THURSDAY, MARCH 23, 1882

TECHNICAL EDUCATION

THE Second Annual Report of the City and Guilds of London Institute for the Advancement of Technical Education is one of great interest. It enables us, for one thing, to see what progress has been made since the issue of the first Report. There can be no doubt that, during the short period that this Institute has been in existence, it has begun effective work on a plan which will commend itself to and command the confidence of those interested in education. The movement has been started with vigour, and very soon we shall probably have a widespread system of technical schools all over the country. In London the City schools belonging to the Institute have been eminently successful, to judge from the increasing attendance, especially upon the physical and chemical classes of Professors Ayrton and Armstrong. The classes of Prof. Ayrton on "Electric Lighting and Transmission of Power" and "Electrical Instrument Making" have been so well attended that it has been necessary to make arrangements for providing additional tutorial assistance, in order that his students might receive the individual attention they require. During the past session, 551 tickets of admission to the evening classes on Technical Physics have been sold, showing a considerable increase on the attendance last year. Dr. Armstrong, Professor of Technical Chemistry, has given special instruction in the subjects of "Coal Tar Distilling" and "Spirit Rectification." The number of tickets sold has been 265. Considerable progress has been made in providing suitable new buildings for these schools, and in adding additional means of instruction and practical laboratory work. Professors Ayrton and Armstrong have been inspecting some of the best technical schools on the Continent, with a view to assist them in organising the institutions in London. Last summer, moreover, the foundation of the great Central College was laid by the Prince of Wales, who is now President of the Institute, at South Kensington, and its construction is being actively proceeded with. In this college, as the Lord Chancellor stated on that occasion, from which the entire work of the Institute will be directed, instruction of a higher and more advanced character will be given, adapted to the wants of those who will be engaged in professional or commercial pursuits, in which a knowledge of some branch of mechanics, physics, or chemistry, in its practical applications, will be found, not only serviceable, but almost indispensable. The building, when completed, will be supplied with laboratories in which the most delicate operations may be carried on; with workshops in which the various branches of mechanical and electrical engineering will be taught; with studios in which applied art may be practised, and with lecture-theatres, and class-rooms in which the principles of science will be explained.

We see from the Report that the candidates for the Institute's certificates and diplomas have greatly increased during the last year. While in the year 1880, 816 candidates were examined in 85 centres, of whom 515 passed; in 1881, 1563 candidates were examined, some of them in two subjects, in 115 centres, of whom 895 passed, 66 of these passing in two subjects. Of the 895 successful candidates, 466 obtained the full certificate, having already passed in pure science as well as in technology. According to the new regulations, the science qualifications for the honours grade are raised, the candidate being required to produce *two* certificates in the *advanced* stage of the Department's examination. The Council attach so much importance to the preliminary training in pure science, that they look forward to the time when they will be able to require all candidates to adduce evidence of adequate scientific knowledge before being admitted to examination in technology. It was thought that the alterations in the regulations might possibly have the effect of lessening the number of students in attendance at the registered classes of the Institute during the present session; but so far is this from being the case, that whilst, according to the returns received in November, 1880, the number of students preparing for examination at 78 centres did not exceed 2500, the returns, received at the central office in November last, show that over 3300 candidates are now receiving instruction in 29 subjects at 115 centres. All this is very gratifying, for unless the candidates undergo a really testing examination both in the principles and practice of their art, unless care be taken to see that practical knowledge is based on a knowledge of scientific principles, we shall be no better off than before, but probably worse. Moreover a considerable increase is shown in the number of teachers who have been placed on the books of the Institute. The new rule which comes into operation after March 30, and which requires, except under special conditions, the candidate to have obtained the Institute's full certificate in honours in order to be registered as a teacher, will, doubtless, prevent the rate of increase of teachers from being as great in the future as it has been during the past two years. But the Council rightly feel that the success of the work in which they are engaged depends, to a great extent, on the efficiency of the teachers who are associated with it, and they think that the time has now arrived when they are justified in requiring from those who wish to be registered as teachers such evidence of their qualifications to impart technical instruction as is furnished by the Institute's Honours certificates, or by a strong recommendation from persons of recognised authority.

This is as it should be, and there is no doubt that the Institute will go on raising their examination standard, till it reaches the highest limit of efficiency as a test of knowledge of the principles and applications of science. The Institute is quite alive to the value of laboratory and workshop practice, and it does all it can to encourage and compel students not to rest content with mere book-knowledge, but to become familiar with the tools and processes themselves. The Institute is anxious to encourage the system in provincial schools, and so far as funds allow are willing to lend instruments for the purpose.

There is such evident anxiety to give both principles and practice equally fair play, that in drawing up the syllabuses of the several examinations the Council have availed themselves of the suggestions and co-operation of manufacturers; and with the view of making the examination a fair and satisfactory test of the candidate's acquaintance with his subject, they have instructed the examiners to make the questions as practical as possible, and are endeavouring to secure the services of two examiners for each subject, one of whom at least shall be actually engaged in manufacture. At the same time it is interesting to notice that of those candidates who have not attended the ordinary registered classes, 47 in all presented themselves from University College, London; the Royal School of Mines; the Yorkshire College, Leeds; the Glasgow Technical College; the St. Mark's Technical College, Grosvenor Square; and from other similar Institutions; and of these 41 succeeded in passing, 23 in the first, and 18 in the second division, the percentage of failures being remarkably less among this than among any other class of candidates. Among changes in the technological examinations, all in the way of improvement, we may note that the subjects have been so arranged as to group together allied industries; examinations in electric lighting, the transmission of electrical energy, and electrical instrument making have been added; more sensible arrangements have been made as to the grades of the examinations; these and several other changes all tend to the efficiency of the examinations as real tests of the attainments of candidates.

From all this it seems clear that the Council of the Institute are impressed with the truth on which we have so often insisted in these columns, that there can be no efficient practice without sound principles, that instruction in the practical applications of science must be based upon a knowledge of the science which is applied, that instruction in the latter must precede instruction in the former, otherwise technical education is little better than the old empirical rule-of-thumb methods. Therefore we are glad to see, as the Lord Chancellor indicated in his speech at the laying of the foundation of the Central College last July, that the aim of the Institution will be to supplement the work of those institutions, especially the Science and Art Department, whose aim is to afford a knowledge of the principles of science and art. There is distinct evidence in the examinations of the new Institute of a gradual tightening of the tests, both for students and those who aim at being technical teachers. At the distribution of the prizes last December Sir Frederick Bramwell said that "the value of these certificates and prizes depends upon the thoroughness of the test that is applied, and it is in the interest of the certificate and prize-holders themselves that the standard of the examination should be maintained, in order that the value of the rewards may be duly appreciated. The Institute's certificates are intended to be regarded as diplomas of efficiency, and with this view they are awarded to those only who give evidence of possessing a practical as well as a theoretical knowledge of the subjects embraced by the examinations. Mere book-learning will not suffice to pass our examinations."

The City Companies have so far been wonderfully liberal in their donations to the Institute, but we hope those which have not contributed will take the advice of the Prince of Wales at the recent meeting, and lose no time in doing so. Compared with what has been spent in the Paris Conservatoire, the sum so far spent by the Institute has been a mere pittance; the City Guilds have ample funds at their command, and they could not spend them on a better object, or one more likely to yield a rich return for the benefit of London and the country generally than in an institution that we hope one day will be comparable to that of Paris. The success already achieved is a guarantee that money devoted to the purposes of the Institute will be well spent.

The Council of the Institute are even already hindered in their work from want of funds; all over the country opportunities occur for starting technical schools in important industrial centres, but this requires a little expenditure on the part of the Institute, to encourage an adequate response from local sources. It would indeed be extremely useful if, in connection with some more of the numerous science schools of the Science and Art Department, a technical School were available for those who desired to learn some of the practical applications of the principles they had learned at the science school. This would greatly help to impress upon the public the natural order of connection between the two departments. In the arrangement for awarding the Holl Scholarships and prizes in connection with the Institute, this order is insisted on, for, among other qualifications of the scholars, they must have passed an examination in mechanics (or physics), mathematics, and chemistry, to the satisfaction of examiners appointed by the Institute. All this seems to us very encouraging; the Institute is yet young, and technical education in the real sense is in this country only a thing of yesterday; but if it be developed along the lines indicated by this report, there is every reason to hope that in time it will become an Institution of the highest national importance.

THE ART OF DINING

Aristology; or, The Art of Dining. By Thomas Walker, M.A. With Preface and Notes by Felix Summerly. 8vo. Pp. 96. (London: George Bell and Sons.)

Food and Feeding. By Sir Henry Thompson, F.R.C.S., &c. 8vo. (London: Frederick Warne and Co.)

THE two dinners which stand out in our memory as events in our life were of very different characters. The one consisted of brown bread and lard, washed down with some rough country wine, and was eaten in the middle of a Tyrolese glacier. The other embraced every delicacy the heart could wish. Our appreciation of the first was due to compulsory fasting for some time previously. Our appreciation of the second was due to its intrinsic merit. In it the dishes seemed to be so arranged that each one stimulated the palate for the one that succeeded it, and the wines given with each course were so selected as to increase the appetite for, and appreciation of, the solids. We then, for the first time in our life, began to realise that cookery was a fine art. In speaking of the fine arts we generally include only those which appeal to the special senses of sight and hearing, such as sculpture, painting, architecture, music, and we rarely think of modes of appealing to the special senses of smell and taste. Yet the latter two are perhaps quite as closely connected with our emotions as the former, and as capable of exciting keen sensations of pain and pleasure. Smell and taste differ from sight and hearing in being much more easily fatigued, and this may partly be the cause of their imperfect cultivation. Another cause is, probably, the closer connection which smell and taste have with the process of nutrition, and the consequent alterations which repeated impressions upon them may have upon the general well being. A man may pass long hours in a picture gallery or concert room, receiving impressions good, bad, or indifferent, without much effect upon digestion or circulation, but a bad odour would quickly excite nausea or sickness. The impressionable natures of Southern Italy object to strong perfumes, even though pleasant. The sense of taste differs in one particular from the other three, viz. that while the agents which excite them may remain outside the body, the substances which excite taste are taken into the body, and thus have an action upon it independently of their mere effect upon the sense itself. In gratifying this sense, therefore, we have to consider not merely what will give the greatest pleasure at the moment, but what will be most satisfactory in its after results. Fortunately, pleasure to the palate usually aids digestion, if obtained in the proper way; but comparatively few people know the art of dining properly themselves, and still fewer know how to give good dinners to their friends.

The two works before us are intended to supply this lacking knowledge, both by giving general rules and special examples. Walker's "Aristology" deals more with the general rules of dining, and especially of dining as a social duty, and Sir Henry Thompson more particularly with the details of food and cookery. In discussing food, the latter author makes some very sound remarks regarding the excessive amount of butcher's meat eaten by Englishmen, and its injurious consequences. In the working classes it leads to wasteful extravagance, although the manual labour which they have to undergo may lessen its deleterious effect upon their health. In the upper classes, where its price has but little effect upon the purse, its injurious action upon the body is increased by want of exercise, and tends, as the author truly says, to shorten or embitter life. The food of middle class Englishmen might be rendered not only much more palatable, but much more healthy, by the introduction of larger proportions of fish, vegetables, and farinaceous substances, as well as by greater variations in the modes of preparation. Both these subjects are well considered by Sir Henry Thompson.

