spears. It was a source of some gratification to us that the points were gleaming, for it showed that at any rate they were not poisoned. There was now a pause. Then our Fulbe interpreter, under my directions, opened fire in a dialect of the Battawa, with satisfactory results, for they appeared to understand him. Their first question was as to whether 'we were Mahommedans? because if so we could not pass, as they were the outposts of the Pagan tribes, and had orders not to allow Mahommedans to pass.' We assured them that we were not Mahommedans. They then told us, in answer to our queries, that the name of their village was Katsho, and that it lay back from the river amongst the hills; they said that if we went on we would come to more villages. After a great deal of persuasion two of their number consented to come on board. So we sent a six-oared gig, which we had towed up with us in case of accidents, to fetch them. They were fine, well-made men, but were trembling with fright at the sight of the steamer and white men, and prostrated themselves on the deck at our feet. These two men wore loin cloths of native manufacture; the great majority of the others were, as I have said, naked. After getting as much information out of these men as we could, which information, on account of their terror and the difficulty in interpreting, was somewhat meagre, we proceeded on our way. By this time large numbers of men and boys had assembled, and ran along the banks gesticulating and pointing at our little ship. They, men and boys alike, were all armed, mostly with spears; we saw very few bows and arrows.

"The scenery now was very picturesque; to our right, *i.e.* the south of the river, some few yards from the water's edge, the mountains rose in some places quite abruptly. These mountains were for the most part covered with green wavy grass very pleasant to the eye. One or two streams trickled down the mountain side, forming now and again picturesque waterfalls. The river had suddenly broadened out to a lake, or, more properly speaking, marsh, some three miles long by two wide. The range of grassy mountains I have mentioned ran along the southern shores of the lake and terminated with it. The country on the east and north shores of the lake, as far as the eye could see in the direction of the Tuburi marsh (near the Shari river) was open and gently undulating, while from the western shores of the lake the beautiful range of mountains, with their needle-shaped peaks, stretched back apparently for many miles. In the north-east corner of the lake we saw a very large village some two miles distant; this we afterwards ascertained was Bifaré. The channel of the river evidently followed the base of the southern hills. We accordingly steamed gaily along, followed on the shore by an ever-increasing crowd, till we arrived at a large village prettily situated almost on the edge of the lake. The houses or huts were built in clusters, each cluster apparently belonging to a different family. The huts were very well constructed, having round walls some 6 feet high, with flat roofs formed by beams covered over with mud and thatch. The walls of the huts were made of black and in some places red mud, and the workmanship of both walls and roof was excellent. Several hamlets were prettily situated on the slopes of the hill, surrounded with patches of cultivation, and had the appearance of the country places of the richer inhabitants of the village.

"A large crowd had now assembled, and regarded our movements with great curiosity. We asked to see the chief of the village, and after a good deal of palaver, a man appeared attired in a very tattered 'tobe' or gown. He had something of the Fulbe in his countenance, and was a tall fine man, though of rather a forbidding appearance. He came on board, and we endeavoured to get what information we could out of him. He said the name of the big water we saw was Nabaret, but that it was only a fourth that size in the dry season. The name of his village was Kaku. The channel of the river ran along by the mountains. He knew of the Tuburi marsh, but had never been there; he did not think the river came from there as it was distant many days' journey. He knew of no other big water, but would give us a guide to show us the way. The people of the Nabaret district are possessed of cattle, but no horses; they live principally on dhurra, which they cultivate largely, and on fish which abound in the lake. They also hunt the hippopotami, of which we saw a dozen in the lake, though doubtless there may be many more.

