THURSDAY, FEBRUARY 1, 1883

POPULAR ASTRONOMY

The Sun, its Planets, and their Satellites. A Course of Lectures upon the Solar System, read in Gresham College, London, in the Years 1881 and 1882. By Edmund Ledger, M.A., Rector of Barham, Suffolk. Pp. 432. (London: Stanford, 1882.)

ANOTHER work on Astronomy! It must have demanded some courage to venture on such an attempt in these days, so unprecedentedly fertile in similar undertakings. We are not speaking of the inundation of lighter productions—the magazines, the lectures, the newspaper articles—by which the lower grounds of modern society are overrun, to the benefit no doubt, in many cases, of those who may thus be led to find out in what a glorious world they live. Provided only that such efforts have something clear and something pleasant about them, we shall not be disposed to say "the fewer the better cheer." It is a worthy and honourable attempt, to introduce one new interest, one fresh and innocent pleasure, into the dull round of a careworn plodding life. The shepherd will love his work none the less for learning something of the movements of his "unfolding star:" the evening of the weary mechanic will bring unalloyed refreshment, if he is enabled to turn an inquiring gaze upon "the fields of light that lie around the throne of GOD." But not only is provision being thus made for the development of thought and intelligence among those whose lives too often are divided between uninteresting labour and debasing gratification, but a corresponding advance has been made in the production of treatises addressed to the possessors of more cultivated minds and leisurely opportunities. A full collection of such treatises during the last half-century would be at once voluminous and interesting. What would come out in strong relief from a comparison of them would be the comprehensiveness and many-sidedness of the subject. It is indeed a glorious subject—the "consideration of the heavens"; the subject of a life-time, of many life-times—in all its complexity of magnificence. No one mind, no one book, can do it justice. It is as boundless as the spaces of which it treats, and the mechanism which it professes to explain. It embraces no small part of the history of human intelligence; it demands the utmost power and subtlety of the most consummate analysis; the picturings of the most poetical imagination will be tame and feeble in the presence of its realities; and yet so simple are many of its elementary truths as to invite and recompense familiar inquiry. There may be room then for another, and another, and yet another work on astronomy; and provided they are thoughtfully designed and accurately wrought out, there will be little question as to their success; for it arises from the very comprehensiveness of the matter, that every writer will address himself to the task from his own point of view, and all readers may find something to interest them in every varied presentation of the subject.

We are pleased to give a welcome reception to the treatise which is now before us. In many respects it will be found worthy to take rank among the best. Where, as we have said, the study is so many-sided, it is obviously better to work on certain lines; not to attempt too wide a grasp, with the inevitable annoyance of bulk and costliness; not to be led into the opposite course of saying a little about everything, and enough about nothing. The author of these Lectures has chosen his own line, preferring to give us a good deal that is explanatory of the mechanism of the solar system, and a good deal that is descriptive of its wonders. And he has executed his task on the whole remarkably well. He has evidently a clear apprehension of what he is going to write about, and therefore succeeds in making it clear to other minds; and there is a pleasant facility in his style which imparts readableness to matters intrinsically somewhat dry. And if we meet with little of vivid and imaginative description, its place is supplied by a truly valuable amount of caution and discretion in dealing with the theories of the day. If he does not lead us far he will certainly not lead us wrong: and "when," as he characteristically tells us, "we know so little, we must not let our ignorance suggest unnecessary difficulties. Rather let it teach us to wait, and watch, and learn." Availing himself of no common extent of reading, he has used his materials with conscientious accuracy; and if we may venture to point out a few matters to which in our view some exception might be taken, we hope it may be looked upon as only the fulfilment of his own express desire to receive friendly communications of this nature.

A comparatively undeveloped point, we venture to think, in the programme, is the very brief notice that has been taken of the theory of the tides. Granted that its minuter details are affected by some complicated considerations, its general outline admits of easy explanation, and is at the same time the cause of occasional misconceptions which ought to be removed; and it would be probably considered by many persons an improvement if the larger space allotted to it were obtained at the expense of the refutation of the fallacy of the exploded Ptolemaic system.

We do not meet with any reference to outbursts of light on the surface of the sun; so interesting a proving that the brilliancy of the photosphere may be far outshone, and so suggestive as to their possible origin.

The author's usual lucidity is scarcely exemplified in the explanation of phases in p. 63, where we venture to think a more familiar treatment might have been adopted.

The larger map by Beer and Mädler, notwithstanding its able reduction by Neison, might have found place in the enumeration of aids to selenographical study.

There seems a little confusion on p. 77 between Sir W. Herschel's idea that Aristarchus and some other spots visible in the earthshine were volcanoes in actual eruption, and the observations by Schröter and others of minute illuminations on the dark side, which seemed to point to an unreflected origin, and are still, unlike the former, not accounted for.

With regard to Mercury, we feel it right to say that Sir W. Herschel's failure to confirm the statements of Schröter may not be entitled to much weight; as is sufficiently indicated by their controversy in the *Phil. Trans.* respecting the phenomena of Venus. As far as this latter planet is concerned, it may be concluded, without accepting the measures of Schröter, that the irregularities witnessed by many observers prove the existence of elevations much more considerable than any upon the earth: as to Mercury, notwithstanding Schröter's deficiency as an artist, and his occasional mistakes of preconception, his observations are always too honest and faithful to be set wholly aside; and we are not sure that the uselessness of devoting time to this planet may not be found a mistake at some future day.

As to the physical condition of Mars, we venture to think that our author has dealt very fairly and judiciously with a subject of controversy, which might have become less pleasant but for the unassuming modesty of Schiaparelli and the liberal candour of Green, so honourable to each of them. We are not sure that it is always borne in mind, how much of the difference may have been due to the early return of the English observer from Madeira to a far inferior climate, previous to the development of the additional features which were subsequently perceived at Milan, and which may possibly, like their strange gemination, become more visible from prolonged solar influence. The less favourable position of the planet at the next opposition is much to be regretted; but Schiaparelli's experience has warned us that increase of distance may possibly be compensated by improvement in definition: to which we would add on the one hand the constantly verified adage of Sir W. Herschel, that "when an object is once discovered by a superior power, an inferior one will suffice to see it afterwards"; and on the other, the advantage which may be expected from the 18 inches of aperture with which the Italian Government are about to mark their appreciation of their astronomer's ability, and their willingness to enable him to meet the emergency. It will be matter of regret, if in this honourable contest no corresponding preparation should be made among ourselves; though it is difficult indeed to counteract the disadvantage of the English sky. It is not easy to forecast the result; but we think there are indications that possibly the supposed terrestrial analogy has been pushed quite far enough. As to the interesting question of the habitability of Mars by beings like ourselves, it deserves more attention than it perhaps has often received, that none of the supposed correspondence with our own constitution could be maintained excepting on the supposition of a higher internal temperature on the globe of Mars, or possibly a very different composition of atmosphere. We are not so much struck as the author with the progressive diminution of the measured diameter of Mars effected by the employment of modern instruments; at least Schröter's determination by the mode of projection in 1798 scarcely exceeds by 0''.5 that adopted by Newcomb for 1850. Irradiation no doubt is a fact; and a very troublesome one; but we suspect that its effects have been sometimes over-estimated, or mixed up with those of diffraction; and possibly the subject might bear further investigation.

As to the internal heat of Jupiter, so interesting an inquiry ought not to have been left so long in abeyance. If it exists, it would hardly be less capable of detection than that of Arcturus; and the bolometer of Langley seems to offer a fair chance for the discovery. The satellite whose strange reappearance is so difficult of explanation was, it will be found, about to enter on transit instead of suffering occultation. It may be noted, *en passant*, that a telescope must have had a marvellous power of indistinctness, that could show M. Flammarion the third satellite with a disc as large as that of Uranus (p. 409).

It seems a pity that the traditional misrepresentation of the ball of Saturn, at p. 358, as carrying a faint shadow on one side, should still be adhered to; and we may venture to suggest that there is a good deal of inequality in the execution of the diagrams in various parts of the book.

We are confident that the author will not misunderstand our remarks, or hesitate to accept our assurances that they are made in the most friendly spirit. If we are in error, he is fully able to hold his own; and he has our cordial wishes not only for his success on the present occasion, but for the extension of his labours, at no distant time, to a wider review of the glorious works of Nature.

THE ZOOLOGICAL RECORD

The Zoological Record for 1881. Being Vol. xviii. of the "Record of Zoological Literature." Edited by E. C. Rye. (London: John Van Voorst, 1882.)

IT is gratifying to be able to announce that the persevering efforts of the editor of the *Zoological Record* to publish the record of one year's work before the termination of the next year have been at last crowned with success; nor do we doubt but that this very desirable effort will be continued, and indeed become even less difficult with the advance of time, so that through the good will of the Recorders the date of publication will recede backwards from December to September or August in each year, enabling the worker to begin his autumn session with the volume in his hand. The facilities of intercourse are now so great with all parts of the world, that the Transactions and Proceedings really published in the month of December in any one year can be, nay are on our bookshelves in these British Isles, long ere the spring is on its wane, and no doubt long ere 1882 was out, some of the Recorders of this very volume had the record for that year well in hand. However grea may be our expectations for the future we cordially welcome this present volume, and acknowledge that our thanks are due to both Editor and Recorders for what they have already done.

To those who have time for reflection and dare to look back over those eighteen years since Dr. Günther and his friends launched this work upon the world of science, the thought naturally arises of the vastness of the amount of work that is year after year being accomplished without apparently in any way leading to exhaustion. The Editor's own comments are naturally in the volume very few, but how full of meaning is the following: "This volume is 36 pages longer than its predecessor," that is, even the very enumeration of the zoological literature of 1881 requires about 800 closely printed pages; and again we read, "the number of new genera and sub-genera contained in the present volume is 1438"—a simply appalling number. The Insecta are credited with 543, and the Protozoa with 517 of these genera. An enthusiastic zoologist once contemplated the posting up to date of Agassiz's "Nomenclator Zoologicus," that was when the generic increase was some 400 to 500 a year; what would he say or think now of these new births at the average of over 1000 a year. The "examination of this large number of new names, as regards prior occupation," the Editor states was necessarily superficial, we quite sympathise with him; before we read his footnote we rushed into the subject with the A's, but on turning over to page 2 we saw how matters stood and we gave the critical business up at once, and it was obvious at a glance that the greatest genus maker of the year was Ernest Haeckel.

The year 1881 showed a lull so far as the works on recent Mammalia were concerned—at least in comparison with 1880—but the flood of new extinct mammalian forms from North America shows no sign of abatement. In 1881 the lamented Balfour completed his excellent and masterlike treatise on Embryology. The account of the Mammalia in Messrs. Salvin and Godman's work on the Biology of Central America has been finished, and Peters and Doria have published an important work on the Mammals of New Guinea.

The contribution to Bird Literature has been considerable, and the year was marked by the appearance of two more volumes of the Catalogue of the Birds in the British Museum (vols. v. and vi.). Among the Reptiles, Batrachians, and Fishes, no work of any very special importance seems to have appeared. Dr. von Martens still records the Mollusca and Crustacea. The record of the former group extends to 108 pages, and of the latter to 38 pages; both are most painstakingly executed.

The literature of the Arachnida is more extensive than usual, and the year's work is marked by the appearance of several important contributions by the Recorder, Holmberg, Karsch, Keyserling, Koch, Pavesi, Simon, and Thorell, so that it is evident that the Arachnid treasures of the world are at last being worked. Among the Myriopods, Cantoni's Monograph of the Lombardy forms seems to call for notice.

The enormous group of Insecta is recorded by Mr. Kirby, with the exception of the Neuroptera and Orthoptera, which fall to the skilled hands of Mr. McLachlan.

The Vermes and Echinoderms are recorded by Prof. Jeffrey Bell; the Cœlenterata by A. G. Bourne and Sydney J. Hickson. It is remarkable that not a single new genus or species of any recent Octactiniæ seems to have appeared in 1881, nor indeed any separate paper on the group. The Sponges and Protozoa have engaged the attention of Stuart O. Ridley; while nothing very striking seems to occur among the Sponge literature. Kent's Manual of Infusoria, and Haeckel's Prodomus of the Radiolaria mark the year; among the Protozoa, the latter work records 483 new genera and 2000 new species—an almost embarassing number of pretty things.

We are truly glad that the importance of this Record is still practically witnessed to by the generous help rendered to the Zoological Record Association by the British Association for the Advancement of Science and by the Grant Committee of the Royal Society.

OUR BOOK SHELF

The Brewer, Distiller, and Wine Manufacturer. (London: J. and A. Churchill, 1883.)

THE little work before us is the first of a series of technological handbooks to be issued by the publishers, "each of which will be complete in itself, will appear in a handy form and at a low price." Practically they will be a re-issue of articles in Cooley's "Cyclopædia of Practical Receipts and Collateral Information in the Arts, Manufactures, Professions, and Trades," with a somewhat fuller treatment and with reference to the more recent developments which have taken place in industrial processes. As this, the first of these handbooks, treats of Alcohol and Alcoholimetry, Brewing and Beer, Cider, Liqueurs and Cordials, Distillation of Alcoholic Liquors, and lastly, Wine and Wine Making, necessarily much of the Encyclopædic form of treatment must remain, when such important industries are discussed in so small a compass.

Though we cannot endorse the statement of the publishers, that each handbook will be *complete* in itself, we are compelled to admit that the Editor has given a remarkably well condensed *précis* of what has been written on industrial fermentation processes.

The first chapter describes the sources of alcohol, its detection in liquids, and its estimation by volume and by weight: numerous tables are given for this purpose. In addition to the usual distillation process, the methods of Balling, Gröning, Brossard-Vidal, Silbermann, Geissler, and others are described; this part of the book must prove of much use to the technologist.

Brewing is described in fifty pages; this is sufficient to show how condensed the treatment of one of the largest industries in the kingdom must necessarily be. Brief though it be the Editor deserves the highest praise for the manner in which he has condensed the vast mass of facts now accumulated in this department of fermentation chemistry. In addition to the description of the English processes of malting, mashing, and fermentation, a brief account is given of the German Lager beer system now almost universal on the continents of Europe and North America. This is supplemented by a large number of elaborate analyses of English and "Lager" beers, showing the characteristic differences in the products of the two methods. Brief though this part of the handbook is, it will be found of interest to the general reader and of value to the practical brewer, who may not have hitherto given much attention to the scientific part of his manufacturing process.

In Chapter V. we have a full account of the mashing and fermentation processes adopted by the distiller and rectifier, including the methods followed by the latter to remove some of the fusel oils and to flavour "still" spirit so as to produce gins, whiskies, &c., of various taste and aroma. A useful feature in this part of the work will be found in the descriptions and drawings of the stills of Coffey, Siemens, Derosne, Laugier, Dorn, Pistorius, Pontifex and Wood, and others; this will be found of much interest to distillers, more especially in our colonies, where technical information is more difficult to obtain than in the old country.

The sixth and last chapter treats of Wine and its Manufacture.

After a brief description of the soils and manures best suited to the culture of the vine, we have an enumeration and description of some of the better known wines, such as Lafitte, Latour, Margaux, Haut Brion, Leoville, and other red wines of the Gironde, and of the white Graves, as Sauterne, Barsac, Château Yquem, Latour, &c.; of the Burgundies, Romanée Conti, Chambertin, Clos Vougeot, Clos St. George, and La Tache, and of some of the wines of the Champagne, Beaujolais and other vine districts of France.

A brief account is given of the so-called Hocks of the Rhine, and those of the valleys of the Moselle, Ahn, and other rivers of Germany. In the description given of wine-making some use is made of the invaluable treatise by Messrs. Dupré and Thudichum ("On the Origin, Nature, and Varieties of Wine," Macmillan), a work which will amply repay the technologist who consults it.

In conclusion we may add that this little book, though far from being "complete," or exhaustive of any one subject it treats of, is yet compiled with great care and discrimination by the Editor, and will be found of much value by those unable to consult larger treatises or original papers. C. G.

Il Potentiale Elettrico nell' Insegnamento Elementare della Elettrostatica. Per A. Serpieri, Prof. di Fisica nella Università e nel Liceo Raffaelo di Urbino. (Milano, 1882.)

THIS treatise is an elementary exposition of the theory of the Potential in its application to Electrostatical Phenomena. It is founded, as we learn from the preface, on the author's lectures at the Raphael Lyceum of Urbino; and is intended for the use of the Lyceums and Technical institutes of Italy. It is well known to all who interest themselves in such matters that a promising young school of physicists has recently been springing up in Italy, and that those who wish to be abreast of their time can no longer neglect the Italian scientific literature. If the treatise of Prof. Serpieri may be taken as a fair specimen of the scientific instruction given in the secondary schools of Italy, it is clear that this harvest of physicists is due in no small degree to careful sowing.

The work deserves its title of Elementary, inasmuch as nothing is demanded of the student beyond a knowledge of elementary geometry and algebra, and a slight acquaintance with trigonometry. The author is mistaken, however, in supposing that an elementary treatment of electrical theory has not hitherto been attempted; for the English work of Cumming, published some six years ago, is almost identical in its aims with his own. Although Cumming's treatise is an excellent one in many ways, we cannot help thinking that the Italian one is better fitted for the purposes of elementary instruction. Prof. Serpieri appears to us to have happily kept the middle way alike between poverty and redundance of matter, and between excess of mathematical and excess of merely experimental detail.

In the first four chapters are developed the relation between potential and charge, and the theory of lines of force and equipotential surfaces. The fifth, sixth, and seventh chapters contain the theory of capacity, of electrostatic induction, and of the measure of potential. The eighth chapter contains a short sketch of the centimetre-gramme-second system of units, now universally adopted in accordance with the decision of the Electrical Congress at Paris; farther details on this all-important matter are given in one of the appendices, and a considerable number of numerical examples is provided to familiarise the student with the practical use of the system. The last seven chapters are devoted to the theory of condensers. Not only is the theory explained in a simple and interesting way, but abundance of experimental results and numerical illustrations are given to enable the learner to judge how far the mathematical theory represents the actual facts. The account of the experiments of Villari on the heat developed in the electric spark under various circumstances is interesting, and would probably be new to most English readers.

The main fault we have to find with Prof. Serpieri's work is that he has a tendency to cite second-hand authorities where it would have been quite as easy, more instructive for his youthful readers, and *more just* to give the original sources. Again, why of all the results concerning specific inductive capacity should he quote (p. 69) those of Gordon only, which have been precisely the most questioned, and why on the same page should the results of Boltzmann for the specific inductive capacity of gases not be coupled with the name of their author?

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. 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.]

Hovering of Birds

IN your last number I observe an interesting letter on the "Hovering of Birds," by Mr. Hubert Airy. In that letter he refers to an opinion which I have expressed, that this "hovering" cannot be accounted for by the mere supporting agency of an upward current of air. The writer quotes this opinion as it was expressed in a letter to you (NATURE, vol. x. p. 262). But he does not seem to have read the fuller explanation which I have given on this subject in Chapter III. of the "Reign of Law." To that chapter I must refer your correspondent for an explanation, which shows that hovering can be, and is perpetually accomplished under the ordinary conditions of horizontal currents of air. It is very commonly performed (especially) by the whole tribe of Terns, or sea-swallows, over the surface of the sea, where there are no hills or mountains to deflect aërial currents from the usual horizontal course.