The question of the best combination of dishes in a meal, and the arrangement of the meals, next engages the author's attention; and after this he discusses the question of wines, coffee, water, and tobacco, gives a scheme for a dinner, and a number of *menus* for different months in the year, finishing up with suggestions for the improvement of public dinners, and for the better teaching of cookery and supply of food throughout the kingdom. The contents of Sir Henry Thompson's book thus corresponds to its title, "Food and Feeding," and it gives the elements of the dinner. Walker's "Art of Dining" aspires to a higher gastronomic level. It is written in a series of most readable little essays, in which the directions which concern the kitchen are omitted, and the foods are discussed as they appear upon the table. The key-note of the book may be found in the little sentence, "The chief maxim in dining with comfort is to have what you want when you want it," and in order to attain this the writer shows how the attendants should be ordered, and how the little adjuncts to the dishes should be arranged, so that no one shall have to wait for anything a moment after the desire for it has arisen. But more than this. It often happens that people do not know what to desire, and this the author tries to show them, by giving them illustrations of little dinners which he has had with his friends, and in which dishes and wines were so arranged in quantity and quality as to give the maximum of enjoyment. A puzzle in physics is the question whether a glass of water containing a cork would be heavier when the cork was fastened to the bottom of the glass or allowed to float on the water. The answer is that it would be heavier when the cork was at the top, because its place at the bottom of the glass would be taken by an equal bulk of water, which is heavier, and thus the attraction of gravity would act on the greater mass at the lesser distance. The author would apply a similar principle to the art of dining, and, instead of as usual keeping the delicacies until the last, when the appetite is palled by the previous dishes, he would give them first, when their enjoyment would be heightened by an excellent appetite.

"At a party of six persons, if the dinner consisted of soup, fish, a joint, and three woodcocks, I maintain it would be much better to serve the woodcocks before the joint, both on the score of enjoyment and of health—of enjoyment, because a delicacy, when the appetite is nearly satisfied, loses a great part of its relish, and is reduced to the level of plainer food whilst the appetite is keen—of health, because it is much more easy to regulate the appetite when the least tempting dishes are brought last. By serving delicacies first, people would dine both more satisfactorily and more moderately, and entertainments would be less costly and less troublesome."

This quotation may serve as an example of the book. To quote all that is worthy would be to transcribe the volume, and if it were read carefully and acted up to by every host, dinners would become a source of pleasure, instead of being, as they too often are at present, weary stale, and unprofitable.

OUR BOOK SHELF

Studies on Apus, Limulus and Scorpio. By E. Ray Lankester, M.A., F.R.S. (London: J. and A. Churchill, 1881.)

In these exceedingly clever memoirs we have a proof of how much can be made out of even well-known subjects by assiduous research, when combined with some speculative talents. The first memoir on *Apus cancriformis* is a valuable contribution to our knowledge of this most interesting Crustacean. The second on Limulus, an Arachnid, is even more interesting, and in its conclusions more startling, with it is combined a very elaborate comparison of the various systems of Limulus with those of Scorpio, and starting with the undoubted affinity of Limulus to the strange extinct Eurypterina, we have the suggestion that the Merostomata, including under this head the Xiphosura, the Trilobita, and Eurypterina diverged from the main stem of the Arthropod pedigree at a point between that indicated by the grade of organisation of Peripatus, and that occupied by the Pro-Phyllopoda or earliest Crustaceans, and it was in the time that these three great groups began to be formed, that each carried off with it some distinct evidence of their common departure.

The illustrations vastly assist in explaining the various technical details, and we are glad to see a large number incorporated in the text, thereby being rendered much more easy of reference to the reader, than when relegated to plates at the end of a memoir.

Fashion in Deformity, as Illustrated in the Customs of Barbarous and Civilised Races. By William Henry Flower, LL.D., F.R.S., F.R.C.S., &c. With Illustrations. 8vo, pp. 85. (London: Macmillan and Co.)

If Prof. Flower by this little work has not rendered good service to medicine, and tended greatly to prevent the diseases due to the prevalence of absurd fashions, it is certainly not his fault. He discusses the curious fashion which has prevailed among all nations, of inflicting upon themselves serious pain and inconvenience, as well as rendering themselves abominably ugly, in their endeavours to conform to a false standard of beauty. He begins with the epidermal appendages—nails, hair, teeth, and skin, proceeding to alterations in the bony skeleton. After discussing the modes of dressing the hair, the first figure he gives is that of the hand of a Chinese ascetic, in which the finger nails appear to be nearly a foot long, and twisted almost like the tendrils of a vine. The custom of tattooing perhaps inflicts upon the votary of fashion more pain than almost any other. The process varies from making gashes with sharp stones, and rubbing wood-ashes into them, to pricking delicate patterns into the skin by pieces of shell cut into a number of fine points, or by a bundle of sharp needles, and then rubbing colouring-matter into the punctures. The custom of wearing rings and plugs in the lips, nose, and ears is sometimes carried to a most exaggerated extent, one man, in an island near New Guinea, having such holes in his ears, that the lobes were converted into great pendants of skin, through which he could easily pass his arms. Such deformities of fashion, although most disagreeable to our ideas, are of much less importance than those which affect the bony skeleton. The author gives a full description of the various modes of altering the shape of the head adopted by various tribes, and of deforming the foot amongst the Chinese. But from savage tribes, Mr. Flower passes on to deformity in fashion amongst ourselves. He shows, by drawings of deformed English feet, and of the modern Parisian shoe, that, much as we may ridicule the Chinese, we are very little better than they. In one particular, indeed, we may be said to be very much worse than either Chinese or savages; for, while they deform the foot, we deform that part of the body which contains our vital organs. How far removed from nature is the form imparted to the figure by fashion, is seen by comparing the figures of the Venus of Milo, and of a lady dressed in the fashion of 1880.

We fear that no amount of warning regarding the pain, suffering, and danger to life which such fashions entail, will ever prevent them from being followed; but it is possible that when fashionable people come to see that their absurdities reduce them to the same level of taste as a Botocudo Indian or Bongo Negro, they may be induced to seek after a higher standard, which shall at once be beautiful, and true to nature.

Cameos from the Silver-Land; or, The Experiences of a Young Naturalist in the Argentine Republic. By E. W. White. In two Volumes. Vol. I. (London: John Van Voorst, 1881.)

This is the first volume of an interesting work which would appear to give a true and vivid sketch of the great Argentine Republic as it is at the present day. The great Republic seems, by the test of the London Exchange, to be well holding its own, but the notions current in England about it are often absurd in the extreme. Mr. White has in this volume given us a very good guide-book to the province, detailing the chief peculiarities of its climate, giving an account of its various races, of the state of the education of the people in the province, and of its natural resources. Buenos Ayres is described in a very enthusiastic way, and the behaviour of its inhabitants is spoken of in glowing terms. The first few chapters are devoted to the experiences of our young naturalist in the large cities. When he left these for trips to Cordoba and such like distant places his experiences as a naturalist began, and we follow such wanderings with real pleasure. At one time he journeyed to Cosquin to hunt the Condor; again to Mendoza for the Guanaco; but wherever he went he was sure to observe and record some interesting incident about the flowers and birds and insects that he met with.

Select Extra Tropical Plants Readily Eligible for Industrial Culture or Naturalisation, with some Indications of their Native Countries and some of their Uses. By Ferdinand, Baron von Mueller, K.C.M.G., M.D., F.R.S. New South Wales Edition, enlarged. (Sydney: Government Printers, 1881.)

It would be difficult to convey an accurate idea of the large amount of information which the author has brought together within the compass of the 400 pages forming this volume, an edition of which was some years ago published by the Victoria Acclimatisation Society, and also not long since in Calcutta by the Central Government of India. While the present edition does not put in a claim for completeness, either as a specific index or as a series of notes on the respective technologic applicability of the plants enumerated, still, we have here brought together an immense assemblage of useful plants arranged in alphabetical order, but with a systematic index and also their correct scientific names, and the chief facts of interest that concern each as to its uses to mankind. Some of these plants, all of which are presumed to be capable of cultivation in extra-tropical countries, are good for food, either as yielding pot-herbage, or roots, or fruits. Others are useful for dyes, for their fibre, as fodder-plants, as medicinal plants, or as timber-trees. The information in all cases is given in the fewest possible words. Baron von Mueller is to be congratulated on the honourable part he has taken now for many years in enriching the culture-resources of his adopted country, and we echo his hope that this most valuable manual of useful plants may be placed in the leading library of every State school in the Australian colonies, when it will be sure to aid in educating the youth instructed therein, in a special knowledge that may be of immense service in the future of Australasia. E. P. W.

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.*]

Vignettes from Nature

If Mr. Grant Allen does not mean what he says, I should strongly recommend him, alike for his readers' sake and his own, to say what he means.

When he wrote, "As a matter of fact, it seems probable that our *actual* fauna and flora are on the whole not only quite as big as any previous ones, but even a great deal bigger," and went on to cite the "modern" whales, the "living" forty-feet shark, and the elephants of the "recent period" (which not I, but his friendly reviewer, Mr. Wallace, converted into the "present time"), I naturally understood him to mean that the "actual," "modern,' or "living" forms of these types are larger than any corresponding "extinct" forms of the same. It now appears, however, that he meant to include *extinct* whales, *extinct* sharks, and the *extinct* mammoth (with, of course, its contemporaries) as members of the "actual" fauna.

To me it seems far better that science should not be taught to the public at all than that by the the use of the "vague but comprehensible language of ordinary life," such erroneous ideas should be propagated. I can assure Mr. Grant Allen, from no small experience of popular science-teaching, that the public mind is quite capable of drawing a very clear distinction between "living" and "extinct" animals, and would urge him to keep that distinction steadily in view in anything he may hereafter write on the subject.

With reference to the sharks' teeth brought up in the *Challenger* dredgings, in that part of the Pacific between Polynesia and South America, I may mention that only within the last week I have seen a collection of sharks' teeth from a "coprolite" digging in South America, among which was one of five inches by four, closely corresponding in its mineralised condition with the large *Challenger* teeth; and associated with them were rolled fragments of elephantine molars, presumably *Mastodon Andium*. And yet, according to Mr. Grant Allen, they belonged to our *actual* fauna! W. B. CARPENTER

56, Regent's Park Road, London, N.W., March 20

Fisher's "Earth's Crust"

THE verdict of NATURE on the Rev. O. Fisher's "Physics of the Earth's Crust" is that "One or two points do seem to emerge from this assemblage of calculation as fairly clear and established on tolerably firm foundation. Such as that the contraction of the earth by cooling is inadequate to the production of its greater inequalities." . . That "there must be subterranean irregularities of density." I ask for a fresh trial on the ground that the evidence is insufficient.