"We took our guide on board and endeavoured to make for Bifaré, already mentioned, which appeared to be a village of quite 6000 inhabitants, situate on the north-east shores of the lake, and distant some two miles from where we were. After proceeding about 100 yards we found that the water shoaled about a foot, and even less, and though we made every effort to proceed, we were completely baffled; turning back, by direction of the guide, we went for an opening in the high dhurra, which grew in immense quantities about here, and found ourselves once more in the channel of the stream, which was, however, only some 8 yards wide and 2½ feet deep, flowing with a swift current. After proceeding with great difficulty for almost a mile, with fields of dhurra growing to a height of 8 feet on either side and completely shutting out the view, the navigation became so difficult that we had to turn back, having already smashed in the bow of our gig, bent our rudder into the shape of a bow, and more than once berthed our little ship amongst the dhurra stalks. The stream was so narrow that we could not turn, but had to float down backwards for a good half mile. The highest point reached was a mile and a half from the village of Kaku, and from what the people said, a good thirty miles from Dawa, in the Tuburi country, the furthest point reached by any European entering Africa from the north, viz. Dr. Vogel in 1854. The stream at the point where we reluctantly turned back was not more than 2 feet deep, and from 15 to 20 feet wide, and this at the period of high water. I should say that in the dry season (and this is corroborated by the natives themselves) that a man could step across it. It is more than probable, therefore, that had we been able to proceed another three miles or so, we should have arrived at its source."

It seems evident, then, from Major Macdonald's observations, that no connection can exist between the Shari and the Benué.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. J. W. Clark, Superintendent of the Museum of Zoology and Comparative Anatomy, has been elected to the Office of Registrary of the University, vacant by the death of Dr. Luard.

The degree of M.A. *honoris causâ* has been conferred on Mr. J. Y. Buchanan, F.R.S., University Lecturer in Geography.

The Electors to the new Isaac Newton Studentships, founded by Mr. F. McClean, are Sir G. G. Stokes, Profs. Darwin and Thomson, Dr. Glaisher, and Mr. Glazebrook.

SOCIETIES AND ACADEMIES.

LONDON.

Zoological Society, May 5.—Prof. Flower, C.B., F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of April 1891, and called special attention to the arrival of what appeared to be an adult male example of the Lesser Orang (*Simia morio*) of Owen, presented by Commander Ernest Rason, R.N., who had obtained it at Sarawak, and to a Great-billed Tern (*Phaethusa magnirostris*), obtained by purchase, new to the collection.—Mr. Sclater opened a discussion on the fauna of British Central Africa, by pointing out the limits of this new territory, which was computed to embrace some 54,000 square miles of land lying immediately north of the Zambezi and west of Lake Nyassa. Mr. Sclater gave an account of the principal authorities that have already written on the subject. Mr. Sclater was followed by Mr. G. A. Boulenger, who read a paper "On the State of our Knowledge of the Reptiles and Batrachians of British Central Africa." The discussion was continued by Mr. Edgar A. Smith, who read a note on the Molluscan fauna of British Central Africa; and by Mr. E. T. Newton, who communicated some general remarks on what is known of the geology of British Central Africa, stating several points to which special attention should be directed. Remarks on various branches of the same subject were made by Dr. Günther, Mr. O. Thomas, Mr. Stebbing, Mr. Slavin, and Mr. Beddard.—Mr. T. D. A. Cockerell read notes on some Slugs of the Ethiopian Region, based on specimens in the collection of the British Museum.—Dr. C. J. Forsyth-Major read a paper containing a summary of our knowledge of the extinct Mammals of the family Giraffidæ.—A communication was read from the Hon. L. W. Rothschild, F.Z.S., containing the description of a new Pigeon of the genus *Carpophaga*, from Chatham Island, South Pacific, proposed to be called *Carpophaga chathamensis*.—Colonel Beddome read descriptions of some new Land-Shells from the Indian Region.

PARIS.