Mr. Airy himself uses words which indicate that this agency of upward currents is quite superfluous. He says: "It is easy to see that a bird, with the exquisite muscular sense that every act of flight demands and denotes, might so adapt the balance of its body, and the slope of its wing-surface to the wind, as to remain motionless in relation to the earth." He prefaces these words by these others: "given such a slant upward current." But no such "gift" is needed. The bird has only to slope his wing-surfaces to the current, and precisely the same effect is produced as if the current had been otherwise "sloped" upwards against a horizontal wing-surface. Mr, Airy's own letter contains an excellent explanation of this correspondence.

Cannes, France, January 29 ARGYLL

WITH respect to Mr. Hubert Airy's interesting note (vol. xxvii. p. 294), I beg to say that I have very often seen the kestrel hovering over the perfectly level meadows of Middlesex with obvious ease, where no undulation of the ground could possibly affect the currents of air. Of the twelve instances Mr. Airy enumerates, I see only six refer to hawks (species undetermined), so this fact must be taken into consideration; the conduct of rooks and crows under such circumstances seems to me to come under quite a different category from that of hawks, and in some instances gulls, thus "prospecting" for their prey. Mr. Airy does not ignore this aspect of the question, but I think that by confusing objective with subjective "hovering" he complicates his theory. HENRY T. WHARTON

39, St. George's Road, Kilburn, N.W., January 27

Action of Light on India-rubber

IT may be in the recollection of some of your readers, that in 1876 I pointed out that the deterioration of ebonite surfaces was due to the combined action of light and air. Some time afterwards it was remarked to me that our laboratory (an old greenhouse) was too light, and as a result all our india-rubber tubes would rapidly deteriorate. This led me to submit some pieces of ordinary black india-rubber to the same treatment as the ebonite in the former experiments. On October 11, 1879, four pieces of caoutchouc connector of 5 mm. internal diameter were taken, two were placed in test-tubes plugged with cotton-wool, and the remaining two inclosed in hermetically sealed tubes. One of the sealed tubes, and one of those plugged with cotton wool were placed in a dark drawer, and the other pair in the laboratory window, with a north aspect, and in such a position that they were not under the influence of direct sunlight in the summer. To-day the specimens were examined. Both the sealed tubes were found to be slightly moist inside, and on opening them an organic odour, like that of an india-rubber shop, was perceived. The caoutchouc which had been exposed to air and light, was covered with a thin brown coating, and on being bent this coating cracked; the end which had been most exposed to the light was rather brittle, and could not be stretched without splitting. The other three specimens were unaltered. All four specimens were slightly acid to test paper, but the quantity of acid was too small to be determined.

Mareck (*Chem. News*, xlvii. 25, from *Zeitschr. für Anal. Chem.* xxi.), has lately recommended the preservation of caoutchouc tubes, by keeping them in water when not in use. This is, no doubt, efficacious in consequence of the exclusion of air.

Cooper's Hill, January 22 HERBERT MCLEOD

A Possible Cause of the Extinction of the Horses of the Post-Tertiary

A TRAVELLER in the Park region of northern Colorado, and the central portion of Wyoming, fifteen years ago, could not fail to notice the immense numbers of skulls and other bones of bisons in districts at that time no longer frequented by these animals. Scattered specimens were to be seen in all directions, some of them bearing marks of bullet and knife which left no doubt as to the agent of destruction. Others were to be found in numbers in localities which suggested that they had been surprised by death while seeking shelter from the weather rather than the human destroyer. In such cases, tumbled and mixed by the scavengers, they were thickly strewed over small areas, and the contour of the surface often was such as to bring them closer together with the movement of water or soil. When asked the cause of the wholesale slaughter, the reply of the natives was almost invariably "the hunters killed a great many, but the most died in the deep snow and cold weather some twenty-five years ago."

The great losses experienced by the cattle men of the Medicine Bow and Elk Mountain region, only a couple of winters ago, are too recent to have been forgotten. The next spring and summer the unfortunate owner found the carcases of his cattle in positions similar to those occupied by the bands of bisons. In small parties they had huddled in sheltered basins or nooks, and some, upheld by the snow through the winter, were still on their feet. Since then these "bone yards" have become similar in appearance to those of earlier date.

Last summer the kindness of Prof. Agassiz enabled me to make some discoveries in the Mauvaises Terres of the eastern slope of the Rocky Mountains which vividly brought to mind the pockets full of recent skeletons. Sections in the Post-Tertiary beds here and there disclosed groups or herds of fossil horses (*Equus*) in circumstances so similar as to leave no alternative to the conclusion that the same causes had filled the bone basins in the olden and in most recent time.

Stripped of the strata above them, the contour of the surface would have been similar, and the old-time Coyotes in their feasting had evidently brought about an equal amount of confusion in the remains. About the time of the deposition of these fossils the horses became extinct. *Why* is still an open question. Such evidence as was gathered there has led to the belief that, in that region at least, occasional "cold waves" of days—perhaps weeks in duration, which deep snows caused, or were the principal causes of the extermination of the horses. Other causes that may be suggested are these: lack of water, and an extended glacial period. A consideration of the character of the deposits, the drainage of the mountains at the time, the absence in these beds of proof of a glacial period affecting them since, and the continued existence in the same locality of other creatures, somewhat less sensitive to the cold, would seem to be sufficient objections to their acceptance. The tradition of the Indians, that there is a winter of terrible destruction to the animals once or twice in the lifetime of a man—say once in about forty years—appears to be confirmed by the testimony of the whites. A few degrees or a few days added to the measure of the "wave," or "blizzard," and a few inches added to the depth of the snow would suffice to sweep the herds from the pastures. Weather of this character is a possibility every winter in the Bad Lands, though we hardly expect it. Apparently the rocks contain evidence of such weather in post-Tertiary times. And it may not have differed so very much from that we are having to-day. S. GARMAN

Cambridge, Mass., U.S., January 12

Suicide of Scorpions

SPEAKING of scorpion suicide, Mr. G. J. Romanes in his "Animal Intelligence writes: "Still I think that so remarkable a fact unquestionably demands further corroboration before we shall be justified in accepting it unreservedly" (p. 225). Some years ago I made some experiments and observations on a smaller and a larger species of scorpion found on the Cape Peninsula. I am unable to ascertain the specific names; the smaller are found beneath the bark of decaying tree-stumps, the larger, which often weigh upwards of seventy grains, are found beneath stones and ant-balls. I have recently resumed these experiments and observations. The conclusion I come to is that neither of these species have any suicidal instinct. Only in one case have I found, after death, any sign of such a wound as the sting might inflict; in this case, though one of the tergal plates showed a largish irregular fracture, the wound did not seem a fresh one, and was dry and apparently skinned over; in this case, too, though I watched the death of the scorpion (caused by the gradual application of heat to the bottom of the glass vessel in which the creature was inclosed), I was not able to detect anything like the act of suicide. I will now briefly describe the nature of my experiments.

1. Condensing a sun-beam on various parts of the scorpion's body. The creatures always struck with the sting round, across, and over the heated spot, and seemed to try and remove the source of irritation.

2. Heating in a glass bottle. As this admits of most careful watching, I have killed some twenty or thirty individuals in this way. The creatures very commonly pass the sting over the body as if to remove some irritant. The poison exudes from the point of the sting and there coagulates.

3. Surrounding with fire or red hot embers. I first took a newspaper, moistened a ring about a foot in diameter with alcohol, and placed a scorpion within the ring. The paper was, by this time, ignited. He walked without hesitation through the fire, and tried to make his escape. I made a ring of red-hot wood-embers, and placed a scorpion in the middle. He pushed his way out, displacing two of the embers. I made a better fire-wall, and put him in the middle again. He crept over the embers. I placed him in the midst of a ring of embers on the flat and much-heated stones of the fire-place. He crept over the embers again, but this time got baked before he could escape.

4. Placing in burning alcohol. I placed a layer of an eighth of an inch of alcohol in a shallow vessel, lit the alcohol, and placed the scorpion in the midst of the burning spirit.

5. Placing in concentrated sulphuric acid. I moistened the bottom of a large beaker with a very thin layer of concentrated sulphuric acid, and put in a scorpion. The creature died in about ten minutes. (I have also tried other strong acids, a concentrated solution of sodium hydrate, and a potassium cyanide solution.)

6. Burning phosphorus on the creature's body. I placed a small pellet of phosphorus near the root of the scorpion's tail, and lit the phosphorus with a touch of a heated wire. The creature tried to remove the phosphorus with its sting, carrying away some of the burning material.

7. Drowning in water, alcohol, and ether.

8. Placing in a bottle with a piece of cotton-wool moistened with benzene.

9. Exposing to sudden light. I have not tried special experiments as to this point, but have, on turning over an ant-ball, suddenly exposed a scorpion, hitherto in complete or almost complete darkness, to the full glare of South African sunshine.

10. Treating with a series of electric shocks.

11. General and exasperating courses of worry.

I think it will be admitted that some of these experiments were sufficiently barbarous (the sixth is positively sickening) to induce any scorpion who had the slightest suicidal tendency to find relief in self destruction. I have in all cases repeated the experiments on several individuals. I have in nearly all cases examined the dead scorpion with a lens. My belief is that the efforts made by the scorpion to remove the source of irritation are put down by those who are not accustomed to accurate observation as efforts at self-destruction. On one occasion I called in one of my servants to watch the death of a scorpion by gradually heating it in a glass bottle. The creature at once began moving its sting across and over its back, upon which my servant exclaimed, "See it is stinging itself." I do not wish to imply that all the cases of alleged scorpion suicide are merely instances of careless observation. All I wish to do in this note is to record my individual experience, and to state clearly that after making a series of observations as carefully as I could on a large number of individuals, I cannot place on record a single instance of clear and unmistakable scorpion suicide.

Rondebosch, January 1 C. LLOYD MORGAN

Mimicry in Moths

I HAVE read with great interest the observations of the Duke of Argyll on Mimicry in Moths. I remember more than one similar occurrence during my travels. The most curious was as follows:—

Whilst in Japan, a messmate brought on board, in an ordinary pot, a beautiful trained shrub with a leaf much resembling that of an orange. It was placed on the ward room table where we all sat, the steward removed it from the table to the top of an harmonium at least three times a day, and I watered it when required, and often examined and admired it; in about eight or ten days it began to show signs of failing; and, thinking it might be infected with spider or green fly, I examined it carefully, and in doing so I disturbed a large green smooth-skinned caterpillar. Like all animals on board ship he soon became a great favourite, and we often asked strangers to point him out and in no case did they succeed.

He always lay along the edge of the leaf, with his head to the point and eat at each bite, exactly the breadth necessary to preserve the contour of the leaf as far as possible, when he reached the point, by a few sharp convulsions he returned to the stem and began another row. When he had finished one half of the leaf he began the other; and when nothing but the centre rib of the leaf was left he eat backwards along the stem. He was the most economical feeder I ever saw, only a very little bit of the centre rib of the leaf was bitten off and fell to the ground, and the hard stem of the leaf was left.

I soon observed that he could assume the exact shade of the leaf he was feeding on, and I frequently shifted him and watched the process.

In due time he assumed the chrysalis form; he partly suspended, partly glued, himself to the stem of the plant and it was very difficult to detect him; but not nearly so difficult as in the caterpillar state.

He remained a very short time in the pupa, and one day I was called by a messmate who informed me that "My beastly bug had hatched out," and at first I thought this was the case, as a beautiful black and gold butterfly was expanding his wings and legs on the table, and soon took wing, but was captured and handed over to our bug collector, who by the way took no interest whatever in the prior stages; he was neither butterfly, moth, nor beetle, so nothing to him.

I went to observe how he had broken out of the sheath and was astonished to find that my chrysalis was safe and sound, the butterfly we had certainly did not come from it. Then where did it come from? We were still in Yokohama harbour, and it was a common occurrence that insects flew off to the ships. But how did a butterfly in the state I saw this one get on the ward room table? I came to the conclusion that the pupa had been attached to the plant or pot; I did not anticipate what took place. In a few days another butterfly, to all appearance the brother of the first one, was seen (but not by me), to emerge from the chrysalis we had at first observed; and I have no doubt the first insect had eluded all our prying, and that there were two caterpillars all the time on the plant.

I do not get NATURE until it is a fortnight old, and I have waited with anxiety to see if any one better able than I am would endeavour to show that mere physical causation is sufficient to account for all the phenomena disclosed by the Duke's admirable observation of the moth.

I look upon the Duke as one of the best observers of Nature we have, and his opinions must carry great weight; and believing as I do that in the Theory of Natural Selection the future existence of our race and all hope of advancement in morality is bound up, I am anxious that his doubts on this subject should not carry weight with others.

I think the whole question lies in this—were either of these caterpillars, or the Duke's moth perfect, or even the most perfect of their kind?

I believe I have had more opportunity of observing cases of mimicry than his Grace has, and I have always found that the individuals vary as much in these forms of life as in any other. At Labuan one of the Engineers of the coal works sent a native out and in half an hour he returned with seven leaf-insects. I had picked one up in my walk from the settlement, and although at first each appeared a perfect leaf to my eye, I soon found great differences between the individuals; some being much better specimens than others—just as all sheep are not sheepish to the shepherd—and I think it is quite possible that not one of these eight insects would deceive the eye of an average natural enemy. Let us suppose that anyone of these were so perfect as a mimic, that it would deceive this enemy, it might be wanting in the advantage of perfect rest whilst under inspection, and thus be detected. It was by the movement of the insect that I was enabled to get the one I picked up. The Duke's moth was betrayed by his "beaded eyes and thorax;" and last of all, there was a small hole in the covering of the bright wings, which the Duke considers one of the mysteries of nature, and through all the mimicry of this moth the Duke with very little trouble detects the imposter; as far as he was concerned, all the effort of nature was wasted. If I may be allowed the paradox, it is only when one has come to see what a botch nature has made of its work that its beauties can be properly appreciated. I admire quite as much the quickness of eye that belongs to the lizard that may have been on the watch to capture the moth; these "mysteries" have gone on together; and where a moth or a lizard failed ever so little it went down whilst its better appointed brother was the fittest to survive. Until the mind has taken in how constant the battle is, how small the advantages must be when the enemy is travelling the same path, it is difficult to resist the feeling of wonder and the desire to account for all by a fiat of creation.

I remember some remarks by the Duke of Argyll in a similar strain, when he observed three water-oozels take the water for the first time. He was struck with the way in which they all dived and swam, so perfectly; but I think he failed to consider this view of the matter—did any one of these surpass the others in the art, even were his advantage so little that the Duke was unable to detect it? if so, then provided he was equal of his brothers in all other respects, he was the fittest to survive; and as we evolutionists only claim little by little; its ordinary phrases are no lean and empty formulæ to me.

Nothing but the conviction that, in the new light thrown on nature by Charles Darwin and his numerous disciples, lies the happiness or misery of our race, would have emboldened me, so indifferently educated for the task, to take up the subject and your time. DUNCAN STEWART

Knockrioch, January 25

Clerk-Maxwell on Stress

CAN any of your readers give me a reference to the note in which Maxwell, commenting on or replying to a correspondent of NATURE, gave his ideas as to the nature of stress in a beam or cord? T.

The Comet

MAY I ask space to make some observations about the orbit of the Great Comet of 1882?

Looking on the many elements published in NATURE, in the *Dunecht Circulars*, and in the *Astronomische Nachrichten*, I find very great differences between one and another. Especially the elliptical elements calculated by Mr. S. C. Chandler, Mr. Frisby, Mr. Kreutz, and Mr. Morrison present periods peculiarly different.

Now this fact can be produced but by two causes; either it may be that the different observers considered different parts of the nucleus as the brightest part; or it may be that the movement of the comet has been much perturbed by some bodies of the solar system.

The first hypothesis is very probable, as you remark in the "Astronomical Column" in NATURE, vol. xxvii. p. 300.

The division of the head in two, and perhaps three portions, is a fact well observed by many astronomers, and well shown in the drawings published by Mr. A. A. Common, Dr. Doberck, and Mr. W. T. Sampson in NATURE, vol. xxvii. pp. 109, 129, and 150.

But I observed that with small magnifying power the appearances of the brightest part of the head maintained always a certain unity, which would not admit great mistakes in the observations. Therefore it seems to me that, unless we suppose considerable and unknown variations in the form of the nucleus, only the difference of appreciation of the point observed can hardly explain such a great, and I say regular, difference between one orbit and another.

I say *regular difference*, because I remark a certain peculiarity.

The first elliptical orbit calculated by Mr. J. C. Chandler, using observations from September 18 to October 20, gave a period of about 4000 years.

Afterwards Mr. Kreutz, using observations from September 8 to November 14, gave a period of 843 years, and lately Mr. Morrison, keeping observations from September 19 to December 11, has an elliptical orbit with only 642·5 years.

This fact induces me to believe that an accurate study of the perturbations of the motion of this comet may be as important as it was for Biela's comet.

It is my purpose to go, as far as I can, through a complete discussion of all the observations, and I shall be very glad if those of your readers, who are possessors of good unpublished remarks both about the appearance and about the positions of the comet, would kindly let me know of them.

E. RISTORI

13, Pembridge Crescent, Bayswater, W., January 30

The Aurora of November 17, 1882

I SHOULD like to ask H. J. H. Groneman whether he tried to find out if a curved path for the auroral beam would agree better with the observations than a straight one; because, if it was purely an auroral phenomenon, we should naturally expect its path to be a curve, maintaining a uniform height above the surface of the earth, and to be approximately a small circle having its centre at the magnetic pole, this being the ordinary position of the auroral arches. Of course the motion of the parts of the arch is often not exactly in this direction, because the arch has frequently a transverse motion in addition to the movements that take place longitudinally; and if there was any such transverse motion in the case of this beam, that would prevent its moving strictly along a parallel of magnetic latitude, though it is hardly likely it would deviate far from it. It would be well to ascertain whether such a motion would not agree better with the observations of the beam than Dr. Groneman's hypothesis that it was in a straight line; for the establishment of a curved motion would do away with the idea that the phenomenon was caused by a meteor.

In the other cases cited by Dr. Groneman of supposed meteoric masses passing through our atmosphere and producing auroral effects, the paths, so far as given, seem all to have been approximately along the parallels of magnetic latitude, which circumstance militates against their having had anything to do with meteors, because these traverse the atmosphere in all directions, and would be just as likely to go in a northerly or southerly direction as in an easterly or westerly one. Possibly, however, Dr. Groneman's theory may be that meteors only produce an auroral effect when they happen to go in such directions as may be calculated to produce it.

Sunderland, January 29 THOS. WM. BACKHOUSE

As Dr. Groneman in his most interesting paper on the phenomenon of November 17 asks for my authority for the Swedish observation, I may say that I merely saw it in the "Notes" in NATURE (vol. xxvii. p. 113). There seems a misprint in that statement, however, as "Eskibstuna, fifty-four miles south of Stockholm" would be in the sea, whereas Eski*l*stuna is fifty-four miles *west* of Stockholm.