On the first head, what Mr. Fisher has done is this. He has started with the assumption that no part of the earth became solid till the whole had cooled down to a uniform temperature of 7000° F. With this and some other minor assumptions he has been led to the conclusion stated above. And he has high authority on his side, for this is the assumption made by Sir W. Thomson in his well-known paper, "On the Secular Cooling of the Earth." But the facts that an assumption is not in itself physically impossible, and that it enables you to integrate a tiresome differential equation and obtain numerical results, are not sufficient to establish the truth of the assumption. There are other ways in which the earth may have passed from a fluid to a solid state, some of them, to say the least, quite as probable as that which Sir W. Thomson adopts. I very much fear then that Mr. Fisher cannot be said to have established even his negative proposition. Indeed, to my mind, Mr. Fisher's work seems rather to show that the earth did not consolidate in the way supposed by Sir W. Thomson.

The second point, strongly insisted on not only by Mr. Fisher but by many other eminent physicists, which the reviewer looks upon as finally settled, is the doctrine that the material of the crust must be denser beneath the ocean basins than beneath continents. The belief is grounded on the following argument. If this were not so, the preponderance of land on the northern hemisphere would attract the water, and the consequence would be that the sea-level would be higher in the northern than in the southern hemisphere. The answer is: How do we know that this is not so? At the outside the difference of level would not amount to more than a few hundred feet, and what is there to prove that the mean level of the sea in St. George's Channel is not a few hundred feet farther from the earth's centre than the mean level of the sea at the point diametrically opposite. It might be so, and we should none of us be a bit the wiser. The famous Indian deviation of the plumb-line, too, can hardly be looked upon as conclusive, when we reflect that it has been found capable of explanation in several ways by the ingenuity of the former Astronomer-Royal and the late Archdeacon Pratt. No problem that admits of several solutions can be appealed to as conclusive on a point like this. Mr. Fisher's treatment of the Revelations of the Thermometer cannot either be accepted as satisfactory. Any one who has roughly plotted to scale a section over the St. Gothard sees that a segment of a circular cylinder does not represent, even to a very loose degree of approximation, the contour of the mountain.

The reviewer speaks of the cause to which Mr. Fisher would assign the contortion of the rocks of the earth's crust as hardly adequate; he might have safely gone further. That cause is the injection of lava into fissures, or, in other words, the formation of dykes. That contorted rocks are often traversed by countless dykes is a well-known fact. Take for instance the dykes which seam so thickly the Palæozoic rocks of Scotland; but here the dykes were formed long after the contortions, and besides their general direction does not coincide with the longer axes of the folds into which the rocks have been bent. In other cases of violently-contorted rocks there is a striking absence of dykes; this is so along the coast of Glamorganshire and Pembrokeshire, where we have about the most marked case of intense folding and inversion in the British Isles. And this is still more strikingly the case in that marvellous example of contortion and inversion to bs seen in the Canton of Glaurus, which has been so graphically described by Heim. Nowhere, as far as we know, on the earth's surface has inversion gone to the length it has here; dykes do traverse the Palæozoic rocks, but they none of them run up into the Secondary and Tertiary beds, and the contortion did not begin till towards the end of the Eocene period.

I for one should be only too relieved to think that some certainty, even if it were only of a negative kind, had been arrived at in the problems of the Physics of the Earth's Crust, but I fear we are a long way off this happy consummation at present. A. H. GREEN

Yorkshire College, Leeds, March 14

An Equatorial Solar Spot

THE occurrence of a spot close to the equator is so rare a phenomenon that it may interest some of your readers to know that there is such a spot now on the disc.

On the 6th I noticed a spot, not long entered, and close to the equator; it was a large, well-defined, regular, oval spot, with a mag. axis exactly parallel to the sun's equator. On the 10th, at 10h. 45m. G. Astron. Time, this spot crosssed the prime meridian, at a distance from the centre of the disc equal to 0·120 the radius, measured towards the true north limb, in the direction (156°–336°), *i.e.* parallel to the sun's axis.

This distance, 0·120 R., corresponds to hel. lat. 6°·9, measured from the centre towards the north; therefore, as the latitude of the centre is now 7°·2 south, the true hel. lat. of the spot is 0°·3 S.

The observations for determining the place of the spot were made on the 10th at 4.45, and on the 11th at 12.45; during the interval the spot had crossed the prime meridian, and the "position" of the axis of the spot, which had remained constant from the 6th till 4.45 on the 10th, had, during the interval, changed from (90°–270°), having reference to the sun's axis, to (38°-218°), *i.e.* 52° in 20 hours.

There was no further change from 12.45 to 4.45 on the 11th: and on the 12th the character of the spot was so altered that you could not distinguish any maj. axis at all. The instrument used was the 7½-inch equatoreal refractor. WENTWORTH ERCK

Sherrington, Bray, Co. Wicklow, March 13

Seasonal Order in Colours of Flowers

SIX years ago the question was brought forward in this journal (Vol. xiii. p. 427) whether light has any influence on the colour of flowers. I then called attention to the experiments made by Askenasy in 1875 (*Botanische Zeitung*, 1876, No. 1), from which he inferred that the action of light was different, some flowers being changed by darkness, but others not. Having myself from time to time studied this subject, I have seen, like other observers, that several kinds of pigment appear in complete darkness, but that in many of these cases daylight strengthens the tint and increases the hue. Not only flowers, but also other parts of the plant are thus affected. I found for instance that the shoots of several potatoes grown in the dark were coloured pink, that a bud of an elder-tree formed under the same circumstances two red-coloured internodes, and that crocuses, tulips, and hyacinths produced coloured flowers, whereas *Aucuba japonica* gave red-coloured fruits. It seems from these experiments that the plant is able to produce colouring matter without help from any source of light. But it is an important fact that the colours formed in the dark and those formed in the light often do not possess the same beauty. To prove this, I raised a bulb of hyacinth with two buds (or "noses" as they are called by Dutch florists); one of the two buds was covered by a piece of thick opaque paper to prevent the sun shining upon it, while the other bud was uncovered, and thus could enjoy the sun's influence. After some weeks the difference was very marked, the covered flowers being less intensively coloured than the others. This way of experimenting gave more striking results than that proposed by Askenasy, who kept the hyacinths in the dark until the whole inflorescence had opened, then cut off the upper part of the flower-stalk with half of the flowers and exposed it to daylight, while the others remained in their dark place. But he also saw that the latter flowers remained less coloured.

Experiments were also made by me with *Aucuba japonica*, which plant produced white flowers in the dark instead of purplish-brown ones in the light, just as the lilac does when cultivated in winter by florists. On the other hand, flowers of *Crocus vernus* and *Tulipa gessneriana* did not perceptibly change their colour. Among the leaves I experimented upon, I mention a Coleus which in the light produced green leaves with red lines, whereas the same sort gave in the dark yellow leaves with almost colourless lines. Achyranthes behaved very curiously, producing in the dark two normally coloured leaves, but the new-formed internode which supported them was almost white instead of red. This result calls to mind that of Batalin, who found ("Acta horti petropolitani," t. vi.) that plants of *Polygonum Fagopyrum*, which he had raised from seeds in the dark, were quite colourless, so that the difference from those grown in the light is very great, the latter, as is generally known, being of a dark red colour.

It appears from these examples, that those organs which are put into the dark in a very undeveloped state (so that they must much enlarge there) undergo much discolouring, whereas those parts that are hidden in a more advanced stage of growth, lose less colour, in some cases almost none.

Lilacs, for instance, develop in the dark from quite small studs, and so do flowers of Aucuba. Buck-wheat plants grow from small seeds containing a small hypocotyl, that enlarges afterwards to an exceedingly long part. The above-mentioned colourless long internode of Achyranthes came from a very small stud. In all these cases there can be only a small quantity of pigment (or chromogene) in the part before its development, and only this small quantity seems to be spread over the same part when many times enlarged. These cases, accordingly, render it very probable that light is necessary to increase the quantity of pigment, and that the pigment present, originates from the time when the plant was exposed to sunlight.

But tulips, hyacinths, crocuses, berries of Aucuba, &c. lose very little of their colour in the dark. Why is this? Because the buds of the flowers named, when hidden in the bulb are quite complete, and the green fruit of Aucuba had reached nearly their natural size at the time they were deprived of light. I think these flowers and fruits had already stored up a great deal of chromogene which they received from the leaves during the time when the bulb and the fruit were formed, just as they possessed a sufficient quantity of food to reach perfect development of all parts in the dark.

According to this opinion, the colour of a flower, fruit, leaf, &c. when grown in the dark, depends only on the quantity of colour-making matter or chromogene that is contained in the part at the moment when it is withdrawn from the light. This opinion is supported by an observation of Askenasy's (*Bot. Ztg.*, *l.c.*), who found that buds of *Pulmonaria officinalis* in different stages of development, when placed into the dark, got their colours the more perfectly the larger the buds were at the time of darkening. The buds which were smallest at that time exhibited almost white flowers.

My conclusion is then, that light is necessary for forming, if not the colouring matter itself, yet a matter (chromogene) which can easily pass into the pigment. J. C. COSTERUS

Amsterdam, March 12

The Electrical Resistance of Carbon under Pressure

I AM indebted to Mr. Herbert Tomlinson for drawing my attention to his most interesting comparisons between the behaviour of carbon and of metals in respect of change of electric resistance under mechanical stress, and am glad to see that his delicate determinations entirely support my conclusion that the excessively slight change of specific conductivity produced by stress in hard coke carbon cannot possibly explain the great variations of resistance observed in the carbon telephone, carbon rheostat, &c. I have also learned that a similar conclusion was arrived at some time ago by Professors Naccari and Pagliani, and that some experiments made by Prof. W. F. Barrett of Dublin on the buttons of compressed lamp-black prepared in Edison's laboratory for use in his well-known carbon telephone lead to a precisely similar result. I am therefore perfectly willing to admit that before the publication of my experiments this question was virtually settled. It is quite clear that the carbon telephone does not work by any variation in the specific resistance of the carbon, but by the partial opening and closing of the circuit at certain surfaces where the intimacy of the contact can be varied by the vibrations.

