THURSDAY, JANUARY 27, 1881

UNCONSCIOUS MEMORY

Unconscious Memory, &c. By Samuel Butler. Op. 5. (London: David Bogue, 1880.)

MR. BUTLER is already known to the public as the author of two or three books which display a certain amount of literary ability. So long therefore as he aimed only at entertaining his readers by such works as "Erewhon," or "Life and Habit," he was acting in a suitable sphere. But of late his ambition seems to have prompted him to other labours; for in his "Evolution, Old and New," as well as in the work we are about to consider, he formally enters the arena of philosophical discussion. To this arena, however, he is in no way adapted, either by mental stature or mental equipment; and therefore makes so sorry an exhibition that Mr. Darwin may well be glad that his enemy has written a book. But while we may smile at the vanity which has induced so incapable and ill-informed a man gravely to pose before the world as a philosopher, we should not on this account have deemed "Unconscious Memory" worth reviewing. On the contrary, as a hasty glance would have been sufficient to show that the book is bad in philosophy, bad in judgment, bad in taste, and, in fact, that the only good thing in it is the writer's own opinion of himself—with all that was bad we should not have troubled ourselves, and that which was good we should not have inflicted on our readers. The case, however, is changed when we meet, as we do, with a vile and abusive attack upon the personal character of a man in the position of Mr. Darwin; for however preposterous, and indeed ridiculous, the charges may be, the petty malice which appears to underlie them deserves to be duly repudiated. We shall therefore do our duty in this respect, and at the same time take the opportunity of pointing out the nonsense that Mr. Butler has been writing, both about the philosophy of evolution and the history of biological thought.

The great theory which Mr. Butler has propounded, and which with characteristic modesty he says seems to himself "one, the importance of which is hardly inferior to that of the theory of evolution itself"—this epoch-making theory is as follows. The processes of embryonic development and instinctive actions are merely "repetitions of the same kind of action by the same individuals in successive generations." Therefore animals know, as it were, how to pass through their embryonic stages, and, after birth, are taught by instinctive knowledge, simply because as parts of their ancestral organisms they have done the same things many times before; there is thus a race-memory as there is an individual memory, and the expression of the former constitutes the phenomena of heredity.

Now this view, in which Mr. Butler was anticipated by Prof. Hering, is interesting if advanced merely as an illustration; but to imagine that it reveals any truth of profound significance, or that it can possibly be fraught with any benefit to science, is simply absurd. The most cursory thought is enough to show that, whether we call heredity unconscious memory, or memory of past states of consciousness the hereditary offspring of those states, we have added nothing to our previous knowledge either of heredity or of memory. All that lends any sense to the analogy we perfectly well knew before—namely, that in the race, as in the individual, certain alterations of structure (whether in the brain or elsewhere) when once made, tend to remain. But the analogy throws no light at all upon the only point which requires illumination—namely, how is it that, in the case of heredity, alterations of structure can be carried over from one individual to another by means of the sexual elements. We can understand in some measure how an alteration of brain structure, when once made, should be permanent, and we believe that in this fact we have the physical basis of memory; but we cannot understand how this alteration is transmitted to progeny through structures so unlike the brain as are the products of the generative glands. And we merely stultify ourselves if we suppose that the problem is brought any nearer to a solution by asserting that a future individual while still in the germ has already participated, say in the cerebral alterations of its parent—and this in a manner analogous to that in which the brain of the parent is structurally altered by the effects of individual experience. But Mr. Butler goes even further than this, and extends his so-called theory even to inorganic matter. He "would recommend the reader to see every atom of the universe as living, and able to feel and remember, though in a humble way." Indeed he "can conceive of no matter which is not able to remember a little"; and he does "not see how action of any kind is conceivable without the supposition that every atom retains a memory of certain antecedents." It is hard to be patient with such hypertrophied absurdity; but if the bubble deserves pricking, it is enough to ask how it is "conceivable" that an "*atom*," even if forming part of a living brain, could possibly have "a memory of certain antecedents," when, as an atom, it cannot be conceived capable of undergoing any structural modification.

So much for Mr. Butler's main theory. But he has also a great deal to say on the philosophy of evolution. "Op. 4" was called "Evolution, Old and New," and now "Op. 5" continues the strain that was struck in the earlier composition. This consists for the most part in a strangely silly notion that "the public generally"—including, of course, the world of science—was as ignorant of the writings of Buffon, Dr. Erasmus Darwin, and Lamarck as was Mr. Butler when he first read the "Origin of Species." That is to say, "Buffon we knew by name, but he sounded too like 'buffoon' for any good to come from him. We had heard also of Lamarck, and held him to be a kind of French Lord Monboddo; but we knew nothing of his doctrine. . . . Dr. Erasmus Darwin we believed to be a forgotten minor poet," &c. No wonder, therefore, when such was our manner of regarding these men, that we required a Mr. Samuel Butler to show us our error. And no wonder that Mr. Charles Darwin, who doubtless may have peeped into the literature which Mr. Butler has discovered, should so well have succeeded in his life-long purpose of concealing from the eyes of all men how much he owes to his predecessors. No wonder, also, that Mr. Darwin, when he chanced to see an advertisement of a forthcoming work by Mr. Butler with the title "Evolution, Old and New," should have inferred, as Mr. Butler observes, "what I was about," and forthwith began to tremble in dismay that at last the Buffoon, the French Lord Monboddo, and the forgotten minor poet had found a champion to vindicate their claims. For now the hideous corruption of the monster was about to be exposed who had fed as a parasite upon these "dead men," till he stands before our eyes bloated with honours undeserved, and extending "his power of fascination all over Europe," not only "among the illiterate masses . . . . but among experts and those most capable of judging." No wonder then that Mr. Darwin, knowing that at last a wise young judge had come to judgment and to open the eyes of the "experts," should at once have set about a book on his own grandfather to disarm by anticipation the justice of the avenger. But natural as all this unquestionably appears, it scarcely prepares us, as it did not prepare Mr. Butler, for the depths of deceit and depravity to which Mr. Darwin would "condescend" in order to thwart the arm of justice. Yet the fact is that Mr. Darwin entered into a foul conspiracy with Dr. Krause, the editor of *Kosmos*, to slay by infamous means the righteous but damning work of Mr. Butler. "The steps," as he points out, "are perfectly clear." A whole number of *Kosmos* was devoted to Mr. Darwin and his antecedents in literature, at about the time when "Evolution Old and New" was "announced" as in preparation. Soon afterwards arrangements were made for a translation of Dr. Krause's essay, and were completed by the end of April, 1879. Then "Evolution Old and New" came out, was read by Dr. Krause, who modified a passage or two in a manner that "he thought would best meet 'Evolution Old and New,' and then fell to condemning that book in a *finale* that was meant to be crushing." So far all was fair enough; but now comes the foul play. "Nothing was said about the revision which Dr. Krause's work had undergone, but it was expressly and particularly declared in the preface that the English translation was an accurate version of what appeared in the February number of *Kosmos*, and no less expressly and particularly stated that my book ["Evolution Old and New"] was published subsequently to this. Both these statements are untrue," &c. Having discovered this erroneous conspiracy, Mr. Butler wrote to Mr. Darwin for an explanation. With almost incredible complacency this arch-hypocrite had the hardihood to answer that it "is so common a practice" to modify articles in translation or republication, that "it never occurred to him to state that the article had been modified," but that now he would do so should there be a reprint. This, as Mr. Butler says, "was going far beyond what was permissible in honourable warfare, and it was time in the interests of literary and scientific morality . . . to appeal to public opinion." He therefore communicated the facts to the *Athenæum*, expecting as a consequence to raise a "raging controversy." Strange to say, however, the thing fell flat. "Not only did Mr. Darwin remain perfectly quiet, but all reviewers and *littérateurs* remained perfectly quiet also. It seemed . . . as if public opinion rather approved of what Mr. Darwin had done." Nevertheless Mr. Butler had a salve to his disappointment in that he saw "the 'Life of Erasmus Darwin' more frequently and more prominently advertised than hitherto," and "presently saw Prof. Huxley hastening to the rescue with his lecture 'On the Coming of Age of the Origin of Species.'" Truly, therefore, in some, if not quite in full measure, Mr. Butler's "vanity," as he himself observes, "was well fed by the whole transaction"; for he saw by it that Mr. Darwin "did not meet my work openly," and therefore that Prof. Huxley had to "hasten to the rescue" with a Royal Institution lecture. How sweet it doubtless was, if Mr. Butler attended that lecture, to think what a large proportion of the audience must have seen through the whole plot! Enough, surely, to "feed" any ordinary "vanity." But Mr. Butler's vanity is inordinate, and so requires a more than ordinary amount of nourishment. He therefore felt it desirable to give a detailed exposition of the whole affair, and this we have in some charmingly temperate and judicious chapters of "Op. 5."

But to be serious. If in charity we could deem Mr. Butler a lunatic, we should not be unprepared for any aberration of common-sense that he might display. His "Op. 5," however, affords ample evidence that he is not a lunatic, but a man who wants to make a mark somewhere, and whose common sense, if he ever had such a thing, has been completely blinded by self-conceit. To us, no less than to him, "the steps are perfectly clear." A certain nobody writes a book accusing the most illustrious man in his generation of burying the claims of certain illustrious predecessors out of the sight of all men. In the hope of gaining some notoriety by deserving and perhaps receiving a contemptuous refutation from the eminent man in question, he publishes this book, which, if it deserved serious consideration, would be not more of an insult to the particular man of science whom it accuses of conscious and wholesale plagiarism, than it would be to men of science in general for requiring such elementary instruction on some of the most famous literature in science from an upstart ignoramus who, until two or three years ago, "considered" himself "a painter by profession." The eminent man however did not administer the chastisement: hence these tears of rage and chagrin; hence too the morbid fancying of the great man's discomfort—of the rallying round of his friends, Krause's article, Huxley's lecture, &c., till such an explosive state of feeling was fermented that a mere omission to supply a reference to a book was magnified into a dark conspiracy—notwithstanding that a moment's thought might have shown how such a conspiracy, even if attempted, would not have been worthy of imbeciles.

But, in conclusion, let us ask what this work on "Evolution, Old and New" contained to produce, as its author imagines, such a scare among the leading "experts" in science. The work has already been reviewed in these columns (June 12, 1879) by Mr. Wallace, who, while fully exposing its weakness, treats the author with more consideration than he deserves—doubtless because Mr. Wallace is himself so personally associated with the theory of "natural selection." It is therefore sufficient for us here to say that "Evolution, Old and New," conveys a confession on the part of its author that until two or three years ago he was totally ignorant concerning the history of biological thought. His attention having at length been directed to the fact that some of the best naturalists had speculated on the probability of evolution, he for the first time found, as he innocently enough observes, that evolution and natural selection are not quite the same thing. Having made this highly original discovery, he forthwith proceeds to display a feebleness of judgment even more lamentable than his previous ignorance. For he concludes that the older speculations on the causes of evolution are more satisfactory than those advanced by Mr. Darwin. In the columns of a scientific journal any comment on such a conclusion might well be deemed superfluous, although Mr. Wallace, in his review above mentioned, had the courtesy to expose its folly. The older evolutionists deserve indeed all honour for having perceived early in the day that some theory of descent must be true, even though they were not able to find the theory that could be seen to be in any measure satisfactory. But a man who in the full light of Darwin's theory can deliberately return to "the weak and beggarly elements" of Lamarck—such a man only shows that in judgment he is still a child. The extreme weakness of Mr. Butler's argumentation has, as we have said, already been shown by Mr. Wallace; but it is of more interest to ask what infatuation it can have been that led him to suppose "all Europe and those most capable of judging" required him as an author to make himself ridiculous as an expounder of this subject. The answer is not far to seek. As Mr. Butler himself has told us, he has vanity, and his vanity is not less childish than his judgment. Thus, to give only one illustration. Of so much importance does he deem his own cogitations, that in the book we are reviewing he devotes two chapters, or more than thirty pages, to "How I wrote 'Life and Habit,'" and "How I wrote 'Evolution, Old and New'"; entering into a minute history of the whole course of his speculative flounderings. This is the only part of the book that repays perusal; but that this part well repays perusal may be judged from the following, which we present as a sample:—

"The first passage in 'Life and Habit' which I can date with certainty is one on p. 52, which ran as follows: . . . '"Do this, this, this, which we too have done, and found our profit in it," cry the souls of his forefathers within him. Faint are the far ones, coming and going as the sound of bells wafted on to a high mountain; loud and clear are the near ones, urgent as an alarm of fire.' This was written a few days after my arrival in Canada, June 1874. I was on Montreal Mountain for the first time, and was struck with its extreme beauty. . . . Sitting down for a while, I began making notes for 'Life and Habit,' of which I was then continually thinking, and had written the first few lines of the above, when the bells of Nôtre Dame in Montreal began to ring, and their sound was carried to and fro in a remarkably beautiful manner. I took advantage of the incident to insert then and there the last lines of the piece just quoted. I kept the whole passage with hardly any alteration, and am thus able to date it accurately. . . . Early in 1876 I began putting these notes into more coherent form. I did this in thirty pages of closely-written matter, of which a pressed copy remains in my commonplace-book. I find two dates among them—the first 'Sunday, February 6, 1876'; and the second, at the end of the notes, 'February 12, 1876.'"

This historical sketch, which is without the smallest interest to any one but Mr. Butler himself, winds up with the following burst of eloquence:—

"Here, then, I take leave of this matter for the present. If it appears that I have used language such as is rarely seen in controversy, let the reader remember that the occasion is, so far as I know, unparalleled for the cynicism and audacity with which the wrong complained of was committed and persisted in. I trust, however, that, though not indifferent to this, my indignation has been mainly roused, as when I wrote 'Evolution, Old and New,' before Mr. Darwin had given me personal ground of complaint against him, by the wrongs he has inflicted on dead men, on whose behalf I now fight, as I trust that some one—whom I thank by anticipation—may one day fight on mine."

Mighty champion of the mighty dead! When our children's children shall read in Westminster Abbey the inscription on the tomb of Mr. Samuel Butler, how will it be with a sigh that in their day and generation the world knows nothing of its greatest men! But as it is our misfortune to live before the battle over Mr. Samuel Butler's memory has been fought, we respond to his abounding presumption by recommending him, whatever degree of failure he may have experienced in art, once more to "consider" himself "by profession a painter"—or, if the painters will not have him, to make some third attempt, say among the homœopathists, whose journal alone, so far as we are aware, has received with favour his latest work. GEORGE J. ROMANES

NEWTON'S BRITISH BIRDS

A History of British Birds. By the late William Yarrell, V.P.L.S., F.Z.S. Fourth Edition, revised by Alfred Newton, M.A., F.R.S. Part 10, November, 1876; 11, September, 1877; 12, October, 1878; 13, June, 1880. (London: Van Voorst.)

WE call this work advisedly "Newton's British Birds," although the title-page would seem to signify that it is only a fourth edition of Yarrell's well-known "History." It is however in fact a new book. The text has been completely rewritten, and the familiar woodcuts and vignettes alone remain to remind one of the former author.

The parts of Prof. Newton's work now before us conclude the account of the Passeres and contain the commencement of the history of the British Picariæ. We need hardly say that the article upon each species is worked out in the same careful and accurate way as in the former portion of this work. Prof. Newton, as every ornithologist knows, is our leading authority on this subject, which, during a course of many years of constan attention, he has made specially his own. We observe with great pleasure the elaborate manner in which the distribution of each species is described, not only within the area of the British Islands, but also wherever it is known to occur on other parts of the world's surface. We may likewise notice the entire absence of misprints and the excellence of the type and paper, which do credit alike to the author and publisher, and will no doubt greatly contribute to extend the circulation of the work. Having said thus much, it is with regret that we must add one word of discontent, for which we trust Mr. Van Voorst and Prof. Newton will alike forgive us. The rate of issue of the numbers is so slow that it is difficult to calculate when the new edition will be completed. As will be seen by the heading of the article, only four parts have been published during the four past years. If, as we suppose, about twenty more parts are required to finish the work, it is manifest that unless the present rate of progress be expedited it will be twenty years before we are able to send our new "History of British Birds" to the binders. The edition was commenced, we believe, in 1871. Now thirty years seems rather long for the execution of a new edition of any work, even with all the improvements which, as we have shown above, the present editor has doubtless bestowed upon it. We would fain ask therefore whether the author and publisher cannot manage to move on a little faster. If this cannot be done it appears to us that the first portion of the work will be almost out of date before the last part is published, and that the subscribers will have good reason to complain.

OUR BOOK SHELF

Jahrbücher für wissenschaftliche Botanik. Herausgegeben von Dr. N. Pringsheim. Elfter Band, drittes und viertes Heft. With twenty-four plates. (Leipzig: W. Engelmann, 1877 and 1878.)

DR. JAKOB ERIKSSON describes in a lengthened paper the protomeristem of the roots of Dicotyledons, and directs attention to the four great types of structure observable in these roots. In the first type the apex consists of three separate zones of meristem: the plerome, periblem, and dermocalyptrogen. In the second type only two zones are present: the plerome and a common zone for primary cortex, epidermis, and root-cap. In the third type there is a common meristem zone from which all the others develop; while in the fourth there are two zones, the periblem and the plerome. Two additional types are met with in Monocotyledons: (1) in which there are four zones of meristem: calyptrogen, dermatogen, peroblem, and plerome; and (2) in which there are three zones: the calyptrogen, the plerome, and a common zone for cortex and epidermis.

The germination of Equisetum and Schizæaceæ forms the subject of two papers, one by Sadebeck and the other by Bauke, whose work was arrested by premature death. Woronin contributes a paper on the *Plasmodiophora Brassicæ*, the remarkable Myxomycete which seems to be the cause of the so-called Hernia of the cabbage plant, which has recently attracted so much attention.

The remaining papers are by Reinke, on *Monostroma bullosum* and *Tetraspora lubrica.* Wydler discusses at great length the morphology of certain forms of inflorescence, chiefly dichotomous; and lastly there is a paper by Pitra on the pressure in stems during the appearance of bleeding in plants. The contents of the parts are, as will be seen, very varied and deal with many different departments of botany, and will be found to sustain the reputation of the "Jahrbücher" so long associated with the name of Pringsheim.

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. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Unconscious Memory—Mr. Samuel Butler

WILL you kindly allow me a portion of your valuable space in order that I may demonstrate the completely groundless character of a series of insinuations which Mr. Samuel Butler has made not only against myself, but also against Mr. Charles Darwin, in the work which he has recently published, entitled "Unconscious Memory" (Op. 5).

1. Mr. Butler insinuates that Mr. Darwin caused my essay on Dr. Erasmus Darwin to be translated simply in order to throw discredit on his work, "Evolution, Old and New" (Op. 4), which was published in May, 1879. Upon this point I have to observe that Mr. Darwin informed me of his desire to have my essay published in English more than two months before the appearance of Mr. Butler's book; that the translation did not appear earlier is due to the fact that I asked for a delay in order that I might be able to revise it.

2. The assumption of Mr. Butler that Mr. Darwin had urged me to insert an underhand attack upon him (Mr. Butler) in my sketch, is not only absolutely unfounded, but, on the contrary, I have to state that Mr. Darwin specially solicited me *to take no notice whatever* of Mr. Butler's book, which had in the meantime appeared. Since however I thought it desirable to point out that Dr. Erasmus Darwin's views concerning the evolution of animated Nature still satisfy certain thinkers, even in our own day (a fact which must add greatly to Dr. Darwin's reputation), I have made some remarks upon the subject in a concluding paragraph, without however naming Mr. Butler. And I may here emphatically assert, that although Mr. Darwin recommended me to omit one or two passages from my work, he neither made nor suggested additions of any kind.

3. Mr. Butler's assertion that the revision of my translation was made "by the light" of his book is only in so far justifiable that I looked over the latter before sending off my work, and that my attention was thereby called to a remark of Buffon's. From Mr. Butler's book I have neither taken nor was I able to take the slightest information that was new to me concerning Dr. Erasmus Darwin's scientific work and views, since in it practically only one portion of the "Zoonomia" is discussed at any length, and this portion I had already quoted and analysed, while Mr. Butler only refers to one comparatively unimportant part of the "Botanic Garden," and absolutely ignores the "Phytologia" and the "Temple of Nature." So that no *single line* of Mr. Butler's far from profound work was of the slightest use to me.