As the spectroscope observation is said to put the auroral nature of the "spindle" beyond doubt, I would observe that until we know that gas excited by the passage of particles through it at fifteen miles a second does not give the same spectrum as when incandescent by an electric discharge, the observation of certain lines cannot prove anything of the exciting cause. Further, a good deal of the light might be reflected sunlight, as that would be scattered over the whole spectrum, and would thus be masked by the faint diffused spectrum of the moonlight at the time. W. M. F. PETRIE

Bromley, Kent

REFERRING to Dr. Groneman's communication, possibly it may be of service to say that at 9 p.m., October 14, 1870, besides some ruddy auroræ, chiefly in the west and north, I saw a band having a very close resemblance to that figured in the illustration, p. 297. It, however, stretched all the way across the sky from west to east, and continued for some time without much apparent alteration in figure or locality. An appointment called me away before it had vanished. HENRY MUIRHEAD

Cambuslang, January 26

The Sea Serpent

I HAVE seen four or five times something like what your correspondent describes and figures, at Llandudno, crossing from the Little Ormes head across the bay, and have no doubt whatever that the phenomenon was simply a shoal of porpoises. I never, however, saw the *head* your correspondent gives, but in other respects what I have seen was exactly the same; the motions of porpoises might easily be taken for those of a serpent; once I saw them from the top of the Little Orme, they came very near the base of the rock, and kept the line nearly half across the bay. JOSEPH SIDEBOTHAM

Erlesdene, Bowdon, January 26

Influence of "Environment" upon Plants

REFERRING to Prof. Thiselton Dyer's letter on the above subject in NATURE (vol. xxvii. p. 82), it may interest your readers to know that I have had several trees of *Acacia dealbata* 30 feet high, in the open air, in flower for ten days past, but not so fully as they will be in a fortnight's time. I have had *Desfontainea spinosa* in flower during the past eight months; this shrub is 6½ feet high, and also in the open air.

Rosehill, Falmouth, January 29 HOWARD FOX

THE PEAK OF TENERIFFE ACTIVE AGAIN

A PRIVATE letter which I have just been privileged to see, from a native lady in Santa Cruz to her sister in this country, tells how the inhabitants of that present capital of Teneriffe had remarked for several months past, that there was no snow on the upper part of the Peak; though all the "Cumbree," or moderately high land over the rest of the island, was whitened with it in the usual manner for the season. But within the past few days, "fire, like three great bonfires" had been seen on the summit of the Peak, and a lava stream had begun to flow down from it.

Now this is interesting both chronologically, and chorographically. Chronologically, I had remarked at p. 150 of my little book "Teneriffe an Astronomer's Experiment," (published in 1858), that the lava eruptions there only break out about once in a century; the last eruption having occurred in 1798, and the previous one in 1703; and now we have one in 1883, but in what part of the mountainous island called Teneriffe has this last eruption appeared?

So far as I can gather from the said private letter, it has issued, if not from the very mouth of the craterlet which forms the tip-top of the Peak, yet from its sides or foot where it stands on a filled up crater of much larger size, otherwise to be looked on as the Peak's proper and effective summit; and it is from that crater's lips that have proceeded all the later, and yet prehistoric, streams of black lava, which score and frill the Peak on every side; and contrast so strikingly with the far more ancient red, and the still more ancient, more abundant, and once hotter yellow streams from the older and larger craters lower down, before ever yet, the Peak, or final cinder heap, was formed.

But though in the Nature-primeval history of the Mountain, the black, unoxydised lava streams of the Peak, were its latest exudations, still nothing more of that kind was locally expected to occur there within the human period. This was partly because no addition to them had been made since the Spanish Conquest; and partly because the lava outflow of 1798 avoided the Peak, and broke out on the Western side of the general mountain mass, while the eruptions of 1703, which threatened the town of Guimar to the south, and destroyed Garachico to the north, filling up its once beautiful bay—broke forth nearer the sea-level than the peak's top. Whence the idea arose, that the central vent of the peak must have clogged up with time, and that nothing more than its merry little jets of steam and sulphurous acid were to be looked for in that quarter; yet now we are told of red hot lava pouring forth.

Nevertheless on the whole, and in the long course of time, the forces of the grand old volcano may be dying out. For in an earlier work than any other that I had ever met with before about Teneriffe, I have lately read a very different account of the average state of the summit crater, to what it has been in, ever since the days of modern travelling and visitation began.

The book I allude to, in the possession of the Earl of Crawford and Balcarres, is an exquisitely illuminated MS. volume in vellum, by the Chevalier Edmund Skory, of the date of about 1582, and dedicated to that name so dear to all the students of Natural Science, viz.:

"Sir Frances Bacon,
"the knower and lover of all good Arts."

The very first dipping into its old MS. pages brought out a quaint proof of its antiquity, by its involuntary allusions to Garachico, as a city that was necessarily the island's chief delight and glory; the seat of its Government, the abode of its commerce, the place of all its shipping, and of course, because it was so prosperous, destined to live a queen for ever, and to be the joy of all peoples. Yet it is now, and has been for nearly two centuries as deserted as another Tyre; hardly fit to be the habitation of foxes, a mere howling wilderness of black rocks, for a few fishermen to spread their nets upon.

This happily preserved author then in the Earl's valuable library, who had abundant experience of Teneriffe more than a century previous to Garachico's Herculaneum fate, speaks of—

"Great stones being, with noyse, fyre and smoke, many "times cast forthe" out of the craterlet on the top of the peak.

Also that, "On the sommer time the fyers doe ofte breake forth from out the hole in the topp of this hill; into which, if you throw a great stone, it soundeth as if a great weight had fallen upon infinite store of hollow Brasse."

C. PIAZZI SMYTH

JOHANN BENEDICT LISTING

ONE of the few remaining links that still continued to connect our time with that in which Gauss had made Göttingen one of the chief intellectual centres of the civilised world has just been broken by the death of Listing

If a man's services to science were to be judged by the mere number of his published papers, Listing would not stand very high. He published little, and (it would seem) was even indebted to another for the publication of the discovery by which he is most widely known. This is what is called, in Physiological Optics, *Listing's Law*. Stripped of mere technicalities, the law asserts that if a person whose head remains fixed turns his eyes from an object situated directly in front of the face to another, the final position of each eye-ball is such as would have been produced by rotation round an axis perpendicular alike to the ray by which the first object was seen and to that by which the second is seen. "Let us call that line in the retina, upon which the visible horizon is portrayed when we look, with upright head, straight at the visible horizon, the horizon of the retina. Now any ordinary person would naturally suppose that if we, keeping our head in an upright position, turn our eyes so as to look, say, up and to the right, the horizon of the retina would remain parallel to the real horizon. This is, however, not so. If we turn our eyes straight up or straight down, straight to the right or straight to the left, it is so, but not if we look up or down, and also to the right or to the left. In *these* cases there is a certain amount of what Helmholtz calls "wheel-turning" (*Rad-drehung*) of the eye, by which the horizon of the retina is tilted so as to make an angle with the real horizon. The relation of this "wheel-turning" to the above-described motion of the optic axis is expressed by Listing's law, in a perfectly simple way, a way so simple that it is only by going back to what we might have thought nature should have done, and from that point of view, looking at what the eye really does, and considering the complexity of the problem, that we see the ingenuity of Listing's law, which is simple in the extreme, and seems to agree with fact quite exactly, except in the case of very short-sighted eyes." The physiologists of the time, unable to make out these things for themselves, welcomed the assistance of the mathematician. And so it has always been in Germany. Few are entirely ignorant of the immense accessions which physical science owes to Helmholtz. Yet few are aware that he *became* a mathematician in order that he might be able to carry out properly his physiological researches. What a pregnant comment on the conduct of those "British geologists" who, not many years ago, treated with outspoken contempt Thomson's thermodynamic investigations into the admissible lengths of geological periods!

Passing over about a dozen short notes on various subjects (published chiefly in the Göttingen "*Nachrichten*"), we come to the two masterpieces, on which (unless, as we hope may prove to be the case, he have left much unpublished matter) Listing's fame must chiefly rest. They seem scarcely to have been noticed in this country, until attention was called to their contents by Clerk-Maxwell.

The first of these appeared in 1847, with the title *Vorstudien zur Topologie*. It formed part of a series, which unfortunately extended to only two volumes, called *Göttinger Studien*. The term Topology was introduced by Listing to distinguish what may be called qualitative geometry from the ordinary geometry in which quantitative relations chiefly are treated. The subject of knots furnishes a typical example of these merely qualitative relations. For, once a knot is made on a cord, and the free ends tied together, its nature remains unchangeable, so long as the continuity of the string is maintained, and is therefore totally independent of the actual or relative dimensions and form of any of its parts. Similarly when two endless cords are linked together. It seems not unlikely, though we can find no proof of it, that Listing was led to such researches by the advice or example of Gauss himself; for Gauss, so long ago as 1833, pointed out their connection with his favourite electromagnetic inquiries.

After a short introductory historical notice, which shows that next to nothing had then been done in his subject, Listing takes up the very interesting questions of Inversion (*Umkehrung*) and Perversion (*Verkehrung*) of a geometrical figure, with specially valuable applications to images as formed by various optical instruments. We cannot enter into details, but we paraphrase one of his examples, which is particularly instructive:—

"A man on the opposite bank of a quiet lake appears in the watery mirror perverted, while in an astronomical telescope he appears inverted. Although both images show the head down and the feet up, it is the dioptric one only which:—if we could examine it:—would, like the original, show the heart on the left side; for the catoptric image would show it on the right side. In type there is a difference between inverted letters and perverted ones. Thus the Roman V becomes, by inversion, the Greek Λ; the Roman R perverted becomes the Russian Я; the Roman L, perverted and inverted, becomes the Greek Γ. Compositors read perverted type without difficulty:—many newspaper readers in England can read inverted type. * * * The numerals on the scale of Gauss' Magnetometer must, in order to appear to the observer in their natural position, be both perverted and inverted, in consequence of the perversion by reflection and the inversion by the telescope."

Listing next takes up helices of various kinds, and discusses the question as to which kind of screws should be called right-handed. His examples are chiefly taken from vegetable spirals, such as those of the tendrils of the convolvulus, the hop, the vine, &c., some from fir-cones, some from snail-shells, others from the "snail" in clock-work. He points out in great detail the confusion which has been introduced in botanical works by the want of a common nomenclature, and finally proposes to found such a nomenclature on the forms of the Greek δ and λ.

The consideration of double-threaded screws, twisted bundles of fibres, &c., leads to the general theory of paradromic winding. From this follow the properties of a large class of knots which form "clear coils." A special example of these, given by Listing for threads, is the well-known juggler's trick of slitting a ring-formed band up the middle, through its whole length, so that instead of separating into two parts, it remains in a continuous ring. For this purpose it is only necessary to give a strip of paper one *half*-twist before pasting the ends together. If three half-twists be given, the paper still remains a continuous band after slitting, but it cannot be opened into a ring, it is in fact a trefoil knot. This remark of Listing's forms the sole basis of a work which recently had a large sale in Vienna:—showing how, in emulation of the celebrated Slade, to tie an irreducible knot on an endless string!

Listing next gives a few examples of the application of his method to knots. It is greatly to be regretted that this part of his paper is so very brief; and that the opportunity to which he deferred farther development seems never to have arrived. The methods he has given are, as is expressly stated by himself, only of limited application. There seems to be little doubt, however, that he was the first to make any really successful attempt to overcome even the preliminary difficulties of this unique and exceedingly perplexing subject.

The paper next gives examples of the curious problem: —Given a figure consisting of lines, what is the smallest number of *continuous* strokes of the pen by which it can be described, no part of a line being gone over more than once? Thus, for instance, the lines bounding the 64 squares of a chess-board can be drawn at 14 separate pen-strokes. The solution of all such questions depends at once on the enumeration of the points of the complex figure at which an odd number of lines meet.

Then we have the question of the "area" of the projection of a knotted curve on a plane; that of the number of interlinkings of the orbits of the asteroids; and finally some remarks on hemihedry in crystals. This paper, which is throughout elementary, deserves careful translation into English very much more than do many German writings on which that distinction has been conferred.

We have left little space to notice Listing's greatest work, *Der Census raümlicher Complexe* (Göttingen *Abhandlungen*, 1861). This is the less to be regretted, because, as a whole, it is far too profound to be made popular; and, besides, a fair idea of the nature of its contents can be obtained from the introductory chapter of Maxwell's great work on Electricity. For there the importance of Listing's Cyclosis, Periphractic Regions, &c., is fully recognised.

One point, however, which Maxwell did not require, we may briefly mention.

In most works on Trigonometry there is given what is called *Euler's Theorem about polyhedra*:—viz. that if S be the number of solid angles of a polyhedron (not self-cutting), F the number of its faces, and E the number of its edges, then

$$S+F=E+2.$$

The puzzle with us, when we were beginning mathematics, used to be "What is this mysterious 2, and how came it into the formula?" Listing shows that this is a mere case of a much more general theorem in which corners, edges, faces, *and regions of space*, have a homogeneous numerical relation. Thus the mysterious 2, in Euler's formula, belongs to the two regions of space:—the one inclosed by the polyhedron, the other (the *Amplexum*, as Listing calls it) being the rest of infinite space. The reader, who wishes to have an elementary notion of the higher forms of problems treated by Listing, is advised to investigate the modification which Euler's formula would undergo if the polyhedron were (on the whole) ring-shaped:—as, for instance, an anchor-ring, or a plane slice of a thick cylindrical tube. P. G. T.

CLAUDE BERNARD

UNDER the title of "Notes et Souvenirs sur Claude Bernard," Prof. Jousset de Bellesme, of the School of Medicine of Nantes, has published an interesting sketch of the life and labours of the great French physiologist, his master, which those who are admirers of Claude Bernard will be glad to have their attention called to. The essay was meant for the opening address to be delivered at the commencement of the present session of the Nantes School. It seems to have been a little too outspoken to meet with the approbation of the director of the school. On the representation of a majority of the professors of the school, it was forbidden to be delivered *ex cathedrâ* by the Minister of Public Instruction, in an Order dated October 28, 1882. In the pages of the November number of the *Revue Internationale des Sciences biologiques*, the address appeals in type to a wider audience than the assembled professors and pupils of the School of Nantes. Commencing with an extremely graphic account of the author's first introduction to Claude Bernard, which concludes as follows:—"With a kind gesture of his head he bid me attend his laboratory; I thanked him, and was retiring. Just as I was about to close the door, he, taking his attention off his experiment, turned his eyes upon me and said, 'Have you read Descartes' "Discours de la Méthode?" Read it, and read it again." At the time of this interview Claude Bernard was in his forty-fifth year, and a great number of his striking works had been achieved. Having assisted for many years with astonishment at the apparently inexhaustible series of discoveries, Bellesme ventured to ask him one day, what was the secret which enabled him to penetrate so easily into things hidden from others. "Do not seek for a mystery," said Bernard, "nothing can be simpler, or less mysterious. My secret is open to all. When I was a young man, I lived greedily on the writings of Descartes. His 'Discourse' always completely satisfied my soul, and I was passionately fond of it. His rules appeared to me so just, that I came to the conclusion that by a strict observance of them all questions might be solved. That is all." The most important of these rules, Bellesme reminds his readers, is as follows:—"Ne recevoir jamais aucune chose pour vraie qu'on ne la connaisse évidemment être telle, éviter soigneusement la Précipitation et la Prévention dans ses jugements." The author, then, in a very striking manner, draws a series of comparisons between Descartes and Cl. Bernard. Passing from this, he criticises somewhat severely the tendency of a modern school, which without taking notice of the complexitiness of biological phenomena, seem to have culminated in the idea that no contagious disease can be conceived of which has not some special microbe as its cause; but the disciples of this school, he urges, have not meditated on the third rule of Descartes: "Conduire par ordre ses pensées, en commençant par les objets les plus simples et les plus aisés à connaître, pour monter peu à peu comme par degrés jusqu'à la connaissance des plus composés."

We are afforded a little glimpse of the private life of the great French physiologist, which explains a sadness about his domestic relations—possibly not understood by many of his foreign admirers and friends. Married late in life—and even in his very youth never having had much place in his mind for love—still his agreeable and quiet character, his inexhaustible kindness, his open frank cordiality, which so often secured the sympathy of others, seemed to promise an abiding union between him and his wife, but the liberal ideas of the husband, and his devotion to his very peculiar studies, did not please Madame Bernard. The state of things became irritable—intolerable; even the birth of two children did not improve the condition of affairs. In 1869 the separation came. The husband and the father was left alone; and from then to the end of his days he lived his solitary life in an apartment in the rue des Écoles, *vis à vis* to the College of France. His life was all too full of work to leave much time for a morbid appreciation of his solitude. Some slight rest was taken each year at the vintage period at Saint Julien, near Villefranche, and he almost every year took part in the French Association for the Advancement of Science, an Association which he assisted in founding, and of which he was the first president. During these latter ten years Bellesme was his very constant visitor, his trusty friend. They were times not to be recalled, he tells us, without emotion, and he regards them as among the happiest of his life. Often he would spend the evening with him by his fire-side in the small bedroom, where by preference he would pass the afternoon, and which his old servant would keep with a quite canonical neatness. In the background was the bed with its curtains of blue damask, to the left the fireplace; at the side of the bed, a large armchair in which Claude Bernard would sit enveloped in a dressing-gown, which, on his ample shoulders, took the folds and plaits of an ancient toga; his head covered with a cap, which he would often remove while talking, with an action peculiar to him, as if his thoughts made him find it too tight. Close to him, opposite the fire, a small square table, on which the lamp is placed amidst a mountain of reviews, *brochures*, new books sent to him from all parts. At this epoch of his life he read, however, but little, nor did he write much. The volumes, which were published during these last ten years, were composed of extempore lectures of his, very carefully edited. "With our feet on the fender," writes Bellesme, "our conversation would begin with the striking events of the day, but speedily we turned to physiology. This was almost the sole object of the master's thoughts. About this he would wax eloquent, and speedily we would be entering on the higher regions of the science. These were charming excursions on the very mountain-tops, with the clear light of his mind illuminating all the dark valleys." No wonder that time was little thought of, or often altogether forgotten.

Up to 1865 Claude Bernard's health was excellent. About then he was attacked by an ill-defined chronic enteritis, from which, after eighteen months, he had only recovered. After this he had some rheumatic attacks, which did not frighten his friends, as he still preserved an alas deceitful appearance of vigorous health. Still nothing seemed to presage his approaching end. Towards the last days of 1877, after passing a long morning in the damp and unhealthy laboratory of the College of France, he returned home shivering, and with a feeling of intense uneasiness. The next day nephritis set in; he kept his room, and was not disquieted as to his state, but after a few days it was evident to all that his career was run. On February 7, 1878, after a six weeks of suffering, he lost all consciousness, and expired on February 10, at half-past nine o'clock in the evening. In Claude Bernard France lost a noble son, one who cultivated science purely and disinterestedly. His works will not ever perish, and in future years they will serve as a demonstration of the excellence of the "Discours de la Méthode," and as a very sure guide towards arriving at a knowledge of truth.

E. P. W.

THE FINSBURY TECHNICAL COLLEGE

THE Finsbury Technical College and the programme of instruction which we have recently received represent a *fait accompli* of the City and Guilds of London Institute.

Judging of the education to be given in the new College from the Programme forwarded to us, we may congratulate the Council of the Institute on having steered clear of the Scylla and Charybdis which overhang the narrow channel of technical education proper. In all such educational movements, there is the danger that the teaching shall either be too exclusively of the ordinary scientific type, or, by being too distinctly practical, shall attempt to take the place of workshop instruction. Theory and practice promise to be judiciously combined in the new school, and the experiment about to be tried in Tabernacle Row is interesting not only as a new departure in education, but also as showing the effect of beginning science teaching from the practical rather than from the theoretical side, as is still so frequently the case.