Bristol, March 20 SILVANUS P. THOMPSON

Vivisection

As I am named in your article thus headed (p. 429), I shall be obliged if you will permit me a brief explanation of my position in regard to the question. There is doubtless in point of suffering a very great difference between such experiments on animals as those tabulated by Prof. Yeo (and cited in your article) and those common upon the Continent, and of which the horrible tortures perpetrated by Professors Schiff, Mantegazza, and Paul Bert may be taken as fair types. But I fail to see by what means, whether legislative or otherwise, the atrocities of vivisection are to be prevented, while its more moderate practices are to be permitted. Had the sins of the experimentalists been confined to the "hypodermic injection" of a few mice, as Sir James Paget has tried hard to make the public believe, I venture to assert that no outcry would ever have been raised on the subject. I for one should certainly have thought the cause unworthy of support. But the truth is, unfortunately, far otherwise. It is not against the inoculation of "vermin," or the pricking of a "tadpole's tail," that the indignation of the English people has been stirred, but against the prolonged and exquisite torments to which such highly sentient creatures as horses, dogs, and other domestic animals have been and still are subjected, often under the influence, not of chloroform, but of curare. So long as English physiologists continue to cite these things with approval, and openly to regret that the effects of public opinion in this country are such as to throw obstacles in the way of "free" and unlimited vivisection, as practised in the Continental schools, so long, I apprehend, the agitation deprecated in your article, will go on, and gather strength. It is the influence of public opinion only, which in this country finds expression as it does in no other, that has hitherto prevented the physiological laboratory of England from becoming as notorious a scene of horror as that of France, Italy, or Germany. And, let it be borne in mind, that the stir on this subject, which has expressed itself recently in the pages of various reviews and magazines, was set going by the demand of the Medical Congress for *unrestricted* vivisection. Sir William Gull's opening sentence in the *Nineteenth Century* of this month is, therefore, wide of the mark. Not less does he mis-state the case, when, in the course of his concluding observations, he infers that I bring against physiological research the charge of Atheistic tendency. That charge is distinctly made by me, not against legitimate research of any kind, but against a method which I wholly dissociate from "science" properly so called. And it is a charge not lightly made, but based on sound experience and thoughtful observation, unbiassed by "emotion" of any kind.

As it has not been my privilege to escape the customary personal retort of pro-vivisectors with regard to the wearing of fur, feathers, and the utilisation of animals, whether for food, pleasure, or clothing, I shall hold myself indebted to your courtesy for permission to make the following statement in justification of my own consistency:—I never buy furs, feathers, ivory, kid gloves, stuffed birds or other creatures. I have long been engaged in trying to introduce the use of vegetable leather for making boots and shoes, and have devoted much of my time to the question. I detest all "sport" which necessitates the pain and suffering of living creatures, and have written many articles and letters to various journals for several years past against seal-hunting, pigeon-shooting, coursing, battues, and rabbit-gins. Of late years I have added "vivisection" to the list. My husband's horses wear no bearing-reins; and I never see cruelty without interfering at the risk—as I know but too well—of personal insult. Finally, it is twelve years since I tasted flesh or fowl of any kind. ANNA KINGSFORD

11, Chapel Street, Park Lane, W., March 16

[It seems a somewhat unwarranted and unworthy argument that because some foreign experimenters have been guilty of excess, therefore we are totally to suppress in England that which all competent persons know and declare to constitute a most important branch of physiological method; and there is certainly no ground for the statement that it is "the influence of public opinion only" which has hitherto prevented abuse in this country. As pointed out in our article, the English physiologists themselves took the initiative in laying down a code of rules against abuse some time before the sense of the general public was aroused, and it does not appear that since this has been aroused there has been any change in our physiological practices. Moreover, we cannot think that many of our readers will resemble our correspondent in failing to see that "atrocities" may be guarded against while "more moderate practices" are allowed; and in this connection it is always to be remembered that our English physiologists have gladly acquiesced in legislation directed against the former.—ED.]

TOWARDS the close of an interesting article on Vivisection in a recent number of NATURE (p. 429), the following remarks from an article in the *Fortnightly Review* by Lord Coleridge occur: "What would our Lord have said, what looks would He have bent upon a chamber filled with unoffending creatures which He loves, dying under torture deliberately and intentionally inflicted." Prof. Yeo in answering this, quotes "Ye are of more value than many sparrows," "How much then is a man better than a sheep." But there is one passage in Scripture which I think has even a closer connection with vivisection than those mentioned above, namely, the healing of the man possessed with devils (Mark v. verse 13), "and the unclean spirits went out and entered into the swine; and the herd ran violently down a steep place into the sea (they were about two thousand;) and were choked in the sea." If our Lord therefore considered it expedient to permit the destruction of a whole herd of swine, numbering 2000, in order to alleviate the sufferings of the demoniac, surely the labours of a man like Hunter must be justified, who by experiments on living animals has been the means of reducing death from aneurism of the principal artery of the lower limb from 95 per cent. to 10 per cent., as stated by Sir James Paget.

CHARLES ALEX. STEVENSON

45, Melville Street, Edinburgh, March 14

Muffs and Vivisection

YOUR correspondent, Mr. H. H. Johnston, of the Zoological Gardens, has offered your readers some "facts," which, he says, he "knows to be true." He says that some little time ago I called on "a distinguished member of science"; that "three things were observable in my outward presentments"—to wit, ostrich feathers in my bonnet, a bird of paradise on, *or near* my muff, and an ivory-handled umbrella; and that the man of science took each of these articles as a text for a rebuke to me for encouraging cruelty. Sir, these "facts" may possibly be "*accurate enough for scientific purposes*," like some others which we heard of at Bond Street, last winter, but they have given much merriment to those who happen to be acquainted with my real "outward presentments." Suffice it to say, that I never paid such a visit as Mr. Johnston describes; never received such a rebuke; never used an ivory-handled umbrella; never wore a bird of paradise, or any other bird, either in *or near* my muff, or any other portion of my attire; and, finally, having never possessed such an object in my whole life, am driven to think that the only Muff connected with the ridiculous story, must be the person who assures us he "knows" it to be true.

1, Victoria Street, S.W. FRANCES POWER COBBE

Pasteur's Inoculations

IN the *Proceedings* of the St. Louis Medical Society, recorded in the *St. Louis Medical and Surgical Journal* for December last, Dr. Spinzig is reported to have said:—"Splenic fever only prevails where there is low ground, or what is called 'bottom ground.' Sheep that are fed and shepherded on such ground will take this disease, while those that are shepherded in lofty regions never take it. But Prof. Pasteur had perfect confidence in his antidote, and asserted that if the sheep were properly 'vaccinated' it would make no difference where they were, because the specific poison could not reach them. In Germany, however, *they found this was not the case.*" May I ask, through your columns, what is Dr. Spinzig's authority for this statement?

Again, I have been informed, that although it has been abundantly proved that cattle and sheep inoculated with attenuated virus are protected against the otherwise deadly effects of uncultivated virus, they do not enjoy similar immunity from the ravages of a natural epidemic of splenic fever. I should be glad to know whether any record is forthcoming as evidence that the inoculation of the attenuated virus has proved as successful against natural epidemic arising under ordinary conditions as against artificial introduction of the virulent matter into the system.

A STUDENT

"Eophyton"

AFTER all that has been written, both in England and abroad, about the inorganic nature of Eophyton, it is strange to see such statements as those made by Mr. Elsden, in NATURE (vol. xxv. p. 409). As long ago as 1873, I pointed out that in Sweden it occurs under circumstances, which most decidedly prove that it *cannot* have been any plant or organic being whatever, and Principal Dawson has been led to precisely the same conclusions, from his studies of Eophyton at St. John. Last year, I further, by way of experiment, produced Eophytons, which cannot be distinguished from the Cambrian ones.

A. G. NATHORST

Geological Survey Office, Stockholm, Sweden, March 16

"Telescopic Definition in a Hazy Sky"

MR. ROYSTON-PIGOTT'S letter in NATURE, vol. xxv. p. 77, reminds me that during the Transit of Venus in 1874, the sky here for the greater part of the day, being covered with thick but varying clouds, I several times, for a considerable time together, saw the planet on the sun's disk, with the naked eye; the cloud at these times stopping out the superfluous light, but not stopping distinct vision.

I may add (though off the line) that, towards evening, the sun shone out brilliantly, and from my garden, through a bit of smoked glass, I watched the whole egress. Unfortunately, the subsidiary station, which was to have been here, had to be abandoned at the last moment, a matter of great regret to all concerned, and to all interested, and not the least to myself, having witnessed a great chance which there was no one to use.

Nelson, New Zealand, January 27 A. S. ATKINSON

The Weather in South Australia

MRS. MERRIFIELD, with her compliments, begs to inclose extracts from two letters she has recently received, describing the hot weather in South Australia during last January, also mentioning the curious fact, that the water in the Sturt river is more abundant in dry seasons than in wet ones. Perhaps the Editor may consider the statements sufficiently interesting for insertion in NATURE.

Stapleford, near Cambridge, March 14

"On Tuesday, January 17, the thermometer was registered at 180° in the sun, and 114° in the shade. I believe we had it hotter here; for, in the afternoon, the hills were covered with bush-fires. Fortunately, they were on the other side of the railroad, about two miles off. At night, when the fire was nearly out, and only the stumps and dead branches left burning, it looked like a large town lighted up. A strong south-east wind blew the heat over to us. In the morning, a strong hot blast came from the north, as hot as I have felt it in West Australia. Yesterday, the thermometer fell, out of doors, to 90°. To-day (January 19) was, I think, as hot in doors as on Tuesday. Metals in the room were unpleasantly hot to the back of the hand. The leaves are falling off the trees, from the intense heat and dryness of the air. The ants, which up to 9 a.m. were busy, forming a column varying in width from eighteen inches to three feet, and almost colouring the ground, retired to their nests. Not one was to be seen. Neither a bird, fly, nor other insect was visible, unless disturbed by the rustling of the leaves. The following statement will show the hottest days in certain years:—

In 1860	...	thermometer	158°
1862	...	,,	159
1871	...	,,	153
1880	...	,,	172
1882	...	,,	180

"*February* 2

"It is a curious circumstance that the Sturt river or creek, which runs through Mr. L.'s garden, has more water in it in dry summers than in wet years. The present is a most unusually dry season, and there is much more water in the creek than there was last year. The Sturt is called 'a river,' but, like most Australian rivers, it is at times only a series of water holes; the water really running under the bed of the river. One can walk across the bed of the river, dryshod, except where the deep holes are. We cannot account for there being a greater body of water, that is, that the springs are stronger during drought than at other times. There are bad accounts from the northern areas. There is no water. The crops, about six inches high, yielding four bushels to the acre. Water is obliged to be carted by rail to some districts, and at the mines on the Peninsula, they are obliged to set the stills to work. In the far north, the new settlers are in fearful straits for want of water, even for drinking. The weather has been more tolerable lately; but hot in the sun, very cold wind in the evening, when a fire is agreeable."

Variable Cygni (Birmingham) 1881

UNDER the above heading, in the *Astr. Nachrichten*, No. 2421 (March 7), Dr. Schmidt, of Athens, gives the results of his observations of this star, which became invisible to him on November 24, and remained so up to his last search for it on December 22. It must have been from rather superior telescopic power—scarcely from a better *atmosphere*—that I was able not alone to see it on December 21, but to recognise its deep crimson colour when no more than 12 mag., and probably less. I used a power of 53 on a 4½ inch object-glass.