Mr. Butler's contention that I have quoted from his book a remark from Coleridge is entirely without foundation. I have been acquainted with this remark for years, and from the source quoted. It is also quoted in Zoeckler's work (vol. ii. p. 256), mentioned by me on p. 151, which appeared *prior* to Mr. Butler's book (Op. 4). The whole of my indebtedness to Mr. Butler reduces itself therefore to a single quotation from Buffon.

4. Finally, as concerns the main accusation that no mention is made in the preface of the fact that my essay had been revised previously to publication, it is clear, as even a child could not fail to see, that this is not due to design, but is simply the result of an oversight. It would be simply absurd for a writer intentionally to attack a publication which appeared subsequently to the date indicated on his title-page; and the so-called falsification, so far from injuring Mr. Butler, could only be most agreeable to him, because it might induce the careless reader to fancy that no reference whatever was intended to Mr. Butler in the closing sentence. Should however such a reference be clearly intended—and to every reader posted up in the subject this could not be doubtful—every man of common sense would recognise this terrible falsehood to be a simple oversight.

Berlin, January 12 ERNST KRAUSE

Hot Ice

I VENTURE, in referring to Dr. Lodge's letter of this week, to put before your readers the meaning of the remarks made on Dr. Carnelley's experiment at the Chemical Society by Prof. Ayrton, who is now away from England. I understood him to say that as Dr. Carnelley's hot ice is obviously in a condition which cannot be represented within the as yet known fundamental water surfaces, it is necessary to produce these surfaces beyond the places at which, hitherto, abrupt changes have been supposed to take place in them. He took as an instance the *ice-water* surface which has hitherto been assumed to stop at Prof. James Thomson's "triple point," and showed that although Sir Wm. Thomson's experiments have proved that it is nearly plane for the stable state of water and ice, yet in the imaginary district beyond the triple point a change of latent heat might give such a change of curvature as to bring this surface into the hot-ice region.

With Prof. Ayrton I have done for water what Prof. James Thomson did for carbonic acid; we constructed in stiff paper a surface or surfaces which represent the relations of p, v and t for a given quantity of water-stuff. Three parts of the whole are cylindric surfaces and divide space into three regions; in one of them the substance is in the form of ice, in another in the form of water, in another in the form of vapour; and they meet in Thomson's triple point. Any one looking at this model must feel that Prof. Ayrton was right in looking for the hot-ice state in a region bounded by imaginary productions of the *all-ice*, the *all-water*, the *all-vapour*, and the above-mentioned three cylindric surfaces beyond their lines of intersection. This is what Prof. James Thomson did to indicate the state of water before boiling by bumping begins. He assumed that the *all-water* surface changed into the *all-vapour* surface gradually, and not through a purely cylindric *water-vapour* surface, and this is really what Dr. Lodge himself does for hot ice. That is, he imagines the *all-ice* surface to change into the *all-vapour* surface gradually, and not by sudden changes through a purely cylindric *ice-vapour* surface. According to Mr. Ayrton the imaginary production is even of a more complicated kind than Dr. Lodge supposes, as the ice probably changes into unstable water before it changes into steam. There can be no doubt that such imaginary productions find their place in the fundamental equation of water, but I cannot agree with Dr. Lodge in thinking that we have at present an explanation of such unstable conditions. If his explanation were satisfactory we ought to be able in the same way to explain the unstable position which precedes boiling by bumping, and this we cannot do. Where the explanation seems to me to fail is in the assumption that the hot vapour filling a cavity, being of lower temperature than the surface of the cavity, is always at a pressure less than that of saturation, in spite of the evaporation going on. Now when we consider how large the surface of a minute cavity is as compared with its volume, the very great increase in bulk when the solid is changed into vapour and the lowering of temperature which the surface must undergo on account of latent heat, we see that the condition which Dr. Lodge assumes to be maintained during the whole experiment would be instantaneously destroyed in a very minute cavity. In explaining hot ice I am afraid that neither Prof. Ayrton nor Dr. Lodge has given us more than Prof. James Thomson has given in explaining "boiling by bumping." The cause of the phenomenon is a molecular one probably, and must be left to the guesses of molecular physicists. JOHN PERRY

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Mr. Bottomley's Experiments with Vacuum Tubes and the Aurora

MR. BOTTOMLEY'S extremely interesting experiments briefly described in NATURE, vol. xxiii. pp. 218 and 243, appear to have a very important bearing on the question of atmospheric electricity; for if such high vacua are good conductors of electricity we have reason for thinking that the electrical conditions of our globe will be very different from what we have been accustomed to regard them. The layers of denser air surrounding the conducting matter of the globe will act like the glass of Mr. Bottomley's tubes in maintaining by a Leyden-jar-like action any difference of potential that there may be between their inner and their outer surface. Again, in the piercing of the glass tube by a minute spark, we have the analogue of the lightning flash between the clouds and the earth; the insulating layer in each case giving way, when, owing to an excessive increase in the surface density of the charge at any point, the dielectric stress exceeds the limits of the dielectric strength of the medium. The internal luminous effects observed by Mr. Bottomley as the result of change in the distribution of the external charge of electricity will be the physical analogues of the *aurora*, with this difference, that they take place in the ultra-gaseous interior, whereas in the case of our globe the luminous phenomena take place in the ultra-gaseous (*i.e.* highly rarefied) exterior regions of the atmosphere. It would be interesting to learn whether such discharges present any other analogies with auroral phenomena. I should be particularly interested in learning whether the conditions under which such luminous effects are obtained give any support to the theory which I think to be the only consistent one, that the aurora is due *not* to electrical discharges from regions of less atmospheric density to regions of a greater density (or *vice versâ*), but to electrical discharges in a region of pretty uniform (and small) density, and in which region differences of electric potential exist. According to this view the auroral streaks which appear to be radial should in reality lie approximately parallel to the earth's surface, and not stand (as most persons imagine) normal to it. A series of horizontal parallel lines drawn across the sky in a direction approximately north and south would necessarily appear to an observer on the earth's surface foreshortened into a set of lines diverging in fan-like forms at either the north point or the south point of the horizon. Their divergence would therefore be apparent only, like the "beams" diverging from the sun at sunset on a cloudy day, or like the beams of the *rayons du crépuscule*, or like the "radial streaks" which I have pointed out as frequently accompanying rainbows. SILVANUS P. THOMPSON

University College, Bristol, January 22

P.S.—The behaviour of a hollow sealed glass tube containing a conducting substance in its interior was noticed just one hundred years ago by Cavallo, who sealed up a glass tube in which mercury was at its boiling-point, thus obtaining a fairly perfect vacuum.—S. P. T.

The Geological Age of the North Highlands of Scotland

FROM the abstract of *Proceedings* of the Geological Society (January 5) I learn with surprise that Sir R. Murchison's interpretation of the succession of the beds over the region north of the Caledonian Canal is disputed, and that the relations of the fossiliferous limestone of Durness to the quartzites "are" (according to Dr. Callaway) "by no means satisfactorily established, and that their conformity is rendered dubious by a marked discordance of strike"; in fact that the limestone lies in a synclinal basin amongst the quartzites, so that if the limestone be of Lower Silurian ("Arenig") age the quartzites and schists must be older; this I presume to be the inference Dr. Callaway intends to draw, as he says there "is no proof of the Lower Silurian age of the quartzite and newer series of flaggy gneiss and schist" constituting the interior mountainous district.

Having had an opportunity last spring of visiting the district lying between Lochs Broom and Inchard under the guidance of Prof. Geikie and in company with my colleague of the Irish Survey, Mr. Symes, I take the opportunity offered by Dr. Callaway's paper of expressing my entire concurrence in the interpretation of the structure of the country given by my late chief, whose elaborate and graphic descriptions in the pages of the *Quarterly Journal* of the Geological Society (vols. xv. and xvii.) will, I feel sure, never be invalidated.

After seeing the clear infra-position of the limestone to the upper quartzite and schists first in the section at the Bridge of Ault-Corry near Ullapool, then in the cliffs near Ullapool, next at Inchnadamff and the head of Loch Assynt, then again in the Forest of Arkle and the hills bordering Loch Stack, where the limestone band is clearly interbedded between the lower and upper quartzites, and this latter as clearly passes under the schists of the interior, it required no further evidence to prove that all these beds belong to one conformable formation; and that the geological age of the whole group is determined by the fossils discovered by Mr. Charles Peach in the limestone of Durness or Assynt, and named by the late Mr. Salter. The geological sequence is so clear throughout that region, and so entirely bears out the description given by Murchison and Geikie, that "he who runneth may read"; and I have no hesitation in saying that the evidence that the Millstone Grit overlies the Carboniferous Limestone, and that the New Red Marl overlies the New Red Sandstone is not more clear than that the upper quartzites and schists overlie the Assynt limestone.

I wish to point out in conclusion that the trough-shaped arrangement of the Durness limestone and its faulted position, described by Dr. Callaway, has already been described by Murchison in the *Quarterly Journal*, vol. xv. Any one visiting the grand tract of country lying between Durness and Loch Maree need have no better guide than the papers I have referred to, and a good geological map. He will find that there is little, if anything, to add to the details and conclusions there given, and were it not that Dr. Callaway's objections seem to find support with some geologists of more experience than himself, it would not have been necessary to enter a *caveat* against them.

As regards the question whether in any part of the Highlands of Scotland except along the western coast the Laurentian (or "pre-Cambrian") rocks reappear, as has been stated or suggested, I do not wish to offer an opinion. As regards the region north of the Caledonian Canal, it seems to me that this is extremely improbable, as along the two traverses we made—one from Garve to Ullapool, the other from Laxford to Lairg—the prevalent dips are eastward, and the upper quartzites forming the elevations of Ben Dearig and Ben More are of great thickness. One may therefore assume that the Laurentian gneiss (even in the absence of the Cambrian sandstone) is deeply buried beneath these beds and their succeeding schists. The region of the Grampians of Aberdeenshire, on the other hand, is of great extent, and until it has been explored by the officers of the Geological Survey it would be injudicious (as it appears to me) to come to any opinion on the subject. EDWARD HULL

Geological Survey Office, Hume Street, Dublin, January 18

Geological Climates

HAVING considered the effects of Mr. Wallace's proposed redistribution of land and water, intended to raise the mean annual temperature of Bournemouth 15° or 20° F. above its present amount, I now, with your permission, shall say a few words on some minor questions, which have arisen during our discussion of the difficult problem of Geological Climates.

1. *The Clump of Bamboos at Cooper's Hill Engineering College.*—Prof. McLeod has kindly forwarded me a specimen of the foliage of the bamboo now growing in his garden, and has promised to send me the fruit when it ripens.

My botanical friends cannot decide its species, with certainty, from the foliage alone, without the seeds, but think that it, probably, is the bamboo called *Thamnocalamus Falconeri*, formerly called *Arundinaria falcata* (not *Arundinacea*) and also called *Bambusa gracilis*. If this opinion be correct my rejection of its evidence in favour of Cooper's Hill now having the climate of "torrid India" was also correct; for this bamboo is one of the hardiest of the "hardy bamboos" growing in the Himalayas, as high as the limit of perpetual snow, and being exposed, at night and in winter, to extremes of cold, which are never experienced in the British Islands. Whether our summers are hot enough to ripen its seeds, and fully acclimatise it amongst us, remains to be seen.

It is a suggestive fact that at Fota, in the Cove of Cork, where it grows in clumps 20 feet in circumference, from each of which spring over 400 canes reaching a height of 25 feet; the seeds ripen with difficulty and take a long time to germinate, some two months elapsing before they come through the soil, even in a temperature of 70° F.

2. *The Moreton Bay Pine at Bournemouth.*—Mr. William Ingram's letter, stating that an individual of this species, surrounded with "wooded heights" about it, has lived for forty years in Leicestershire, and attained a height of 35 feet, shows what the gardener's skill can accomplish in protecting a sub-tropical tree from the injurious effects of English winters, but throws no light whatever upon the possibility of the Moreton Bay pine living spontaneously in this country.

In order to do so it must ripen its fruit and produce seedlings, which (as I am informed) it cannot possibly do with the moderate heat of our cool summers.

3. *Tertiary Climates in England.*—Mr. Gardner states, that independently of the evidence afforded by the Moreton Bay pine, the Tertiary fossil plants of the Eocene require an increase of temperature of, at most, 20° F.

When we add to this that the London clay contains true *Crocodiles*, true *Palms*, many species of *Nautilus*, of *Volutes*, and large species of *Cypræa*, we may be certain that 20° F. increase of temperature is the very minimum required.

The question of importance is, whence did this required heat come from? This is a question of number and magnitude, and not of mere "naturalist talk." This question cannot be settled by redistributions of land and water, nor by repeating continually the assertion that all former causes of change of climate were the same as existing causes, not only in kind, but in degree.

SAMUEL HAUGHTON

Trinity College, Dublin, January 14

I AGREE with Prof. Haughton in his conclusion that no increase in the quantity of water sent into the Arctic Ocean by currents like the Gulf Stream would make much practical difference in the Arctic climate, though not altogether for his reasons. I think the question of total quantities of heat is irrelevant, and that the extent of glaciation and the distribution of plants and animals are almost exclusively determined by summer temperatures.

Respecting the distribution of plants and animals, I believe this is the general testimony of naturalists, and it is certainly confirmed by Nordenskjöld's observations on the Siberian flora. Respecting glaciation, I rely for proof on the well-known fact that the extent of perpetual snow on mountains—in other words, the height of the snow-line—depends, not on mean temperature, but on summer temperature.

If this is true it shows that no change in the ocean currents would make much difference; for a glance at Dové's isothermal lines for July and January shows that the effect of the Gulf Stream on the temperatures of Europe and Asia and the Arctic Ocean is chiefly confined to winter. The late Mr. Hopkins, in his well-known paper on changes of climate (Geological Society, December, 1851) estimated that the effect of the Gulf Stream on the July climate of London is null.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, January 17

Prof. Whitney on the Glaciation of British Columbia

IT must be gratifying to all geologists interested in the western part of America to find that a portion of the general results of the work of the Californian Survey is at length being published under the auspices of the Museum of Comparative Zoology at Harvard College, Prof. Whitney's "Auriferous Gravels of the Sierra Nevada" being now supplemented by the first part of a volume on the "Climatic Changes of Later Geological Times," dealing chiefly with the evidences of glaciation on the Pacific slope. No one will question Prof. Whitney's observations and deductions on this subject when he deals with that portion of the region with which he is personally familiar, especially as these are in substantial agreement with the already-published facts of Clarence King. The general result arrived at in the areas of Whitney's and King's surveys is that comparatively only a very small portion of the highest ranges of mountains has ever been covered with glaciers, and that there has never been in this region anything like a northern drift period or a transportation of material in any given direction independent of the present topographical features of the country.

This accords also with the statement published by Prof. Whitney in 1866 (*Proc.* Col. Acad. Sci. vol. iii. p. 271) as to the absence of glacial traces of a general character from California, but—as it appears to me unfortunately—a clause was added to this statement embracing in the generalisation the whole north-western extension of the Cordillera region. Now in 1866, as Prof. Whitney himself says, almost nothing was definitely known of the coast north of Oregon, and for that portion of it included in the province of British Columbia I have since maintained, as the results of observation, that there is conclusive proof of the occurrence of a period of general glaciation comparable in its effects with that of eastern North America (see *Quart. Journ.* Geol. Soc. vol. xxxiv. p. 89; *Canadian Naturalist*, vol. viii. No. 7; vol. ix. No. 1; also the following *Reports* of the Geological Survey of Canada, 1875-76, p. 261; 1877-78, p. 133 B.; 1878-79, p. 89 B.) In summarising and discussing the evidences of glaciation in British Columbia however Prof. Whitney still thinks it necessary to support the correctness of his paper of 1866. As Prof. Whitney's volume appears to be intended as a general, and, so far as the facts now known go, final review of the glaciation of the Pacific slope, and professes to contain "all that is necessary to set forth in regard to the former glaciation of the western side of the American continent," it may not be amiss to state that in my view the account given of the evidences of glaciation in British Columbia is in some cases insufficient, and that in the interpretation of other points misconceptions as to the nature of the facts have arisen. The tendency of the whole treatment of the subject is to minimise the glacial phenomena of the northern part of the coast and assimilate the conditions there found to those of California, which appear to me to be essentially different. (For a comparison of these see "Travelling Notes on the Surface Geology of the West Coast," *Canadian Naturalist*, vol. viii. No. 7.)

To criticise minutely the numerous features which seem open to such treatment in the account of this region, with which seven seasons' work in connection with the Boundary Commission and Geological Survey of Canada has rendered me familiar, would require a lengthened article, and would at best be an ungracious task. I will therefore touch on a few salient points only.

In dealing with the interior region of British Columbia lying between the Rocky and Coast Mountains no mention is made of the actual evidence obtained of a movement of ice from north to south in this plateau district, though it is afterwards incidentally alluded to in a quotation connected with a proposed explanation of the facts observed. The drift-covered and erratic-strewn character of the country is also ignored; and while the lower terraces bordering the rivers are mentioned, and attributed to fluviatile action—a view doubtless substantially correct—the fact that terraces are found beyond the river-valleys attaching themselves to the higher parts of the plateau and to the mountain-sides to an elevation of 5270 feet is passed over in silence. The conclusion is then easily arrived at that the "statement" of 1866 is "entirely borne out by an overwhelming weight of evidence."

Turning now to the coast of the province, Prof. Whitney of course admits the marked glaciation of the south-eastern extremity of Vancouver Island, which has been noticed by a number of observers, and which he has himself seen during a hurried visit. He states however that the markings he saw were everywhere parallel to the coast, and appeared to him more like iceberg than glacier work. Now as the coast is very sinuous in outline, while the main glaciation pursues *within a few degrees a uniform direction* (S. 11° W.), the two must in some places coincide, but an intimate acquaintance with the south-eastern part of Vancouver Island enables me to state that the glaciating agent has swept completely and steadily over it entirely, without reference to the present coast outlines. With regard to the second statement, I believe that a reference to the description of the character of the glaciation given in one of my papers already referred to (*Quart. Journ.* Geol. Soc. vol. xxxiv. p. 92) will be sufficient to convince any one who is familiar with ice action that a glacier has done the work. It is of course easier to be personally assured, where so much depends on judgment of local details, than to demonstrate the actual conditions to others; but the parallel grooving and furrowing out of hard rocks in the manner illustrated on pp. 93, 94, and 96, one has been accustomed to consider as characteristic of glaciers.

Further on Prof. Whitney assumes that the "manifestations" of the supposed Strait of Georgia glacier are "almost or quite exclusively limited to its termination." Some evidence to the contrary is however given in the publication to which special reference has just been made, while subsequent exploration—the published account of which Prof. Whitney appears to have overlooked—has brought to light similar and concordant glacier-work at Nanaimo, ninety miles to the north-west of Victoria, and has also demonstrated that a second branch of the great ice mass which choked the space between Vancouver Island and the mainland, comparable in size with that of the Strait of Georgia, discharged north-westward by Queen Charlotte's Sound (*Canadian Naturalist*, vol. ix. No. 1). In the lately-issued volume of the Geological Survey (1878-79) additional facts tending to show the importance of ice-action in the Queen Charlotte Islands and extreme north of the coast of British Columbia are given.

Not being in the position of having any favourite theory of glaciation to maintain, I wish merely to indicate by a few examples the inadequacy of the portion of Prof. Whitney's monograph which is intended to summarise the glacial conditions of British Columbia. Prof. Whitney appears to have been beset by observers "entirely inexperienced in the study of glacial phenomena" to such an extent as to render him unduly suspicious of the evidence obtained by other workers. He states, for example, that in passing to the region north of the boundary of the United States "we have to depend largely on the observations of others," and that "an attempt will be made to sift the evidence offered." Now while it is a little discouraging to find that one must belong to the class of "others," I feel confident that to any unprejudiced inquirer the facts already accumulated and published are sufficient to prove the general and pronounced character of the glaciation of British Columbia. It is perhaps not too much to ask that in this matter purely negative shall not be put on an equality with positive evidence. Prof. Whitney's profound distrust of the "others" again appears where he qualifies a reference to my statements by the clause "even if his observations be accepted as entirely trustworthy." It is, however, so far satisfactory to find oneself in good company, for Dr. Hector, who has also had the misfortune to have had something to say about this region which does not conform to Prof. Whitney's hypotheses, is referred to as "evidently quite inexperienced," and one whose "statements must be received with some caution," while Dr. R. Brown for a similar sin is characterised as "an entirely unpractised observer."