During the last three years the conception of the Finsbury College has undergone considerable development, and corresponds now much more nearly to what a technical school should be than appeared probable at its inception. According to the plans published in March, 1880, in the Report to the Governors, the College was to consist in the first place of chemical and physical laboratories only. These laboratories were to be adapted to instruction in various departments of applied chemistry and physics, but no provision was made for the teaching of mechanics, drawing, or of other subjects which find a place in the new programme. Such a school would scarcely have realised the idea of a technical college properly so called, least of all a college for the instruction of artisans. It is doubtful whether many of the pupils who frequent the excellent classes of Prof. Ayrton and Prof. Armstrong are really of the artisan class, for which instruction was originally intended to be given by the City Guilds. The progress that is being made in the completion of the Central Institution at South Kensington, which is expressly intended for the education of a higher class of students, renders it the more important, in order that the two schools may not clash with one another, that the instruction at Finsbury should be not only nominally, but really, of a different grade, and adapted to the improvement of artizans and workpeople.

The programme recently published shows that provision has been made for other branches of industry besides electrical lighting and technical chemistry.

The Technical College, Finsbury, consists really of two distinct schools: a day school and an evening school. It has for its objects the education of—

(1) Persons of either sex who wish to receive a scientific and practical preparatory training for intermediate posts in industrial works.

(2) Apprentices, journeymen, and foremen who are engaged during the day-time, and who desire to receive supplementary instruction in the art practice, and in the theory and principles of science connected with the industry in which they are engaged.

(3) Pupils from middle class and other schools who are preparing for the higher scientific and technical courses of instruction to be pursued at the Central Institution.

The College therefore fulfils the functions of a finishing technical school for those entering industrial life at a comparatively early age; of a supplemental school for those already engaged in the factory or workshop; and of a preparatory school for the Central Institution.

The College embraces the following four chief departments: (1) Mathematical and Mechanical; (2) Physical; (3) Chemical; (4) Applied Art.

It is under the general direction of a principal or superintendent of studies; and the Council of the Institute appear to have acted wisely in asking Mr. Philip Magnus, who has directed the work of the Institute up to the present time with so much ability, and whose exceptional experience of Continental technical schools renders him particularly fitting for such a position—to occupy this post, pending the completion of the Central Institution, and to carry into effect the general scheme of instruction indicated in the programme.

In the day school of the Finsbury College, pupils from middle class and higher elementary schools will have the opportunity of continuing their studies, and of preparing, at the same time, for the particular branch of industry in which they purpose to be engaged.

Such a school is a technical school in the true sense of the word, for it gives the pupil the best training he can receive for his future occupation.

The instruction is not limited to the application of one branch of science only; the future electrician is taught chemistry and mechanics, the chemist is taught mechanics and physics, the mechanician is taught physics and chemistry, and, what is almost equally important, all are taught drawing, French, German, and the manipulation of tools in the workshops.

The evening school is intended for those who are already engaged in practical work, and in this department of the College noteworthy changes have been introduced, with a view of adapting the teaching to the special requirements of artisans. To the courses of Applied Physics and Chemistry originally provided for, courses of Mechanical Engineering have been added; but besides these courses, which are adapted to the higher class of artisans, a complete syllabus of instruction has been added to the programme, suited to the requirements of the special industry of the district of Finsbury, viz. cabinet-making. To provide a systematic course of instruction for cabinet-makers it was necessary to add to the other departments of the College, a Department of Applied Art; and in order to secure a good number of students to start with, the Council affiliated to the College the City School of Art, one of the oldest art schools of the country, and appointed Mr. Brophy as head master.

Moreover, to satisfy the demand of workmen engaged in numerous small industries, the Council have arranged courses of instruction, on a more systematic basis than has been previously attempted in this country, for carpenters, joiners, metal-plate workers, bricklayers, &c., thereby supplying that popular element in the instruction provided by the City Guilds, which at first seemed likely to be wanting in their scheme of technical education.

By undertaking to admit apprentices to the evening classes at half the fees, which are small enough, charged to ordinary workmen, those who have had the direction of the work of the Institute have shown a just appreciation of the importance of encouraging apprentices of fifteen to twenty to follow the evening courses of instruction; for there will be far less difficulty in inducing youths, during their apprenticeship, to attend regular systematically-arranged lessons, covering a period of two or three years, than is generally found in the case of adult workmen.

Indeed, it is in the arrangement of systematised and progressive courses of instruction adapted to various industries and involving the application both of science and of art to the student's occupation, as well as in the practical methods of instruction adopted, that the Technical College, Finsbury, is differentiated from other science schools.

The programme of studies now before us is a publication that can hardly fail to prove useful to all persons who are interested in the establishment of technical schools, and shows unmistakably that the Council of the Institute and their advisers are fully conscious of the difficulties that beset the problem of technical education, and may be trusted to deal judiciously with them in the schools established under their direction.

The fittings of the new College, which are most complete and admirably adapted for practical teaching, have been designed and executed under the direction of Professors Armstrong, Ayrton, and Perry.

ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE[1]

III.

THE determination of H and the measurement of a current in absolute units, can be effected simultaneously by the method devised by Kohlrausch, and described in the *Philosophical Magazine*, vol. xxxix. 1870. This method consists essentially in sending the current to be measured through two coils, of which all the constants are accurately known. One of these is the coil of a standard galvanometer, the other is a coil hung by a bifilar suspension, the wires of which convey the current into the coil. The latter coil rests in equilibrium when no current is passing through it, with its plane in the magnetic meridian. When a current is sent through it, it is acted on by a couple due to electro-magnetic action between the current and the horizontal component of the earth's force, which tends to set it with its plane at right angles to the magnetic meridian; and this couple is resisted by the action of the bifilar. The coil comes to rest, making a certain angle with the magnetic meridian, and as the couple exerted by the bifilar suspension for any angle is supposed to have been determined by experiment, a relation between the value of H and the value of the current is obtained. But, as the same current is sent through the coil of the standard galvanometer, the observed deflection of the needle of that instrument gives another relation between H and C. From the two equations expressing these relations the values of H and C can be found. Full details of the construction of Kohlrausch's apparatus and of the calculation of its constants will be found in the paper above referred to.

In this method it is assumed that the value of H is the same at both instruments, an assumption which for rooms not specially constructed for magnetic experiments cannot safely be made. An instrument which is not liable to this objection has been suggested by Sir William Thomson. A short account of this instrument and its theory will be found in Maxwell's "Electricity and Magnetism," vol. ii. p. 328.

In the application of what has gone before to the graduation of galvanometers, we shall have to deal with the quantities resistance and potential, and in our calculations to measure potentials in volts, resistances in ohms, and currents in amperes. A full explanation of the terms resistance and potential would require a treatise on electricity, but perhaps a very short explanation of what is meant by a volt, by an ohm, and by an ampere may not be here out of place.

Two conductors are at different potentials when, on their being put in contact, electricity passes from one to the other. The difference of potential between them will be made manifest if one of them be connected with an electrically insulated plate which forms one of the scales of a delicate balance, and the other with a second insulated plate parallel to, and at a very small distance from the first plate. If the conductors be at different potentials the plates will attract one another, and the force of attraction may be weighed by means of the balance. With certain arrangements to ensure accuracy, a balance may be constructed by means of which the difference of potentials between two conductors can be measured. Such an instrument has been made by Sir William Thomson, and called by him an Absolute Electrometer.

[1] Continued from p. 108.

It is found experimentally by measuring with a delicate electrometer that, between any two cross-sections A and B of a homogeneous wire, in which a uniform current of electricity is kept flowing by any means, there exists a difference of potentials, and that if the wire be of uniform section throughout, the difference of potentials is in direct proportion to the length of wire between the cross-sections. It is found further that if the difference of potentials between A and B is kept constant, and the length of wire between them is altered, the strength of the current varies inversely as the length of the wire. The strength of the current is thus diminished when the length of the wire is increased, and hence the wire is said to oppose *resistance* to the current; and the resistance between any two cross-sections is proportional to the length of wire connecting them. If the length of wire and the difference of potentials between A and B be kept the same, while the cross-sectional area of the wire is increased or diminished, the current is increased or diminished in the same ratio; and therefore the resistance of a wire is said to be inversely as its cross-sectional area. Again, if for any particular wire, measurements of the current strength in it be made for various measured differences of potentials between its two ends, the current strengths are found to be in simple proportion to the differences of potential so long as there is no sensible heating of the wire. Hence we have the law, due to Ohm, which connects the current C flowing in a wire of resistance R, between the two ends of which a difference of potential V is maintained,

$$C = \frac{V}{R} \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

In this equation the units in which any one of the three quantities is expressed depend on those chosen for the other two. We have defined unit current, and have seen how to measure currents in absolute units; and we have now to show how the absolute units of V and R are to be defined, and from them and the absolute unit of current to derive the practical units—volt, ampere, coulomb, and ohm.

We shall define the absolute units of potential and resistance by a reference to the action of a very simple but ideal magneto-electric machine, of which, however, the modern dynamo is merely a practical realisation. First of all let us imagine a uniform magnetic field of unit intensity. The lines of force in that field are everywhere parallel to one another: to fix the ideas let them be vertical. Now imagine two straight horizontal metallic rails running parallel to one another, and connected together by a sliding bar, which can be carried along with its two ends in contact with them. Also let the rails be connected by means of a wire so that a complete conducting circuit is formed. Suppose the rails, slider, and wire to be all made of the same material, and the length and cross-sectional area of the wire to be such that its resistance is very great in comparison with that of the rest of the circuit, so that, when the slider is moved with any given velocity, the resistance in the circuit remains practically constant. When the slider is moved along the rails it cuts across the lines of force, and so long as it moves with uniform velocity a constant difference of potentials is maintained between its two ends, and a uniform current flows in the wire from the rail which is at the higher potential to that which is at the lower. If the direction of the lines of force be the same as the direction of the vertical component of the earth's magnetic force in the northern hemisphere, so that a blue pole placed in the field would be moved upwards, and if the rails run south and north, the current when the slider is moved northwards will flow from the east rail to the west through the slider, and from the west rail to the east through the wire. If the velocity of the slider be increased the difference of potentials between the rails, or, as it is otherwise called, the electromotive force producing the current, is increased in the same ratio; and therefore by Ohm's law so also is the current. Generally for a slider arranged as we have imagined, and made to move across the lines of force of a magnetic field, the difference of potentials produced would be directly as the field intensity, as the length of the slider, and as the velocity with which the slider cuts across the lines of force. The difference of potentials produced therefore varies as the product of these three quantities; and when each of these is unity, the difference of potentials is taken as unity also. We may write therefore $V = I L v$, where I is the field intensity, L the length of the slider, and v its velocity. Hence if the intensity of the field we have imagined be 1 c.g.s. unit, the distance between the rails 1 cm., and the velocity of the slider 1 cm. per second, the difference of potentials produced will be 1 c.g.s. unit.

This difference of potentials is so small as to be inconvenient for use as a practical unit, and instead of it the difference of potentials which would be produced if, everything else remaining the same, the slider had a velocity of 100,000,000 cms. per second, is taken as the practical unit of electromotive force, and is called one *volt*. It is a little less than the difference of potentials which exists between the two insulated poles of a Daniell's cell.

We have imagined the rails to be connected by a wire of very great resistance in comparison with that of the rest of the circuit, and have supposed the length of this wire to have remained constant. But from what we have seen above, the effect of increasing the length of the wire, the speed of the slider remaining the same, would be to diminish the current in the ratio in which the resistance is increased, and a correspondingly greater speed of the slider would be necessary to maintain the current at the same strength. We may therefore take the speed of the slider as measuring the resistance of the wire. Now suppose that when the slider 1 cm. long was moving at the rate of 1 cm. per second, the current in the wire was 1 c.g.s. unit; the resistance of the wire was then 1 c.g.s. unit of resistance. Unit resistance therefore corresponds to a velocity of 1 cm. per second. This resistance, however, is too small to be practically useful, and a resistance 1,000,000,000 times as great, that is, the resistance of a wire, to maintain 1 c.g.s. unit of current in which it would be necessary that the slider should move with a velocity of 1,000,000 000 cms. (approximately the length of a quadrant of the earth from the equator to either pole) per second, is taken as the practical unit of resistance, and called one *ohm*.

In reducing the numerical expressions of physical quantities from a system involving one set of fundamental units to a system involving another set, as for instance from the British foot-grain-second system, formerly in use for the expression of magnetic quantities, to the c.g.s. system, it is necessary to determine, according to the theory first given by Fourier, and extended to electrical and magnetic quantities by Maxwell, for each a certain reducing factor, by substituting in the formula, which states the relation of the fundamental units to one another in the expression of the quantity, the value of the units we are reducing from in terms of those we are reducing to. For example, in reducing a velocity say from miles per hour, to centimetres per second, we have to multiply the number expressing the velocity in the former units by the number of centimetres in a mile, and divide the result by the number of seconds in an hour; that is, we have to multiply by the ratio of the number of centimetres in a mile to the number of seconds in an hour. The multiplier therefore, or *change-ratio* as it has been called by Professor James Thomson, is for velocity simply the number of the new units of velocity equivalent to one of the old units, and may be expressed by the formula $\frac{L}{T}$, where L is the number of new units of length contained in one of the old, and T is the corresponding number for the unit of time. In the same way the change-ratio for rate of change of velocity or acceleration is $\frac{L}{T}$; and the change-ratio of any other physical quantity may be found by determining from its definition the manner in which its unit involves the fundamental units of mass, length and time. Now the theory of the change-ratios of electrical and magnetic quantities, in the electromagnetic system of units, shows that the change-ratio for resistance is the same as that for velocity; that in fact a resistance in electro-magnetic measure is expressible as a velocity; and hence we may with propriety speak of a resistance of one ohm as a velocity of 10^9 centimetres per second.

It is obvious from equation (14) that if V and R, each initially one unit, be increased in the same ratio, C will remain one unit of current; but that of V be, for example, 10^8 c.g.s. units of potential, or one volt, and R be a resistance of 10^9 cms. per second, or one ohm, C will be one-tenth of one c.g.s. unit of current. A current of this strength—that is, the current flowing in a wire of resistance one ohm, between the two ends of which a difference of potentials of one volt is maintained,—has been adopted as the practical unit of current and called one *ampere*. Hence it is to be remembered one ampere is one-tenth of one c.g.s. unit of current.

The amount of electricity conveyed in one second by a current of one ampere is called one *coulomb*. This unit although not quite so frequently required as the others, is very useful, as, for instance, for expressing the quantities of electricity which a secondary cell is capable of yielding in various circumstances. For example, in comparing different cells with one another their capacities, or the total quantities of electricity they are capable of yielding when fully charged, are very conveniently reckoned in coulombs per square centimetre of the area across which the electolytic action in each takes place.

The magneto-electric machine we have imagined gives us a very simple proof of the relation between the work done in maintaining a current, the strength of the current, and the electromotive force producing it. By the definitions given above of a magnetic pole and a magnetic field, a unit pole must produce at unit distance from itself a magnetic field of unit intensity. Again, unit current is defined as that current which flowing in a wire of unit length, bent into an arc of a circle of unit radius, acts on a unit magnetic pole at the centre of the circle with unit force. Hence, as the reaction of the pole on the current must be equal to the action of the current on the pole, this wire carrying the current is acted on by unit force tending to move it in the opposite direction to that in which the pole is moved, and it plainly does not matter which we suppose held fixed and which moved. Therefore a conductor in a magnetic field, and carrying a unit current which flows at right angles to the lines of force, is acted on by a force tending to move it in a direction at right angles to its length, and the magnitude of this force for unit length of conductor, and unit field, is by the definition of unit current equal to unity.

Applying this to our slider in which we may suppose a current of strength C to be kept flowing, say, from a battery in the circuit, let L be the length of the slider, v its velocity, and I the intensity of the field; we have for the force on the moving conductor the value ILC. Hence the rate at which work is done by the electromagnetic action between the current and the field is $ILC\frac{dx}{dt}$ or $ILCv$, and this must be equal to the rate at which work is done in generating by motion of the slider a current of strength C. But as we have seen above ILv is the electromotive force produced by the motion of the slider. Calling this now E, the symbol usually employed to denote electromotive force, we have EC as the rate of working, that is, the rate at which electrical energy is given out in the circuit.

By Ohm's law this value for the rate of working may be put into either of the two other forms, namely: $\frac{E^2}{R}$, or C^2R. In the latter of these forms the law was discovered by Joule, who measured the amount of heat generated in wires of different resistances by currents flowing through them. This law holds for every electric circuit whether of dynamo, battery, or thermoelectric arrangement.

We have, in what has gone before, supposed the slider to have no resistance comparable with the whole resistance in the circuit. If it has a resistance r, and R be the remainder of the resistance in circuit, the actual difference of potentials between its two ends will not be ILv or E, but $E\frac{R}{R+r}$. The rate per unit of time at which work is given out in the circuit is however still EC, of which the part $EC\frac{r}{R+r}$ is given out in the slider, and the remainder, $EC\frac{R}{R+r}$, in the remainder of the circuit. In short, if V be the actual difference of potentials, as measured by an electrometer, between two points in a metallic wire connecting the terminals of a battery or dynamo, and C be the current flowing in the wire, the rate at which energy is given out is VC, or if R be the resistance of the wire between the two points, C^2R.

One of the great advantages of the system of units of which I have given this brief sketch, is that it gives the value of the rate at which work is given out in the circuit, without its being necessary to introduce any coefficient such as would have been necessary if the units had been arbitrarily chosen. When the quantities are measured in c.g.s. units, the value of EC is given in terms of the centimeter-dyne or *erg*, the recognized dynamical unit of work. Results thus expressed may be reduced to *horse-power* by dividing by the number $7{\cdot}46 \times 10^9$; or if E is measured in volts, and C in amperes, EC may be reduced to horse-power by dividing by 746. Thus, if 90 volts be maintained between the terminals of a pair of incandescent lamps joined in series, and a current of 1·3 ampere flows through these lamps, the rate at which energy is given out in the lamps is approximately ·157 horse-power.

ANDREW GRAY

(*To be continued.*)

NATURAL SCIENCE IN THE OPEN COMPETITIVE EXAMINATIONS FOR CLERKSHIPS (CLASS I.) IN THE CIVIL SERVICE

THE Civil Service Commissioners have done much to encourage the thorough study of natural science in our Universities by the weight which they have assigned to it in the competitive examinations for first-class clerkships in the Government service. These posts are of sufficient value to attract young men of one or two-and-twenty, fresh from the University. It will be seen from the list of marks assigned to subjects, which we print below, that 1000 marks may be made in two branches of natural science, for instance, Zoology and Geology; whilst Greek and Roman language, literature, and history only stand for 1500. Hence a candidate who makes science his strong side and can do something in either English, classical, or foreign literary subjects, is by no means at a disadvantage.

We take this opportunity of prominently drawing attention to the encouragement thus given to the pursuit of natural science as a branch of culture.