Millbrook, Tuam, March 11 J. BIRMINGHAM

A Strange Phenomenon

THE letter under the above heading in NATURE, vol. xxv. p. 410, does not describe a phenomenon altogether unique. A good many years ago a clergyman, well known to me, was passing over a low hill in this parish; while doing so, he encountered a sharp shower of hail, and on approaching the highest point of the ground, he was astonished to find an electrical display similar to that described by Mr. Moir, an elevated walking-stick behaving like a pointed rod on the prime conductor of an electrical machine. I understood that when the clergyman left the summit of the rising ground, the phenomenon disappeared, and that on at once retracing his steps it was again visible. Mr. Moir does not state his position with regard to the contour of the ground, but I strongly suspect that he occupied a position similar to that described above, and that he witnessed a natural display of the common class experiment of presenting a pointed metallic rod to the charged conductor of an electric machine. B.

Fyvie, March 13

STENO[1]

IN the galaxy of genius that glowed in the still dark sky of the seventeenth century some spots shone forth more bright than others, and the keener vision and greater knowledge of later times has detected there stars of surpassing brilliancy. It was a period of great intellectual activity, and there was much independence of thought and freedom of research. In natural science this was quickly felt, and in Italy—elastic Italy!—that first rebounds to every movement, the results were soon made visible. It is not always in the heaving mass itself we first detect it; a foreign body resting on the surface sometimes more clearly indicates the motion. So in the country of Frascatoro, Scilla, Cardano, Cesalpino, Imperati, Aldrovandi, it was a Dane who first put geological Science into shape.

[1] *Phil. Trans.*, vol. ii. p. 225.
Fabroni.—"Vitæ Italorum," vol iii. p. 7.
Manni.—"Vita del Litteratissimo Mgr. Stenone," Florentiæ, 1775.
Pilla.—E dissertatione N. Stenonis, "de solido intra solidum naturaliter contento excerpta in quibus doctrinas geologicas quæ hodie sunt in honore facile est reperire," Florentiæ, 1842.
A. G. S.—"Biographie Universelle," Paris, 1825.
Lyell.—"Principles of Geology," vol. i., 1830.
Ramsay.—"Passages in the History of Geology." Inaugural Lecture, Univ. Coll. Lond., 1848, p. 10
Capellini.—"Di Nicola Stenone e dei suoi studii geologici in Italia," Inaugural Lecture, University of Bologna, 1869.
Huxley.—Discourse at York meeting, Brit. Assoc., 1881, revised by the Author (NATURE, vol. xxiv. p. 452).

But a very remarkable man was this Dane, Nicholas Steno, and a curious history his. Born at Copenhagen in 1638, the son of a goldsmith in the service of Christian IV., he was brought up in the strictest principles of the Lutheran faith. Instead of following the calling of his father, he was educated for the medical profession, studied under Thos. Bartholin, and attended the lectures of Borrichius and of Simon Paul. Hence the work that first brought him into notice was human and comparative anatomy. Soon after he had obtained the degree of doctor at Copenhagen, he went to Leyden, attracted by the fame of Francis Sylvius, Van Horn, and others. Here he made the acquaintance of Gerard Blasius, to whom without any distrust or reserve he showed his recent discoveries of the parotid gland, and associated ducts, one of which is named after him *Ductus Stenonianus*; but Blasius seems to have dealt unjustly by him in this matter, and to have put forward as his own the discoveries communicated to him by Steno. It was soon, however, apparent that Blasius did not know enough about it to avail himself of the information he had thus gained and unfairly tried to make use of. Steno worked on, tracing by observation and experiment the relations between the salivary and mucous secretions and the blood.

He next turned his attention to the organs of vision and of smell, and in his comparison of the human body with that of the lower animals he may be considered one of the founders of the science of comparative anatomy.

About the year 1657 he published the results of his experiments on the eye of a calf, but he assumed too hastily an exact correspondence between that and the eye of other animals, especially man. In his work on the heart, too, he did not himself arrive at satisfactory results, but he did much to set others on the right line of inquiry, and we do not know how much Lower and other later writers were indebted to the earlier investigations of Steno on this subject. It will be seen that his chief work was that on the glands and various secretions, but it also was incomplete, and it remained for Richard Hall (*Phil. Trans.* vi. p. 3) to make out the true relations of the submaxillary glands.

In 1664 he published some embryological researches in a letter "On the manner in which the chick is nourished in the egg," which, with a letter "On the anatomy of the ray," is appended to his essay entitled "Observationum Anatomicarum de Musculis et Glandulis Specimen" (Copenhagen, 1664, 4to). On embryology he seems to have adopted the views of Marcello Malpighi.

While engaged in these various studies at Amsterdam he heard of the death of his mother, and returned to Copenhagen. After a short stay there he went for a tour through Italy and France, and in 1664 arrived at Paris with a view of carrying on his anatomical researches, now especially devoting himself to the investigation of the brain. In Paris he became intimate with Thevenot, and here also he made the acquaintance of Bossuet. The eloquence and earnestness of that remarkable prelate had such an effect upon Steno, that in 1667 he went over to the Catholics, which perhaps helped somewhat to secure for him the warm reception accorded to him by the Grand-Duke Ferdinand II. and his brother Leopold. He explains the reasons which had induced him to take this step, in a letter published by Fabroni ("Lettere inedite di uomini illustri," vol. ii.). Steno, after leaving Paris and making a tour through the chief towns of Italy, settled at Florence in 1666, where he met Carlo Dati, Francesco Redi, Vincenzo Viviani, and Lorenzo Magalotti. They, in spite of the jealous opposition of Jean Alphonse Borelli, who had had a controversy with Steno respecting the action of the muscles, all agreed in doing him honour, and invited him to become a member of the Academia del Cimento. He was appointed Physician to the Grand-Duke Ferdinand II. de' Medici, and under his protection and patronage had great opportunities of prosecuting his anatomical studies, as are shown by his further observations on the heart, among which he gives the results of his experiments on the heart of a dog; by his memoirs on the muscles of eagles; on the intestinal movements in cats; and on the bile ducts, &c.

Three years after the publication of these treatises on special points, he brought out his "Elements of Myology," in which he treated the subject more from a mechanical than an anatomical point of view. In a letter to Thevenot, published with this work, he gives an account of the dissection of a shark which had been captured off Leghorn in 1666, and especially discusses the character and mode of growth of the teeth of that animal. This seems to have been a favourite line of inquiry about that time, for next year (1667) Agostino Scilla published his work, "La Vana Speculazione disanganata dal senso," in which, with a view of proving the organic origin of fossils, he figures and describes sharks' heads and teeth, in order to compare them with the glossopietre, or fossil sharks' teeth, so commonly found in the Tertiary beds of Italy.

It was about this time 1667-9 that Steno extended his researches into the field of geology, and began to write a dissertation in Italian for the Academia della Crusca, of which unfortunately only the introduction has been handed down to us. This bears the title "De Solido intra solidum Naturaliter Contento Dissertationis Prodromus." (Florence, 1669, in 4to.)

In this work he showed that he held views far in advance of his age, at any rate that no one else had clearly stated them, for we cannot but feel that in most cases of this sort we have got the wisdom of many and the wit of one. The independent researches of a number of different observers suggest the same explanation, but each is afraid to bring it forward on the evidence that he alone has gathered. A bold clear-headed generaliser steps forward and says, why not accept the conclusions that naturally follow from the hypothesis that each of you severally feel would best explain the various phenomena you have been investigating?

At any rate Steno did give a fair sketch of the Principles of Geology, and showed that he had considered it from the petrological, palæontological, and stratigraphical point of view. He pointed out the difference between rocks of mechanical origin, and those which were due to chemical agencies, and further clearly distinguished those that were to be referred to ordinary subaqueous sediment from those which were the products of eruption.

He found it necessary to mention by way of illustration what any one would admit as soon as their attention was called to it, that if we found a deposit containing sea salt and the remains of marine animals, planks of ships, &c., we should allow that the sea had once been there, whether the bed was exposed in consequence of the sea having retired or because the land had been raised.

A great quantity of timber and things washed down from the land suggest transport by torrents and rivers. Charcoal, cinders, and calcined objects we refer to the action of fire.

If the strata are of the same kind we infer the same causes. But if the character of the deposits which make up a set of beds in one and the same place varies, we refer this to changes in the surrounding conditions affecting the flow, or the source from which the material was derived from time to time.

He further shows that although the lowest beds deposited over any area must conform to the shape of the underlying rock, the tendency of all sediment must be to assume a horizontal position; and so, when we find them highly inclined, we must refer this to subsequent movement, excepting, of course, in the case of false bedding, which probably he would include under his aqueous causes of inclination of strata.

He observes that mountains, often with flat tops, are made up of both horizontal and inclined strata, as may be seen along their flanks, and from all his observations inferred that once the mountains were not, that they do not grow, that there is no constant direction in mountain chains, and he infers that mountain regions are raised and depressed, and subject to rending and fissuring. Discussing the origin of springs, he shows that he had a clear idea on the subject of Artesian wells, which had been previously treated by Ramazzini [De Miranda fontium mutinensium scaturigine, 1596.]

As he had clear notions of the structure of the crust of the earth and of the origin of the sedimentary rocks, we are not surprised to find that he entertained correct views respecting the nature of fossils. He pointed out that some shells were preserved just as they had been left by the sea or lake; others had undergone a slight change, the original shell being altered or replaced, while, in a third case, the shell had perished and left only the cast in the rock. We must remember what queer ideas he had to meet when we read of his explanations and arguments to prove what seems now so clear; for example, how he dwelt upon the occurrence in the rock of a large shell bored by lithodomous mollusks, and had to combat the view that they were concretions! Again a common idea with regard to the sharks' teeth of Malta was that they were the spontaneous productions of the soil, while popular superstition referred them all to the miracle by which St. Paul deprived all the snakes in the island of their venom. So Steno had to meet the argument derived from the great numbers that are there found. He pointed out first that each fish has an enormous number of teeth; next that the sea often carries and collects into one place bodies of the same kind, sorting them, as we know now, according to their size, specific gravity, and so on; and thirdly, that these sharks herd together, so that it was likely there should be a large number of their teeth in one place; and he adds that as there are teeth of different fish as well as shells in the same beds, it was clear that we had to do with an ordinary marine deposit.

He does not seem to have determined the bones of the large mammalia or to have studied their mode of occurrence very carefully, for though he recognised elephants, he did not see the difficulty that arose from the occurrence of a great number of other large animals, nor did he realise in what ancient deposits they were found, and so he thought it a sufficient explanation to say that the elephants had been brought over by Hannibal. He was hampered by the attempt to classify the events of geology under six periods, and had rather to wrest his facts to make them fit with his explanation of the Noachian deluge.

It is less interesting to dwell upon these difficulties than to follow him where he made the great advances of his age, and laid down the simple law of palæontology that when we find a body imbedded in the rocks, and it is similar in all important points to a recent organism, it is a fair inference that it also did belong to such an organism, or when he gave as the result of his investigations that the deposits of past ages and their included remains were produced in just the same way as similar accumulations are formed in modern times, and that the succession of beds with marine shells such as were seen in various parts of Tuscany clearly proved that there had been alternate periods of submergence and of elevation over large areas.