GEORGE M. DAWSON

Geological Survey of Canada, Montreal, December 22, 1880

Lophiomys Imhausi

IN NATURE of January 1, 1880, I published a note on the "habitat" of that strange and excessively rare rodent *Lophiomys Imhausi*; it may interest many of your readers to know that I have recently received from Count Lodovico Marazzani a splendid specimen of that species from a new locality, viz. Erkauid, on the mountains between Suakin and Singat, where it was captured quite accidentally on April 12 last by a shot from a small revolver. It was also secured and preserved by mere chance, for it was found by a small terrier and killed at the bottom of a deep fissure in the granitic rocks, and its value was quite ignored by those who first handled it; thus the skeleton and viscera were lost, but happily the skin was in excellent condition, and the skull had been left attached. It is an adult female and has four teats, two pectoral or rather axillary, and two inguinal; it is rather larger than the fine specimen at Genoa, but does not differ in colour or richness of fur. The luxuriant dorsal mane to which this creature owes its name is separated from the long hairs of the body by a narrow stripe of short stiff greyish green bristles. The iris was dark brown, and the animal emanated no special odour.

This is the *fourth* specimen of *Lophiomys Imhausi* that has been secured to science; the first was the type specimen accidentally brought alive by M. Imhaus at Aden and described by Prof. A. Milne-Edwards: it is in the Paris Museum, skin, skeleton, and viscera preserved. The second is the skull accidentally picked up by Dr. Schweinfurth at Maman, north of Kassala, and described in 1867 by Prof. Peters as *Phractomys æthiopicus*; it is I believe at Berlin. The third was accidentally killed by a blow on the head with a stick in the Seriba of Beccari and Antinori at Keren in the Bogos country in 1870; the mounted skin and skeleton are in the Civic Museum at Genoa. The fourth is the subject of this note; its skin has been splendidly mounted by my able taxidermist Signor R. Magnelli, and it and the cranium form an important item of the Florence Zoological Museum. The natives told Count Marazzani that *Lophiomys* is rare, that it lives in deep holes in the strangely fissured rocks of that country, and that it is a vegetable feeder; the stomach of the specimen I have was much distended with leaves and young shoots when Count Marazzani skinned it.

The "habitat" of this species is now pretty well defined by lines drawn from Suakin to Maman and Kassala, and thence southward towards the Somali coast.

HENRY HILLYER GIGLIOLI

Reale Istituto, Florence

Parhelion

YESTERDAY a parhelion or mock sun was seen here. At 3h. 20m. I was at the Observatory, and the true sun was sinking in the south-west upon a somewhat dense cloud-bank with light and long cirro strati about and above it. The air was comparatively calm, the anemometer cups moving only occasionally and slowly. The horizon was foggy and misty. The spectral sun appeared as a bright diffused circular spot of light tinged with prismatic colours about 30° to the left (E.) of the true sun, and in a horizontal line with it.

I could trace a segment of a circle having the sun for its centre, for a few degrees above and below the mock image.

To the west I could not trace any false image or continuation of the circle. The phantom image slowly faded away in about ten minutes from its being first observed. The weather has been severe here (something over 200 feet above sea), but hardly so sharp as in some other (probably lower-lying) places. With Negretti and Zambra's standard minimum in cage four feet from the ground, 11° is the lowest I have registered.

During, however, the past seven days the maximum has only twice risen above freezing-point, and then but 1°.

Guildown, Guildford, January 21 J. RAND CAPRON

Girton and Newnham Colleges

SOME of your readers may perhaps be glad to help the natural science students of Girton and Newnham Colleges to raise about 800*l.*, needed for a physical and biological laboratory. The present provision for practical work is very inadequate, and the number of students has largely increased, while the required money is not forthcoming. I have already received the following donations, and shall gratefully acknowledge any further help:—Mr. Charles Darwin, 5*l.* 5*s.*; Mr. Edward Dormer, 5*l.*; Mr. T. Newland Allen, 3*l.* 3*s.*; Mr. William Fasseridge, 2*l.* 2*s.*; Anonymous, 2*l.*; Mr. Frank Dethridge, 1*l.* 1*s.*; Anonymous, 1*l.*; Mr. G. Eves, 1*l.*; Mrs. Eves, 1*l.*; Mr. R. Wilkinson, 1*l.* 1*s.*; Rev. C. T. Mayo, 1*l.* 1*s.*; smaller subscriptions, 4*l.* 15*s.* Any further particulars will be most willingly given.

FLORENCE EVES,
Science Student of Newnham College

Mitton House, Uxbridge, January 22

Minerva Ornaments at Troy *v.* Net-Sinkers

NOT having seen the numbers of NATURE regularly during the autumn, I did not observe Mr. Sayce's reply to my letter on the above subject until lately. I may perhaps trespass on your space with a few lines in reference to it.

I certainly did not observe any markings upon the stones in question that could be construed into any likeness to a human face or to that of an owl. Not having the opportunity of re-examining them I must take this as granted according to Dr. Schliemann's judgment. Of course an expert can see, and see with certainty, what to one less experienced seems quite invisible. At the same time an enthusiast, as we all know, is rather apt to "oversee," and find in his relics more than actually exists. I say this, as it is a common occurrence, and not in any way to disparage Dr. Schliemann's valuable work.

But admitting the existence of such outlines upon the stones in question is it not far more probable that the half-savage natives of the Troad may have taken advantage of certain suggestive lines and roughly outlined an image upon a net-sinker, than that they made so large a number of rough and uncouth things as likenesses of Minerva? The use of stones similarly chipped in the middle as net-sinkers seems common to savages all over the world, and it would seem to me therefore wiser to name them net-sinkers (with outlines, &c.) than to ticket them "Minerva ornaments."

One point, if I understand him aright, which Dr. Schliemann endeavours to prove, is that Ancient Troy stood close to the river. Hence the occurrence of net-sinkers may be considered as probable.

E. W. CLAYPOLE

Antioch College, Yellow Springs, O., December 18, 1880

THE PROVOST OF TRINITY COLLEGE, DUBLIN

THE Rev. Humphrey Lloyd, D.D., was born in 1800. He was the eldest son of the Rev. Bartholomew Lloyd, who was Provost of Trinity College, Dublin, from 1831 to 1837. Humphrey Lloyd entered his father's college in 1815, graduated as a Gold Medallist in Science in 1820, and was elected a Fellow in 1824. In 1831 he was appointed Professor of Natural Philosophy. He was co-opted a Senior Fellow in 1843, was made Vice-Provost in 1862, and was appointed by warrant from the Crown to the Provostship in 1867. He died, after a few days' illness, in the Provost's house on the 16th inst.

Full of years and honours, a very distinguished life has been brought to a close. Part of it was spent in laborious scientific research, part as the head of a great teaching establishment. Both portions of his life were a success, as even a short sketch of that life will show.

Lloyd was an excellent, though by no means a profound, mathematician. On becoming the Professor of Natural Philosophy he devoted himself with some ardour to the study of physical optics, and his report on this subject, laid before the fourth meeting of the British Association, was quite a masterpiece of reporting, and may still be consulted with pleasure. He was not however by any means content with having a knowledge of the work done by others, but was determined to enter on the field of original work himself; an opportunity soon offered. About 1832 Sir William Hamilton had been investigating the relations between the surface of wave-slowness and that of the wave, and thereby had been led to the discovery of some new geometrical properties of the latter. These properties he demonstrated by means of certain transformations of the equations of the wave-surface, and he showed that this surface had four conoidal cusps at the extremities of the lines of single ray-velocity, at each of which the wave is touched not by *two* planes as Fresnel supposed, but by an *infinite* number forming a tangent cone of the second degree; while, at the extremities of the lines of single wave-velocity, there were four circles of plane contact, in every point of each of which the wave-surface is touched by a single plane. These singular properties led Hamilton to anticipate two new laws of refraction called by him external and internal "conical refraction." Hamilton was naturally desirous of having his theoretical conclusions proved by experiment; such experiments required a wonderful patience, delicacy of touch, and an almost instinctive sagacity. As possessing all these he selected Lloyd to solve his problem; and by his labours in a short time the reality of this interesting phenomenon was established.

The memoir by Hamilton and the experimental researches by Lloyd appear in the same volume (xvii.) of the *Transactions* of the Royal Irish Academy.

Lloyd published several treatises and memoirs relating to optical science, but he was persuaded by Sir Edward Sabine to turn his attention, about 1836, to the subject of terrestrial magnetism. At his request the Board of Trinity College, Dublin, built a magnetical observatory, and the Professor entered with zeal upon those studies of magnetism which will for ever remain connected with his name. It would be unnecessary here to enumerate his very numerous writings on this subject.

In 1838 the British Association resolved that having regard to the high interest of the simultaneous magnetic observations which have been for some time carried on in Germany and various parts of Europe, and the important results to which these have led, they regard it as highly desirable that similar series of observations should be instituted in various parts of the British Dominions, and they suggested, as localities particularly important, Canada, Ceylon, St. Helena, Van Diemen's Land, and the Cape of Good Hope, also in the Southern Hemisphere. They further appointed as a Committee to approach the Government on this question Sir J. Herschel and Mr. Whewell, Dr. Peacock and Prof. Lloyd. The Committee, appointed late in August, at once set about their arduous work, and their memorial was laid before Lord Melbourne in the November following. The President and Council of the Royal Society strongly supported the memorial, and these concurrent representations were attended with full effect. In the Report of the Committee to the British Association in 1839 it is stated, "probably at the very moment when this report will be read, two ships, the *Erebus* and the *Terror*, under the command of Sir James Clark Ross, will be already on their voyage to the Antarctic Seas, carrying with them every instrument requisite for the complete and effectual prosecution of important magnetical researches in the high southern latitudes, and also complete establishments, both personal and instrumental, of the fixed magnetical observations to be placed at St. Helena, the Cape of Good Hope, and Van Diemen's Land. It was no wonder that the Committee were proud of the result of their labours, and that they acknowledged in strong terms the ample and liberal manner in which every demand on the national resources had been without exception granted, expressing at the same time the hope that this splendid example might be followed up by other nations. The report is signed J. F. W. Herschel and H. Lloyd.

In 1843 Dr. Lloyd pointed out a mode of reducing the error attending the determination of the intensity of the earth's magnetic force to less than one-fifth of that by the ordinary method.

In 1858 he again pointed out a fatal imperfection attending the ordinary mode of calculating the same force, and proposed instead a method requiring for its application only the use of the dip-circle, a vast advantage to the traveller, as it reduced to the smallest possible number the instruments which he would have to carry.

Along with his friend Sabine he visited the chief Continental cities in 1839, going as far as Berlin. This tour was altogether undertaken for the purposes of establishing still further a system of joint records of magnetical phenomena. His chief work in connection with magnetism was published under the title of "The Dublin Magnetical and Meteorological Observations" (2 vols. 4to, 1865-69). In 1857, when the British Association visited Dublin for a second time, Lloyd was their president, and many will still remember his dignified and courteous behaviour as such.

When, in 1867, Dr. Lloyd was appointed provost, there was scarcely one dissentient voice. He had distinguished himself in his college career; his researches had reflected lustre on his university, and the belief in him was never shaken. During his period of office as Senior Fellow the study of the experimental sciences was introduced into the curriculum; in 1851 it was even possible to graduate as a Gold Medallist in these. To the experimental sciences were at first joined the natural sciences. During his provostship, these two groups were separated, to the great encouragement of the students in both. It was something wonderful to find how the now aged provost kept pace with the time, encouraging in every way the more modern view of things. Among the Professors and Fellows of his college he was very popular; he was always affable, while he possessed a quiet dignity. Proudly conscious of the position he held as Provost of Trinity College, he was singularly unambitious of worldly honours, but the honorary degree of D.C.L. from the sister University of Oxford, conferred on him in 1856, was grateful to him, and he always spoke with pleasure of the recognition of his scientific merits by the Emperor of Germany, who conferred on him in 1874 the order "Pour le Mérite;" he was a F.R.SS. Lond. and Edin. He received the Cunningham gold medal of the Royal Irish Academy in 1862.

GEOLOGISING AT SHEPPEY

So much has been said about the abundance of fossil fruits at Sheppey that most geologists picture them lying plentifully upon the shore waiting to be picked up, and their only concern might well be at the outset to provide baskets strong and ample enough to convey their collectings home. A day spent upon the beach would dispel these preconceived ideas.

The cliffs in a wet season are streams of liquid mud alternating with freshly-fallen landslips rendering them practically unapproachable. The wet and frost have this year proved exceptionally disastrous, and mere shreds of coast-paths remain. In places slabs of freshly-ploughed land are arrested half-way down the cliff, and at one point a cabbage garden with the produce still only partly cut is streaming down to the beach. It is a good time for the cement works, but when Roman cement falls into disuse, as it seems likely to, then perhaps steps will be taken to stay this perpetual removal of fine arable land into the channels of the Thames. The beach itself is gravelly, and at low water there are extensive mud-flats. Among the gravel are patches of rolled pyrites, and among these pyrites the fruits are found, though valuable specimens are rare. This Christmas five experienced collectors, including Mr. W. H. Shrubsole, F.G.S., Mr. O. A. Shrubsole, F.G.S., Dr. Häusler, F.G.S., myself and brother, searched for several hours without a single fairly perfect fruit being found, and no greater success attended us on subsequent days. The vast bulk of the pyrites is amorphous; the majority of that which retains any recognisable shape is made up of twigs; a considerable percentage is of nearly obliterated casts of shells; and here and there are broken up Nipadites and other water-worn fragments of fruits. The best way to collect is to lie down upon the pyrites and examine it closely, when seeds and twigs that are passed over by the copperas-gatherers may be picked out. In this way I found seeds and scales of Araucaria, twigs of Ephedra, and many other shapes that may some day be recognised as parts of still-existing plants. No rest, short of doing absolutely nothing, could be more perfect to an overworked geologist's brain than to sprawl and smoke upon this beach.

The fruits themselves are so rare in the London Clay that they are seldom if ever found *in situ*, no prolific patches are known, and to attempt to dig for them would be futile. Their abundance in collections is due to the facts that for several miles there are lofty cliffs perpetually wasting away, and that the whole of the clay that reaches the beach is slowly removed in suspension by the sea, every particle of pyrites remaining behind until picked up for copperas or dissolved away. For two hundred years they have been known and searched for daily by the septaria and copperas collectors, and any one may quickly purchase an extensive collection. I have within a few months received from my friend Mr. Shrubsole enough Nipadites to fill a twenty-gallon cask, besides other fruits innumerable. Bowerbank's collection numbers many thousands, 300 specimens of a rare cone alone from Herne Bay having been in his possession. There is in the British Museum a MS. catalogue by a Mr. Crowe of Faversham, with 831 very rough drawings representing, as he supposed, 700 varieties. Ettingshausen, when he examined the British Museum collection, made 200 species. How many there may really be is still unknown, but the number doubtless is very considerably beyond the latter. Among the Coniferæ alone I have to add, besides the Ephedra, a *Podocarpus* near to *P. elata*, a *Frenella* almost indistinguishable from *F. Endlicheri*, and an *Araucaria* near *A. Cunninghami*. I have grave doubts about the correctness of the determination of all the other Coniferæ except a few of Bowerbank's Cupressineæ, and am still at work upon them. The state in which they are preserved is not sufficiently taken into account. The woody matter is generally preserved as lignite, and easily removed when rolled upon the beach, and the pyrites which remains filled the cavities between the more solid parts, as well as replacing the fruit itself. The densest and most salient part now is the purest pyrites, and was therefore at the time of fossilisation probably the most open part of the fruit or the filling in of cavities. The casts that are found are thus, in the case of hard-shelled fruits, more often casts of the space between the outer ligneous shell and the kernel, than of either the kernel or the shell itself. In the case, for instance, of an almond, we should have most frequently a smooth cast of the inside of the shell, but in perfect fruits the pitted exterior would be preserved, and in fruits partially dissolved the wrinkled kernel would show. In fruits with septa the variety of aspect presented in different stages of preservation is very great, and has doubtless led to the same species being catalogued under several names. The so-called Sequoia or Petrophiloides of Herne Bay is another instance, for the filling-in between the open scales of the cone was thought by Bowerbank to represent confluent scales inclosing cells, the supposed cells being really the cavities left by the true scales which have decayed away, while the infiltrated pyrites has enveloped the seeds which lay under them.

On Monday we took the 8 a.m. train to Herne Bay and searched at Swalecliffe for cones. At Whitstable we set sail in an oyster-boat for Shellness, but some delay occurred in getting it off the ground; the wind dropped in the meantime, and we had to row. Shellness was reached at dusk, and we experienced some difficulty in landing across the mud, which stretches a long way from shore at low water. We reached Warden Point at 6, and found that the fly we expected to meet us had driven home an hour before. The position of two mud-covered and complete strangers on a dark night on a most desolate spot, in drenching rain, eight miles from, and two hours late for dinner was not particularly enviable; yet a well-arranged excursion from Whitstable to Sheerness, *viâ* the singular shores of Shellness, would under pleasanter circumstances well repay any naturalist.

J. STARKIE GARDNER

THE CONSERVATOIRE DES ARTS ET MÉTIERS[1]

ONE of the most eminent English men of science said to us one day:—"You have at Paris collections, libraries, museums, observatories, faculties, schools; we have the equivalent of all that. There is only one thing we have not, which I always admire among you, and that is the Conservatoire des Arts et Métiers."

The National Conservatoire des Arts et Métiers[2] is, in fact, an establishment unique of its kind both in its scientific interest and practical utility. No institution is more worthy of the solicitude of the Government, since it has for its object the occupation of the workers and the instruction of the people. The Conservatoire is about to make a fresh start in consequence of the construction of a new block of buildings. There is even reason to hope that these works will only be the prelude of constructions still more important, and that very soon a law will insure the completion of our fine national establishment. The following are some of the improvements which have been recently introduced into the institution.