The schoolboy who is excused from verse-composition and sent into the chemical laboratory, is distinctly recognised, and has a fair chance given to him by the Commissioners. So too the Oxford undergraduate who breaks with the wearisome iteration of Greek play and Latin odes in the College lecture-room and escapes to the fascinating microscopes and dissecting troughs of Prof. Moseley, or the verniers and milligram-weighing pans of Prof. Clifton, is marked out for patronage. And not only indeed are Oxford and Cambridge students of science thus benefited.

The courses of instruction in scientific subjects given at the London Colleges, University and King's, are pre-eminently such as will enable a candidate to do justice to his abilities in this examination. The examination is practical, and no mere smattering of a subject will obtain any marks for a candidate. Hence the "crammers" are at a disadvantage, and the teachers in duly-organised and properly-furnished laboratories, are rightly encouraged in their efforts to carry on thorough courses of instruction. It is indeed, a matter for satisfaction that hitherto the various cramming establishments where young men are "prepared" for public examinations have failed to enable any candidate to gain a success in any branch of natural science in these higher competitive examinations, those candidates who have scored marks in natural science having been University students. We subjoin an extract from the Regulations issued by the Civil Service Commission, to the secretary of which body application for further information should be made.

1. The limits of age for these situations are 18 and 24, and candidates must be of the prescribed age on the first day of the competitive examination.

2. At the competitive examinations exercises will be set in the following subjects only; the maximum of marks for each subject being fixed as follows, viz.:—

	Marks.
English Composition (including Précis-writing)... ...	500
History of England—including that of the Laws and Constitution...	500
English Language and Literature	500
Language, Literature, and History of Greece	750
" " " Rome	750
" " " France	375
" " " Germany ...	375
" " " Italy	375
Mathematics (pure and mixed)	1250
Natural Science: that is, (1) Chemistry, including Heat; (2) Electricity and Magnetism; (3) Geology and Mineralogy; (4) Zoology; (5) Botany	1000
*** The total (1000) marks may be obtained by adequate proficiency in any two or more of the five branches of science included under this head.	
Moral Sciences: that is, Logic, Mental and Moral Philosophy	500
Jurisprudence	375
Political Economy	375

Candidates will be at liberty to offer themselves for examination in any or all of these subjects. No subjects are obligatory.

No candidate will be allowed any marks in respect of any subject of examination unless he shall be considered to possess a *competent knowledge* of that subject.

NOTES

A TELEGRAM, dated December 21, has been received by the Finnish Academy of Sciences from Prof. S. Lemström, chief of the Finnish Meteorological Observatory at Sodankylä. He states that, having placed a galvanic battery with conductors covering an area of 900 square metres on the hill of Oratunturi, he found the cone to be generally surrounded by a halo, yellow-white in colour, which faintly but perfectly yields the spectrum of the aurora borealis. This, he states, furnishes a direct proof of the electrical nature of the aurora, and opens a new field in the study of the physical condition of the earth. A further telegram, dated Sodankylä, January 5, has been received, in which Prof. Lemström states that experiments with the aurora borealis made December 29, in Enare, near Kultala, on the hill of Pietarintunturi, confirm the results of those at Oratunturi. On that date a straight beam of aurora was seen over the galvanic apparatus. It also appears from the magnetic observations that the terrestrial current ceases below the aurora arc, while the atmospheric current rapidly increases, but depends on the area of the galvanic apparatus to which it seems to be proportional. The Professor regrets that with the means at his disposal further experiments cannot be made, and that he intended, on the 13th inst., to withdraw the apparatus.

THE Report of the Royal Gardens, Kew, for 1881, shows what a large amount of varied and highly useful work is got through in the space of a year at that great national establishment; perhaps *imperial* would be more accurate than national, for it is really the botanical and horticultural centre of the whole empire. One important feature is the lessons given during the year to the young gardeners in the science of these subjects; this will certainly tend to secure that the work of the gardens throughout are conducted with intelligence and on a sound scientific basis. The Report contains extracts from the reports of various Colonial curators, on the progress of experiment in the culture of certain important plants, such as Cinchona and india-rubber. Mr. Jamieson reports from the Nilgiris that he has found the Cape Coast and Liberian coffee-plants to be really two varieties. Queensland may yet add coffee to its other industries, a vastly important addition. The Report contains an illustration of *Cinchona Ledgeriana*, Moens.

IN preparation for the International Fisheries Exhibition there is a large number of artificers now employed in erecting and completing enormous buildings for the reception of the exhibits on the ground known as the Royal Horticultural Gardens, South Kensington. Some four or five immense structures have been already erected, two standing side by side on the western side of the gardens—one being about 180 yards, the other some 140 yards in length, with a width of about 20 yards, and of great height and capacity. Arched roofs contain in the centre, running the whole length of the building, a wide breadth of glass, which throws below as ample an amount of light as can be desired. Other similar buildings are in the course of completion at the north-eastern corner of the gardens, close to the Albert Hall; and when the capacity of all these structures is considered, some estimate can be formed of the enormous proportions the International Exhibition will assume. The arcade at the south-western side of the gardens, well known for the horticultural and other expositions which the Royal Horticultural Society has held in it, is being devoted to the purposes of an aquarium, which will soon be completed, and in which both fresh-water and sea fish will be exhibited. The spacious long arcade affords ample room for all the tanks that may be required, and it is expected that the aquarium will form one of the most attractive features of the exhibition. Arrangements will be made to provide easy access from one building to another, and such portions of the gardens as remain uncovered by the necessary structures will serve as an agreeable promenade. All the works are so forward that everything will be ready in good time for the reception of the exhibits of our own and of foreign countries.

CONSIDERABLE success has attended the Sunday Evening Association, its object being to bring together all persons who, estimating highly the elevating influence of music, the sister arts, literature and science, desire, by means of meetings on Sunday evenings, to see them more fully identified with the religious life of the people. The president is Dr. Geo. J. Romanes, F.R.S. The fifth series of meetings will be concluded next Sunday with a lecture by Dr. W. B. Carpenter, F.R.S., C.B., on "Niagara." A sixth series will be commenced on Sunday, February 11, and will include lectures by Dr. G. J. Romanes, F.R.S., on "Star

Fish;" J. Cotter Morison, M.A., on "A Glimpse of England in the Fifteenth Century;" Dr. P. Martin Duncan, F.R.S., on "Metamorphosis of Insects," and J. Norman Lockyer, F.R.S., on "The Recent Eclipse of the Sun." The meetings are held in the Working Men's College, Great Ormond Street.

THE Report of the Commissioner of the Imperial Japanese Mint, Ôsaka, for the year ending June, 1882, being the twelfth report of the Japanese Mint, shows that the high standard of excellence of the work done at this establishment is still kept up. Rather more gold was coined than during the previous year, viz. 803,645 yen, all in 5 yen pieces; the silver coined during this year was all 1 yen pieces, and amounted to 3,294,988 yen; whilst the nominal value of the copper coins, in 2 sen, 1 sen, and half sen pieces was 1,130,548 yen. The total nominal value of the coins of all denominations struck since the commencement of the Mint to the end of the last financial year is 102,888,478 yen, of which more than one-half is gold and two-fifths silver. Besides this a large number of medals have been struck and refined ingots produced. This year a large number of old bronze guns and field pieces have been melted down, refined, and converted into copper coins, and also additional improvements and economies have been made in the treatment of old Japanese silver coins prior to their re-coinage. The sulphuric acid works in connection with the Mint have been more busy than last year, and nearly a million pounds of acid have been exported to China in addition to that produced for home consumption. The soda works are now in working order, and a considerable outturn of sulphate, black ash, white ash, and crystallised soda has been made; caustic and bicarbonate of soda will shortly be produced, and it is proposed to add works for the production of bleaching powder so as to utilise the whole of the hydrochloric acid formed. There was a considerable increase in the amount of Corean gold dust received during the year, but it was not generally of a high standard. The curve showing the variation in weight of the silver yen issued, as also the report of the trial of the pyx and the reports of the assays on the pyx pieces made by Prof. Chandler Roberts of the Mint in this country, and by Mr. Lawner, of the American Mint, show that the greatest care and attention is given to every department, both by the foreign *employés*, Mr. Wm. Gowland, chemist, assayer, and technical adviser, and Mr. R. MacLagan, engineer, and also by the native officials. The report affords abundant evidence that excellent work is being done by the above-named European technical advisers of the Japanese Government.

WE have received an excellent little pamphlet on "The Rudiments of Cookery, with some Account of Food and its Uses." It is called a manual for the use of schools and homes, is written by "A. C. M.," examiner to the Northern Union of Training Schools for Cookery, dedicated to the Countess of Derby, and published by Simpkin and Marshall. Besides conveying practical information on plain cookery, the writer is careful throughout to explain the why and the wherefore of every point by briefly stating the principles of elementary science which bear upon the subject. We can recommend the pamphlet to the "schools and homes" for whose use it is designed.

AT the meeting of the Royal Geographical Society on Monday evening, Sir Henry Rawlinson, who presided, stated that Mr. Leigh Smith, in acknowledgment of the assistance which the Royal Geographical Society had afforded him in fitting out his expedition, and also to mark the extent of the interest he takes in Arctic discovery, had presented 1000*l*. for the purpose of extended Arctic exploration. Sir Henry referred briefly, also, to the recent services of one of the native explorers which the Indian Government are in the habit of sending beyond the Himalayas, which are closed to Europeans by the jealousy of the natives. The paper from which he quoted said: "One of General Walker's native explorers has returned to India after an absence of four years through Thibet, in which he has obtained a large amount of new geographical information, and has finally disposed of the question of the Sanpo River, which does not, according to him, fall into the Irawaddy, as was generally supposed. The traveller got as far north as Santu, lat. 40° N., 92° E., which is supposed to be the Sorchia of Marco Polo. Returning, he proceeded to Batang, and tried to reach Assam by the direct route, but was stopped at the frontier of the Mishmi country by the assurance that the natives were savages, who would murder him. He, therefore, took a circuitous route to Lhassa, *via* Alanto and Gjamda. But from the latter place he turned and made for Chetang, on the Sanpo, thence by Giangze, Leng, and Phari, to Darjeeling. He reports that Sama is the place where two Europeans coming from Assam were murdered some thirty years ago. If so, it must be Wilcox's Simé, where the priests Kirch and Bsury were murdered in 1854. He is positive he only crossed the Sanpo once at Chetang. He says that on the road from Sama to Gjamda there is a great range of hills to the west, separating the basin of the affluents of the Sanpo, from that of the affluents of the Irawaddy to the east."

LIEUT.-COL. BERESFORD LOVETT, her Majesty's Consul at Astrabad, read at the same meeting a paper, which was illustrated by an itinerary map from his plane table survey of four inches to the mile. The route from Teheran northwards to Asolat is well known, but new ground was traversed between Asolat and the Lur Valley, on the south of Mount Damavand, and again between the Horas River and Firnshuh, and onward to Kurrand, and also between Fulhad Mahala and Shu Kuh. The survey throws considerable light on the untrodden parts of the Elburz Mountains, and on the entire route no part of which had been previously delineated with any approach to accuracy. The author's route was from Teheran to Astrabad, *viâ* Ahar to Sarak, thence to Husan Ikdir, Gutchisir, Wohbad, Towar, and Arsmkern. The route was along the ridge of the Shamran mountain country, which runs south of the Caspian, the author desiring, as the journey was made in the middle of the summer heats, not to descend below 5000 feet, while on the journey an altitude of over 9000 feet was attained, and one mountain 12,500 feet was measured and ascended. The author found in one position a plateau of considerable height full of oyster shells, while in his paper and in the discussion which followed, it was shown that at one geological period the Caspian must have been a sea of very large extent to the north and east.

UNDER the presidency of the Marquis of Exeter, a National Fish Culture Association has been established, its object being to increase the supply of food by increasing the supply of fish of all kinds.

FROM the preliminary report of the Princeton Scientific Expedition (the third of its kind), whose ground was Wyoming, Colorado, and the west, it would seem that the students who formed the party covered a very considerable field, did some good work in geology and natural history, and endured just enough of hardship to give them the feeling of real explorers.

THE trial of the electro-magnetic engine, aërial screw, and bichromate elements constructed by MM. Tissandier for their directing balloon took place in their aëronautical work-shop at Point du Tour, on January 26, before a large number of electricians and aëronauts. It was shown that the twenty-four elements, each of which weighs about six kilogrammes, give during almost three hours a current which rotates a screw of 2·85m. diameter, and about 5 metres of path, with a velocity of 150 turns in a minute. The motive power really developed may be estimated at that of four horses per hour. The weight of all the machinery and elements is a little less than 250 kilogrammes. The real effect on the air can only be found by experiments in the air, but according to measurements taken with a dynamometer of the horizontal tendency to motion, it is about the same as in the experiment tried by Dupuy de Lome. The motive power of Dupuy de Lome having been obtained with eight men working his large screw, whose diameter was 9 metres, it may be inferred that the results in the present case will be more advantageous in the ratio of *two and a half* to *one*. These results are not very powerful when compared with the immense power of aërial currents. But MM. Tissandier have no intention of directing their balloon against strong winds. Their object is to organise an apparatus with which rational experiments may be made in the air, and they have taken advantage of the most recent improvements of science. If their elongated balloon answer their wishes, a real advance will be registered in the history of aëronautics.

EXCAVATIONS are being carried out on Blackheath for the purpose of exposing the "deneholes" which have puzzled geologists and archæologists, and of which we gave some account in vol. xxiii. p. 365.

IN 1884 a general Italian exhibition will be opened at Turin. Among the exhibits will be works in mathematics, physics, and general chemistry.

THE "Treatise on Marine Surveying," reviewed in last week's NATURE, is published by Messrs. Macmillan and Co., and not by Mr. Murray.

THE additions to the Zoological Society's Gardens during the past week include a Mona Monkey (*Cercopithecus mona* ♀) from West Africa, presented by Mr. J. N. Flatau; a Crested Porcupine (*Hystrix cristatus*) from West Africa, presented by Mr. Joseph J. Doke; two Pileated Jays (*Cyanocorax pileatus*) from La Plata, presented by Capt. Gamble; two Grey-breasted Parrakeets (*Bolborhynchus monachus*) from the Argentine Republic, presented by Mr. Tomas Peacock; an European Tree Frog (*Hyla arborea*), European, presented by Mrs. M. B. Manuel; a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from East Africa, a Macaque Monkey (*Macacus cynomolgus* ♀) from India, deposited; a Water Chevrotain (*Hyomoschus aquaticus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—The following are Greenwich times of heliocentric minima of Algol:—

	h. m.		h. m.
February 3,	8 47	Feb. 26,	7 18
17,	16 52	March 12,	15 23
20,	13 41	15,	12 12
23,	10 29	18,	9 1

The light equation (geocentric—heliocentric) in seconds, may be found from the expression—

$$460{\cdot}2\text{s. R. sin } (S + 35^\circ\ 28'\ 7),$$

where R is the earth's radius-vector, and S the longitude of the sun. S Cancri will be at a minimum about the following times:—February 2, at 9h. 40m.; February 21, at 8h. 55m.; and March 12, at 8h. 2m. A minimum of U Cephei occurs on February 5, about 13h. 26m. χ Cygni is at minimum on March 17. This year's maximum of Mira Ceti is not observable. According to the observations of Mr. Knott in 1881 and 1882, a maximum of T Cephei, when the star is about 6·5m., may be expected towards February 17; the position of this variable for 1880 is in R.A. 21h. 7m. 57s., Decl. +68° 0'·1; it is No. 3731 in Fedorenko's catalogue from Lalande.

REPORTED DISCOVERY OF A COMET.—A Reuter's telegram from Puebla, Mexico, January 23, states that a comet had been discovered there near the planet Jupiter, of which no further account has been received at the time we write, nor has a somewhat hurried examination of the vicinity between clouds revealed anything brighter or more cometary in aspect than our very old friend, the first nebula of Messier's catalogue near ζ Tauri, which has proved "a mare's nest" for more than one incipient comet-hunter. Jupiter was close at hand on January 22, but there was a full moon on that date, which hardly favours the suggested explanation. Messier 1, it may be remembered, led to more than a single false alarm when observers were on the look out for Halley's comet in 1835.

THE NEXT RETURN OF D'ARREST'S COMET.—At the sitting of the Paris Academy of Sciences on January 22, M. Leveau communicated elements of the orbit of D'Arrest's comet of short period, for the approaching return to perihelion. He states that on account of the great perturbations suffered by the comet from its passage near Jupiter during the period 1859-1863 (in April, 1861, it passed within 0·36 of the earth's mean distance from the planet), and the want of observations at its third appearance in 1864, it has not been possible to combine in the same system of elements the observations made in 1851 and 1857 with those of 1870 and 1877. He has consequently been obliged to determine the osculating orbit in 1883, from the elements which best represent the observations of 1870 and 1877 alone. The following are the elements of the comet's orbit for 1883, June 12·0, M.T. at Paris:—

	° ′ ″	
Mean anomaly	328 13 20·3	
Longitude of perihelion	319 11 10·8	Mean
,, ascending node ...	146 7 21·0	equinox
Inclination	15 41 47·1	1880·0
Angle of eccentricity	38 46 33·4	
Mean daily sidereal motion ..	530″·65245	

It is M. Leveau's intention to prepare and circulate among astronomers an ephemeris for what appears to be the most likely period during which to obtain observations, or from April 23 to November 25 in the present year, but from the comet's great distance or unfavourable position it is probable that only the largest telescopes will command it. By the above elements the comet will not arrive at perihelion until 1884, January 13·5765 Greenwich M.T.

MERIDIAN OBSERVATIONS OF NEBULÆ.—Dr. Engelmann publishes the positions of about 120 nebulæ, determined with the 6-inch meridian circle of the Leipsic Observatory, and reduced to the beginning of the year 1870, with the mean epoch of observation and the annual precessions, thus aiding by meridian observations the extension of our knowledge of accurate places of these bodies, which has engaged the attention of d'Arrest, Vogel, Schönfeld, Schultz, and others, with equatorial instruments. Valuable material is thus being collected for the investigation of proper motion amongst the nebulæ, which for want of reliable positions in past times, is not practicable at present, except perhaps in a few isolated cases.

ERRATUM.—In last week's "Astronomical Column," p. 300, lines seven and six from bottom, for *Washington* read *Washburn*.

PHYSICAL NOTES

A DOUBLE-ACTION mercury air-pump, invented by Signor Serravalle, who was awarded a gold medal for it at a recent exhibition in Messina, is described in the *Rivista Scientifico-Industriale* (Nos. 21-22). By a simple mechanical method two similar vessels are raised and lowered alternately with each other on opposite sides of a vertical support. A long caoutchouc tube connecting their bottoms lets mercury pass from one to the other. Each has at top a three-way cock; one port of which in a certain position leads into a small open vessel to receive any excess of mercury, and another is connected by means of a caoutchouc tube with a spherical piece fixed laterally about the middle of the vertical support. This piece has three passages, communicating together; two of them are opposite each other, and lead into the tubes from the mercury vessels; the other is connected by tubing to the vessel to be exhausted of air. The three-way cocks at the tops of the vessels are mechanically shifted at the top and bottom of their course by means of a toothed sector and rack in the one case, and a pin and projecting piece in the other.