In discussing the possible causes of these earth movements, he touches the question of internal heat, and in inquiring into the causes of hot and cold springs, currents of air of different temperatures, emanation of gases, &c., he speculates upon the effects of the internal heat of the earth, and here and there throws out hints of larger questions working in his mind, as, for instance, modifications of the earth's crust, such that the centre of gravity should no longer so nearly coincide with the centre of figure; and we cannot but regret that, owing to the sudden interruption which now fell upon his scientific life, that work of which we have but the introduction was never finished, and the many interesting facts as to the changes which had taken place in historic times in Southern Italy were never recorded, and the many curious disquisitions we were led to anticipate are lost for ever.

In 1668 Christian V. offered Steno the Chair of Anatomy at Copenhagen, which he accepted, and entered upon the duties of his office with the delivery of a remarkable inaugural address pointing out the direct benefits that have been derived from the study of anatomy, not only in the alleviation of suffering in others, but from the pleasure of the intellectual pursuit itself. But though his talent was universally recognised, jealousy and bigotry combined to make it uncomfortable for him in his native place, and so he returned to Tuscany, where the Grand-Duke Cosmo III. intrusted to him the education of his son Ferdinand. Steno now began to turn his attention to religious questions, and gave up natural science. He thought he must endeavour to bring about the conversion of his old co-religionists, and wrote several theological works which involved him in a controversy with the reformed clergy of Jena. Innocent XI. rewarded his zeal by appointing him, in 1677, Bishop of Titopolis (*in partibus*), and Apostolic Vicar of Northern Europe.

Steno fixed his residence in Hanover, when the Duke John Frederick of Brunswick had just embraced the Catholic faith; but on the death of this prince in 1679, the electorate fell under the domination of the Bishop of Osnabruck, who belonged to the reformed communion, and would not allow any proselytising to go on in his states. Steno therefore had to leave; and after spending some time at Munster and Hamburg, withdrew to Schwerin, where he died November 25, 1687. His body was, at the request of the Grand-Duke Cosmo III., carried back to Tuscany and laid in the Basilica of S. Lorenzo.

A simple slab of marble, put up by the Catholics whose cause he had espoused, marks the spot. The inscription gracefully records the pious prelate's end. As far as we know, no relative stood by[1]—no man of science pronounced a eulogy over Steno's grave.

The epitaph runs thus:—

NICOLAI STENONIS
EPISCOPI TITOPOLITANI
VIRI DEO PLENI
QUIDQUID MORTALE FUIT HIC SITUM EST
DANIA GENUIT HETERODOXUM
ETRURIA ORTHODOXUM
ROMA
V RTUTE PROBATUM SACRIS INFULIS INSIGNIVIT
SAXONIA INFERIOR
FORTEM EVANGELII ASSERTOREM AGNOVIT
DEMUM
DIUTURNIS PRO CHRISTO LABORIBUS AERUMNISQUE
CONFECTUM
SVERINUM DESIDERAVIT
ECCLESIA DEFLEVIT
FLORENTIA SIBI RESTITUI
SALTEM IN CINERIBUS VOLUIT
A.D. 1687

In this epitaph nothing is said of what Steno did for science, and when the President of the International Geological Congress led the congressists from Bologna to Florence last autumn to place a wreath upon the tomb of Steno, and called upon the distinguished Danish antiquary, Waldemar Schmidt, to say a few words to those assembled round the last resting-place of his illustrious compatriot, it was felt that it would be a fit and pleasing thing to put another slab beside the old one, in memory of that gathering round his grave, and telling of the full appreciation of his worth as a man of science by those who came two centuries after him.

THOS. MCKENNY HUGHES

[1] Jacques-Benique Winslow, an illustrious name in the annals of anatomy, was descended from a sister of Steno, but otherwise we hear no more of his relations.

WIND MEASUREMENTS

SINCE the time of Hooke the accurate measurement of the wind has formed an object of experimental research. That philosopher, if not actually the first to invent an anemometer, at any rate appears to have been the first to write upon the subject, which since then has occupied the attention and exercised the ingenuity of many scientific men. The main result of these efforts was well shown last week at the exhibition of anemometers organised by the Meteorological Society. The President, in an interesting historical address, stated that the number which had been invented was at least one hundred and fifty, and upwards of forty of these were collected, besides photographs and drawings of many others. The exhibition was by kind permission held in the library of the Institution of Civil Engineers, at whose weekly meeting two papers, on the design of structures to resist wind, and the resistance of viaducts to gusts of wind, were very opportunely read.

It is not by any means generally recognised that there are two distinct objects for which the measurement of the wind is necessary; these are: (1) the determination of the actual motion or transference of the air itself; (2) the investigation of the effect of the wind. The two societies above mentioned well represent these two objects of anemometry, and all the instruments are included in one or other of the two classes, which are said to measure respectively the velocity and pressure of the wind. These terms, though convenient, are slightly misleading, as it is really the impulse of the wind which is in both cases measured —in one by its effect in producing the continuous rotation of a vane or set of cups, in the other by its statical effect upon a pressure board or column of air or liquid.

From the nature of the wind it is evident that nothing less than a continuous graphic record could be of much service, and but little progress was made until the invention, about fifty years ago, of self-recording instruments of both classes. The late Dr. Robinson, F.R.S., contributed more than any one else to the establishment of the velocity anemometer which, by the addition of Mr. Beckley's self-recording apparatus, is undoubtedly a model of mechanical invention. Mr. Follet Osler, F.R.S., as the result of much persevering labour and skill, has given to the world a pressure instrument of great excellence, and of this and the former, both of which may be regarded as the best types of the two classes, it may fairly be said that much improvement, at any rate in mechanical construction, can hardly be expected.

As to the tabulation of results, this is conducted with the most scrupulous regularity. Since 1874 the Meteorological Office has published hourly numerical records, from its various stations, of the direction and other elements of the wind. Quarterly records containing engravings of the actual curves are also published. These latter have rather fallen into arrears, the first volume of the new series for 1876 having been only published in 1881; but it is satisfactory to hear that the work of completing them up to the year 1880 is progressing, and it is to be hoped that they will always be continued.

In the face of all this expenditure of time and skill the meteorologist and the engineer alike proclaim the unsatisfactory state of the science. The engineering aspect of the question, viz. the effect of the wind, has recently excited considerable attention in consequence of the Tay Bridge disaster in this country, and of similar accidents abroad. It is evident that with the increase in the size of engineering structures, particularly in exposed situations, the force of the wind may become as great as that impressed upon the structure by the action of gravity. The recent account, in this paper, of the proposed new Forth Bridge, was a good example of the provision made for wind pressure, not only on the completed structure, but also during its construction. Notwithstanding this, the report of the recent Commission on Wind Pressure substantiates the statements already alluded to. This distribution of wind pressure over any surface appears to be very little understood, though the matter is being carefully investigated by more than one experimenter, and some results have recently been published. It seems, however, hardly credible that the maximum pressure to which a structure may be exposed is almost as great a matter of uncertainty; yet such is the case. The papers on wind pressure, above referred to, in spite of the existence of so many anemometers, endeavour to ascertain from a variety of sources, such as previous accidents, and reports of the effect of wind in storms, what the probable maximum pressure has been, both, however, assuming values for purposes of calculation far less than are actually reported. In the same manner, the Commission decided upon a limiting value only a little more than 62 per cent. of a pressure recorded by an anemometer, and believed by them to have actually taken effect in this country.

The fact is, that the motion of the air is, beyond all expression, most complicated. Were it not for this, there would be no necessity for obtaining both the velocity and pressure of the wind, for there is, by a first principle of dynamics, a fixed relation between these two elements; and if one were known, the other could be, at any rate, approximately deduced. In reality, any attempt to treat the wind as having steady motion for more than a very small distance in space, is certain to involve serious error, and the complications which are introduced, from even slight disturbing causes, seem quite beyond the powers of investigation. The engineer is concerned both with prejudicial effect of the wind upon structures, and its useful effect upon wind-motors. In both these cases the conditions are such as to greatly interfere with the steady motion of the wind, and the effect due to locality must be estimated and allowed for. The meteorologist needs observations of the wind at all elevations, and as pointed out by Mr. Laughton in his address, particularly at higher ones, where, judging from the experience of aëronauts, the motion of the wind is nearly as complex as below. Until the motion of the wind is better understood, weather forecasts must be more or less unreliable, and what has been said with reference to the mechanical excellence of the present anemometers and the regular tabulation of results, must not lead to the idea that there is no room for improvement. On the contrary, there is yet much to be done in directions which can here be only briefly indicated.

First, there is great necessity for improvement in the lubrication of the instruments, especially of that portion recording direction, so that in viewing a weather chart of the *Times* it may be certain that in light winds the arrows *really* show the direction and not directly the opposite one. Such an error as this, perhaps from some distant station, causes whole columns of the bulky hourly records to be worse than useless.

Secondly, the reductions for the relative velocity of the wind and cups, if made at all, ought not to be made, as is at present the case, by a factor now well known as the result of much costly investigation, to be erroneous.

Lastly, the locality of anemometers should be more carefully selected, or at least taken more closely into account, in discussing the effect of wind in storms.

The importance of some reform in the matter of wind measurement is obvious, since it is only by continued observations, under improved conditions, that a more reliable and satisfactory knowledge can be obtained of the aërial ocean in which we live. H. S. H. S.

THE ZOOLOGICAL SOCIETY AND "JUMBO"

At the General Meeting of the Fellows of the Zoological Society on Thursday last, Prof. Flower made the following remarks with reference to the subject of the elephant, "Jumbo":—

Before the Meeting separates I wish to make a few observations upon the subjects which have just been under discussion. It has been said that there should be power in the Bye-laws to call Special Meetings of the Fellows of the Society; and the subject is certainly deserving of the consideration of the Council. The probable explanation why there is no such power already, lies in the fact that there are regular Monthly General Meetings at which all Fellows are able to be present, to ask any questions or to make any observations they think fit upon the management of the affairs of the Society and, upon notice having been given, to propose any resolutions.

With reference to the action of the Council in the particular case under consideration, their legal powers to part with any of the animals under their care have now been fully affirmed by Mr. Justice Chitty's judgment, and the expediency of their being able to exercise these powers at their discretion in all ordinary cases does not seem to be doubted by any sensible person. It has, however, been asserted that there was something exceptional in the case of the elephant in question. I would ask when, and by what means, can the line be drawn between an ordinary and exceptional animal? Two elephants have been sold within my recollection (one in 1854, the other in 1873), and no one ever disputed the power or discretion of the Council in parting with either. Certainly neither of them was called "Jumbo," a name which has clearly done much to foster the present agitation. If our "Jumbo" had been called by some name as unpronounceable as that of the two Indian elephants now in the Society's possession we should have heard much less of his virtues.