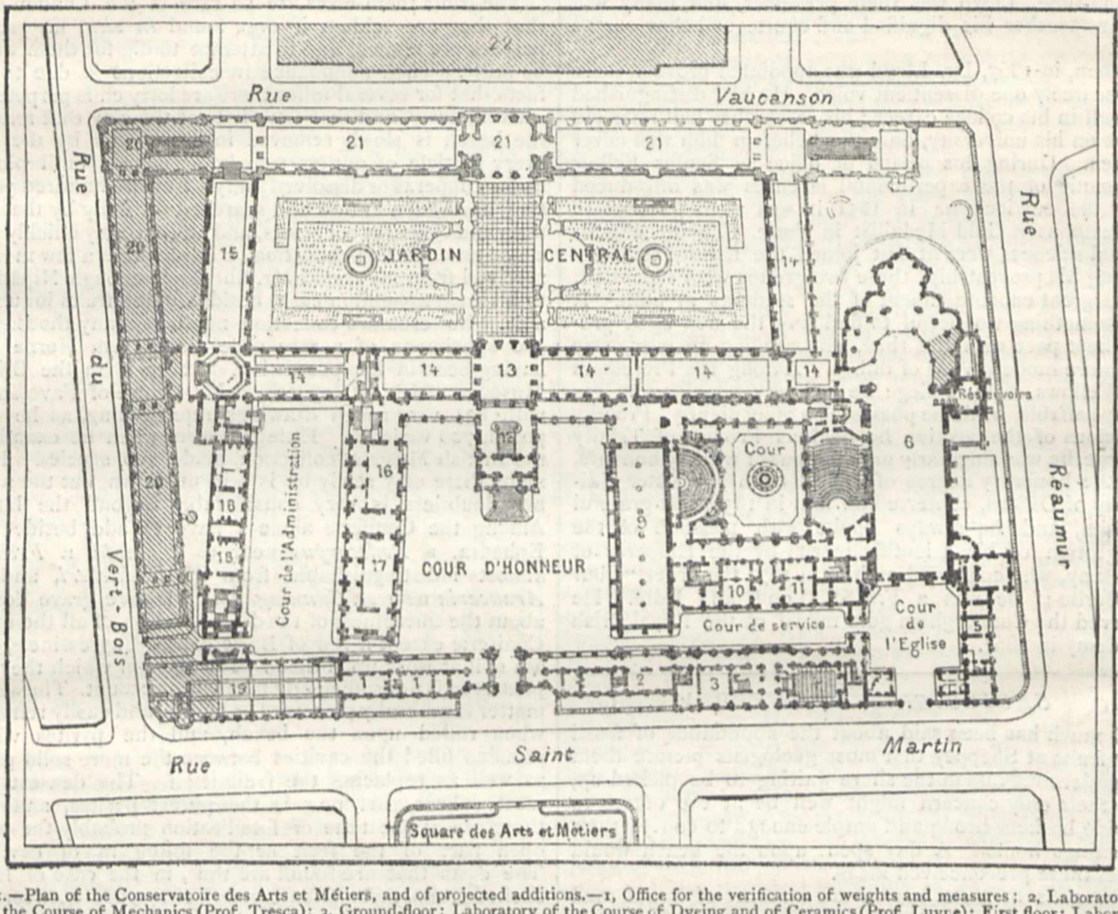

FIG. 1.—Plan of the Conservatoire des Arts et Métiers, and of projected additions.—1, Office for the verification of weights and measures; 2, Laboratory of the Course of Mechanics (Prof. Tresca); 3, Ground-floor: Laboratory of the Course of Dyeing and of Ceramics (Prof. Luyne); First floor: Laboratory of Agricultural Chemistry (Prof. Boussingault); 4, Ground-floor: Amphitheatre; First Floor: Physical Laboratory (Prof. Becquerel); 5, Provisional location of the Agronomic Institute; 6, Great Hall of Machinery in motion; 7, Great Amphitheatre; 8, Old Amphitheatre; 9, Library; 10, Laboratory of Industrial Chemistry (Prof. Girard); 11, Laboratory of General Chemistry (Prof. Peligot); 12, Great staircase; 13, The Echo Hall; 14, Galleries and Collections; 15, Ground-floor: Gallery in construction; First floor: Gallery of Ceramics and Optics; 16, Ground-floor: Weights and measures; First floor: Gallery of Spinning; 17, Exhibition Hall and Gallery of Spinning; 18, Administration and Gallery in construction; 19, Industrial drawings, patents, and trade-marks; 20, Projected construction, gallery of collections; 21, Projected construction; 22, Proposed location for the Central School of Arts and Manufactures.

The service of patents and of the industrial department has been recently installed in the new buildings in the rue St. Martin. Early in November there was placed at the service of the public the old and remarkable collection of Vaucanson's drawings. These drawings, which form a considerable series, comprised between the years 1775 and 1829, have a great historical interest. We find in them the germ of a considerable number of apparatus or of systems realised in our time, and which the want of processes of execution condemned to remain in the condition of projects. We see there a great number of curious objects, and notably the original drawing of Fulton's first steamer.

Among recent additions we may mention the great gallery of machinery (No. 6 in the accompanying plan, Fig. 1), to which is added the entire apse of the old

[1] From an article in *La Nature*, by M. Gaston Tissandier.

[2] Descartes had the idea of its foundation; Vaucanson formed the first germ of it by his public collection of machines, instruments and utensils, intended for the working classes; and the Convention decided on its definitive creation by a decree of 8 vendémiaire of the year xii.

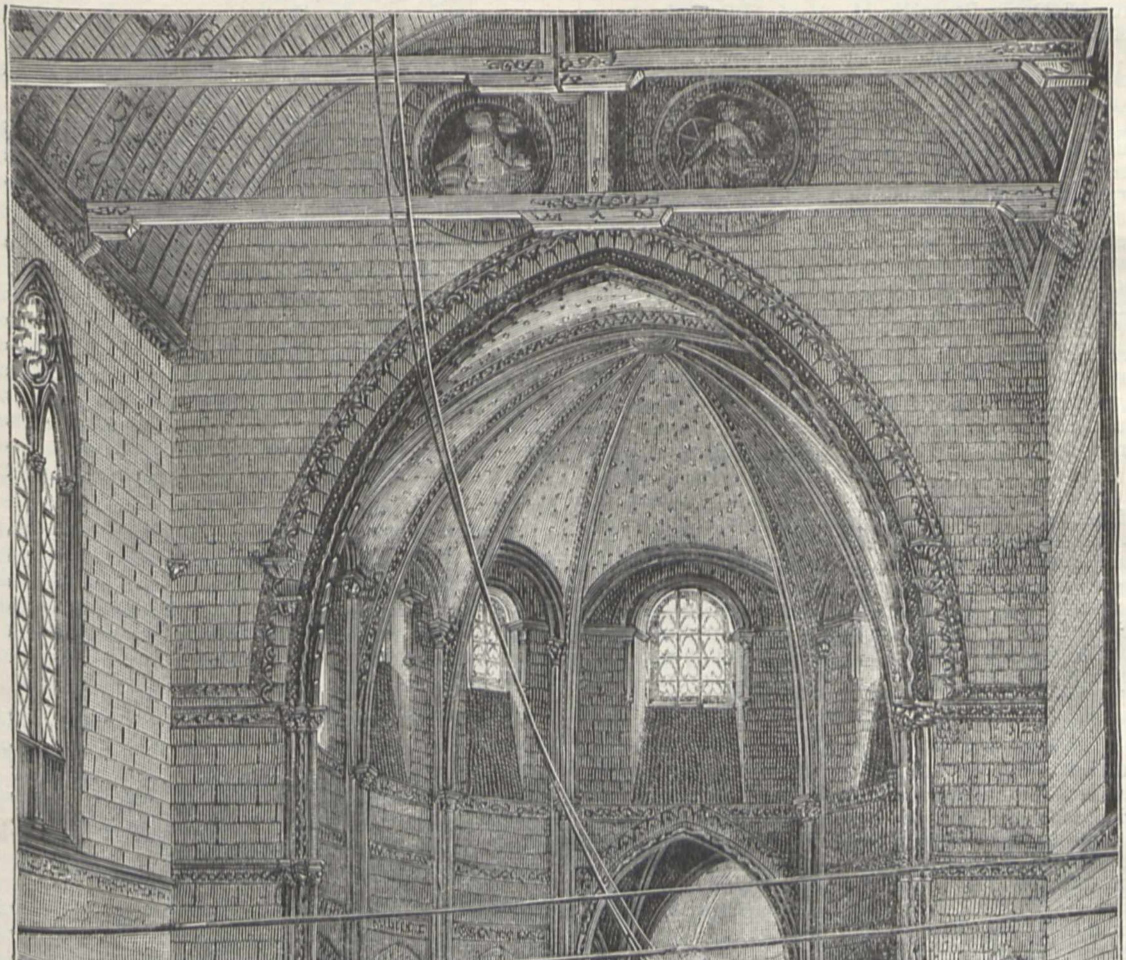

FIG. 2.—The Great Machinery Hall of the Conservatoire.

church of St. Martin's priory. We see here the curious steam-carriage of the mechanician Cugnot, and the fine statue of Denis Papin by M. Aimé Millot, the bronze duplicate of which was inaugurated at Blois some weeks ago. Besides the machinery which has long been at work in this gallery, the new administration of the Conservatoire is endeavouring to show visitors all the new and interesting apparatus used in the great Parisian industries. More than 3000 visitors witness every Sunday these experiments, very beautiful and very instructive for every one. Among the most notable apparatus are those connected with electrical phenomena. The beautiful experiments of M. Gaston Planté have obtained the greatest success, as also those relating to the transmission of power to a distance by electricity. The Conservatoire is thus becoming the museum of machinery in action.

While the machinery is thus at work in the great nave, other experiments are going on in the galleries. The great electrical machine throws off sparks in the physical hall, and projections by means of the oxyhydrogen light are made elsewhere by M. Molteni. Visitors show great interest in the Echo room, the Lavoisier room, in which is a great number of instruments used by the founder of modern chemistry, the Agricultural room, where are exhibited all the newest models of agricultural machinery. It is scarcely necessary to speak of the courses of lectures by eminent professors, many of whom are known beyond France; the gratuitous courses here and at the Sorbonne for 1880-81 comprise almost every branch of pure and applied science. The public library of more than 30,000 special works is freely placed at the disposal of workers.

Among the less known departments is the public service for testing the resistance of materials, very useful to architects, contractors, and builders. Any one may take advantage of it. It is sufficient to send to the Conservatoire specimens of stone, marble, pottery, metals, tubes, &c., which are crushed, broken, or bruised by special machinery, and the results accurately registered. The most powerful of these machines is a hydraulic press of 500,000 kilograms.

Such, in few words, is the Conservatoire des Arts et Métiers. By its collections, its public courses, its library, its eminently practical services, it may be regarded as one of the most valuable institutions of France.

NOTES

THE Faraday lecture will be delivered by Prof. Helmholtz in the theatre of the Royal Institution on Tuesday, April 5. The subject will be "The Modern Development of Faraday's Conception of Electricity." The lecture will be delivered in English.

PROF. HOLDEN, of the U.S. Naval Observatory, Washington, has published, through Scribner, a biography of Sir William Herschel. Prof. Holden is also publishing, through the Smithsonian Institute, a subject index and synopsis of the scientific writings of the great astronomer.

THE Kent's Cavern Committee, when presenting their *final* Report in August last to the British Association stated that, from the first day of the exploration in 1865 to its close in 1880, George Smerdon had been continually engaged on the work, and for nearly thirteen years had been the foreman; that during that period he had always discharged his duties in a most exemplary manner, and without the least misunderstanding with the superintendents; that he was nearly sixty years of age, and so crippled with chronic rheumatism—induced by working for so many years in the damp Cavern—as to be incapable of any ordinary labour, and that it was proposed to raise by subscription a fund sufficient to secure him a small annuity. The proposal was cordially received, and Mr. Pengelly was encouraged to carry it into effect. Several contributions have already been received from Mr. G. Busk, Prof. W. B. Dawkins, Dr. John Evans, Mr. J. E. Lee, Sir John Lubbock, Bart., M.P., F.R.S., Mr. W. Pengelly, Mr. E. Vivian, M.A., and others. Further contributions to the "Smerdon Testimonial Fund" may be paid directly to Mr. W. Pengelly, Lamorna, Torquay, or to Messrs. Vivian, Kitson, and Co., Bankers, Torquay.

A MARBLE statue of Nicephore Niépce, the inventor of photography, is now being executed by the celebrated sculptor, M. Guillaume of Paris, and will be erected and unveiled in May next at Châlons-sur-Saône.

PROF. MASKA of Neutitschein writes that the excavations now going on in the Schipka Cave, near Stramberg (Moravia), have yielded some interesting results. Among the numerous remains of Post-Tertiary animals (such as mammoth, rhinoceros, urochs, horse, lion, hyæna) the jaw-bone of a supposed diluvial human being has been found. It was imbedded in the immediate vicinity of a place where carbonised animal bones, stone implements, and bone utensils were found. The jaw-bone, described as having belonged to a child of some eight years of age (according to the development of the teeth), is of very large, indeed of colossal dimensions.

THE director of French Lighthouses has sent to the Minister of Public Works a communication recommending the lighting, by electricity, of all the great lighthouses on the French coasts. It will involve an expenditure of several millions of francs, which will end in a large economy and an extension of the range of illumination. A system of steam-trumpets is also to be established in connection with these improved lighthouses.

WITH the January number the *Quarterly Journal of Microscopical Science* enters on the twenty-first volume of its second series. First published in 1853, under the editorship of Dr. Edwin Lankester and Mr. George Busk, it now appears under the editorship of Prof. E. Ray Lankester, assisted by Mr. F. M. Balfour, Mr. W. T. Thiselton Dyer, and Dr. E. Klein. Mr. William Archer has withdrawn from the editorial staff.

THE minutes of the *Proceedings* of the Dublin Microscopical Club, which since 1865 have been published in the *Quarterly Magazine of Microscopical Science*, will for the future, we understand, be published in the *Annals and Magazine of Natural History*.

WE understand that Mr. Richard Anderson, the author of the well-known work on Lightning Conductors, has nearly ready for publication a treatise—based on the "Instruction sur les Paratonnerres adoptée par l'Académie des Sciences" of France—to be entitled "Information about Lightning Conductors."

AT its last session the French Parliament voted a grant of several millions of francs for the completion of an underground system of telegraphic wires connecting the principal cities with Paris.

SEVERAL electric railways are to be tried on the occasion of the forthcoming Electrical Exhibition at Paris. The most important will be built by Siemens Brothers, and will form consequently a prominent part of the British display. At the last sitting of the General Council of the Exhibition M. Georges Berger announced that a steam-engine of 800 horse-power will be arranged for the working of the electric light, and the number of lamps in operation is estimated at 600. A number of these will be in the large hall, but a large proportion in the gardens, in the *annexe*, and in a series of saloons fitted up magnificently with tapestry-work by the Government. The *annexe* is to be the Pavillon de la Ville de Paris, which was one of the wonders of the 1878 Exhibition, and will be transported to the vicinity of the Palais de Champs Elysées.

IN connection with our recent note on the Young Men's Home Education Society, a lady in Cork sends us some information concerning the Minerva Club, whose head-quarters seem to be in that city, and which aims at enabling ladies to educate themselves at home. The regulations of the Club seem well adapted for this purpose, and the programme includes natural science. The books recommended are all standard ones, and the ex-examiners men of good standing in literature and science. The specimens of the examination papers sent us in geology and geography show that a high standard is aimed at. The honorary secretary of the Club is Mrs. W. S. Green, The Rectory, Carrigaline, Co. Cork.

IN the course of dredging operations in the bed of the Limmat, at Zurich, some very interesting objects have been brought to light, among others ancient coins (including fifty gold pieces of Brabant), swords, and the skeleton of a stag of a species now extinct in Switzerland. The piers of a Roman bridge which once spanned the river have also been laid bare. All the finds are being placed in the Zurich Historical Museum.

A REPORT has reached Vienna, January 24, which has not yet been confirmed, of a fresh earthquake at Agram, attended with disastrous consequences. At Landeck (Tyrol) three shocks were noticed on January 10, at 9 p.m. The first one was the most violent, and the other two followed at intervals of five minutes. The earthquake which was felt at 5.15 p.m. on December 25, in Southern Russia, extended as far as Odessa and Kishineff in south-west, Tiraspol, Byeltzy, in Bessarabia, and the Ouman district of the province of Kieff in north-east; it was strong enough in the villages Leghezino and Vishnepolie of this district. At Molokishi, district of Balta, and at Byetzy, it was very strong.

A METEORIC stone fell at Wiener Neustadt a few days ago, near the telegraph office, and penetrated deeply into the gravel-covered road. The phenomenon was witnessed by several persons, who all declare that the meteor showed a brilliant light. Upon inspection a triangular hole was discovered of 5 centimetres width; the ground was frozen at the time. The meteoric stone was excavated in the presence of Dr. Schober, director of the Wiener Neustadt High School. It weighs 375 grammes, is triangular in shape, its exterior is crystalline, with curious blackish, greyish, and yellow-reddish patches. Here and there metallic parts give a brilliant lustre. Its specific weight is very high, its hardness about 9. An analysis is now being made.

THE second series of Evening Lectures delivered at the Royal College of Science, Dublin, has commenced with satisfactory entries. The classes have been voluntarily undertaken by the professors in order to afford a systematic course of study available to beginners and to those who are earning their livelihood in various avocations during the day. Artisans and others receiving weekly wages are admitted at half fees. The courses consist of from fifteen to twenty lectures in each of the following subjects: Biology, Dr. McNab; Physical Geography, Prof. O'Reilly; Geology and Mineralogy, Profs. Hull and O'Reilly; Chemistry, Prof. Hartley; Physics, Prof. Barrett; and Mathematics, Mr. Stewart, the Demonstrator in Physics. During the session of 1879-80 the numbers in attendance at the various classes were 336. In order to give assistance needful for the continuance of this course of instruction the Worshipful Company of Drapers have generously voted the sum of 100*l*. per annum for a period of three years. The earnestness, intelligence, and regularity of those attending the evening classes is remarkable, giving evidence of a hearty desire for sound and solid scientific instruction in Ireland, as well for the love of knowledge itself as for the purposes of technical information.

NEWS from Cairo states that to the north of Memphis, near Saggarah, two pyramids have been discovered which were constructed by kings of the sixth dynasty, and the rooms and passages of which are covered with thousands of inscriptions. The discovery is said to be of the greatest scientific importance.

THE Prefect of the Seine has opened in Paris a public laboratory for the analysis of any substance used for food; the fees are very moderate, and vary from 5 francs to 20 francs, according to the difficulty in the determinations.

A MEDICAL gymnasium was opened on January 22 at Paris. It has been built in the Chausée d'Antin at an expense of 20,000*l*., by a public company. About seventy mechanical contrivances of different descriptions have been arranged in a series of rooms. The greater number of these are worked by a steam-engine, and all of them can be graduated by screws, so that the extent, duration, and velocity of motion can be regulated according to the direction of the physicians.

THE electric steamer *Pouyer-Quertier*, belonging to MM. Siemens Brothers, has arrived at Havre, after having successfully repaired the French cable, which had been discovered to be faulty.

IN his last report to the Foreign Office H.M.'s Consul at Shanghai points out that the Chinese are much more disposed to allow the opening of coal-mines than the construction of railways. Without referring to the work being done in Formosa, he mentions that operations are in progress under English engineers for the opening of coal-pits at Kaiping, near Tientsin, and near Nganking, the capital of the Nganhwuy province. Both districts have plenty of coal, but unfortunately no navigable waterway, and for this reason the engineer of the Nganking coal-mines intends to remove to another locality near by, where there is an equal quantity of coal and better water-power. At Kaiping matters are worse, for the nearest navigable stream is at Lutai, forty miles away. To reach this it is expected that a railway may be constructed, but, as it can hardly be a commercial success, it would not much promote the cause of railway enterprise in general. The engineers find no difficulty at Kaiping in obtaining Chinese labour, but the English workmen sent there have not given satisfaction, and the Chinese are getting rid of them.

M. MAGITOT, a member of the Prehistoric Congress which met at Lisbon last autumn, reports on a Portuguese Pompeii, which he had occasion to inspect while on a tour to the territory of Tertiary Silex at Otta. The place is called Santarem and Citania. The latter is the general Portuguese name for ruins of ancient towns, which cover entire hills in the neighbourhood of Braga. The most important of these very old town-ruins is the Citania di Briteiros, which occupies nearly a kilometre square, and is supposed to be of Celtic origin. Circular walls, streets, squares, large architectural monuments, and even a number of houses have retained their typical forms. For twenty centuries this Citania was buried below *débris*, soil, and a rich vegetation; only a few years ago a zealous archæologist, Senhor Sarmento, succeeded, by costly and troublesome efforts, in clearing away the covering of centuries and to lay open to the world an ancient city in which quite a primitive state of civilisation is apparent. Its architecture and plastic ornamentation point to a somewhat advanced state of art and industry. Many stone monuments are covered with sculptures and inscriptions, which in their general character recall those of India and China, which the well-known Lyons archæologist, M. Guimet, declares to be of a symbolic and religious character, similar to those found upon the oriental monuments. It is possible that this fact might be adduced as a proof that the tribes who built these Citanias had originally emigrated from Turan.

NEWS from Washington territory states that the volcano Mount Baker was in full eruption quite recently

WE have received the first number of the *Revista* of the Society of Instruction of Oporto. There are various papers bearing on education, and one by Mr. E. J. Johnston on the Phanerogamous Flora of Oporto. The number of English names on the list of this Society is remarkable; the first name among the Foundation Members is that of Isaac Newton, followed by W. C. and A. W. Tait; there are several Allens, a Johnston, several Kendalls and Coverleys, F. C. Rawes, Henrique Rumsey, a Grant, a Hastings, and an Archer. This no doubt indicates the close commercial relations between Oporto and England.