To observe directly the action of gravity on gases, M. Kraievitsch, of the Russian Chemical Society (*Jour. de Phys.*, December, 1882), sets up two baro-manometers, one on the (low) ground, the other at the top of a high building, or of a hill. The manometric branches are connected by means of a long metallic tube. On rarefying the air in the tube through an adjutage adapted near one of the manometers, the rarefaction is propagated towards the other, but owing to gravity, the lower one always shows a greater pressure than the other. By varying the conditions, the hypsometric formula may be established directly. To ascertain whether gases have or have not a limit of elasticity, two baro-manometers are placed below and connected by separate tubes with the one above. On rarefying through a tube near one of the lower manometers, a limit is reached at which gravity prevents the air of the latter manometer from rising, and it remains stationary while the other continues to fall, if the limit of elasticity exist. The author was fitting up his apparatus for these experiments on a very high old building at St. Petersburg.

IN another paper to the same Society (*loc. cit.*), M. Piltschikoff describes an arrangement for measuring the refractive index of liquids of which one has but small quantities. A hollow lens is filled with the liquid, and with the aid of a graduated scale and a microscope, one measures exactly the focal distance of a monochromatic flame placed at a given distance from the lens. The author gives a simple formula for calculating the index of the liquid, when the constants of the apparatus have been determined once for all. In one set of experiments, the index of glycerine was found = 1·47298, with a probable error estimated at ± 0·00001.

IN the common practice of referring the electromotive force of galvanic combinations to the Daniell element as unit, some difficulty and confusion have arisen from differences in the construction of that element by different physicists. In a recent investigation of this matter (*Wied. Ann.*, 13, 1882), Herr Kittler gives the name of "normal element" to a combination, which is as follows:—Amalgamated, chemically pure zinc, in dilute sulphuric acid of specific gravity 1·075 at 18° C.; and chemically pure copper in concentrated copper sulphate solution of specific gravity 1·190 to 1·200 He finds that the electromotive force of the Daniell element (Zn, H_2SO_4, $CuSO_4$, Cu) increases with percentage proportion of the acid to a maximum occurring at the same place, whether the copper sulphate solution be concentrated or dilute, viz. with 25 to 30 per cent. of the acid; with further hydration of the acid there is decrease. The increase is greater, however, the more dilute the $CuSO_4$ solution used, and greatest with pure water. It is further found that, if very weak acids are used, there is decrease of the electromotive force with dilution of the copper-sulphate solution. Accordingly, there is a degree of concentration of the acid, with which a Daniell element furnishes the same tension, whether the $CuSO_4$ be concentrated or diluted to any extent. The solution in question has the specific gravity 1·011 at 16° C., and is compounded of 750 ccm. H_2O and 100 ccm. dilute H_2SO_4 of sp. gr. 1·007. Herr Kittler compares the action of his "normal element" with that of other practical units.

IN a recent paper to the Vienna Academy (*Wied. Ann.*, 13, 1882), Prof. Stephan describes an investigation of the magnetic screening action (*Schirmwirkung*) of iron (which is exemplified in Thomson's marine galvanometer and the Gramme machine). His experiments were made with hollow iron cylinders and iron rings, and were of three kinds, viz. deflection, oscillation, and induction.

THE sound-vibrations of solid bodies (glass cylinders) in contact with liquids has been lately studied by Herr Auerbach (*Wied. Ann.*, No. 13, 1882). He finds that the *geometrical lowering of tone*, represented by the ratio of the vibration number (n_0) of the empty vessel to that of the same vessel filled with water (n), is smaller the higher the tone of the empty vessel, and greater the narrower the vessel. The *arithmetical lowering of tone* (represented by $(n_0 - n)n_0$) in a vessel of mean pitch, is inversely proportional to the square root of the vibration-number of the empty glass, and (approximately) to that of the number of wave-lengths which the sound of the empty vessel traverses from the wall to the axis. In glasses of different width it is (approximately) inversely proportional to the square root of the width. The specific lowering of tone of a liquid depends primarily on the density, and is greater, the greater this is, though it does not increase so quickly; next, on the compressibility, being greater the smaller this is.

AN INQUIRY INTO THE DEGREE OF SOLUBILITY REQUISITE IN MANURES, WITH SPECIAL REFERENCE TO PRECIPITATED CALCIC AND MAGNESIC PHOSPHATES

SOME remarkable field trials, recently conducted in Scotland by Jamieson and others, have tended to raise serious doubts concerning the correctness of the high relative values, hitherto assigned by chemists to dissolved phosphates, commonly termed super-phosphates, for manurial purposes. We propose, therefore, to examine briefly the action of phosphates in the soil; the conditions under which they become available for the nutrition of plants, and the degree of solubility which, considering these facts, would appear to be most advantageous fot the purposes of the agriculturist. We hope to be able to show the great value of precipitated calcic and magnesic phosphates as manure-ingredients, and to assign some reasons for the comparative neglect which the salts of magnesia have hitherto received from agricultural chemists.

The careful and elaborate series of experiments undertaken by Dr. Voelcker respecting the "solubility of phosphatic materials" may be said to constitute the basis of our present inquiry, as the behaviour of phosphates in water is perhaps the readiest test of their activity as manures. Dr. Voelcker ascertained that one gallon of distilled water will dissolve the following amount of calcic phosphates, derived from the sources quoted:—

	Per gallon.
Estremadura phosphorite...	0·10 grains.
Norwegian apatite	0·44 ,,
Coprolites (mean of Suffolk and Cambridgeshire)	0·62 ,,
Monk's Island phosphate	1·00 ,,
Pure bone ash (from very hard bone)	1·18 ,,
Pure tribasic phosphate of lime, precipitated, burnt and finely ground	2·20 ,,
Guano	2·52 ,,
Pure tribasic phosphate, precipitated and still moist	5·56 ,,

The general deductions arrived at from these experiments, made about fifteen years ago, were that the phosphates in coprolites, apatite, and other phosphatic minerals were very little acted upon by water, and that "for agricultural purposes phosphatic minerals, as well as bone ash, should be treated with a quantity of sulphuric acid sufficient to convert the whole of the insoluble phosphates, therein contained, as completely as possible into soluble combinations. It is a waste of good raw materials to leave much of the insoluble phosphates unacted upon by acid." Broadly speaking, the above may be said to constitute the creed of the agricultural chemist at the present day, and the farmer buys his manure at a relatively high price, per unit of soluble phosphate.

On applying manures containing dissolved phosphates to the soil, nearly the whole of the phosphoric acid is at once neutralised by the various salts present therein, be they lime, alumina, or iron, and the chemist assures us that the superior estimation in which soluble salts are held arises from the property possessed by these salts of becoming rapidly diffused through the soil and precipitated therein, in an extremely fine state of sub-division. Voelcker, in some recent observations on this question, lays down certain propositions which are thus set forth in the abstract of the *Journal* of the Chemical Society, vol. xi. (1881) p. 640. These appear to us to state very clearly and briefly the accepted theories respecting the action of phosphates in manure.

1. Phosphates are not readily taken up by plants in a soluble form, but must be returned to an insoluble condition before they yield their useful properties.

2. The efficacy of insoluble calcium phosphate corresponds with the minuteness of division in which it is found in a manure.

3. The finer the particles in a phosphatic material, the easier it is dissolved in water, and the more energetic its action as a manure. Coarsely-ground coprolites and other minerals are less useful than the same materials in fine powder.

4. Calcium phosphate in porous soft bones is more soluble and energetic than in hard bones, and is more available in bone meal than in crushed bones.

5. Calcium phosphate in crystallised mineral phosphates—Norwegian, Canadian, and Spanish apatites, for example—is less

soluble and energetic than the same amount contained in porous phosphatic materials, such as certain descriptions of phospho-guano.

6. Treatment with acids renders the material completely soluble in water, and the so-formed superphosphate, when put into the ground, is precipitated in a very fine state of division.

7. In the precipitated state the insoluble phosphate is immeasurably more finely divided than it could be obtained by mechanical means, and is consequently more energetic than any raw material mechanically ground.

8. The author's conclusion is that the chemical treatment with acid is the cheapest and best way of rendering mineral phosphates useful for agricultural purposes.

We think that it will be generally admitted that these propositions give a very reasonable statement of the case; but for the purposes of our inquiry we must supplement them with the following additional proposition. This has reference to a matter which has escaped the attention of Dr. Voelcker, but which is strongly supported by the results of the numerous recently-recorded practical trials.

"By reprecipitating the acid in a super-phosphate previous to its employment for agriculture by means of a suitable base, it becomes possible to obtain a neutral phosphate, possessed of a sufficient degree of solubility to be readily distributed through the soil, in an extremely fine state of subdivision, and capable of affording nutriment to the plant under highly favourable conditions."

It is to this further proposition to which we now desire to call special attention, and we may allude first to the assumed loss of the power of spontaneous diffusion through the soil, which is stated by Sibson, in his work on "Artificial Manures," to render the precipitated phosphates inferior in value to soluble acid phosphate. We think that no chemist will doubt that the phosphates in guano are sufficiently soluble to be available for plant food, and precipitated phosphate is certainly more soluble than the earthy phosphates in Peruvian guano. It must be remembered, moreover, when studying the table of solubilities of phosphates, as ascertained by Dr. Voelcker, that these are stated with reference to distilled water, which does not occur in nature, whereas in water containing small percentages of many of the salts, commonly present in the soil, the solubility of phosphates is largely increased. Thus the addition to the water of a trifling amount of ammonic chloride (1 per cent.) increases the solubility of precipitated calcic phosphate fourfold.

This matter has not then received a due share of attention, for, as we have seen, arguing on the analogy of guano, phosphates, in the precipitated form, are undoubtedly so far soluble as to possess the power of diffusion to an extent amply sufficient for agricultural purposes, and there must be a point, short of perfect solubility, which adequately satisfies all requirements in this respect. A careful consideration of the subject has led us to the conclusion that the effect of phosphoric acid added to the soil, after having been fixed by a suitable base, in a condition sufficiently soluble for every need of the plant, and in a state of subdivision far finer than anything which could be obtained by mechanical means, would be in theory, if not superior, at least equal to that of a similar amount of soluble phosphate, applied to a soil promiscuously, in cases in which it is impossible to predict by what bases the phosphoric acid will be fixed, or even whether it will be fixed at all. Indeed, the foregoing considerations would almost lead us to the belief that the employment of such ready-formed compounds as calcic or magnesic phosphates would be preferable to the haphazard use of soluble phosphoric acid in a super-phosphate.

Chemists in treating of the magnesic phosphates appear to have overlooked the dibasic phosphate and to have conducted their experiments and to have founded their observations mainly, if not entirely, on the behaviour of the far less soluble tribasic phosphate. The freshly-precipitated magnesic phosphate is soluble in about 322 times its weight of pure water, while calcic phosphate, as we have seen when newly precipitated, is soluble to the extent of 5·56 grains per gallon. Both of these salts are therefore much more soluble than the earthy phosphates present in guano. We must not overlook the fact also that although it has not yet received much attention, the magnesia would appear to possess in itself considerable manurial value. A recent French authority assigns to it a value approaching 5*s.* 8*d.* per unit, almost three-fourths of the price he sets down for phosphoric acid, and we are convinced from the study of the composition of numerous fertile soils, the ashes of plants, and recent field-trials, that the day is not far distant when the magnesia will rank as high in a manure as a salt of potash.

Another fact which the foregoing considerations have forcibly brought before us is the value of organic matters, in bringing about the solubility of the phosphates. This is perhaps scarcely within our present scope, but we have mentioned, incidentally, that small quantities of ammonia and carbonic acid, dissolved in the water, produce a very marked effect on the solubility of the phosphates. So valuable is their office in this respect, that it seems a false system to deny that organic matter, when present in a manure, possesses any value whatever. It was formerly the practice with agricultural chemists to allow 1*l.* per ton (2·4*d.* per unit) for organic matter, and we think that the important office which it fulfils in supplying carbonic acid for bringing into solution additional quantities of the phosphates, fully justifies the assignment to it of the above valuation.

We have thus endeavoured to explain the true conditions under which phosphoric acid becomes, in the soil, a source of plant-food. We have shown that there must be a limit to the value of solubility, merely considered as a means of securing diffusion through the soil, because partially soluble salts also possess the property to a degree sufficient for all practical purposes. In conclusion we have claimed for a ready-formed, partially-soluble phosphate, in a finely divided condition, and, in the case of the magnesic phosphate, possessing the property of fixing at the same time a portion of the ammonia, a value at least as great as that of a soluble acid phosphate, which runs the risk of being fixed by iron, or alumina (should lime be deficient in the soil), or, which may sink below the roots of the plants before it is neutralised. We trust we have thus shown a good case for a more liberal valuation of precipitated phosphates, and have indicated, with some measure of success, the reasons for the excellent results that have been recently obtained by the use of manures containing phosphoric acid in this form.

THE ELECTROLYTIC BALANCE OF CHEMICAL CORROSION[1]

THIS paper treats of some fundamental points in silver electroplating, and shows how a large amount of the electric power may be wasted by the use of too large a proportion of free potassic cyanide in the plating solution, or by using the liquid in a heated state.

In it is also described a method of ascertaining the degree of energy of chemical corrosion of metals in electrolytes, by means of the strength of electric current per unit of surface necessary to prevent such corrosion; the metals and liquids employed for the purpose in the present research being silver, and solutions of argento cyanide of potassium containing free potassic cyanide. Numerous examples, chiefly in the form of tables, are given of the strength of current required to enter cathodes of a given amount of surface, in order to exactly balance the chemical corrosive effect upon them at atmospheric temperatures, and at higher ones, of solutions of potassic cyanide of various degrees of strength.

The method employed was to take a given solution of cyanide of potassium, pass through it by means of a sheet of platinum anode and a burnished sheet of silver cathode, a weak electric current, and add gradually to the liquid (with stirring) small portions of argento potassic cyanide, until the faintest perceptible deposit of silver occurred. The verge of deposition thus attained was called "the balance point;" and the conditions which determine and influence it, constitute the subject of this research.

The effect of various conditions upon the point of balance of electric and chemical energy were investigated, and the experiments are described. The influences examined were: composition of the liquid, strength of current, size of cathode and density of current, electro-motive force, temperature, ordinary chemical corrosion, nature of the cathode, etc. The circumstances were also investigated which affect the measurement of the current by the method employed in this research, viz. by depositing silver from a solution of argento potassic cyanide; and the sources of error, (and their limit), in that method, are pointed out. The effect of varying the proportions of free potassic cyanide, and of argento potassic cyanide, upon the strength of current at the balance point, are shown in tables of results. The strengths of current just sufficient to prevent all

[1] Abstract of paper by G. Gore LL.D., F.R.S., read before the Birmingham Philosophical Society, Dec. 14, 1882.

corrosion and to deposit the whole of the silver from a solution of argento potassic cyanide of given composition and containing free cyanide are also shown. The influence of varying the proportions both of argento potassic cyanide, and free cyanide of potassium, upon the transfer resistance[1] of the solution, and thereby upon the balance point, are also investigated and the results described.

A number of results and conclusions were arrived at, some of which are as follows:—variation either of the number of battery elements, the proportion of water, of free potassic cyanide, or of argento potassic cyanide, destroys the balance. The effect of altering the proportion of water is opposite with strong solutions to what it is with weak ones. The electric current at the point of balance appears to be entirely conveyed by the free potassic cyanide, and does not divide itself between the two salts until the liquid contains a certain proportion of argentic salt. In strong solutions of potassic cyanide, decreasing the number of battery cells, necessitates more cyanide of silver to restore the balance. The alteration of the point of balance by alteration of proportion of free potassic cyanide cannot be much accounted for by alteration of corrosive power of the liquid. A current from ten Smee's elements is about sufficiently strong to prevent all corrosion of silver at 60° F. in a solution of cyanide of potassium containing a mere trace of argento potassic cyanide. The addition of nitrate, chloride, iodide, or sulphate of potassium to the cyanide solution has but little effect upon the balance point. Variation of strength and of "density" of current affect greatly the point of balance. Greater "density" irrespective of strength of current usually increases the amount of silver deposited. Difference of electro-motive force of current had no conspicuous effect in altering the balance point. Rise of temperature of the liquid acts in two opposite ways, it increases the corrosive action, and by diminishing conduction-resistance it increases the current, and as the latter effect is usually a little stronger than the former one, rise of temperature alters slightly the point of balance, and enables the current to produce a sparing deposit of silver. The ordinary chemical corrosion of silver in a solution of potassic cyanide without an electric current is increased slightly by partial immersion (through capillary corrosion), and greatly by rise of temperature; it is also slightly greater in a weak solution than in a strong one, with solutions of a certain range of strength; and it is distinctly increased by contact with platinum. In consequence of the latter circumstance, a platinum cathode requires a somewhat stronger current than a silver one to enable the point of balance to be attained. In a mixed solution of potassic and argento potassic cyanides, even the smallest proportion of the former salt conveys a portion of the current, and if the cathode is large or the current is sufficiently weak, the whole of it is conveyed by that salt, however much of the double salt is present, an error is thereby introduced when deposition of silver in such a liquid is used as a measure of current. But with a large amount of the double salt, a small amount of potassic cyanide, and a current sufficiently strong, the proportionate amount of error is small. During the act of deposition the cathode surface is not at all corroded, and any deficiency in the weight of deposit is not due to corrosion, but to a portion of the current being conveyed by other ingredients of the liquid than the argentic salt. A current which produces deposition of silver, prevents all corrosion of a silver cathode in the same liquid. The addition of free potassic cyanide to a solution of the double cyanide alters both the resistance and the balance point. The quantity of current diverted from the argentic salt in solution is directly proportional to the amount of free potassic cyanide present, but not always in the same ratio. The presence of a large proportion of free cyanide, together with the employment of a feeble current conduce to the passage of a large amount of current through the liquid without depositing silver; and a current of ·001057 Ampere (which would deposit ·132 grain of silver in two hours) was hardly strong enough to prevent all corrosion or to deposit any silver from a solution composed of 37·5 grains of argento potassic cyanide and 112·5 grains of free cyanide of potassium in three ounces of water. Whilst also a current if sufficiently weak, may traverse a solution of potassic cyanide containing double cyanide, without any of the current decomposing the latter, it cannot traverse a solution of double salt containing free potassic cyanide without some of it traversing the cyanide of potassium. With a very dense current also, a portion of it enters the cathode without depositing silver, and evolves gas.

It requires a much stronger current to balance the corrosion in a hot solution of the two cyanides than in a cold one, and in an instance given, a rise of temperature from 60 to 120° F. was attended by the passage of 21 per cent increase of current without deposition of silver. Addition of free potassic cyanide to a weak solution of the double salt *at the balance point*, first decreases and then increases the current by altering the transfer resistance, probably at the cathode. An amount of current equal to ·14857 Ampere, entering a surface of $\frac{3}{8}$ths. of a square inch, was found to be sufficiently strong to deposit nearly the whole of the silver from a solution at 60° F. composed of 70·11 grains of free potassic cyanide, ·0297 grain of double cyanide, and three ounces of water, the liquid retaining dissolved a little less than that amount of silver at its balance point under those conditions. The strength of current *at the balance point* in a weak solution of potassic cyanide, varies inversely as the amount of silver salt added, and at about eight times the rate. A certain strength of current must enter a given surface of silver in a given liquid under stated conditions in order to prevent all corrosion and produce deposition. The addition of the double cyanide reduces the amount of current conveyed by the free potassic cyanide into the cathode *at the balance point*. Successive additions of double salt to a solution of potassic cyanide *not at the balance point*, first decreases and then increases the current by altering the transfer resistance; it alters the relation of the molecules of potassic cyanide to the cathode so as to diminish their power of transmitting current into that surface without depositing silver. The greater the proportion of double salt present, the greater the tendency to the deposition of silver. Addition of potassic cyanide to a weak solution of the double salt *not at the balance point*, first decreases and then increases the current by altering the transfer resistance at the cathode; in this respect it behaves like addition of the double salt to a weak solution of potassic cyanide. With cathodes of platinum, a solution of potassic cyanide offered less resistance to the current (*not at the balance point*) than one of the double cyanide, but with silver cathodes the reverse effect occurred.