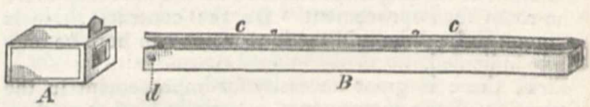
To speak of this animal as is done by Sir George Bowyer in the *Times* of to-day, as in any way comparable to the *Codex Alexandrinus*, is only equalled in absurdity by the statement lately made in a letter to the same paper by another Fellow of the Society, that if a certain Chancery suit were successful the animal would remain as a "permanent" inhabitant of the Gardens. How immortality was to be conferred on "Jumbo" I do not know. Our animals are only temporary possessions. All experience tells us that even elephants die, and, moreover, that whatever may be the case in their native land (a subject on which strangely exaggerated notions prevail), in this country they are never long-lived animals. Whatever means were tried to preserve "Jumbo," whether lawsuits, chains, or stone walls, it is absolutely certain that a few years would have seen his end in one way or another.

Then as to "Jumbo" being "unique," as is constantly said, I am not quite certain what is meant by this, as there are many African elephants at present in Europe, and one other in our own Gardens. As an elephant he is by no means perfect, wanting the most characteristic ornament of his race—the tusks. He is certainly large, but probably not larger than many other male elephants of his species would grow, if kept for a sufficient length of time. This very size, however, while in one sense adding to his value, is in another a serious detriment. It was, in fact, the principal cause of the desire to part with him. Then it is said that he was exceptional on account of his great money-value; but of what that value was no one could form any idea: in the general market it was literally nothing. I doubt whether, at all events a month ago, any one but the actual purchasers would have taken him off our hands at any price. I know, for my own part, so great has been my anxiety about him for several years past—so sure did I feel that he would one day or other bring us into trouble (although I can scarcely say that I anticipated it in its present form), that I would willingly have consented to giving him away *gratis* if an opportunity had offered. Probably few of us admired the animal more than I did; but I have considerable knowledge of those who have attempted to keep such elephants in captivity in Europe. It is said that, as we have no difficulty in keeping lions, which are more dangerous, there need be no danger with elephants; but the deduction is not sound. A lion is always dangerous, and can be treated accordingly; an elephant, which inspires confidence by its usual docility, is on that very account a far more difficult and dangerous animal to deal with. In many zoological gardens on the Continent I have seen elephants boxed and chained up, without being allowed to take a foot of exercise, sometimes for years together; and on inquiry I have always found that it had been necessary to restrain the animal, because at some unexpected moment it had killed or injured its keeper. In India this would only be looked upon as an ordinary incident in an elephant's life; but if such an event were to happen in our Gardens (as I must say I have felt morally certain it would do sooner or later, if Jumbo remained there), what should we have had to do with the animal? Could we have ever again let him pace about the Gardens with his precious freight of little children on his back? But much worse than even killing a single keeper might have happened if the animal had once got beyond control. We have been warned by high legal authority of our responsibilities on this subject. It is possible that we may have been *too* apprehensive, *too* careful, about the lives of our servants and of our visitors; we may possibly have looked at difficulties incident to the management of our gardens, into the details of which it would be useless to detain this meeting by entering upon, in too serious a light; but this was a case in which we felt that to be on the safe side was the right course to pursue. I do not say that other bolder and more enterprising managers, who might look upon the attractions of the Gardens in a more commercial spirit, might not possibly have taken a different course; for we were quite aware that the loss of the animal might for a time be detrimental to the income of the Society. For this reason we also, as custodians of the Society's finances, thought it not right to decline to avail ourselves of the very unexpected opportunity of diminishing that loss, as far as possible, by the animal's sale. Some persons have called in question the "morality" of this transaction. How any one who has ever sold a horse, cow, sheep, or pig can do so, I cannot imagine. If the purchasers elect to take an animal, knowing all its imperfections, and the vendors are satisfied that it will pass into hands where there is every reasonable prospect of its being properly treated, what more can be required? Then we have been told that we ought to have killed the elephant. To this I decidedly demur, unless the principle is admitted that every one who has a horse, a dog, or any other animal, which has become through any circumstances inconvenient for him to keep, is bound to destroy it. This may be the doctrine of a few visionary enthusiasts, but it is not common sense, it is not humanity. If the life of an animal is of any use to it (and I see no reason why this elephant may not enjoy his life for perhaps a few years longer), there is no reason for taking it away until the time comes when it is absolutely necessary to do so. Besides, as I mentioned before, as trustees and managers of the Society's property, we are bound to look after its finances. You surely all know that the operations of the Society cannot be carried on without means, and that every penny received by the Society is spent upon the purposes indicated in the Charter; and yet many persons (I am almost ashamed to allude to such folly and ignorance), have spoken as if the Council, or the officers of the Society, had some direct pecuniary interest in selling the elephant. This brings me, in conclusion, to the one most serious side among the many ludicrous incidents that have arisen out of this affair. This is the rash or wilful misrepresentations that have been so freely indulged in against a body of gentlemen of whose general qualifications for the offices which they hold it is not perhaps necessary for me to speak in this assembly (their names should be a sufficient guarantee of this), but of whom I may say, from my intimate knowledge, that they are constantly endeavouring, often at considerable personal sacrifice, to bring their varied knowledge and experience to bear upon carrying out the work of the Society for the advancement of science, and for the benefit of the Fellows of the Society and the public generally. Our accomplished Secretary of whose successful general administration of the Society no one who did not know its condition as I happen to do before he took office, and has not watched its growing prosperity for the last five and twenty years, can form an adequate idea, has not been spared, although in his share in this transaction he certainly had no interest but that of the Society at heart.

There is much in this which is to me a novel and painful experience; but I am told that it is what all must expect who undertake the responsibility of any kind of work for the benefit of others. However this may be in political life, it might have been hoped that among those who followed the calmer pursuits encouraged by this Society, there would not have been any found who, either openly or under cover of anonymous slander in newspaper articles, letters, and postcards, would have imputed to us, which I regret to say has been so freely done, motives absolutely contrary to those by which we know we have been ever actuated.

ON DUST-EXPLOSIONS IN COLLIERIES

THE observations and experiments of M. Vital, in France, and of Mr. Galloway, Prof. Abel, and the late Prof. Freire Marreco, in this country, have shown, beyond all question, that we must look to the power possessed by coal-dust, and possibly even by finely-divided incombustible inorganic matter, when suspended in air, of propagating or enlarging the area of an explosion as one of the main causes of those frightful occurrences, which now and then decimate even an entire mining community. There can be little doubt, that so far as the loss of life is concerned, dust-explosions are, as a rule, far more disastrous than mere explosions of gas. A shot is blown out, or, by some mischance, the gas in the goaf, or in some hole in the roof, is fired: the concussion of air raises a cloud of dust, among the particles of which the flame rushes with explosive violence. Fresh dust is



raised, to form fresh fuel for the devouring flame, which, as in the case of the Penygraig explosion, so carefully investigated by Mr. Galloway, is thereby enabled to penetrate and search into the innermost recesses of the workings, provided they be sufficiently dry and dusty. Every particle of free oxygen is thus practically used up, and the resultant atmosphere is a suffocating mixture of nitrogen, carbonic acid, carbonic oxide, hydrocarbons, and partially-coked dust, against which the men, over whom the flame may have passed, with little hurt, have not the slightest chance.

It may possibly be of interest to those who, like myself, have to teach chemistry in a coal-mining district, to know of an experiment which illustrates in a striking manner the main features of a dust explosion. The experiment is to make an explosion at one end of a long and narrow wooden tube, representing the gallery of a mine; to show that the concussion will raise a cloud of finely divided solid matter from the bottom of the tube along which the flame will be propagated and be driven out at the other end. *A* (see figure) is a wooden box 12 inches long, 8 broad, and 6 deep, closed on all sides, with the exception of a rectangular hole ($3\frac{1}{4} \times 2\frac{3}{4}$ inches), into which can be inserted a long narrow rectangular tube (B), also of wood, which may be 20 feet or more in length; the upper side (*cc*) of this tube is hinged, and along the bottom is strewed a thin layer of finely-divided dry coal-dust, or, what is better in the lecture-room, lycopodium powder. Into the wooden box, which in my apparatus has a cubic content of more than a gallon (5 litres) is placed about $1\frac{3}{4}$ pints (say 1 litre) of coal-gas; this can be most readily effected by pouring this amount of water into the box and displacing it over the water-trough by a current of the coal-gas. The opening is then closed by a sliding lid, and the gaseous contents are mixed by violently shaking the box for a minute or so. The end of the long tube (along which the powder or dust has been strewn, and the lid *c c* pushed down) is then inserted into the box, and the gaseous mixture is fired by thrusting a lighted taper through a small hole (*d*) at the end just where the tube enters the box. The mixture of coal-gas and air explodes, and the flame rushes along the whole length of the tube with astonishing velocity, and is driven, often to a distance of six or seven feet, out at the other end, and is followed by a cloud of smoke.

The experiment is unaccompanied by danger, and is so simple that it may be readily performed in a lecture-room. I showed it some time since to a number of colliers and others engaged in coal-mining, and it seemed to bring home to them far more forcibly than possibly any amount of mere description would have done, the real character of the phenomenon. T. E. THORPE

THE PHOTOGRAPHIC SPECTRUM OF THE GREAT NEBULA IN ORION[1]

LAST evening (March 7) I succeeded in obtaining a photograph of the spectrum of the great nebula in Orion, extending from a little below F to beyond M in the ultra-violet.

The same spectroscope and special arrangements, attached to the 18-inch Cassegrain telescope with metallic speculum belonging to the Royal Society, were employed which have been described in my paper on "The Photographic Spectra of Stars" (*Phil. Trans.*, 1880, p. 672).

The exposure was limited by the coming up of clouds to forty-five minutes. The opening of the slit was made wider than during my work on the stars.

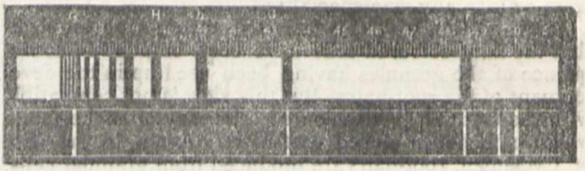
The photographic plate shows a spectrum of bright lines, and also a narrower continuous spectrum which I think must be due to stellar light. The bright stars forming the trapezium in the "fish's mouth" of the nebula were kept close to the side of the slit, so that the light from the adjacent brightest part of the nebula might enter the slit.

Outside this stronger continuous spectrum I suspect an exceedingly faint trace of a continuous spectrum. In the diagram which accompanies this paper the spectrum of bright lines only is shown, which is certainly due to the light of the nebula.

In my papers on the visible spectrum of the nebula in Orion, and other nebulæ (*Phil. Trans.*, 1864, p. 437, and 1868, p. 540; also *Proc.* Roy. Soc., 1865, p. 39, and 1872, p. 380), I found four bright lines. The brightest line, wave-length 5005, is coincident with the less refrangible component of the double line which is strongest in the spectrum of nitrogen. The second line has a wave-length of 4957 on Ångström's scale. The other two lines are coincident with two lines of hydrogen, Hβ or F, and Hγ near G.