Academy of Sciences, May 4.—M. Duchartre in the chair.—On the time of evaporation of water in boilers, by M. Haton de la Goupillière. The author has mathematically determined the rate of lowering of the level of the water in steam boilers of various forms.—A geometrical theorem, by M. Tarry.—On a class of ordinary linear differential equations, by M. Jules Cels.—On the convergence of recurring simple fractions, by M. H. Padé.—On an induction inclination needle, by M. H. Wild. A short note is given on some measures of magnetic inclination made with a new form of needle. It appears from the experiments that the inclination at a place can be determined within $4''\cdot5$ by a single observation. Skilled observers make the determination within $2''\cdot5$. Inclination may therefore now be determined as accurately as declination.—On a process for constructing screws suitable for the instruments to be used for the measurement of the photographic map of the heavens, by M. P. Gautier.—Quantitative studies of the chemical action of light; Part ii., reactions with different thicknesses of glass and with different forms of vessels, by M. Georges Lemoine.—On some compounds formed by mercuric chloride, by M. G. André. The methods of preparation of the subjoined compounds are given, together with analyses establishing their composition :—

(1) $4ZnCl_2, HgCl_2, 10NH_3 + 2H_2O$;
(2) $2ZnCl_2, HgCl_2, 6NH_3 + \frac{1}{2}H_2O$;
(3) $C_6H_5NHHgCl$;
(4) $C_6H_5NH_2, HgCl_2$;
(5) $5C_6H_5NHHgCl + 2HgCl_2$;
(6) $3C_6H_5NHHgCl + 2HgCl_2$;
(7) $C_6H_5.CH_2.NH_2, HgCl_2$;
(8) $C_6H_5.CH_2.NH.HgCl$.

—A general law determining, as a simple function of the chemical constitution of bodies, the temperatures of their changes of state under all pressures, by M. G. Hinrichs.—On boron selenide, by M. Paul Sabatier. The compound is prepared by the action of vapours of selenium upon amorphous boron at a red heat, or of seleniuretted hydrogen on amorphous boron at a bright red heat in a tube of Bohemian glass. The action of water upon the selenide shows it to have the same composition as the sulphide and oxide. Its formula is therefore B_2Se_3, a conclusion supported by the results of a rapid analysis.—On the action of hydriodic acid on boron bromide, by M. A. Besson. At a raised temperature the three compounds BBr_2I, $BBrI_2$, and BI_3, have been obtained.—On the basic chromites of magnesium and zinc, and the neutral chromite of cadmium, by M. G. Viard.—Preparation of disodic erythrate, by M. de Forcrand.—Discussion of the experiments of Biot on aqueous solutions of tartaric acid in presence of potash or soda, by M. G. Aignan.—Formation of dimethacrylic acid in the preparation of the acid amides of isovaleric acid, by M. E. Duvillier.—Methyl-methylcyanosuccinate, methylethenyltricarboxylic ether, by M. L. Barthe.—On the "dextrosity" of certain Gastropods called "sinisters" (Lanistes, Peraclis, Limacina, larvæ of Cymbuliidæ), by M. Paul Pelseneer.—On the structure of the composite eye of certain Crustaceæ, by M. H. Vaillanes.—Comparative structure of the inflated roots of certain umbelliferous plants, by M. Géneau de Lamarlière. It is shown that the anomaly which is observed in the inflated lateral roots of certain umbelliferous plants (Œnanthe, Carum) is more apparent than real. In plants of the same family an intermediate series should be found between the structure called normal and the structure of a normal inflated root (Daucus, Apium).—On the microscopic structure of the phosphate rocks of Dekma (Department of Constantine), by M. Bleicher. The rocks examined are said to show under the microscope the mixture of a fair proportion of osseous *débris*, whence it is thought that this is the origin of the phosphorus in rocks rich in calcium phosphate.—Note on the Quaternary strata of Éragny and Cergy (Seine-et-Oise), by M. E. Rivière.—On the production of diabetes after the destruction of the pancreas, by M. E. Hédon.—Meteorological observations on the Pamir, by M. Guillaume Capus. An account is given of thermometric observations made between March 13 and April 19, 1887, on the high plain of Pamir, the centre of the highlands of Europasia.

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