IT is known that Leverrier, urged by growing infirmities and apprehending that he would not live to accomplish his great work on the theory of Saturn, left a part of his calculations uncompleted, convinced that this would exert no real influence on the total result. But M. Gaillot, the director of the Calculation Service of the Paris Observatory, felt it a duty to fill up the gap left by the late director of the observatory and to revise the whole of his work. We are happy to state that, as far as the revision has gone, the accuracy of the conclusions published by the great astronomer is demonstrated, and none of the neglected terms will exert any appreciable influence.

WE take the following from the *Albury Banner* (New South Wales):—It has long been a matter of popular belief that the great kingfisher was an enemy of the snake, perpetually warring upon the tribe in general, and never happier than when dining on serpent *au naturel*. It is not often, however, that even persons habitually residing in the bush have so good an opportunity as that afforded a few days since to Mr. Christian Westendorff of Jindera, for observing the laughing jackass when in the act of bagging the game referred to. Mr. Westendorff was engaged with another man in clearing some land, and in the course of the day's operations it became necessary to shift a large log. For this purpose levers were applied to each end, and after some straining the log was rolled from its resting-place. The very moment it commenced to move a laughing jackass, which had hitherto been taking a deep but unobtrusive interest in the proceedings, made a swoop down from the limb of an adjacent tree, and seized a large snake which had been lying under the log. The snake was gripped by the back of the neck (if snakes can be said to have necks) and borne away to the bird's previous perch, where the unfortunate reptile was banged against the bough until the body separated from the head and fell to the ground. The jackass then dropped the head, and seizing the body sailed away in triumph with his prize. Whether the bird had seen the snake go under the log and was watching for it to come forth again, or whether it knew by instinct that the reptile was there, is a question that may be left for naturalists to determine; but we are credibly informed that as soon as the log was shifted, and before Mr. Westendorff or his companion had any idea of a snake being in their neighbourhood, the jackass was down and had made good his seizure.

THE Russian Technical Society has created a special branch which will devote its attention to aëronautics, especially to the popularisation of all branches of aëronautics, to recent researches on this field, to the meteorology of the higher regions of the atmosphere, and to the study of the applications of aërostatics to military purposes.

WE note from the *Deutsche Industrie-Zeitung* that during 1879 some 140 tons of amber were obtained at the coast of the Baltic, of which the mine at Palmnicken yielded seventy-five tons, and the digging-engine at Schwarzort the remainder. About fifteen tons were gathered by nets and picked up on the shore. Some 3000 people (including women and children) gain their living by gathering amber.

AT the end of 1880 the Berlin Electro-technical Union numbered no less than 1575 members, 1246 of whom are foreigners.

A NUMBER of Celtic tombs were recently discovered near Lichtenwald, on the frontier between Styria and Carniola, not far from Cilli. Several of them were opened, and numerous urns were found in them. A few objects of more interest have been sent to the local museum at Cilli.

THE well-known Hungarian archæologist, Herr Wilhelm Lipp, continues the excavations of the ancient burial-ground discovered by him at Kessthely. The cost is borne by the Budapest Archæological Society. These tombs are rich in bronze and iron objects dating from the fourth and fifth centuries.

OUR ASTRONOMICAL COLUMN

BRORSEN'S COMET IN 1842.—In September, 1846, it was pointed out by Mr. Hind (*Astron. Nach.* No. 582) that the comet of short-period discovered by Brorsen at Kiel on February 26 preceding must have approached very near to the planet Jupiter about May 20, 1842, possibly within 0·05 of the earth's mean distance, and it was surmised that an entire change of orbit might have been produced at that time. In 1857 D'Arrest examined this point more closely, applying the formulæ of the *Mécanique Céleste* to determine the elements prior to the encounter with the planet. His results were published in *Astron. Nach.* No. 1087. Adopting good elements for 1846, but without taking account of perturbations, since the comet left the sphere of activity of Jupiter after the near approach, he inferred that the closest proximity occurred May 20·6924 Berlin mean time, the distance between the two bodies being then 0·05112; that for April 19·5 the inclination of the comet's orbit was 40° 51′, or 10° greater than in 1846, and that the perihelion distance was greater than 1·5, instead of 0·65 at the time of Brorsen's discovery, and it was considered that the comet would not be visible when the radius-vector was much over unity; hence, perhaps, our ignorance of its existence before the year 1846. Thus the question has remained until within the last two years. Our object now is to record the results of a much more complete investigation of the effect of the comet's encounter with Jupiter, by Herr Harzer, forming the subject of an inaugural dissertation in the University of Leipsic in 1878. He adopts the definitive elements of Prof. Bruhns for 1846, with a small correction to the mean motion indicated by the observations at the comet's reappearance in 1868, and calculates backward with great care the perturbations of Mercury, Venus, the Earth, Mars, Jupiter, and Saturn to 1842, July 16·5, when the distance from Jupiter was 0·305; the total perturbations in the interval 1846, February 25·5—1842, July 16·5 are as follows:—Mean anomaly, − 1° 58′ 32″·6; mean sidereal motion, + 4″·391; longitude of perihelion, + 9′ 52″·8; ascending node, + 24′ 35″·4; inclination, + 1° 48′ 31″·4; angle of eccentricity, + 56′ 30″·0. From the ecliptical co-ordinates of the comet with respect to Jupiter at the latter date and the variations of these relative co-ordinates, the hyperbolic elements of the orbit about the planet are obtained and the perijove is found to have taken place May 27·28488 M.T. at Berlin, when the distance was 0·054714. The hyperbolic elements are assumed to 1842, April 7·5, when the distance between comet and planet was 0·30334, and the radius of the sphere of attraction 0·27149. The elements are then again referred to the sun, and thus the following figures defining the comet's orbit before this near approach to Jupiter, result:—

Epoch, 1842, April 7·5 Berlin M.T.

	° ′ ″	
Mean anomaly	135 0 58·0	
Longitude of perihelion	111 50 20·6	1846·0
,, ,, ascending node ...	103 42 12·8	1846·0
Inclination...	46 18 57·4	1846·0
Angle of eccentricity	49 32 10·0	
Mean daily motion	686″·253	
Log. semi axis major	0·4756809	
Perihelion distance	0·7151810	

The only very striking difference from D'Arrest's figures, which were confessedly a rough approximation, is in the perihelion distance, which, as will be seen, is found to be much smaller, indeed not one-half as great, by Herr Harzer.

The author of this very able dissertation remarks upon the similarity of the elements he has deduced for Brorsen's comet before the near approach to Jupiter, to those of the first comet of 1798, discovered by Messier on April 12 and observed by him till May 24; this comet was computed by Burckhardt and Olbers. Herr Harzer finds, however, that it is not probable Messier's observations will admit of an orbit widely different from a parabola. He considers there is reason to conclude that the orbit of 1842 was impressed upon the comet by a close approach to Jupiter in 1759-60, and that another change may be similarly produced in 1937, as hinted by D'Arrest.

HERSCHEL'S FIRST OBSERVATION OF URANUS.—We are now close upon the centenary of the discovery of Uranus on March 13, 1781. Perhaps some readers may be interested in the following examination of the first evening's measures of distance and angle of position from a small star, with which Herschel compared the planet till March 21. In his "Account of a Comet," for as such the planet was announced in a communication to the Royal Society read on April 26, he gives the distance from the small star "2′ 48″ by pretty exact estimation true to 20″," and the angle "0° 0′ by superficial estimation, liable to an error of 10° or 12°," this angle corresponding in our present system of double-star measures to 270°, or preceding on the parallel: this distance and angle are for 10h. 30m. at Bath.

By Prof. Newcomb's pretty accurate "provisional theory," we find the place of Uranus for 1781, March 13, at 10h. 30m. M.T. at Bath, or 10h. 39m. 20s. Greenwich M.T., to be as follows:—True R.A. 5h. 35m. 47·77s., true Decl. +23° 32′ 58″·3, and the corrections to apparent place are −0·32s. and −0″·2; the log. distance of the planet from the earth being 1·27742. It is clear from this that Herschel's star, which he calls α, is No. 1576 in Rümker's Catalogue (the first impression of 0h.−6h.), where it is estimated a tenth magnitude: Argelander in the *Durchmusterung* has 9·0. The mean place accurately carried back to 1781·0 was—

R.A. 5h. 36m. 0·85s. Decl. + 23° 32′ 3″·7.

For this date and hour we find, in the notation of the *Nautical Almanac*—

Log A. −1·2709. Log B. + 0·3473. Log C. +7·7110.
Log D. −0·8971.

Whence the apparent place of the star was—

R.A. 5h. 36m. 0·84s. Decl. + 23° 32′ 11″·6.

Consequently the calculated distance of planet from star is 3′ 10″·2, and the angle of position 284°·2, agreeing as nearly with Herschel's estimates as under the circumstances can be expected. At his next observation on March 17 the distance by observation was 42″; the computed distance 54″.

GEOGRAPHICAL NOTES

THE last meeting of the Russian Geographical Society was very animated, owing to the presence of Prof. Nordenskjöld. The great hall of the Society was crowded, and the explorer of the northern seas was greeted with loud cheers. The president of the Society, M. Semenoff, opened the meeting with a speech of welcome in which he sketched the long series of expeditions undertaken from Europe to Siberia since the year 1553, when Willoughby directed his three ships to the White Sea, and paid for his undertaking with his life. Prof. Nordenskjöld replied in a short speech, referring to the expeditions which will start next spring for the exploration of the Siberian shores; and Prof. Lentz made a communication on Polar meteorological stations and on their importance for science.

ORENBURG was the first town which enjoyed the pleasure of hearing the story of Col. Prjevalsky's journey told by himself. On his passage through this town, on January 2, the traveller gave a lecture on the adventures of his journey to Tibet, which we have already told. From Orenburg M. Prjevalsky started to visit his relations at Smolensk, whence he proceeded to St. Petersburg, reaching it at the same time as his companions and his collections, which were at the beginning of January on their way from Orsk to Orenburg.

DR. LENZ, the German traveller who lately accomplished the feat of reaching Timbuctoo from the north, has arrived at Bordeaux, and is expected at Berlin soon to give an account of his explorations.

THERE are at present sixty-five geographical Societies in the world. The oldest of these is that of Paris, founded 1821; there is also a Society of Commercial Geography at Paris, founded 1873. Besides these France has geographical societies at Lyons, Bordeaux, Marseilles, Montpellier, Rouen, Nancy, Bergerac, Périgueux, Rochefort, Mont-de-Marsan, Agen, Apinal, Rochelle, Douai, Dunkirk, St. Omer, Lille; and one is about to be founded at Bar-le-Duc. The Berlin Geographical Society was founded in 1828, besides which Germany has similar societies at Frankfort, Darmstadt, Leipsic, Dresden, Munich, Bremen, Halle, Hamburg, Friberg, Metz, Hanover; other societies are in formation at Halberstadt, Magdeburg, and Jena. The London Geographical Society, the only one in England, was founded in 1830. The next oldest society (after Frankfort) is that of Rio Janeiro, founded 1838; then Mexico, 1839; St. Petersburg, 1845; in Russia there are besides—societies at Tiflis, Irkutsk, Vilna, Orenburg, Omsk. The other societies are those of the Hague, 1851; New York, 1852; Vienna, 1856; Geneva, 1858; Rome, 1867; Buda-Pesth, 1872; Amsterdam, 1873; Bucharest, 1875; Lisbon, 1875; Madrid, Antwerp, Brussels, Copenhagen, Lima, all 1876; Stockholm and Quebec, 1877; St. Gall, Berne, Oran, 1878; Tokio, 1879; Buenos Ayres, Algiers, and Oporto, 1880.

THE Hamburg firm of C. Woermann has sent Mr. Hermann Soyaux to the French colony of Gaboon in order to try to cultivate the coffee-tree of Liberia at that place. Soyaux has now been at Gaboon for two years, and has there established the Scibomge farm, which is situated about a day's march inland from the Gaboon River, on the Awandu River, which flows in a north-easterly direction into the Bay of Corisco. He now employs some 100 negroes. Many thousand coffee-trees have been imported from Liberia, and have been planted, and experiments have also been made with sowing the beans, so that at the beginning of 1882 the first coffee-harvest is confidently expected. The Hamburg firm supports the undertaking in a most efficient manner by sending engines, implements, &c., and experiments are also pending to introduce and acclimatise horses and mules. Mr. Soyaux makes meteorological observations for the Leipsic Observatory and natural history collections for the Hamburg Museum.

IN the current number of *Les Missions Catholiques* we find appended to a letter from Mgr. Cluzel, the Apostolic Delegate in Persia, some notes on the Kurds, which are just now of considerable interest. These notes deal with the origin of the Kurds, their country, language, present condition, religion, manners and customs, &c.

THOUGH no doubt much geographical information respecting the Philippines may be obtained from Spanish works, there is but little readily accessible to the English reader. It may therefore be well to call attention to a useful *résumé* furnished by H.M.'s Consul at Manila in his commercial report for 1879. He gives some brief particulars respecting each of the twenty-one provinces into which the principal Island of Luzon is divided, and afterwards deals with some of the other chief islands. The interior of the Island of Mindoro, immediately south of Luzon, he tells us, is not explored, but is supposed to contain much mineral wealth. In the Visayan group much of the interior of Negros, Samar, and Paragua is likewise not explored. Capt. Pauli adds that the archipelago is believed to contain 1200 islands of all sizes. The report is accompanied by an outline map, on which the principal islands are shown, as well as the division of Luzon into provinces.

M. LUCEREAU, a member of the Paris Geographical Society, has been killed by natives on his exploring expedition in Eastern Africa. He had started from Aden in June last in order to reach the Upper Nile by crossing the territory inhabited by the Gallas Negroes.

THE *Bulletin* of the International Geographical Institute at Berne has with the new year begun a new series on a larger scale than the previous issue. In the first number the contents of previous issues are resumed, the chief novelty being a fine map of the South Polar regions, on linen, in connection with the proposed Italian expedition. We cannot yet see exactly what place this *Bulletin* fills in geographical journalism.

DEEP-SEA EXPLORATION[1]

THIS subject is one in which I have for many years taken much interest; and I will give you the result of my experience and studies. It is highly fascinating to all persons of ordinary intelligence, although they may not be naturalists. Our best poets have not disdained to sing its praises; one of them says,

> "There is a magnet-like attraction in
> These waters to the imaginative power
> That links the viewless with the visible,
> And pictures things unseen."

Speculations of this kind were not unknown to the ancients. In the "Haleutica" of Oppian, written nearly seventeen centuries ago, it is stated that no one had found the bottom of the sea, and that the greatest depth ascertained by man was 300 fathoms, where Amphitrite had been seen. But this grand discovery does not seem to have satisfied the poetical philosopher; and he enters into a long disquisition as to the many other wonderful things that may be concealed in the recesses of the boundless ocean, adding, nevertheless, what I will translate from the Greek:—

> "But men have little sense and strength."

However, man has not degenerated in this kind of knowledge since the days of Oppian; for he has now not only explored the greatest depths of the sea, but has mapped out its main features with nearly as much accuracy as he has done with respect to the land.

It will be more convenient to divide the subject into separate heads, viz.:—(1) Historical; (2) Apparatus; (3) Fauna; (4) Food; (5) Light; (6) Temperature; (7) Depth; (8) Inequalities of the Sea-bottom; (9) Deposits; (10) Geological; (11) Incidental; (12) Concluding Remarks.

I hope you will not be frightened at the number of these heads. Some of them you will find to be exceedingly short.

1. *Historical.*—Sir Wyville Thomson's "Depths of the Sea" gives an excellent account of the origin and progress of deep-sea exploration up to a very recent period. To this work I would refer my audience, contenting myself with some supplemental remarks.

In 1868 commenced the systematic examination of the sea-bed at considerable depths in that part of the North Atlantic which surrounds the British Isles. I then took my yacht, the *Osprey*, for another excursion to Shetland, and dredged off the most northern point of our isles. The greatest depth which I attained was 170 fathoms, or 1020 feet, each fathom being 6 feet. This depth, strictly speaking, is beyond the line of soundings, viz. 100 fathoms: and it may be a question whether the fauna of the sea-bed outside of that limit can be regarded as British, although adjacent to our coasts. If it be we ought to take the "medium filum aquæ" (as the lawyers in the time of Coke called it), and extend the geographical limit of the British marine fauna half-way across to North America! But such boundaries are neither national nor rational. We cannot lay claim to so extensive a dominion. International boundaries, for the purpose of naval warfare or as defined by fishery treaties, are limited to a distance of three miles, irrespective of depth. Later in the same year (1868) Dr. Carpenter and Prof. Wyville Thomson explored, in H.M. surveying-vessel *Lightning*, the sea-bed lying between the Butt of Lewis and the Faröe Isles, and reached the depth of 550 fathoms. These tentative excursions showed that the sea-bed everywhere was full of life, not merely of a microscopic and uniform kind, and of a low degree of organisation, but of a considerable size, great variety, and a high degree of organisation. In the following year (1869) our Government placed a better vessel at the disposal of the Royal Society; and I undertook the first scientific cruise in H.M. surveying-ship *Porcupine*. This cruise was off the western coast of Ireland, and the greatest depth dredged was 1476 fathoms. The second cruise was undertaken by Prof. Wyville Thomson, and extended from the south of Ireland to what is probably the deepest part of the North Atlantic in the European seas. The greatest depth dredged by him was 2435 fathoms, or nearly three miles. The third cruise, under the charge of Dr. Carpenter, was in the same direction as the *Lightning* expedition, but embraced a larger area, including the Shetland Isles; the greatest depth was 867 fathoms. In the following year (1870) the *Porcupine* was again placed at the disposal of the Royal Society for further exploration. This expedition was divided into two cruises, North Atlantic and Mediterranean. The former was assigned to me, and comprised the sea-bed lying between Falmouth and the Straits of Gibraltar, along the western coasts of Spain and Portugal. There were 38 dredging and sounding stations, at depths ranging from 81 to 1095 fathoms. The Mediterranean cruise was made by Dr. Carpenter, and extended round Sicily. There were 29 stations, at depths ranging from 51 to 1743 fathoms. Prof. Wyville Thomson was unfortunately prevented by illness from taking part in this year's expedition. In all these cruises an abundance as well as a great variety of marine life occurred at every depth.

The *Lightning* and *Porcupine* expeditions culminated in the celebrated voyage of H.M.S. *Challenger* round the world, which commenced on December 21, 1872, and ended on May 24, 1876, having thus occupied a period of three years and five months. During this expedition about 30,000 nautical miles were traversed, 504 soundings were taken, and 132 dredgings and 150 trawlings were made. The depths of soundings were from 25 to 4475, of dredgings from 4 to 3875, and of trawlings from 10 to 3050 fathoms. The greatest depth reached was five statute miles. The Americans have recorded a greater depth, viz. 5¼ miles, or 4620 fathoms. Even greater depths than this have been given; but they are not now considered reliable, by reason of the imperfect machinery which was formerly used for sounding.

The *Proceedings* of the Royal Society for 1873-1877 contain many "Preliminary Reports" by Sir Wyville Thomson and the other naturalists attached to the *Challenger* expedition; so that all the scientific world were from time to time kept informed of the progress and results of this great national undertaking.

During the last of our arctic voyages, in 1875, I had, through the influence and energy of the Royal Society, another opportunity of exploring a part of the North-Atlantic sea-bed which was not within the limits of the *Challenger* expedition; and I was intrusted with the scientific charge of the sounding and dredging conducted in H.M.S. *Valorous* between Bantry Bay and Hare Island in Davis Strait. This ship accompanied the *Alert* and *Discovery* on their way northwards. After a voyage of three months, which was rendered more eventful by a cyclonical storm and a partial shipwreck on the coast of Greenland, we succeeded in working sixteen stations, with depths of from 20 to 1785 fathoms. Here also, and even in the midst of icebergs, submarine life showed no diminution in number or extent.

To this short recital of our later expeditions I must not omit to add a notice of the valuable and suggestive researches which were accomplished under considerable difficulties by Dr. Wallich in H.M.S. *Bulldog* in 1860, while she was engaged in surveying the North-Atlantic sea-bed for the purpose of establishing telegraphic communication between this country and North America. The results of these researches were published in Dr. Wallich's important work, entitled "The North-Atlantic Sea-bed; comprising a Diary of the Voyage on board H.M.S. *Bulldog* in 1860, and observations on the presence of Animal Life, and the Formation and Nature of Organic Deposits at Great Depths in the Ocean." On the return voyage, about midway between Cape Farewell and Rockall, thirteen starfishes came up from a sounding of 1260 fathoms, "convulsively embracing a portion of the sounding-line which had been payed out in excess of the already ascertained depth, and rested for a sufficient period at the bottom to permit of their attaching themselves to it."