The balance point is a case of equalization of molecular influences, including ordinary chemical corrosion, density of current, nature of cathode, temperature, proportions of water, argento potassic cyanide, free potassic cyanide, and the soluble salts present as impurities, either of which by being disturbed, alters all the others. All these influences also have separate numerical values. A rise of temperature of 60° F. requires an increase of ·000976 Ampere to restore the equipoise. The experiments illustrate the dynamics of electro silver plating; and the method employed in the research is applicable to the detection and measurement of molecular influence in electrolytes. In consequence of the alteration of any one of the conditions having the effect of altering all the remainder, all the above conclusions are limited in their application and are only correct under the conditions given in the paper. The fundamental explanation underlying these conclusions is, that the phenomena are essentially molecular; and that the mere presence and admixture of the double cyanide alters the molecular arrangement of the free cyanide *not at the balance point*, in such a way as to enable the latter to transmit a greater quantity of current into a cathode of given size, notwithstanding its being more diluted by the other salt.

The phenomena of the "balance point" constitute an interesting example of molecular equilibrium, in which the balance point may be compared to a ball suspended by an elastic cord, and having attached to it, a number of other similar cords, each drawing it in a different direction, and all of them being kept in a state of tension. In such a case an alteration of the degree of strain of any one of the cords, changes that of all others, and alters the position of the ball.

The research has a practical bearing both upon the measurement of electric currents by means of deposition of silver from a cyanide solution, and upon the technical process of electro plating. In the former it shows how a large proportion, or even the whole of a current may pass without being measured, and how the error may be reduced to the smallest amount; and in the latter, how a similar waste of current may occur, and how to prevent it.

It is manifest from the foregoing research, that the electrolytic balance of chemical corrosion of cathodes in other depositing solutions, such as those of gold, copper, nickel, etc., might form an extensive subject of experimental investigation.

[1] By transfer resistance is meant the resistance to transfer of the current into the cathode.

Appended Note.—It was constantly found that in using a non-corrodible anode such as platinum, the amount of current passing was very much more easily regulated by varying the size of the anode than that of the cathode, with a corrodible anode however, such as silver, this effect was not observed.

THE ETHER AND ITS FUNCTIONS [1]

II.

Consider the effect of wind on sound. Sound is travelling through the air at a certain definite rate depending simply on the average speed of the atoms in their excursions, and the rate at which they therefore pass the knocks on; if there is a wind carrying all the atoms bodily in one direction, naturally the sound will travel quicker in that direction than in the opposite. Sound travels quicker with the wind than against it. Now is it the same with light: does it too travel quicker with the wind? Well that altogether depends on whether the ether is blowing along as well as the air; if it is, then its motion must help the light on a little; but if the ether is at rest no motion of air or matter of any kind can make any difference. But according to Fresnel's hypothesis it is not wholly at rest nor wholly in motion; the free is at rest, the bound is in motion; and therefore the speed of light with the wind should be increased by an addition of $\left(1 - \frac{1}{\mu^2}\right)$th of the velocity of the wind. Utterly infinitesimal, of course, in the case of air, whose μ is but a trifle greater than 1; but for water the fraction is 7-16ths, and Fizeau thought this not quite hopeless to look for. He accordingly devised a beautiful experiment, executed it successfully, and proved that when light travels with a stream of water, 7-16ths of the velocity of the water must be added to the velocity of the light, and when it travels against the stream the same quantity must be subtracted, to get the true resultant velocity.

Arago suggested another experiment. When light passes through a prism, it is bent out of its course by reason of its diminished velocity inside the glass, and the refraction is strictly dependent on the retardation; now suppose a prism carried rapidly forward through space, say at the rate of eighteen miles a second by the earth in its orbit, which is the quickest accessible carriage; if the ether is streaming freely through the glass, light passing through will be less retarded when going with the ether than when going against it, and hence the bending will be different.

Maxwell tried the experiment in a very perfect form, but found no difference. If all the ether were free there would have been a difference; if all the ether were bound to the glass there would have been a difference the other way; but according to Fresnel's hypothesis there should be no difference, because according to it, the free ether, which is the portion in relative motion, has nothing to do with the refraction; it is the addition of the bound ether which causes the refraction, and this part is stationary relatively to the glass, and is not streaming through it at all. Hence the refraction is the same whether the prism be at rest or in motion through space.

An atom imbedded in ether is vibrating and sending out waves in all directions; the length of the wave depends on the period of the vibration, and different lengths of wave produce the different colour sensations. Now through free ether all kinds of waves appear to travel at the same rate; not so through bound ether; inside matter the short waves are more retarded than the long, and hence the different sizes of waves can be sorted out by a prism. Now a free atom has its own definite period of vibration, like a tuning-fork has, and accordingly sends out light of a certain definite colour or of a few definite colours, just as a tuning-fork emits sound of a certain definite pitch or of a few different pitches called harmonics. By the pitch of the sound it is easy to calculate the rate of vibration of the fork; by the colour of the light one can determine the rate of vibration of the atom.

When we speak of the atoms vibrating, we do not mean that they are wagging to and fro as a whole, but that they are crimping themselves, that they are vibrating as a tuning-fork or or a bell vibrates; we know this because it is easy to make the free atoms of a gas vibrate. It is only in the gaseous state, indeed, that we can study the rate of vibration of an atom; when they are packed closely together in a solid or liquid, they are cramped, and all manner of secondary vibrations are induced. They then, no doubt, wag to and fro also, and in fact these constrained vibrations are executed in every variety, and the simple periodicity of the free atom is lost.

To study the free atoms we take a gas—the rarer the better—heat it, and then sort out the waves it produces in the ether by putting a triangular prism of bound ether in their path.

Why the bound ether retards different waves differently, or *disperses* the light, is quite unknown. It is not easy accurately to explain refraction, but it is extremely difficult to explain dispersion. However, the fact is undoubted, and more light will doubtless soon fall upon its theory.

The result of the prismatic analysis is to prove that every atom of matter has its own definite rate of vibration, as a bell has; it may emit several colours or only one, and the number it emits may depend upon how much it is struck (or heated), but those it can emit are a perfectly definite selection, and depend in no way on the previous history of the atom. Every free atom of sodium, for instance, vibrates in the same way, and has always vibrated in the same way, whatever other element it may have been at intervals combined with, and whether it exists in the sun or in the earth, or in the most distant star. The same is true of every other kind of matter, each has its own mode of vibration which nothing changes; and hence has arisen a new chemical analysis, wherein substances are detected simply by observing the rate of vibration of their free atoms, a branch of physical chemistry called spectrum analysis.

The atoms are small bodies, and accordingly vibrate with inconceivable rapidity.

An atom of sodium vibrates 5×10^{14} times in a second; that is, it executes five hundred million complete vibrations in the millionth part of a second.

This is about a medium pace, and the waves it emits produce in the eye the sensation of a deep yellow.

4×10^{14} corresponds to red light, 7×10^{14} to blue.

An atom of hydrogen has three different periods, viz. 4·577, 6·179, and 6·973, each multiplied by the inevitable 10^{14}.

Atoms may indeed vibrate more slowly than this, but the retina is not constructed so as to be sensible of slower vibrations; however, thanks to Capt. Abney, there are ways now of photographing the effect of much slower vibrations, and thus of making them indirectly visible; so we can now hope to observe the motion of atoms over a much greater range than the purely optical ones and so learn much more about them.

The distinction between free and bound ether is forced on our notice by other phenomena than those of light. When we come to electricity, we find that some kind of matter has more electricity associated with it than others, so that for a given electromotive force we get a greater electric displacement; that the electricity is, as it were, denser in some kinds of matter than in others. The density of electricity in space being 1, that inside matter is called K, the specific inductive capacity. In optics the density of the ether inside matter was μ^2. These numbers appear to be the same.

Is the ether electricity then? I do not say so, neither do I think that in that coarse statement lies the truth; but that they are connected there can be no doubt.

What I have to suggest is that positive and negative electricity together may make up the ether, or that the ether may be sheared by electromotive forces into positive and negative electricity. Transverse vibrations are carried on by shearing forces acting in matter which resists them, or which possesses rigidity. The bound ether inside a conductor has no rigidity; it cannot resist shear; such a body is opaque. Transparent bodies are those whose bound ether, when sheared, resists and springs back again; such bodies are dielectrics.

We have no direct way of exerting force upon ether at all; we can, however, act on it in a very indirect manner, for we have learnt how to arrange matter so as to cause it to exert the required shearing (or electromotive) force upon the ether associated with it. Continuous shearing force applied to the ether in metals produces a continuous and barely resisted stream of the two electricities in opposite directions, or a conduction current.

Continuous shearing force applied to the ether in transparent bodies produces an electric displacement accompanied by elastic resilience, and thus all the phenomena of electric induction.

Some chemical compounds, consisting of binary molecules, *distribute* the bound ether of the molecule, at any rate as soon

[1] A lecture by Prof. Oliver Lodge at the London Institution, on December 28, 1882. Continued from p. 306.

as it is split up by dissociation; and, instead of each nascent radicle or atom taking with it neutral ether, one takes a certain definite quantity of positive, the other the same amount of negative, electricity. In the liquid state the atoms are capable of locomotion; and a continuous shearing force applied to the ether in such liquids causes a continual procession of the matter and associated electricity, the positive one way, and the negative the other, and thus all the phenomena of electrolysis..

What I say about electricity, however, is not to be taken without salt, you will not regard it as recognised truth, but as a tentative belief of your lecturer's which may be found to be more or less, and possibly more rather than less, out of accordance with facts. I can only say that it hangs phenomena together, and that it has been forced upon my belief in various ways.

Now what about the free ether of space, is it a conductor of electricity? There are certain facts which suggest that it is, and Edlund has suggested that it is an almost perfect conductor. When a sun-spot or other disturbance breaks out on the sun, accompanied as it is, no doubt, by violent electric storms, the electric condition of the earth is affected, and we have auroræ and magnetic disturbances. Is this by induction through space? or can it be due to conduction and the arrival of some microscopic portion of a derived current travelling our way?

For my part I cannot think the ether a conductor. Maxwell has shown that conductors must be opaque, and ether is nothing if not transparent; one is driven, then, to conclude that what we call conduction does not go on except in the presence of ordinary matter—in other words, perhaps, that it is a phenomena more connected with bound ether than with free.

But now, looking back to Fresnel's hypothesis of the extra density of the ether inside gross matter, and also to the fact that it must be regarded as incompressible, the question naturally arises how can it be densified by matter or anything else? Perhaps it is not; perhaps matter only strains the ether towards itself, thus slackening its tension, as it were, inside bodies, not producing any real increase of density; and this is roughly McCullagh's form of the undulatory theory. In this form gravitation may be held to be partially explained; for two bodies straining at the ether in this way will tend to pull themselves together. In fact Newton himself pointed out that gravitation could be produced if only matter exerted this kind of strain on all pervading ether, the tension varying as the inverse distance.

He did not follow the idea up, however, because he had then no other facts to confirm him in his impression of the existence of such an ether, or to inform him concerning its properties. We now not only feel sure that an ether exists, but we know something of its properties; and we also have learnt from light and from electricity, that some such action between matter and ether actually occurs, though how or why it occurs we do not yet know. I am therefore compelled to believe that this is certainly the direction in which an ultimate explanation of gravitation and of cohesion is to be looked for.

In thinking over the Fresnel and McCullagh forms of the undulatory theory, with a view to the reconciliation between them which appears necessary and imminent, one naturally asks, is there any such clear distinction to be drawn between ether and matter as we have hitherto tacitly assumed? may they not be different modifications, or even manifestations, of the same thing?

Again, when we speak of atoms vibrating, how can they vibrate? of what are their parts composed?

And now we come to one of the most remarkable and suggestive speculations of modern times—a speculation based on this experimental fact, that the elasticity of a solid may be accounted for by the motion of a fluid; that a fluid in motion may possess rigidity.

I said that rigidity was precisely what no fluid possessed; at rest this is true; in motion it is not true.

Consider a perfectly flexible india-rubber O-shaped tube full of water; nothing is more flaccid and limp. But set the water rapidly circulating, and it becomes at once stiff; it will stand on end for a time without support; kinks in it take force to make, and are more or less permanent. A practicable form of this experiment is the well-known one of a flexible chain over a pulley, which becomes stiff as soon as it is set in rapid motion.

This is called a vortex filament, and a vortex is a thing built up of a number of such filaments. If they are arranged parallel to one another about a straight axis or core, we have a vortex cylinder such as is easily produced by stirring a vessel of water, or by pulling the plug out of a wash-hand basin; or such as are made in the air on a large scale in America, and telegraphed over here, when they are called "cyclones," or "depressions." The *depression* is visible enough in the middle of revolving water. These vortices are wonderfully permanent things, and last a long time, though they sometimes break up unexpectedly.

Vortices need not have straight cores, though they may have cores of various ring forms, the simplest being a circle. To make a vortex ring, we must take a plane disk of the fluid, and at a certain instant give to every atom in the disk a certain velocity forward, graduating the velocity according to its distance from the edge of the disk. We kave as yet no means of doing this in a frictionless fluid, bnt with a fluid such as air and water it happens to be easy; we have only to knock a little of the fluid suddenly out of a box through a sharp-edged hole, and the friction of the edges of the hole does what we want. The central portion travels rapidly forward, and returns round outside the core, rolling back towards the hole. But the impetus sends the whole forward, and none really returns; it rolls on its outer circumference as a wheel rolls along a road. In a perfect fluid it need not so roll forward, as there would be no friction, but in air or water a vortex-ring has always a definite forward velocity, just as a locomotive driving-wheel has when it does not slip on the rails.

We have in these rings a real mass of air moving bodily forward, and it impinges on a face or a gas flame with some force. It is differentiated from the rest of the atmosphere by reason of its peculiar rotational motion.

The cores of these rings are elastic—they possess rigidity; the circular is their stable form, and if this is altered, they oscillate about it. Thus when two vortex rings impinge or even approach fairly near one another, they visibly deflect each other, and also cause each other to vibrate.

The theory of the impact or interference of vortex rings whose paths cross, but which do not come very near together has been quite recently worked out by Mr. J. J. Thomson. It is quite possible to make the rings vibrate without any impact, by serrating the opening out of which they are knocked. The simplest serration of a circle turns it into an ellipse, and here you have an elliptic ring oscillating from a tall to a squat ellipse and back again. Here is a four-waved opening, and the vibrations are by this very well shown. A six-waved opening makes the vibrations almost too small to be perceived at a distance but still they are sometimes distinct.

The rings vibrate very much like a bell vibrates, perhaps very much like an atom vibrates. They have rigidity, although composed of fluid; they are composed of fluid in motion. These vortices, are imperfect they increase in size, and decrease in energy; in a perfect fluid they would not do this, they would then be permanent and indestructible, but then also you would not be able to make them.

Now does not the idea strike you that atoms of matter may be vortices like these—vortices in a perfect fluid, vortices in the ether. This is Sir William Thomson's theory of matter. It is not yet proved to be true, but is it not highly beautiful? a theory about which one may almost dare to say that it deserves to be true. The atoms of matter according to it are not so much foreign particles imbedded in the all-pervading ether as portions of it differentiated off from the rest by reason of their vortex motion, thus becoming virtually solid particles, yet with no transition of substance; atoms indestructible and not able to be manufactured, not mere hard rigid specks, but each composed of whirling ether; elastic, capable of definite vibration, of free movement, of collision. The crispations or crimpings of these rings illustrate the kind of way in which we may suppose an atom to vibrate. They appear to have all the properties of atoms except one, viz. gravitation; and before the theory can be accepted, I think it must account for gravitation. This fundamental property of matter cannot be left over to be explained by an artificial battery of ultra-mundane corpuscles. We cannot go back to mere impact of hard bodies after having allowed ourselves a continuous medium. Vortex atoms must be shown to gravitate.

But then remember how small a force gravitation is. Ask any educated man whether two pound-masses of lead attract each other, and he will reply no. He is wrong, of course, but the force is exceedingly small. Yet it is the aggregate attraction of trillions upon trillions of atoms; the *slightest* effect of each upon the ether would be sufficient to account for gravitation; and no one can say that vortices do not exert some such residual, but uniform, effect on the fluid in which they exist, till second, third, and every other order of small quantities have been taken into account, and the theory of vortices in a perfect fluid worked out with the most final accuracy.

At present, however, the Thomsonian theory of matter is not a verified one, it is, perhaps, little more than a speculation, but it is one that it is well worth knowing about, working at, and inquiring into. It may stand or it may fall, but if it is the case, as I believe it is, that our notions of natural phenomena, though they often fall short, yet never exceed in grandeur the real truth of things, how splendid must be the real nature of matter if the Thomsonian hypothesis turns out to be inadequate and untrue.

I have now endeavoured to introduce you to the simplest conception of the material universe which has yet occurred to man. The conception that is of one universal substance, perfectly homogeneous and continuous and simple in structure, extending to the furthest limits of space of which we have any knowledge, existing equally everywhere. Some portions either at rest or in simple irrotational motion transmitting the undulations which we call light. Other portions in rotational motion, in vortices that is, and differentiated permanently from the rest of the medium by reason of this motion.

These whirling portions constitute what we call matter; their motion gives them rigidity, and of them our bodies and all other material bodies with which we are acquainted are built up.

One continuous substance filling all space: which can vibrate as light; which can be sheared into positive and negative electricity; which in whirls constitutes matter; and which transmits by continuity, and not by impact, every action and reaction of which matter is capable. This is the modern view of the ether and its functions.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Lord Rayleigh has resumed his course of lectures on Electrical Measurements.

Dr. Gaskell's lectures this term deal with the Physiology of the Circulation; Mr. Langley is lecturing on the Physiology of Muscle and Nerve, and the Histology and Pathology of the Secretory Organs.

SCIENTIFIC SERIALS

Transactions of the New York Academy of Sciences, Nos. 2-5, 1881-82.—Outlines of the geology of the North-eastern West India Islands, by Prof. Cleve.—The excavation of the bed of the Kaaterskill, New York, by Dr. Julien.—On the cell-doctrine and the bioplasson doctrine, by Prof. Elsberg.—The discovery of the North Pole practicable, by Commander Cheyne.—The volcanic tuffs of Challis, Idaho, and other western localities, by Dr. Jullien.—The mammoth cave of Kentucky, by Mr. Stevens.—On the determination of the heating-surface required in steam pipes employed to produce any required discharge of air through ventilating chimneys, by Prof. Trowbridge.—On a peculiar coal-like transformation of peat, recently discovered at Scranton, Penn., by Prof. Fairchild.—The parallel drift-hills of Western New York, by Dr. Johnson.—Hypothetical high tides as agents of geological action, by Dr. Newberry.—The international time-system, by Prof. Rees. The moral bearing of recent physical theories, by Prof. Martin.—The discovery of emeralds in South Carolina, by Mr. Hidden.—Obituary notice of Prof. J. W. Draper.—On the behaviour of steam in the steam-engine cylinder, and on curves of efficiency, by Prof. Thurston.—Stereoscopic notes, by Prof. Hines.—A new reversible stereoscope, by Mr. Stevens.—Diphenylamine-acrolein, by Prof. Leeds.

Annalen der Physik und Chemie, No. 1, 1883.—On the radiometer, by E. Pringsheim.—A wave-length measurement in the ultra-red solar spectrum, by the same.—Fluorescence according to Stokes' law, by E. Hagenbach.—The isogyrous surfaces of doubly-refractive crystals; general theory of the curves of like direction of vibration, by E. Lommel.—On the heat-conducting power of liquids, by L. Graetz.—On the ratio of the specific heats in gases and vapours, by P. A. Müller.—The product of internal friction and galvanic conduction of liquids is constant with reference to the temperature, by L. Grossmann.—On M. Guébhard's proposed method of determination of equipotential lines, by H. Meyer.—Further researches on the relation of molecular refraction of liquid compounds to their chemical constitution, by H. Schröder.—On the preservation of oxygen gas in the zinc-gasometer, by J. Loewe.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 11.—"On the Skeleton of the Marsipobranch Fishes. Part I. The Myxinoids (*Myxine* and *Bdellostoma*)." By W. K. Parker, F.R.S. Abstract.

In their cranio-facial skeleton the Myxinoids are very remarkable; where segmentation is perfect in other fishy types there they only exhibit a lattice-work of continuous growth; in the median region of the skull-base, where other types show but little or only temporary distinctness of parts, these fishes develop and retain large independent cartilages.

The lamprey has a large superficial basket-work of soft cartilage (*extra-branchial*), and its gill-pouches keep this related to the rest of the structures of the mouth and throat. But in the Myxinoids the basket-work is *intra-branchial*, and corresponds to the system of segmented arches of the higher Cartilaginous, the Ganoid, and the Osseous fishes. But these non-segmented arches soon lose all relation to the branchial pouches, which are removed so far backwards that they begin under the *twentieth myotome;* whilst the end of the pericardium is under the *fortieth*.

In seeking light upon the primordial condition of the Vertebrata, one naturally looks to such forms as the Myxinoids. For in these types, even in the adult state, there are neither limbs nor vertebræ, and no distinction between head and body, except the beginning, in the head, of a cartilaginous skull; a *continuous structure*—not showing the least sign of secondary segmentation, and by far the greater part of which is in front of the notochord, or axis of the organism. But here our *gradational* work agrees with the *developmental*, for the continuous skull-bars constantly arise before the secondary cartilaginous segments that are found between the myotomes behind the head. Evidently, therefore, the early "Craniata" grew supports to the enlarged and subdivided front end of their neural axis, long before anything beyond strong fibrous septa were developed between the muscular segments of the body. As for the linear growth, the greater or less extension backwards of the main organs—circulatory, respiratory, digestive, urogenital—that, in the evolution of the primary form, was a thing to be determined by the "surroundings" of the type. "Thereafter as *they* may be" was the tentative idea in this case.

Certainly, in the Marsipobranchs and in their relations, the larval "Anura," we have the most archaic "Craniata" now existing; in these the organs may be extended far backwards in a vermiform creature, as in these low fishes, or kept well swung beneath the head—the body and tail together forming merely a propelling organ, as is seen in Tadpoles, especially the gigantic Tadpole of *Pseudis*.

Thus we see that in low limbless types there is no necessity for the development of more than fibrous "metameres"; but the vesicular brain, the suctorial lips, the branchial pouches, and the special organs of sense—these all call for support from some tissue more dense than a mere fibrous mat or web. In the *Myxinoids* we find that *four* special modifications of the connective tissue series are developed for the support of the properly *cephalic* organs, and for them only; thus these fishes are *Craniata*, but are not *Vertebrata;* that is, if we stick to the letter, which of course we do not.

At first some disappointment is felt, after careful study of these types, for, notwithstanding the low level in which they remain, they are mere specialised *Ammocœtes*, keeping on the same "platform" as the larval Lamprey; yet some parts of their organisation do undergo a marvellous amount of transformation, and are, indeed, as much specialised in conformity with their peculiar habits of life *as any Vertebrates whatever, the highest not excepted*.

Yet, on the whole, the Msxinoids are a sort of *Ammocœtine* type, whilst the transformed Ammocœte, the larval Lamprey, comes nearest to the untransformed Frog or Toad—the *Tadpole.* But the mere putting of this shows (suggests at any rate) what *losses* the fauna of the world has sustained during the evolution of the Craniate forms; *now*, the Myxinoids, Petromyzoids, and anurous Amphibia, must all be kept "within call" of each other; but the types that have been culled out between them cannot be numbered. Some other kind of fish are evidently the descendants of primordial "Marsipobranchs," notably *Lepidosteus*, the development of which has been lately studied, and the results are being published in the *Philosophical Transactions*. But the *Chimæroids*, *Dipnoi*, and, still more important, the *Myxinoids* themselves, have still to be followed through their early stages. If the present paper is of any value to the morphologist, one on the embryology of these low forms would be worth much more.

The Myxinoids keep on the low "platform" of the larval Lamprey (*Ammocœte*) in the following particulars, namely:—

a. The notochord has no paired cartilaginous vertebral rudiments in the spinal region.

b. The trabeculæ end in the ethnoidal region, without growing forwards into a cornu (or *two continuous* cornua).

c. There are merely "barbels" round the mouth; no *labial cartilages*.

d. The last character involves this, namely, that the special armature of horny teeth, attached to the labials in the adult *Petromyzon*, is absent.

e. The organs of vision are very feeble, and probably almost useless; in the *Ammocœte* they are arrested for a time.

f. The cranium is a mere *floor*, without side-walls or roof.

The Myxinoids come near to the adult Lamprey in the following particulars, namely:—

a. There are developed outside the skull proper, but not segmented from it, palato-pterygoid and hyoid cartilages.

b. There is a very large median cartilage belonging to both the hyoid and branchial regions.

c. The cranium acquires a floor by the development of a special "hinder intertrabecula."

d. There is a large median cartilaginous olfactory capsule.

The Myxinoids go beyond even the adult Lamprey in the following particulars, namely:—

a. The facial basket-work is much more perfect; and as this is a generalised condition of the true *intra-visceral* system of cartilages, it is a very important character; there is not only an equal development of the "suspensorium," but the *suspensorial part* of the hyoid is developed also (it is suppressed in the Lamprey); and there is, in *Bdellostoma* a large complete first branchial arch, and in both kinds pharyngo-branchial rudiments of the second branchial arch.

b. The respiratory (branchial) pouches are much more specialised by being carried far back under the spine.

c. There is not only a distinct sub-cranial intertrabecula, but also a large pre-cranial or nasal median cartilage of the same nature.

d. The opening of the median olfactory sac is not a mere short membranous passage, but a long tube, encased in a series of cartilaginous (imperfect) rings.

e. Correlated with the non-development of the suctorial labial cartilages, there is an enormous development of the lingual, the basal bar becoming not only double, but, in front, quadruple, and the "supra-lingual" cartilages, which are very small in the Lamprey, and carry only one pair of rows of small second teeth, in the Myxinoids are very large, and carry two pairs of rows of large teeth, with the addition of a median antagonistic "ethnoidal tooth."

Lastly, the greater development of the intra-visceral (= "intrabranchial") cartilages is correlated with the suppression of the extra-visceral basket-work seen both in the larval and adult Lamprey, and also in the larvæ of the "Anura" generally.

January 18.—"On the Skeleton of the Marsipobranch Fishes. Part II. The Lamprey." By W. K. Parker, F.R.S.

The suctorial mouth has its highest development in the Lamprey; in the Myxinoids (*Myxine*), and Bdellostoma, there is no circular disk with horny teeth, but merely an oral fissure surrounded by barbels, and having inside it a huge tongue beset with two oblique rows of recurved and inturned horny teeth, antagonised by a single ethnoidal tooth. In the larva of the Lamprey the mouth is not circular, and the *lower* lip is far back, covered by the *upper*, which is like a hood; there are no teeth of any kind, only moss-like "barbels" or *papillæ* under the upper lip.

In the Tadpole the mouth is suctorial, the *lower* lip being converted into an imperfect ring, which is completed by the *upper* lip. Here the cartilage of the lower lip is not a perfect ring, as in the Lamprey, but is in two parts, and is formed into a sort of *horseshoe*. Inside this compound ring there are sharp horny plates or teeth, and the folds of the lips, all round the mouth, are covered with a horny rasp.

Correlated with the perfectly suctorial *lower* lip of the Lamprey, which, is a *post-oral* structure, entirely, we have the perfectest form of the superficial branchial skeleton, a basket-work of soft cartilage which appears in the early embryo, and only gains enlargement fore and aft, and all its snags and out-growths, after metamorphosis. Besides this there are no rudiments of *internal* branchial arches, such as we find in the Tadpole. The only parts developed *inside* the head-cavities and branchial arches are the generalised and rudimentary mandibular and hyoid arches. In the Tadpole there is no *pier* to the hyoid arch, and the *first cleft* is arrested as a small blind pouch; this state is persistent in the Lamprey. But, after metamorphosis—as the lingering latter part of that profound change of structure—the young Frog and Toad acquire a pier to their hyoid arch, right and left. This, however, does not become functional to the arch, much less assist in supporting the mandible, as a "hyomandibular," but is transformed into an osseo-cartilaginous chain—a *stapedio-incudal* series, specialised correlatively with the expanded rudiment of the first cleft, now enlarged into a *cavum tympani*, with a large "Eustachian opening." The little mandibles of the Tadpole, which served as arms to carry the divided suctorial disk, and lay across the fore face, become very long, and are often hinged on to their pier behind the occiput, and the cartilages of the suctorial disk straighten out and add to the length of the lower jaw in front. These things show how this *temporary* "Petromyzoid," the Tadpole, blossoms out into unthought-of specialisations; it becomes a *quasi-reptile*, worthy of a place far above the Lamprey, and even far above all other *Ichthyopsida*.

Geological Society, January 10.—J. W. Hulke, F.R.S., president in the chair.—T. W. Edgeworth David, the Earl of Dysart, John James Hamilton, Francis Alfred Lucas, and Meaburn Staniland, were elected Fellows, and Dr. Otto Torell, F.C.G.S., of Stockholm, a Foreign Member of the Society.—The following communications were read:—On the Lower Eocene section between Reculvers and Herne Bay, and some modifications in the classification of the Lower London Tertiaries, by J. S. Gardner, F.G.S.—The author noticed Prof. Prestwich's classification of the Lower London Tertiaries, and the introduction by the Survey of the term "Oldhaven Beds" for some of his basement beds of the London Clay. He next discussed the conditions under which the Lower Tertiaries were produced, and showed that throughout the Eocenes there are indications of the close proximity of land and of the access of fresh water. Two types of faunas are to be recognised, namely, those of the Calcaire Grossier and the London Clay, the latter indicating more temperate climatal conditions. The former is represented in England by the Bracklesham series. The areas of these two faunas were separated by land forming an isthmus, as each formation is bounded by a shore-line and separated from its neighbours by freshwater formations; but this isthmus probably shifted its position to the north and south without ever being broken through. A vast Eocene river existed, draining a great continent stretching westward; the indications of this river in Hampshire and Dorsetshire would show it to have been there seventeen or eighteen miles wide.—The Lower Tertiaries have been divided by Prof. Prestwich and the Survey into the marine Thanet beds, the fluviatile, estuarine and marine Woolwich and Reading Beds, and the marine Oldhaven Beds. The mode of occurrence of these was described by the author, with especial reference to the section between Herne Bay and the Reculvers, from his investigation of which he was led to the following conclusions:—The Thanet Sands were probably deposited by a rough sea outside the estuary of the great Eocene river, but within its influence. This area became silted up, rose above the surface, and became covered with shingle and sand. The Thanet Beds closed with a period of elevation, during which the Reading Beds were formed, and this was followed by a subsidence during the Woolwich period, which finally ushered in the Oldhaven and London Clay deposits. The formation of the Oldhaven Beds may be compared with that of the modern beach at Shellness; and during the period of depression the beaches would advance steadily over the flat area of Sheppey, and the earlier formed ones would sink and become covered up by the silt of the great Eocene river. These beaches, forming vast aggregations of sand and shingle between the Thanet Beds and the London Clay, form integral portions of one or other formation, and cannot be recognised as forming a separate formation at all equivalent to the other divisions of the Eocene.—On Mr. Dunn's Notes on the Diamond-fields of South Africa, 1880, by Francis Oates, F.G.S.

Anthropological Institute, January 23.—Anniversary Meeting.—John Evans, V.P., D.C.L., F.R.S., in the chair.—The Treasurer's report and the report of the Council were read and adopted.—The Chairman delivered an address, in which he briefly reviewed the work of the past year, and enlarged on the subject of the antiquity of man, discussing the evidence for and against his existence in Tertiary times.—The following Officers and Council for 1883 were elected:—President, Prof. W. H. Flower, F.R.S. Vice-presidents: Hyde Clarke, John Evans, F.R.S., Francis Galton, F.R.S., Major-Gen. Pitt-Rivers, F.R.S., A. Thomson, F.R.S., E. B. Tylor, F.R.S. Director, F. W. Rudler, F.G.S. Treasurer, F. G. H. Price, F.S.A. Council: J. Beddoe, F.R.S., S. E. B. Bouverie-Pusey, E. W. Brabrook, F.S.A., C. H. E. Carmichael, M.A., W. Boyd Dawkins, F.R.S., W. L. Distant, A. W. Franks, F.R.S., Lieut.-Col. H. H. Godwin-Austen, F.R.S., Prof. Huxley, F.R.S., A. H. Keane, B.A., A. L. Lewis, Sir J. Lubbock, M.P., R. Biddulph Martin, M.P., Henry Muirhead, M.D., J. E. Price, F.S.A., Lord Arthur Russell, M.P., Prof. G. D. Thane, Alfred Tylor, F.G.S., M. J. Walhouse, F.R.A.S., R. Worsley.

Paris

Academy of Sciences, January 22.—M. Blanchard in the chair.—The following papers were read:—On metasulphites, by M. Berthelot.—On selenide of nitrogen, by MM. Berthelot and Vieille.—On the characters of induced currents resulting from reciprocal movements of two magnetic bodies parallel to their axis, by M. du Moncel. Polarisation of an iron core immobilises a certain quantity of magnetism, which thus remains indifferent to exterior magnetic excitation, and is only affected when, being able to act on the inducing body, which over-excites its energy, it may polarise it in its turn, so that action and reaction are in concordance.—On complex units (continued), by M. Kronecke.—Theory of the most general electro-dynamic actions that can be observed, by M. Le Cordier.—On the construction of a dynamo-electric propeller on a long balloon, by M. Tissandier. The system, with a total weight = three men, gives during three hours the work of twelve to fifteen men. The two-vaned propeller (of steel wire and varnished silk) is driven by a small Siemens' dynamo (120 turns of the former to 1200 of the latter); the battery being of thirty-four elements mounted in tension, and divided into four series. An element consists of a vulcanite box (four litres capacity) holding ten zinc and eleven carbon plates. Strong bichromate solution is let in or drawn off by raising or lowering a separate vessel connected by a tube with the battery.—Observations of the transit of Venus at Bragado (Argentine Republic), by M. Perrin. He observed two direct contacts (the second and the fourth), and a certain number of artificial contacts which will supplement the others. The phenomenon was of distinct and well characterised geometrical appearance.—On the approaching return of the periodic comet of d'Arrest, by M. Leveau. He has calculated an ephemerides (which will be communicated to all astronomers) for the period most favourable to observation, viz. April 23 to November 25 this year. Values for the relative brightness are deduced.—Addition to a note on prime numbers, by M. de Jonquières.—On the relations between covariants and invariants of like character, of a binary form of the sixth order, by M. Stephanos.—On the functions of several imaginary variables, by M. Combescure.—On the functions of two variables, by M. Poincaré.—On the curves of the sextant, by M. Gruey.—Mode of distribution among various points of its small supporting base, of the weight of a hard body, of polished and convex surface, placed on an elastic horizontal ground, by M. Boussinesq.—On a communication of MM. Mercadier and Vaschy on consequences deducible from relations between electric magnitudes, by M. Lévy.—Remarks on the expression of electric magnitudes &c. (continued), by MM. Mercadier and Vaschy.—Observations on Dr. Siemens' last paper, by M. Violle.—Photographic positives on paper obtained directly, by MM. Cros and Vorgerand. Paper is covered with a solution of 2 gr. bichromate of ammonia, 15 gr. glucose, and 100 gr. water, is dried, and exposed to light under a positive (*e.g.* a drawing). When the (yellow) bare parts of the paper have become grey, the paper is immersed in a bath of 1 gr. nitrate of silver to 100 gr. of water, with 10 gr. acetic acid. The image appears at once, with reddish tint, produced by bichromate of silver. Drying in light gives a dark brown tint.—On hydraulic silica, by M. Le Chatelier. The only new fact given by M. Landrin (he says) is the non-hydraulicity of silica obtained from manufacture of hydrofluosilicic acid.—On mutual displacements of bases in neutral salts, the systems remaining homogeneous, by M. Menschutkin.—On the causes capable of affecting the amount of ammonia in rain-water, by M. Houzeau. One important consideration is the time that has elapsed between obtaining and analysing; another the monthly quantity of water (the less the rain, the more ammonia present).—On the action of certain metals on oils, by M. Livache. Instead of metallic plates (which M. Chevreul experimented with), he used metals finely divided, as in precipitation, and got much better effects. Of the three, lead, copper, tin—lead acts most strongly. If some of it be moistened with oil and exposed to air, an increase of weight very soon occurs through oxidation, and it is greater the more siccative the oil. A solid and elastic product is formed. The increments of weight with different oils are sensibly proportional to those in fatty acids of the same oils exposed to air several months (cotton-seed oil alone is anomalous; it is siccative, but its fatty acids increase very little in weight). The transformation of the oil is attributed to direct action of the metal, not to that of the air. It suggests a rapid means of distinguishing siccative and non-siccative oils, and an advantageous substitute for the heating of oils.—Calcification of kidneys, parallel to the decalcification of the bones, in subacute poisoning by corrosive sublimate; increase of the proportion of mineral parts of a tibia, following disarticulation of the other tibia, by MM. Prevost and Frutiger.—Physiological action of sulphate of quinine on the circulatory apparatus in men and animals, by MM. Lée and Bochefontaine. It preserves and increases the force of the heart, and is a powerful antipyretic.—Medullary origin of paralyses following cerebral lesions, by M. Couty.—On the lymphatic system of tadpoles, by M. Jourdain.—On the development of the reproductive apparatus of pulmonate molluscs, by M. Rouzaud.—On Suctociliate Infusoria (a reply), by M. de Merejkowsky.—On the morphological nature of the subterranean branches of the root of adult *Psilotum*, by M. Bertrand.—Contribution to the stratigraphic history of the relief of Sinai, and especially on the age of porphyries of that country, by Abbé Raboisson. The last dislocations of the Sinaitic system were posterior to the eocene.

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