A short voyage in H.M.S. *Shearwater* through the Mediterranean in 1871 enabled Dr. Carpenter to have some dredging between Sicily and the northern coast of Africa, on the Adventure and Skerki Banks. This dredging was by no means unproductive; but the depths did not exceed 200 fathoms, which we are now inclined to call "shallow water"; Dr. Carpenter's word was "shallows." Fifty years ago such depths would have been regarded by naturalists as peculiarly "abyssal"!

The elaborate report of my lamented friend Prof. Edward Forbes, on the investigation of British Marine Zoology by means of the dredge, which he submitted to the British Association for the Advancement of Science in 1850, and to which I contributed as a humble fellow worker, was preceded by his equally valuable "Report on the Mollusca and Radiata of the Ægean Sea, and on their Distribution, considered as bearing on Geology." The last-mentioned Report was published by the Association in 1844. Forbes's conclusion that the sea-bottom at a depth of 300 fathoms is lifeless, because he found that life diminished gradually, and almost ceased when he dredged at 230 fathoms, has certainly been proved to be inaccurate as regards

[1] A Lecture by J. Gwyn Jeffreys, LL.D., F.R.S.

the ocean in general; but Dr. Carpenter, in his Report to the Royal Society on his biological researches in the Mediterranean during the *Shearwater* cruise, expresses his belief that "in the Mediterranean basin the existence of animal life in any abundance at a depth greater than 200 fathoms will be found quite exceptional"; and he infers "that Edward Forbes was quite justified in the conclusion he drew *as regards the particular locality he had investigated*, and that his only mistake lay in supposing that the same conditions would prevail in the open ocean." But this eminent naturalist and physiologist, Dr. Carpenter, to whose opinions on such subjects all respect is due, admits that "the history of science is full of instances in which erroneous doctrines have been more *productive*, because more suggestive, than well-determined facts that open no access to the unknown beyond." With the greatest deference to Dr. Carpenter's opinion that animal life is scanty in the depths of the Mediterranean, I venture to point out that very little had previously been done to investigate the fauna of that sea beyond the shores and shallow water to the extent which Forbes reached, viz. 230 fathoms.

Admiral Spratt in 1846 dredged, at a depth of 310 fathoms, 40 miles east of Malta, a number of living Mollusca, which I examined and found to be identical with species which I dredged at considerable depths in the North Atlantic during the *Porcupine* expeditions. Again, during the Mediterranean cruise of 1870 in the *Porcupine*, no fewer than 14 species of Mollusca (also Atlantic), besides a pelagic or surface-water species and a small freshwater shell, which must have been carried out to sea by some river or stream, occurred at a depth of 1415 fathoms, between the coasts of North Africa and Spain. All these species were recent, and some were living, although most of them were known to me as also belonging to the Pliocene formation in Sicily. However, we shall in all probability know a great deal more of this matter if our good neighbours the French are able to carry out their idea of extending their investigation of the deep sea near their own coasts by another dredging and sounding cruise off Marseilles or Toulon.

During the early part of the summer in the present year (1880) our Admiralty placed at the disposal of Sir Wyville Thomson H.M. surveying-vessel *Knight Errant* for a cruise off the Butt of Lewis, in prosecution of his researches in the *Lightning* Expedition as to the "warm" and "cold" areas which were noticed in the Report of that expedition. Mr. Murray took the scientific charge of the cruise; but the weather was boisterous, and unfavourable for dredging and trawling. There were, however, some zoological results of an interesting kind, especially as regards the Mollusca; and it is hoped that the application which has now been made by the Royal Society for another Government vessel will be successful, and will enable Sir Wyville to continue the work and make further discoveries.

Although we have of late years done a great deal to promote submarine researches, as shown by the expeditions of H.M.SS. *Bulldog*, *Lightning*, *Porcupine*, *Shearwater*, *Valorous*, and *Knight Errant*, our comparatively poor neighbours in Scandinavia have been earlier in the field and not less energetic. From the "Notices sur la Suède," published on the occasion of the International Congress of Geographical Sciences in 1875 at Paris, it appears that between the years 1837 and 1875 seventeen scientific expeditions were made from Sweden, fifteen of which explored the Arctic regions. Professors Lovén, Torell, and Nordenskjöld, with other distinguished naturalists, took an active part in these expeditions. The sister kingdom of Norway has since engaged in the same course of discovery; and a well-equipped Government vessel, the *Vöringen*, of the same size as the *Porcupine* (about 400 tons), left Bergen in the beginning of June, 1876. Dr. Danielssen, Professors Mohn and G. O. Sars, Herr Friele, and other scientific men accompanied the vessel, and were engaged in the zoological and physical work. Through the kindness of my friend Prof. Sars I am enabled to give the following particulars of these Norwegian expeditions:—They occupied nearly three months in each of the years 1876, 1877, and 1878. The first expedition was divided into three cruises, and extended along the western coast of Norway to the Faröe Isles and Iceland. There were 24 dredging-stations, at depths of from 90 to 1862 fathoms, besides 5 shore stations in Norway, Faröe, and Iceland. The second expedition was divided into four cruises, and extended from Bergen to outside the Loffoden Isles, and from Tromsö to Jan Mayen; there were 28 stations, with depths of from 70 to 1760 fathoms, besides 6 shore stations in Norway and Jan Mayen. The third expedition was divided into three cruises, and extended to Vardö, and thence westward to Beeren Island, and afterwards to Spitzbergen in 80° N. lat. The last expedition had 36 stations, with depths of from 21 to 1686 fathoms, besides 7 shore stations on the Arctic coasts of Norway, and in Beeren Island and Spitzbergen.

The United States have prosecuted this kind of research with their well-known activity and perseverance. From 1867 to the autumn of 1880 four Government steamers have been continuously employed in surveying the seas which border the coasts of Central and South America. Several hundred stations were investigated, at depths ranging from 6 to 2412 fathoms. Count Pourtales, Prof. Agassiz, and his no less eminent son, have been successively in charge of the scientific department. The results are both extensive and invaluable. In 1871 I was invited by the late Prof. Agassiz to pay him a visit and examine the Mollusca which had been procured during the previous years. The collection was in the custody of the late Prof. Stimpson at Chicago. It was extremely interesting to me, in connection with the expeditions of the *Lightning* and *Porcupine*. I examined the collection in the Museum at Chicago; and, at the request of Prof. Agassiz, I took home with me several of the shells for comparison with my own. On my return to England, after enjoying the kind hospitality of my scientific friends in the United States and Canada, I learnt that Chicago had been utterly burnt down; and I was fortunately enabled to restore the shells, which were the only specimens of natural history that had been saved from the fire. Through the kindness of Prof. Spencer Baird, I had, during this visit to America, an opportunity of joining in a dredging-excursion on the coast of New England, which was conducted under the auspices of the Fishery Commission.

Like a giant refreshed, France has awakened from a rather long sleep, and, with its accustomed spirit, has now rivalled all other nations in deep-sea work. Last summer a scientific commission was appointed, with the venerable Prof. Milne-Edwards as its president; and a large and well-equipped Government steamer, the *Travailleur*, explored the Bay of Biscay with most favourable results. I was obligingly asked to take part in this expedition; and I gave an account of it at the last meeting of the British Association at Swansea, which is published in the Report of that meeting.

Austria, Germany, and Holland have also not been last in the race of maritime voyages, although they have not contributed much to our knowledge of deep-sea life.

The harvest reaped in all the above-mentioned expeditions was most abundant and valuable.

But after all it must be borne in mind that if every civilised nation in the world were every year, during the next century, to send out similar expeditions, with improved appliances, for exploring the sea-bed, the field would be far from being exhausted. Every such expedition must be more or less tentative, and can only form the basis for a more complete investigation of "the deep bosom of the ocean." The area of investigation must be measured by many millions of square leagues; whereas all that has hitherto been effected has been to scrape in an imperfect manner the surface of a few scores of acres.

I here exhibit charts to show the tracks of the expeditions in which I have been personally engaged, as well as those of the *Challenger* and Norwegian expeditions.

2. *Apparatus.*—The sounding-line, ropes, dredge, trawl, tangles, towing-net, sieves, accumulators, steam-engines, and other contrivances for deep-sea exploration have been so fully described and illustrated in the "Depths of the Sea" and Capt. Sigsbee's "Deep-sea Sounding and Dredging," that it is unnecessary for me to do more than mention those books. The latest improvements consist in the substitution of steel-wire for line in sounding, and of galvanised wire-rope for hempen rope in dredging and trawling. Capt. Sigsbee's new towing-net for ascertaining whether floating or swimming animals are found in any zone or belt of water lying between the surface and the bottom will be hereafter noticed. It is still a desideratum to invent a dredge for deep-sea work which shall scrape the surface instead of sinking into the ooze or mud.

3. *Fauna.*—This word is used by naturalists to denote animal life in contradistinction to "Flora" or vegetable life. All the recent exploring expeditions have established the fact that animal life of various kinds abounds everywhere in the deepest parts of the ocean. Nor is such life microscopic or minute only. In the *Challenger* voyage was procured by the trawl, at the depth

of 1600 fathoms, in the South Atlantic (S. lat. 46° 16′, E. long. 48° 27′), a living specimen of a magnificent shell belonging to *Cymbium*, or an allied genus, which is 6¾ inches long and 4 inches broad! I dredged other Mollusca from an inch and a half to nearly double that length in the *Porcupine* and *Valorous* expeditions. Willemöes Suhm mentions among the *Challenger* discoveries a gigantic crustacean or sea-spider from 1375 fathoms, which measured nearly two feet across the legs.

Sir Wyville Thomson gives an eloquent description of life in the deep sea, when he says that the latter "is inhabited by a fauna more rich and varied on account of the enormous extent of the area, and with the organisms in many cases apparently even more elaborately and delicately formed, and more exquisitely beautiful in their soft shades of colouring, and in the rainbow tints of their wonderful phosphorescence, than the fauna of the well-known belt of shallow water teeming with innumerable invertebrate forms which fringes the land. And the forms of these hitherto unknown living beings, and their mode of life, and their relations to other organisms whether living or extinct, and the phenomena and laws of their geographical distribution, must be worked out."

It was formerly supposed that animals could not exist at great depths because of the great pressure to which they were subjected. Mr. Moseley says[1] "the pressure exerted by the water at great depths is enormous, and almost beyond comprehension. It amounts roughly to a ton weight on the square inch for every 1000 fathoms of depth; so that, at the depth of 2500 fathoms, there is a pressure of two tons and a half per square inch of surface, which may be contrasted with the fifteen pounds per square inch pressure to which we are accustomed at the level of the sea." But it must be recollected that water is nearly incompressible, and that marine animals which are surrounded by such a fluid, and are to a certain extent filled with it, would not necessarily be inconvenienced by the superincumbent weight.

Animals from great or even from what may be considered moderate depths are always brought up dead, the cause of death being unknown. This is another problem worthy of being worked out.

The migration or distribution of marine animals throughout the open sea is quite free, and is unobstructed only by great or abrupt changes of level in the bed of the ocean, which operate as barriers. Even animals of a fixed or sedentary nature in their earliest state of growth swim on the surface, and are therefore unchecked in their onward course by any submarine barrier.

The doubt whether any life exists in the intermediate space or zone which lies between that of the surface and that of the bottom of the deep sea has now, I believe, been set at rest. The naturalists in the *Josephine* expedition believed that this intermediate zone is lifeless; and Sir Wyville Thomson seems to have been of the same opinion. The towing-net adopted by Mr. Murray in the *Challenger* expedition for such researches was to some extent successful; but Capt. Sigsbee, of the U.S. Coast-Survey steamer *Blake*, invented a cylinder or machine, called the "gravitating trap," which completely answered the purpose of collecting at any particular depth the animals which occurred there. Prof. Alexander Agassiz, in his communication to the Superintendent of the Survey made last August, and now published, records the experiments thus made, and says that they "appear to prove conclusively that the surface-fauna of the sea is really limited to a comparatively narrow belt in depth, and that there is no intermediate belt, so to speak, of animal life between those living on the bottom, or close to it, and the surface pelagic fauna."

I am not aware that any deep-sea animals adopt or avail themselves of the same means that oceanic or land animals use for purposes of protection and concealment, chiefly by coloration or by what has been termed "mimicry." Many cases of this kind are known to occur in birds, fishes, mollusks, *Salpæ*, insects, crabs, shrimps, and worms.

None of the animals whose remains are found in geological formations older than the Pliocene or latest of the Tertiary strata have yet been detected in any exploring expedition. The late Prof. Agassiz and Sir Wyville Thomson were disappointed in their enthusiastic expectation of finding Ammonites, Belemnites, and other Old-World fossils in a living state. I have dredged Miocene fossils on the coasts of Guernsey and Portugal, the latter at considerable depths; but they were petrifactions, and must have come from some fossiliferous formation in the adjacent land, or perhaps in the sea-bed.

[1] "Notes of a Naturalist on the *Challenger*."

Sir Wyville Thomson, in his "Report of the Scientific Results of the Voyage of H.M.S. *Challenger*," has expressed his opinion as to the doctrine of evolution, that "in this, as in all cases in which it has been possible to bring the question, however remotely, to the test of observation, the character of the abyssal fauna refuses to give the least support to the theory which refers the evolution of species to extreme variation guided only by natural selection." I cannot understand how either "natural selection" or "sexual selection" can affect marine invertebrate animals, which have no occasion to struggle for their existence and have no distinction of sex.

(*To be continued.*)

THE RELATION BETWEEN ELECTRICITY AND LIGHT.[1]

EVER since the subject on which I have the honour to speak to you to-night was arranged, I have been astonished at my own audacity in proposing to deal in the course of sixty minutes with a subject so gigantic and so profound that a course of sixty lectures would be quite inadequate for its thorough and exhaustive treatment.

I must indeed confine myself carefully to some few of the typical and most salient points in the relation between electricity and light, and I must economise time by plunging at once into the middle of the matter without further preliminaries.

Now when a person is setting off to discuss the relation between electricity and light it is very natural and very proper to pull him up short with the two questions: What do you mean by electricity? and What do you mean by light? These two questions I intend to try briefly to answer. And here let me observe that in answering these fundamental questions I do not necessarily assume a fundamental ignorance on your part of these two agents, but rather the contrary; and must beg you to remember that if I repeat well-known and simple experiments before you, it is for the purpose of directing attention to their real meaning and significance, not to their obvious and superficial characteristics: in the same way that I might repeat the exceedingly familiar experiment of dropping a stone to the earth if we were going to define what we meant by gravitation.

Now then we will ask first, What is Electricity? and the simple answer must be, We don't know. Well, but this need not necessarily be depressing. If the same question were asked about Matter, or about Energy, we should have likewise to reply, No one knows.

But then the term Matter is a very general one, and so is the term Energy. They are heads, in fact, under which we classify more special phenomena.

Thus if we were asked what is sulphur, or what is selenium, we should at least be able to reply, A form of matter; and then proceed to describe its properties, *i.e.* how it affected our bodies and other bodies.

Again, to the question, What is heat? we can reply, A form of energy; and proceed to describe the peculiarities which distinguish it from other forms of energy.

But to the question, What is electricity? we have no answer pat like this. We cannot assert that it is a form of matter, neither can we deny it; on the other hand, we certainly cannot assert that it is a form of energy, and I should be disposed to deny it. It may be that electricity is an entity *per se*, just as matter is an entity *per se*.

Nevertheless I can tell you what I mean by electricity by appealing to its known behaviour.

Here is a battery, that is, an electricity pump: it will drive electricity along. Prof. Ayrton is going, I am afraid, to tell you, on the 20th of January next, that it *produces* electricity; but if he does, I hope you will remember that that is exactly what neither it nor anything else can do. It is as impossible to generate electricity in the sense I am trying to give the word, as it is to produce matter. Of course I need hardly say that Prof. Ayrton knows this perfectly well; it is merely a question of words, *i.e.* of what you understand by the word electricity.

I want you then to regard this battery and all electrical machines and batteries as kinds of electricity pumps, which drive the electricity along through the wire very much as a water-pump can drive water along pipes. While this is going on the wire manifests a whole series of properties, which are called the properties of the current.

[1] A lecture by Dr. O. J. Lodge, delivered at the London Institution on December 16, 1880.

[Here were shown an ignited platinum wire, the electric arc between two carbons, an electric machine spark, an induction-coil spark, and a vacuum tube glow. Also a large nail was magnetised by being wrapped in the current, and two helices were suspended and seen to direct and attract each other.]

To make a magnet, then, we only need a current of electricity flowing round and round in a whirl. A vortex or whirlpool of electricity is in fact a magnet; and *vice versâ*. And these whirls have the power of directing and attracting other previously existing whirls according to certain laws, called the laws of magnetism. And, moreover, they have the power of exciting fresh whirls in neighbouring conductors, and of repelling them according to the laws of diamagnetism. The theory of the actions is known; though the nature of the whirls, as of the simple stream of electricity, is at present unknown.

[Here was shown a large electro-magnet and an induction-coil vacuum discharge spinning round and round when placed in its field.]

So much for what happens when electricity is made to travel along conductors, *i.e.* when it travels along like a stream of water in a pipe, or spins round and round like a whirlpool.

But there is another set of phenomena, usually regarded as distinct, and of another order, but which are not so distinct as they appear, which manifest themselves when you join the pump to a piece of glass or any non-conductor and try to force the electricity through that. You succeed in driving some through, but the flow is no longer like that of water in an open pipe; it is as if the pipe were completely obstructed by a number of elastic partitions, or diaphragms. The water cannot move without straining and bending these diaphragms, and if you allow it, these strained partitions will recover themselves and drive the water back again. [Here was explained the process of charging a Leyden jar.] The essential thing to remember is that we may have electrical energy in two forms, the static and the kinetic; and it is therefore also possible to have the rapid alternation from one of these forms to the other, called vibration.

Now we will pass to the second question: What do you mean by light? And the first and obvious answer is, Everybody knows. And everybody that is not blind does know to a certain extent. We have a special sense-organ for appreciating light, whereas we have none for electricity. Nevertheless, we must admit that we really know very little about the intimate nature of light—very little more than about electricity. But we do know this, that light is a form of energy; and, moreover, that it is energy rapidly alternating between the static and the kinetic forms —that it is, in fact, a special kind of energy of vibration. We are absolutely certain that light is a periodic disturbance in some medium, periodic both in space and time: that is to say, the same appearances regularly recur at certain equal intervals of distance at the same time, and also present themselves at equal intervals of time at the same place; that in fact it belongs to the class of motions called by mathematicians undulatory or wave motions. The wave motion in this model (Powell's wave apparatus) results from the simple up-and-down motion popularly associated with the term *wave*. But when a mathematician calls a thing a wave he means that the disturbance is represented by a certain general type of formula, not that it is an up-and-down motion, or that it looks at all like those things on the top of the sea. The motion of the surface of the sea falls within that formula, and hence is a special variety of wave motion, and the term wave has acquired in popular use this signification and nothing else. So that when one speaks ordinarily of a wave or undulatory motion one immediately thinks of something heaving up and down, or even perhaps of something breaking on the shore. But when we assert that the form of energy called light is *undulatory*, we by no means intend to assert that anything whatever is moving up and down, or that the motion, if we could see it, would be anything at all like what we are accustomed to in the ocean. The kind of motion is unknown; we are not even sure that there is anything like motion in the ordinary sense of the word at all.

Now how much connection between electricity and light have we perceived in this glance into their natures? Not much truly. It amounts to about this: That on the one hand electrical energy may exist in either of two forms—the static form, when insulators are electrically strained by having had electricity driven partially through them (as in the Leyden jar), which strain is a form of energy because of the tendency to discharge and do work; and the kinetic form, where electricity is moving bodily along through conductors or whirling round and round inside them, which motion of electricity is a form of energy, because the conductors and whirls can attract or repel each other and thereby do work.

And, on the other hand, that light is the rapid alternation of energy from one of these forms to the other—the static form where the medium is strained, to the kinetic form when it moves. It is just conceivable then that the static form of the energy of light is *electro*-static, that is, that the medium is *electrically* strained, and that the kinetic form of the energy of light is *electro*-kinetic, that is, that the motion is not ordinary motion, but electrical motion—in fact that light is an electrical vibration, not a material one.

On November 5 last year there died at Cambridge a man in the full vigour of his faculties—such faculties as do not appear many times in a century—whose chief work has been the establishment of this very fact, the discovery of the link connecting light and electricity; and the proof—for I believe it amounts to a proof —that they are different manifestations of one and the same class of phenomena—that light is, in fact, an electro-magnetic disturbance. The premature death of James Clerk Maxwell is a loss to science which appears at present utterly irreparable, for he was engaged in researches that no other man can hope as yet adequately to grasp and follow out; but fortunately it did not occur till he had published his book on "Electricity and Magnetism," one of those immortal productions which exalt one's idea of the mind of man, and which has been mentioned by competent critics in the same breath as the "Principia" itself.

But it is not perfect like the "Principia"; much of it is rough-hewn, and requires to be thoroughly worked out. It contains numerous misprints and errata, and part of the second volume is so difficult as to be almost unintelligible. Some, in fact, consists of notes written for private use, and not intended for publication. It seems next to impossible now to mature a work silently for twenty or thirty years, as was done by Newton two and a half centuries ago. But a second edition was preparing, and much might have been improved in form if life had been spared to the illustrious author.

The main proof of the electro-magnetic theory of light is this. The rate at which light travels has been measured many times, and is pretty well known. The rate at which an electro-magnetic wave disturbance would travel if such could be generated (and Mr. Fitzgerald of Dublin thinks he has proved that it cannot be generated directly by any known electrical means) can be also determined by calculation from electrical measurements. The two velocities agree exactly. This is the great physical constant known as the ratio V, which so many physicists have been measuring, and are likely to be measuring for some time to come.

Many and brilliant as were Maxwell's discoveries, not only in electricity, but also in the theory of the nature of gases, and in molecular science generally, I cannot help thinking that if one of them is more striking and more full of future significance than the rest, it is the one I have just mentioned—the theory that light is an electrical phenomenon.

The first glimpse of this splendid generalisation was caught in 1845, five and thirty years ago, by that prince of pure experimentalists, Michael Faraday. His reasons for suspecting some connection between electricity and light are not clear to us—in fact they could not have been clear to him; but he seems to have felt a conviction that if he only tried long enough, and sent all kinds of rays of light in all possible directions across electric and magnetic fields in all sorts of media, he must ultimately hit upon something. Well, this is very nearly what he did. With a sublime patience and perseverance which remind one of the way Kepler hunted down guess after guess in a different field of research, Faraday combined electricity, or magnetism, and light in all manner of ways, and at last he was rewarded with a result. And a most out-of-the-way result it seemed. First you have to get a most powerful magnet and very strongly excite it; then you have to pierce its two poles with holes, in order that a beam of light may travel from one to the other along the lines of force; then, as ordinary light is no good, you must get a beam of plane polarised light and send it between the poles. But still no result is obtained until, finally, you interpose a piece of a rare and out-of-the-way material which Faraday had himself discovered and made, a kind of glass which contains borate of lead, and which is very heavy, or dense, and which must be perfectly annealed.

And now, when all these arrangements are completed, what is seen is simply this, that if an analyser is arranged to stop the light and make the field quite dark before the magnet is excited, then directly the battery is connected and the magnet called into action a faint and barely perceptible brightening of the field occurs; which will disappear if the analyser be slightly rotated. [The experiment was then shown.] Now no wonder that no one understood this result. Faraday himself did not understand it at all: he seems to have thought that the magnetic lines of force were rendered luminous, or that the light was magnetised; in fact he was in a fog, and had no idea of its real significance. Nor had any one. Continental philosophers experienced some difficulty and several failures before they were able to repeat the experiment. It was in fact discovered too soon, and before the scientific world was ready to receive it, and it was reserved for Sir William Thomson briefly, but very clearly, to point out, and for Clerk Maxwell more fully to develop, its most important consequences. [The principle of the experiment was then illustrated by the aid of a mechanical model.]

This is the fundamental experiment on which Clerk Maxwell's theory of light is based; but of late years many fresh facts and relations between electricity and light have been discovered, and at the present time they are tumbling in in great numbers.

It was found by Faraday that many other transparent media besides heavy glass would show the phenomenon if placed between the poles, only in a less degree; and the very important observation that air itself exhibits the same phenomenon, though to an exceedingly small extent, has just been made by Kundt and Röntgen in Germany.

Dr. Kerr of Glasgow has extended the result to opaque bodies, and has shown that if light be passed through magnetised *iron* its plane is rotated. The film of iron must be exceedingly thin, because of its opacity, and hence, though the intrinsic rotating power of iron is undoubtedly very great, the observed rotation is exceedingly small and difficult to observe; and it is only by very remarkable patience and care and ingenuity that Dr. Kerr has obtained his result. Mr. Fitzgerald of Dublin has examined the question mathematically, and has shown that Maxwell's theory would have enabled Dr. Kerr's result to be predicted.

Another requirement of the theory is that bodies which are transparent to light must be insulators or non-conductors of electricity, and that conductors of electricity are necessarily opaque to light. Simple observation amply confirms this; metals are the best conductors, and are the most opaque bodies known. Insulators such as glass and crystals are transparent whenever they are sufficiently homogeneous, and the very remarkable researches of Prof. Graham Bell in the last few months have shown that even *ebonite*, one of the most opaque insulators to ordinary vision, is certainly transparent to some kinds of radiation, and transparent to no small degree.

[The reason why transparent bodies must insulate, and why conductors must be opaque, was here illustrated by mechanical models.]

A further consequence of the theory is that the velocity of light in a transparent medium will be affected by its electrical strain constant; in other words, that its refractive index will bear some close but not yet quite ascertained relation to its specific inductive capacity. Experiment has partially confirmed this, but the confirmation is as yet very incomplete. But there are a number of results not predicted by theory, and whose connection with the theory is not clearly made out. We have the fact that light falling on the platinum electrode of a voltameter generates a current, first observed, I think, by Sir W. R. Grove at any rate it is mentioned in his "Correlation of Forces"—extended by Becquerel and Robert Sabine to other substances, and now being extended to fluorescent and other bodies by Prof. Minchin. And finally—for I must be brief—we have the remarkable action of light on selenium. This fact was discovered accidentally by an assistant in the laboratory of Mr. Willoughby Smith, who noticed that a piece of selenium conducted electricity very much better when light was falling upon it than when it was in the dark. The light of a candle is sufficient, and instantaneously brings down the resistance to something like one-fifth of its original value.

I could show you these effects, but there is not much to see; it is an intensely interesting phenomenon, but its external manifestation is not striking—any more than Faraday's heavy glass experiment was.

This is the phenomenon which, as you know, has been utilised by Prof. Graham Bell in that most ingenious and striking invention, the photophone. By the kindness of Prof. Silvanus Thompson I have a few slides to show the principle of the invention, and Mr. Shelford Bidwell has been good enough to lend me his home-made photophone, which answers exceedingly well for short distances.

I have now trespassed long enough upon your patience, but I must just allude to what may very likely be the next striking popular discovery, and that is the transmission of light by electricity; I mean the transmission of such things as views and pictures by means of the electric wire. It has not yet been done, but it seems already theoretically possible, and it may very soon be practically accomplished.

ENDOWMENT OF RESEARCH IN BIRMINGHAM

THE President of the Birmingham Philosophical Society, Dr. Heslop, recently gave an address to the members, taking for his subject the "Scientific Situation in Birmingham." Having reviewed the various local agencies set up during the past year for the diffusion of knowledge, including the opening of Mason's Science College, he went on to say: I must now allude to the most important work undertaken by the Society, the establishment of the fund for the endowment of research. This action has received warm support in many quarters, and has in fact done more to place it in a favourable light before the country than any previous circumstances. Although the efforts made to raise this fund have been inconsiderable, yet nearly 100*l*. in annual subscriptions, of varying dates, and 900*l*. in donations have been obtained. The Council have invested 600*l*. in order to ensure the permanence of the fund. It is probable that some slight additions may be made to this sum, having the same object in view; but it is, I believe, their intention to recommend the Society to spend the whole income, howsoever derived, in annual grants to persons living in this town or neighbourhood who devote themselves wholly or in part to science research. It is an error to suppose that this fund is to be allotted either to any particular individual or specially to members of this Society. The Council are free to do what they deem best with the money intrusted to them, within the limits of the scheme agreed upon. There is another temporary limit to their powers. One eminent investigator (Dr. Gore) is allotted a certain sum for a certain period. The approval of this step evinced by those who have contributed to the fund, and by others, has been a source of satisfaction to the Council.

I wish now to remind you that the scheme in connection with this subject declares that "the Council are of opinion that this Society would be omitting a principal means of the advancement of science—the end for which all such associations exist—if it neglected the question of the endowment of research. To maintain a successful investigator in his labours, even though no results of immediate or obvious utility can be shown to spring out of them, is of interest to the community at large." It may be that you will pronounce these words to be truisms scarcely requiring formal enunciation. The fact is that though the sense of them has been repeatedly given to the public in late years, practical action has not ensued. Everybody is telling his neighbour what a good thing it would be if men endowed with an aptitude for research into the facts of nature were also endowed with the means of living during their work. The speaker and the listener go by on the other side, and no good Samaritan tenders help to the well-praised searcher after truth. Nay, Mr. Mark Pattison affirms in his late book on Milton that "the England of our day has decided against the endowment of science," and seems to think that the principle on which the decision is based may be wrong, but "is not unreasonable." But the endowment of ministers of science stands on quite another foundation from that of ministers of religion. "To assign a place with a salary," says Mr. Pattison, "is to offer a pecuniary inducement to simulate" the qualification, *i.e.* a state of grace. But in the case of science there is no question of place, and the endowment is offered, not to those who promise much, but to those who have already performed something; not to those who imagine themselves to be in the requisite spiritual state, but to those who, working for an audience, select though few, have demonstrated that they are touched by the divine fire which burns not for other men.

In the opinion of others the only practicable mode of dealing with this question is by bestowing adequate funds on teachers, and by placing them in favourable conditions for research.

The necessity for making provision for the devotion of fit men to scientific work has occurred to many, and doubtless private generosity has often enabled such men to prosecute labours by which the world has greatly gained. One of the most striking instances is that of Priestley. His own remarks contained in his diary are full of interest. He says that Dr. Fothergill "having observed that many of my experiments had not been carried to their proper extent on account of the expense that would have attended them," proposed a subscription from himself and some of his friends, and named 100*l.* per annum. He consented to receive 40*l.*, which was regularly paid to him, three other gentlemen aiding. Afterwards for good reasons Fothergill proposed "an enlargement of the allowance for my experiments, and likewise for my maintenance, *without being under the necessity of giving my time to pupils*, which I must otherwise have done." This was accepted, as Priestley preferred it to any pension from the Court. He gives a minute list of the numerous donations, legacies (one of 2000*l.*) and subscriptions given to him, while he dilates on Mr. Wedgwood's gifts of pottery, retorts, tubes, &c., and presents in glass from another gentleman, among which figures "a capital burning-lens, sixteen inches in diameter." The Duke of Grafton remitted him annually 40*l.* When he went to America forty of his friends, without solicitation, raised the sum of 450*l.*, "which was meant to have been continued annually while he lived," as stated by his son.

You will hear these details with the interest naturally belonging to the subject, and doubtless ask the question, Have succeeding generations improved on this? I believe that there is no example of an equal generosity on the part of their fellow-countrymen to a man of science, although there are some agreeable exceptions to the rule of neglect. A few years ago the Fishmongers' Company presented the sum of 50*l.* to Prof. Parker, and an annual gift of 20*l.* for three years, to assist him in bearing the expenses of his researches on vertebrate animals. For three years the British Pharmaceutical Society voted 80*l.* in aid of pharmaceutical research. The principle of promoting research has also been recognised by the Government in their grant of 1000*l.*, and in the fund of 4000*l.* placed at the disposal of the Royal Society. Our Government has, however, as yet made no payment for the *labour* of pure research in experimental physics or chemistry. "A grant from the above sum," says Dr. Gore, "is often an unprofitable gift to accept, because it is in some cases only sufficient to pay expenses out of pocket for chemicals and apparatus, and allows nothing for skill or labour."

The mention of our able associate's name compels me to draw attention to the invaluable services he has rendered for ten years past to the cause of the endowment of research. His numerous articles and papers form a repository of facts and arguments of which I have largely availed myself. Any one who studies them will need no other evidence of the importance of the question, both in view of the progress of truth and of the maintenance of our national welfare. In one of them he mentions the difficulty experienced in the proper employment of the Government money, and proceeds as follows:—"By far the greater part of the expense of an investigation in physics or chemistry is the exceedingly large amount of time it occupies. Many necessary preliminary experiments have to be made, which yield either negative, unsuccessful, or incomplete results, and make the undertaking expensive." Further, "By order of the Council, all instruments, apparatus, and drawings, made or obtained by aid of the Government grants, shall, after serving the purpose for which they were procured, and in the absence of any undertaking to the contrary, be delivered into the custody of the Royal Society."

Research, then, in any fair sense of the word may be said to be unprovided for by public funds. The British Association annually bestows grants of varying amounts for specific researches. The Royal Institution helps. Yet we feel surprised when told that the average annual expenses relating to experimental research, including salaries to assistants in the laboratory, from the year 1867 to 1871, did not amount to two hundred and fifty pounds.

When it is remembered that not a single college, nor even a professorship, for pure scientific research exists in this country, we must feel humiliated when we cast a glance at what is going on in other countries. In France and Germany varied resources have been placed at the disposal of men of science, which I cannot now minutely specify. Nor will I delay to speak in detail of the importance of pure research in science, of the mighty material results as regards our comfort and national wealth which have sprung from the labours of men of science, for it may be assumed that all this is known. It is certain too that valuable inventions in various arts and manufactures will follow upon fresh discoveries regarding the principles involved in them. When we know more of the materials and forces of nature, new applications of them will soon follow. The progress of invention depends upon that of discovery; the various inventions wanted by manufacturers and others cannot be perfected until "suitable knowledge is found." The money of the capitalist, the hand of the inventor, await the products of the brains of the searcher after truth.

It is only too well known that other countries have for some years past distanced Britain in the field of research; that while Germany is sending her trained sons to all parts of the world, we scarcely even supply our own colonies. A writer in the *Monthly Journal of Science* said last year that "to a very great extent, both in the home kingdoms and the colonies, we find ourselves compelled to import that intellectual eminence which we refuse to cultivate in our midst. Foreigners occupy professorial chairs in our colleges; they fill the posts of botanists and geologists in our colonial governments; they hold high positions in the respective staffs of the British Museum, of the Geological Survey of India, and of our exploring expeditions."

Now as these results cannot be owing to any inbred deficiency in the countrymen of Newton, Faraday, and Darwin, it behoves us to ask if our educational system is at fault, and if fair provision is made for those able and willing to make original research. The latter question is already answered by the facts adduced. How far our great universities have provided for science-teaching can be learned by reading between the lines of certain resolutions passed a few years ago by an "Association for the organisation of academical study," the composition of which was an ample guarantee of competence. The resolutions were as follows:—

"That the chief end to be kept in view in any redistribution of the revenues of Oxford and Cambridge is the adequate maintenance of nature study and scientific research, as well for their own sakes as with the view of bringing the higher education within the reach of all who are desirous of profiting by it."

"That to have a class of men whose lives are devoted to research is a national object."

"That it is desirable in the interests of national progress and education that *professorships* and *special institutions* shall be founded in the universities for the promotion of scientific research."

"That the present mode of awarding Fellowships as prizes, has been unsuccessful as a means of promoting nature study and original research, and that it is therefore desirable that it should be discontinued."

The state of things here pointed at has improved and is improving, yet the verdict of a candid observer must still be summed up in the one word inadequacy.

Numerous suggestions have been made regarding the endowment of research, but most of them are unpractical. Those who imagine that the object will be gained by establishing adequate teacherships of science, seem to be too sanguine. The labour involved in the work of teaching, in the way of acquisition, preparation, and performance, is too great to permit the devotion of sufficient time and thought to the search after new truth. If it is desirable that new facts and principles be searched after, why should fit inquirers be put, either partly or wholly, to other work? I know that many of our teachers have hitherto been at a great disadvantage; that managers of institutions have had a sharper eye on their prospectus than on their internal arrangements; that they have thought of a college rather as a body of bricks than as productive of a body of learning; and that apparatus and assistants, though well enough in Berlin or Leipzig, are needless in prudent England. Yet the ideal Professor is rather the head of a department than a mere speaker by the yard of so many lectures; a man with numerous hands in the shape of demonstrators and assistants, themselves the possessors of well-trained brains; a director of work with all its apparatus freely supplied to him. Such a man adequately remunerated may be safely left to his own tendencies. Contact with nature breeds the desire to know her better. In favourable conditions the teacher becomes the investigator, and while seeking after new truth builds up his own fame.

After all, however, the question recurs, how can we best promote research, as the undivided life's work of fit persons? I believe that the solution lies, so far as this time is concerned, in the establishment of special institutions having no other object than the search after new truth. Their administration would be difficult. The right men can be found for the work, but can the right electors be found? Ardent searchers after a more intimate knowledge of nature do still live, will ever live; but what of First Commissioners of Works like—but I need not name him? What of Lords of the Treasury who refused the request of a great physicist for 150*l.* for the investigation of the tides? Yet these gentlemen assist in governing a maritime state of some importance. Such electors as these are not within my view, and, if they were, how of the detailed management? Men given up to research are not to be tied by the common rules of official life; to be compelled to report in annual blue books the exact measure of work they have done; to show how many drachms of oil of vitriol they have used, and account for every ounce of platinum.

Special institutions will be founded, but they will owe their origin to private individuals like Sir Josiah Mason; who, having taken into their confidence the chiefs of the world of science in making the appointments, will speak to the masters of research in this wise:—

"I have built a house for you replete with every requirement for your work; I have provided you with such assistants as you have asked for; I have given you an income placing within your reach every reasonable comfort. Occupy your lives in the study of nature. If you succeed in your efforts to attain to new truth, the world will be the gainer. If you fail, your efforts will be enough reward for me."

Such language as this will be surely one day heard. In this fortunate town it is already heard. During the past year a member of this Society, Mr. Fulford, has taken a house, and, having admirably fitted it up, has handed it over to our two distinguished associates, Dr. Gore and Dr. Norris, in order that they may enjoy at least the requisite structural conveniences for the prosecution of research. This building is called the "Institute of Scientific Research."

I must, however, remind you that this noble enterprise must be supplemented by some such efforts in the way of endowment as those now made by this Society; and that those who work even in the highest sphere are bound by the same necessities as bind other men.

SCIENTIFIC SERIALS

Annalen der Physik und Chemie, No. 12 (December, first No.).—On the density and tension of saturated vapour, by A. Wüllner and O. Grotrian.—On the application of the electrodynamic potential to determination of the ponderomotive and electromotive forces, by R. Clausius.—On friction in free liquid surfaces, by A. Oberbeck.—Simple methods and instruments for resistance-measurements, especially in electrolytes, by F. Kohlrausch.—Influence of temperature on the phenomena of charge of a liquid cell acting as condenser, by H. Herwig.—On the modes of electric discharge in gases, by O. Lehmann.—On the electric discharge in liquid insulators, by W. Holtz.—On electric figures on the surface of liquids, by the same.—On the increase of danger from lightning and its probable causes, by the same.—On a microprismatic method for distinction of solid substances, by O. Maschke.—Note on Herr Weber's reply, by A. Winkelmann.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xii., fasc. xix.—The Leprosy of Upper Italy, especially of Comacchio (continued), by Prof. Sangalli.—Influence of traction and vibration of a metallic wire on its electric conductivity, by Dr. de Marchi.—On a case of twisted neck; a contribution to the doctrine of transport of spinal influence and to establishment of a hypothesis for its explanation, by Prof. de Giovanni.

Zeitschrift für wissenschaftliche Zoologie, November, 1880, contains: Dr. H. von Ihering, on the affinities and kinship of the Cephalopods.—Dr. J. Bellonci, on the origin of the optic nerve and on the minute structure of the "tectum opticum" in the Teleostei (Plates 1 and 2).—Dr. D. Sochaczewer, on the organ of smell in the terrestrial pulmonates (Plate 3).—Dr. Fritz Müller, on the case-making Trichoptera larvæ of the Province of Santa Catharina (Plates 4 and 5), translated by his brother, Dr. Hermann Müller, from the memoirs in Portuguese in the Archivos de Museu National, Rio de Janeiro.—Dr. William Marshall, researches in the sponge groups, Dysideidæ and Phoriospongiæ (Plates 6 to 8).—Prof. Dr. Krause, on two very early human embryos (Plate 9).—Dr. H. Simroth, on the nervous system in the foot of *Paludina vivipara*, with a woodcut of the nerves as dissected out.

Revue internationale des Sciences biologiques, December 1880 contains:—A. de la Calle, on the formation of language (continued).—M. Decatte, microcephalism, from the point of view of atavism.—M. Zaborowski, historical sketch of the relative knowledge possessed by the ancients and in mediæval times of the large monkeys.—Notices of learned societies.—French Association for the Advancement of Science (the Rheims Meeting).—The Academy of Sciences, Paris.

Schriften der physikalisch-ökonomischen Gesellschaft zu Königsberg (1877, ii.; 1878, i. and ii.).—These parts contain the following papers:—On Baron von Richthofen's loess theory and the alleged steppe character of Europe at the close of the Glacial period, by Dr. A. Jentzsch.—Observations of the station for measuring the temperature of the soil in various depths, at the Königsberg Botanical Gardens, by Prof. E. Dorn.—On the prehistoric-archæological work done by the Society, by Otto Tischler.—On the commercial routes of the ancients to the amber country, by Dr. Krosta.—On the physics of the soil, by Dr. von Liebenberg.—On the discoveries in prehistoric tombs at Fürstenwalde, by Otto Tischler.—On hair-covered human beings and the abnormal growth of hair, by Prof. Hildebrandt.—On the marine fauna near the Prussian coast, by Prof. Zaddach.—On the alleged steppe character of Central Europe, by Dr. Ientzsch.—On the state of civilisation in Denmark during the first centuries after Christ, by O. Tischler.—On Darwin's theory, by Herr Czwalina.—On East Prussian burial-grounds, by O. Tischler.—On the fauna of Madagascar, by Prof. Zaddach.—On the intra-Mercurial planet, by Dr. Franz.—On the geological maps at the Paris Exhibition, by Dr. Jentzsch.—On some special geological maps of Germany, by the same.—On the principles of the kinetic theory of gases, by Dr. Saalschütz.

SOCIETIES AND ACADEMIES

London

Chemical Society, January 20.—Prof. H. E. Roscoe, president, in the chair.—The president announced that the Faraday lecture would be delivered by Prof. Helmholtz in the Royal Institution, On the Modern Development of Faraday's Conception of Electricity. The following papers were read:—On pentathionic acid, by Mr. V. Lewes. The author has succeeded in obtaining beautifully crystallised barium and potassium pentathionates by partially neutralising Wackenroder's solution and evaporation *in vacuo*.—A preliminary note on some hydrocarbons from rosin spirit, by Dr. Armstrong. Cymene, toluene, and metaxylene were found to be present. The hydrocarbons insoluble in sulphuric acid are probably hexhydrides of hydrocarbons of the benzene series. The author does not consider that rosin is directly derived from terpene.—On the determination of the relative weight of single molecules, by Dr. Vogel of San Francisco.—On the synthetical production of ammonia by the combination of hydrogen and nitrogen in presence of heated spongy platinum, by G. S. Johnson. About 0·0144 grm. of ammonia were obtained in two and a half hours.—On the oxidation of organic matter in water, by A. Downes.—Analyses of Queensland soils, by Prof. A. Liversidge. These analyses are interesting, as the soils include samples from districts which were exempt from the disease prevalent in the sugar plantations around.—On the volumes of some compounds of the benzene naphthalene, anthracene, and phenanthrene series, by Dr. Ramsay.—On the atomic volume of nitrogen, by Dr. Ramsay.—On a new theory of the conversion of bar iron into steel by the cementation process, by Dr. Marsden. The author thinks that carbon diffuses in an impalpable powder through the heated iron.—On the action of sulphydrate of potassium on chloral hydrate, by W. W. J. Nicol. Thioglyoxylic and thioformic acids are formed.

Zoological Society, January 18.—Prof. W. H. Flower, LL.D., 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 December, 1880, amongst which special attention was called to a young female Red Wolf (*Canis jubatus*) from the Argentine Republic, presented by Mr. W. Petty of Monte Video, being the second example of this scarce animal received, and to a Pig from Brooker Island, Louisiade Archipelago, presented by Lieut. de Hoghton, of H.M.S.

Beagle.—A paper by Mr. P. L. Sclater and Dr. G. Hartlaub was read, on the birds collected in Socotra by Prof. I. B. Balfour in the early part of the year 1880. The collection contained 124 examples referable to thirty-four species. Of these seven of the Passeres appeared to be new, and were proposed to be called *Cisticola incana*, *Drymœca hæsitata*, *Lanius uncinatus*, *Cinnyris Balfouri*, *Passer insularis*, *Rhynchostruthus Socotranus*, and *Amydrus frater.*—Mr. A. G. Butler read a paper on the lepidoptera collected in Socotra by Prof. I. B. Balfour. The collection contained twenty-four specimens referable to thirteen species, seven of which were stated to be new to science.—Mr. W. A. Forbes read a paper on some points in the anatomy of the Koala (*Phascolarctos cinereus*), as observed in the specimen recently living in the Society's Gardens.—A communication was read from Mr. R. Bowdler Sharpe, in which was given the description of a new form of the family *Timeliidæ*, from Madagascar, proposed to be called *Neomixis.*—A communication was read from Dr. John Scully containing an account of the mammals of Gilgit, a district in the extreme north-western part of Kashmir. Thirty three species were enumerated, and notes on their vertical ranges and habits were added. The mammals of Gilgit were shown to consist of an intermixture of Central Asiatic and Himalayan species, as might have been expected from the position of the country. Two species (a Bat and a Vole), apparently new to science, were named respectively *Harpiocephalus tubinaris* and *Arvicola Blanfordi.*

Meteorological Society, January 19.—Mr. G. J. Symons, F.R.S., president, in the chair.—The report of the Council for the year 1880, which was read by the Secretary, refers to subjects of considerable importance, and affords substantial evidence of the interest taken in meteorology by the scientific and general public. Amongst these may be mentioned the great success of the new climatological stations, as shown by their increased number and by the regularity and care with which the observations have been made and recorded, and the returns forwarded to the Society. The Council also advert to the number of new and improved instruments exhibited at the meeting held in March last, to the increase in the number of Fellows, fifty-two having been elected during the year, and finally the numerous papers which have been sent to the Society from various parts of the world, embracing records of the climate of several important localities, respecting which but little has hitherto been known in this country.—After a vote of thanks had been passed to the Council for their services during the year, and to the Institution of Civil Engineers for allowing free use of their rooms, the President delivered his address, in which he traced the history of English meteorological societies from 1823 to 1880. The earliest English effort at forming an English meteorological society, or at any rate at securing observations made with comparable instruments recorded upon a uniform system, was made in 1723 by Dr. James Jurin, who was then secretary to the Royal Society. In the *Philosophical Transactions* for that year will be found a Latin address by Dr. Jurin, in which he anticipates nearly all the conditions which are now considered essential for comparable observations. This appeal did not lead to much being done, and in 1744 another attempt was made by Mr. Roger Pickering, F.R.S., who read before the Royal Society a paper entitled "Scheme of a Diary of the Weather, together with Drafts and Descriptions of Machines subservient thereunto." The Meteorological Society of the Palatinate was established in 1780 under the auspices of the Elector Charles Theodore, who not only gave it the support of his public patronage, but entered with spirit and ability into its pursuits and furnished it with the means of defraying the expense of instruments of the best construction, which were gratuitously distributed to all parts of Europe and even to America. One of the first acts of the Association was to write to all the principal universities, scientific academies, and colleges, soliciting their co-operation and offering to present them with all the necessary instruments properly verified by standards and free of expense. The offer was accepted by thirty societies, and the list of distinguished men who undertook to make the observations shows the importance which was attached to the plan and the zeal with which it was promoted in every part of the Continent. In 1823 the first meeting of the Meteorological Society of London was held, and was attended by Luke Howard, Thomas Forster, Dr. Birkbeck, and others. After 1824 the Society languished, but it was never regularly dissolved. Owing to several letters and articles which appeared in *Loudon's Magazine of Natural History* a meeting was held on November 15, 1836, at which the Society was revived, Mr. W. H. White appointed secretary, and regular meetings resumed. Application was made to the Royal Society for permission to compare the instruments of the Society with the Royal Society's standards, and leave was granted on March 13, 1838. A volume of *Transactions* was published in 1839, and among other articles contains one entitled "Remarks on the Present State of Meteorological Science, by John Ruskin." The cost of the publication of this volume exhausted the funds of the Society, but in 1841 Mr. Gutch undertook personally the pecuniary risk of a new publication entitled the *Quarterly Journal of Meteorology*, but this does not appear to have been very successful, owing to the high rate of postage. Shortly after this the Society practically came to an end. On April 3, 1850, a meeting of some friends of the science was convened by Dr. Lee at Hartwell, when the British Meteorological Society was established, and Mr. S. C. Whitbread elected president. The first general meeting of the Members was not held till March 25, 1851; but in the meanwhile several important steps had been taken by the Council. Annual Reports were published from 1851 to 1861, and since then five volumes of the *Proceedings* and six volumes of the *Quarterly Journal* have been published. Up to 1858 absolutely nothing had been done towards forming a library, but in 1862 a catalogue was published containing about 200 titles. In 1876 a new catalogue was issued, which extends to eighty pages and contains over 1200 entries. On January 27, 1866, the Society obtained a Royal Charter of Incorporation, and has since been known as "the Meteorological Society." On April 4, 1872, the Council resolved upon taking a room for an office and for the protection of the library, and appointed Mr. W. Marriott as their Assistant Secretary. The work has now become so great that the Society has been obliged to take an additional room and to engage three computers. The subsequent eight years have been characterised by great progress. A series of second order stations has been organised which are systematically inspected, and at which strictly comparable observations are made. On January 1, 1880, another and larger series of stations—called climatological—was started, at which the observations are less onerous than those at the second order stations, but at which they are required to be equally accurate. Observations on natural periodical phenomena are also made at many places, and discussed yearly by the Rev. T. A. Preston. At the request of the Society a conference has been appointed consisting of delegates from several other societies to prepare accurate instructions respecting the erection of lightning conductors. At the conclusion of the President's address the following gentlemen were elected the officers and council for the ensuing year, viz.:—President—George James Symons, F.R.S. Vice-presidents: Edward Ernest Dymond, William Ellis, F.R.A.S., Joseph Henry Gilbert, F.R.S., Charles Greaves, M.Inst. C.E., F.G.S. Treasurer: Henry Perigal, F.R.A.S. Trustees: Sir Antonio Brady, F.G.S., Stephen William Silver, F.R.G.S. Secretaries: Robert Henry Scott, F.R.S., John William Tripe, M.D. Foreign Secretary: John Knox Laughton, M.A., F.R.A.S., F.R.G.S. Council: Edmund Douglas Archibald, M.A., Arthur Brewin, F.R.A.S., Henry Storks Eaton, M.A., Rogers Field, B.A., M.Inst. C.E., Frederic Gaster, Baldwin Latham, M.Inst.C.E., F.G.S., Robert John Lecky, F.R.A.S., Edward Mawley, Hon. Francis Albert Rollo Russell, M.A., Richard Strachan, George Mathews Whipple, B.Sc., F.R.A.S., Charles Theodore Williams, M.A., M.D.

Entomological Society, Annual Meeting, January 19.—Sir John Lubbock, Bart., F.R.S., &c., president, in the chair.—The President delivered his annual address, and the following gentlemen were elected as officers for the ensuing year:—President, H. T. Stainton, F.R.S.; Treasurer, E. Saunders, F.L.S.; Librarian, F. Grut, F.L.S.; Secretaries: E. A. Fitch, F.L.S., and W. F. Kirby, F.L.S.; Council: W. Cole; W. L. Distant, M.A.I.; F. du Cane Godman, F.L.S.; Sir John Lubbock, Bart., F.R.S., &c.; R. Meldola, F.R.A.S.; O. Salvin, M.A., F.R.S.; F. P. Pascoe, F.L.S.; R. Trimen, F.L.S.

Victoria (Philosophical) Institute, January 17.—A paper on Pliocene man in America, by Dr. Southall of Virginia, U.S., was read. In it he showed that the evidence brought forward as to the existence of such was wholly unreliable; the same ground was taken in special communications from the Duke of Argyll, K.G., Principal Dawson, F.R.S., of Montreal, Prof. Hughes (Woodwardian Professor of Geology at Cambridge), and Mr. Whitley, C.E.; also by Mr. S. R. Pattison, F.G.S., and Mr. Hall, F.R.G.S., who had examined the evidences on the spot, and by the Rev. J. M. Mello, F.G.S., Mr. T. K. Callard, F.G.S., and Mr. E. Charlesworth, F.G.S.—About twenty new Members were elected, bringing the total number to nearly 900.

Institution of Civil Engineers, January 18.—Mr. James Abernethy, F.R.S.E., president, in the chair.—The paper read was on deep winning of coal in South Wales, by Messrs. Thomas Forster Brown and George Frederick Adams, MM. Inst. C.E. The authors, who were professionally associated with Harris's Navigation Pits, the deepest winning in the district, described the operations as a fair example of the details connected with winning deep coals in South Wales. The depth of the lowest seam at present sunk to was 760 yards; the pits were each seventeen feet in diameter inside the walling. In addition to the depth a special feature was the thickness of hard and heavily-watered rock penetrated. Guide ropes, upon the Galloway principle, were used in sinking, and the value of this system was shown in the saving of over two minutes in steadying the bowk at the bottom of the pit at depths of 475 and 530 yards, the total time occupied in clearance at the latter depth being three minutes twenty-six seconds. The method of dealing with the various feeders of water during sinking was described: one of the pits was drained by a hole bored by the diamond machine, which was put down, at a depth of 175 yards from the surface, for a farther depth of 860 feet.

Vienna

Imperial Academy of Sciences, January 13.—Dr. Fitzinger in the chair.—On the lacunar resorption in diseased bones, by Dr. Pommer.—On the physiological significance of the transpiration of plants, by Herr Reinitzer.—On the influence of prussic acid on breathing and circulation, by Dr. Lazarski.—On the relations of homogeneous deformations of solid bodies to surfaces of reaction, by Dr. Finger.—Contributions to the photochemistry of silver chloride, by Dr. Eder and Herr Pizzighelli.—On a new derivative of gallic acid, by Dr. Oser and Herr Kalmann.—Influence of form of cathode on the distribution of phosphorescence-light (sealed packet of November 17, 1880), by Herr Goldstein.—On a tetra-hydrocinchonin acid, by Dr. Weidel.—Determination of magnetic and diamagnetic constants of liquids and gases in absolute measure, by Herr Schuhmeister.

January 20.—Herr von Burg in the chair.—Studies on caffeïn and theobromin (first part), by Prof. Maly and Herr Hinteregger.—Researches on the anatomy, physiology, and development of Sternaspis, by Dr. Vejdovsky.—The flight of Libellula; contribution to the anatomy and physiology of organs of flight in insects, by Herr Lendenfeld.—Research on kynurenic acid (first part), by Dr. Kretschy.—Action of hydrate cupric oxide on some kinds of sugar, by Prof. Habermann and Herr Hönig.

Paris

Academy of Sciences, January 17.—M. Wurtz in the chair.—The following papers were read:—Contemporaneous production of native sulphur in the subsoil of Paris, by M. Daubrée. This sulphur occurs abundantly in the ground of the Place de la République, from 0·2m. to 3m. from the surface, and in a space 50m. by 15 to 20m.; one finds a breccia with thin incrusted fragments of crystalline sulphur. The product is due to simultaneous presence of the sulphate of lime of plaster-rubbish, and organic *débris*, with which the ancient moat of the centre of the city was filled up two centuries ago.—Order of appearance of the first vessels in the ear of Lolium (second part), by M. Trécul.—On the treatment of phylloxerised vines, by M. Marès. He finds very successful an application of dilute sulphocarbonate of potassium to the lower parts of the vines twice a year.—Discoveries in equatorial Africa; meeting of MM. de Brazza and Stanley; by MM. de Lesseps and de Quatrefages. M. de Brazza speaks of having descended the Congo and founded the station of Ntamo Ncouna, twelve marches from Ogôuué; it is the most advanced post in the heart of Africa, and will be an important centre for exploration, &c. He had met Stanley on November 7 near Vivi. Capt. Bloyet has established a station near Lake Touquerko.—Observations of the comet *f* 1880 (Pechulé) at Paris Observatory, by M. Bigourdan.—On the displacement of an invariable figure, by M. Darboux.—Integration in finite form of a new species of differential linear equations with variable coefficients, by M. André.—On the theory of vibrating plates, by M. Mathieu.—On complete combinations; number of complete combinations of m letters n to n, by M. Melon.—Remarks on an opinion attributed to me by a note of M. Cornu, by M. Gouy.—Minimum of the power of resolution of a prism, by M. Thollon.—On the production of intermittent luminous signals with the electric light, by M. Mercadier. One carbon is horizontal, and advances a little at each signal. The other is vertical, and is held in a peculiar clip at one end of a horizontal lever, to the other end of which is fixed a vertical rod with terminal friction roller working on a cam. The vertical carbon is connected with the battery by a wire spring, and it is dropped a little by the clip at certain positions of the cam. The cam may be turned by clockwork for regular signals, or with the hand, at a variable rate, for irregular.—Observations *àpropos* of M. Dunand's recent paper on reproducing speech with electric condensers, by M. Herz. He patented the use of a condenser as telephonic receiver on M. Dunand's principle in June last year.—Some facts to serve in the history of nitrification, by MM. Hautefeuille and Chappuis. Electric effluves, intense enough to quickly give much ozone in a mixture of oxygen and nitrogen, but not to form hyponitric acid, produce the new unstable pernitric acid. Using lower tensions, the formation of this acid is found to go side by side with that of ozone. Pernitric acid is decomposed at all temperatures, but at 130° the decomposition is complete in a few seconds (into hyponitric acid and oxygen). Numerous experiments seem to prove that in simultaneous production of ozone and pernitric acid by the effluve the gases have not been raised to a temperature near that named; where hyponitric acid is formed, that temperature has been passed. A consequence of the facts is that effluves corresponding to weak tensions may furnish nitric acid, ultimate product of decomposition of pernitric acid.—On the conservation of grain in reservoirs (continued), by M. Müntz. Oats kept in a ventilated granary thirty months had lost 7·2 per cent. more of fixed matter (chiefly starch) than oats kept the same time in a metallic reservoir (of 220 cubic m. capacity), having its lower part in a subsoil. In the sereservoirs there is a distillation towards the upper part. To get all the advantage of closed reservoirs there should be a comparatively dry grain, a perfect closure, and a maintenance of the walls at pretty constant temperature.—Study of the peat of crystalline strata of Finisterre, by M. de Molon.—On the parts of the pancreas capable of acting as ferments, by M. Béchamp. All the known properties of the pancreas are concentrated in the microzymas.—Anatomical researches on the digestive, nervous, and reproductive apparatus of Onchidia, by M. Joyeux Laffine.—Hypertrophy and multiplication of nuclei in hypertrophied cells of plants, by M. Prillieux.—On the production of *verglas*, by M. Minary. He thinks the theory needs correction. Instead of regarding water in a state of surfusion as composed only of liquid, he supposes it formed of a mixture of liquid and of solid molecules (of ice) held apart by some unknown cause. For the congelation to be complete when surfusion ceases, the ice of the mixture merely requires (in order to rise to 0°) a quantity of heat equal to the latent heat still conserved by the quantity of water in surfusion.

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