In the photograph, these lines which had been observed in the visible spectrum are faint, but can be satisfactorily recognised and measured. In addition to these known lines, the photograph shows a relatively strong line in the ultra-violet, which has a wave-length 3730, or nearly so. The wide slit does not permit of quite the same accuracy of determination of position as was possible in the case of the spectra of stars. For the same reason, I cannot be certain whether this new line is really single, or is double or multiple. In the diagram the line is represented broad, to indicate its relative great intensity.

This line appears to correspond to ζ of the typical spectrum of white stars (*Phil. Trans.*, 1880, p. 677). In these stars the line is less strong than the hydrogen line near G; but in the nebula, it is much more intense than



Hγ. In the nebula, the hydrogen lines F and Hγ are thin and defined, while in the white stars they are broad, and winged at the edges. The typical spectrum has been added, for the sake of comparison, to the diagram.

I cannot say positively, that the lines of hydrogen between Hγ and the line at 3730 are absent. If they exist in the spectrum of the nebula, they must be relatively very feeble. I suspect, indeed, some very faint lines at this part of the spectrum, and possibly beyond λ 3730, but I am not certain of their presence. I hope, by longer exposures and with more sensitive plates, to obtain information on this and other points. It is, perhaps, not too much to hope, that the further knowledge of the spectrum of the nebulæ afforded us by photography, may lead, by the help of terrestrial experiments, to more definite information as to the state of things existing in those bodies.

[1] Paper read at the Royal Society, March 16, by William Huggins, D.C.L., LL.D., F.R.S.

THE ACTION OF CARBONATE OF AMMONIA ON THE ROOTS OF CERTAIN PLANTS, AND ON CHLOROPHYLL BODIES[1]

1. *Roots.*

THE observations which led to the first of these papers were originally made many years ago on *Euphorbia peplus*, and have now been extended to other genera. A plant of *E. peplus* having been dug up and carefully washed, the smaller rootlets may be placed under the microscope without further preparation, the thicker roots may be examined by means of sections. If such roots are left, before being examined, in a solution of carbonate of ammonia (1 to 7 per 1000) for a short time (varying from a few minutes to several hours), they present a wonderfully changed appearance. The most striking alteration is that the surface of the root assumes a *longitudinally striped appearance*, due to longitudinal rows of darker brown cells, alternating with lighter coloured rows. The darker colour is seen under a high power to be due to the presence of innumerable rounded granules of a brown tint, which the lighter-coloured cells are without. Similar brown granules are deposited in cells scattered throughout the parenchyma, and markedly in the elongated endoderm cells surrounding the vascular bundle.

The granules are apparently neither resinous nor fatty, for they are not removed by alcohol or ether; they are,

[1] Abstract by Mr. Francis Darwin of two papers by Mr. Charles Darwin read before the Linnean Society on March 16.

however, slowly acted on by caustic potash, and seem to be of the nature of protein.

It will be observed that the most remarkable part of the phenomenon is that the granules are only formed in some of the external cells, and that these cells are, before the treatment with ammonia, indistinguishable in shape or by their contents from their fellows, which are unaffected by the solution.

There is, however, a curious functional difference between the two classes of cells, namely, that the granular cells do not produce root-hairs, which arise exclusively from the cells of the light-coloured rows. With this fact may be compared an observation of Pfeffer's, that the root-hairs of the gemmæ of Marchantia grow only from certain definite cells. He describes a similar state of things in Hydrocharis, but with these exceptions it seems not to have been hitherto suspected that root-hairs arose from cells in any way specialised.

In connection with this fact, the theory suggests itself that the light-coloured cells have been emptied in consequence of the granules having been used up in the development of the root-hairs. But this view is not compatible with the fact that light-coloured cells may often be found which have not produced root-hairs. Again, in the case of Cyclamen, root-hairs are produced from granular cells. Effects similar to those now described were observed in some other Euphorbiacious plants, *e.g. Phyllanthus compressus*, though not in all the genera of this family which were observed. Among genera belonging to other families may be mentioned Drosophyllum and Cyclamen, as showing the phenomenon especially well. Altogether 49 genera were observed; of these 15 were conspicuously acted on, and 11 in a slight degree, making together 26 genera, while the roots of the remaining 23 genera were not acted on in any plain manner.

Before attempting to draw any conclusions, a few more details must be taken into account. The root must be alive, otherwise no precipitation will take place; the process is therefore a vital one, and seems in some measure to resemble "aggregation," as it occurs in the tentacles of Drosera. In both cases carbonate of ammonia is the most efficient re-agent, but other salts, such as nitrate of ammonia, produce a similar effect. What the nature of the process may be, must remain doubtful. The view here suggested is that the granular matter is of the natuae of an excretion; the arrangement of the dark-coloured cells in rows agrees with what we know of the disposition of certain cells whose function is admittedly to contain excretions. The granules are, moreover, deposited in the loose exfoliating cells of the root-cap where they cannot take part in the life of the root; and this fact points in the same direction.

2. *On the Action of Carbonate of Ammonia on Chlorophyll Bodies.*—The effects of solutions of carbonate of ammonia and of other fluids on the tentacles of Drosera, &c., was described in "Insectivorous Plants," under the name of "*aggregation*." This process consists essentially in the appearance of curiously-shaped masses, of an albuminoid nature, which undergo striking changes of form. The masses were believed to be protoplasmic, but this conclusion has not been generally accepted, and has been called in question by such authorities as Cohn and Pfeffer. The present paper is intended to show that carbonate of ammonia causes a kind of aggregation in chlorophyll bodies; and as these are undoubtedly protoplasmic, the belief in the protoplasmic nature of the aggregated masses in Drosera, and other carnivorous plants, is supported.

The changes which occur in the chlorophyll bodies may be well observed in the case of Dionæa. If a young leaf is immersed for twenty-four hours in a solution of carbonate of ammonia (7 to 1000), and is then examined by making thin sections, the contrast with a normal leaf will be found strikingly great. In most of the cells, not a single chlorophyll-grain can be seen, but in their place are found masses of translucent yellowish-green matter of diversified shapes, resembling in a general way the aggregated masses in the tentacles of Drosera. The matter is not exclusively derived from the chlorophyll-grains, but consists, in part, of matter deposited from the cell-sap, which is often the first to be formed, and is afterwards surrounded by the green matter derived from the chlorophyll-grains.

The same process may be observed in Drosera, and here it is not necessary to make sections, as the chlorophyll-grains may be well seen at the bases of the tentacles. Many observations were made in this way, and also by means of sections. In the case of Drosera it was possible to show that the chlorophyll-grains may recover from the effects of the carbonate—and this is a fact of some importance. After placing drops of various solutions on the discs of leaves still attached to their plants, green spheres or green zones surrounding a central purple mass were to be found in the tentacles. In this case it will be seen that the chlorophyll grains join with the purple cell-contents in forming aggregated masses. These masses were observed to be in constant slow movement. The leaves were then syringed with water and left to themselves for some days. When again examined, the green spheres had in large part disappeared, and instead of them normal chlorophyll-grains were found.

Other observations were made on Drosophyllum, Sarracenia, *Primula sinensis*, Dipsacus, Pelargonium, Cyclamen, and many other genera, with various results. In some cases the chlorophyll-grains disappeared, and the green masses were formed, in other cases hardly any effect was produced; in others again the chlorophyll-grains became confluent and formed curious horse-shoe like masses in the bottoms of the cells.

In the case of Spirogyra the effects of the carbonate were well marked, the spiral chlorophyll body breaking up into variously formed rounded and pear-shaped masses, which slowly changed their outline. Here also plainly-marked deposition of fine granular matter from the cell-sap was caused by the ammonia solution.

Finally, it may be pointed out that whether or not the argument from the facts here given in favour of the protoplasmic nature of the aggregates in Drosera be considered valid, the observations themselves possess some independent interest.

NOTES

In the New Code it is satisfactory to find that science is placed on a fair footing. While in elementary schools, the substratum of instruction, in the form of "obligatory subjects," is reading, writing, and arithmetic, still the grants for optional subjects are such as to encourage teachers to make them a regular part of education. In the class-subjects for older scholars, for example, we find geography and elementary science, and these it is recommended, should be illustrated as far as possible, by maps, diagrams, specimens, and simple experiments. In geography the subjects for the different standards are carefully graduated; in Standard V., for example, such subjects as latitude and longitude, day and night, and the seasons, are set down; under Standard VI., among other subjects, are the "circumstances which determine climate;" and under Standard VII., "the ocean, currents, and tides, general arrangement of the planetary system, the phases of the moon." Under Elementary Science, again, the object of the instruction is stated to be the cultivation of "habits of exact observation, statement. and reasoning." For the first standards, lessons in "common objects, such as familiar animals, plants, and substances employed in ordinary life," are to be given. For Standard IV, there is required "a more advanced knowledge of special groups of common objects, such as (*a*) animals or plants, with particular reference to agriculture; (*b*) substances employed in arts and manufactures; (*c*) the simpler kinds of physical and mechanical appliances, *e.g.* the thermometer, barometer, lever, pulley, wheel and axle, spirit-level." For Standard V. we have "(*a*) animal and plant life; (*b*) the chemical and physical principles involved in one of the chief industries of England; (*c*) the physical and mechanical principles involved in the construction of the commoner instruments, and of the simpler forms of industrial machinery." For Standards VI. and VII. the preceding subjects are set down "in fuller detail." If two class subjects are taken, the second must be, in the lower division, either geography or elementary science; in the upper division, history is added. Grants are also to be given for specific subjects, and in the schedule setting forth the subjects, the instruction is divided into three stages, and includes such subjects as animal physiology, botany, principles of agriculture, chemistry, physics (in two divisions—sound, light and heat, and magnetism and electricity). The syllabus under the various subjects has evidently been carefully considered, so as to give the pupil a fair knowledge of leading facts and principles. It is evident that the New Code, so far as science is concerned, is a great advance on the previous one; science has at last something like fair play, and the next stage will doubtless be to include its elements among the obligatory subjects. There is now, at all events, a real stimulus given to teachers to encourage the pupils to take it up, and every precaution has evidently been taken to stamp out mere learning by rote, and to secure that what science is taught shall be real.

THE first report of the Royal Commissioners on Technical Education has been issued. It states that the Commissioners have conducted their inquiry into the instruction of the industrial classes under the following heads:—The instruction of the proprietors and superior managers engaged in industrial pursuits; that of the foremen, and that of the workmen. During their recent visits to France and the north of Italy, they have collected data bearing on each of those heads, but they consider it is not desirable to publish the whole of the information thus obtained, until they have possession of the corresponding facts about other countries, including the United Kingdom. To publish the information at present without comment, would involve great risk of its not being properly understood, and the Commissioners are not yet themselves sufficiently informed to be able, in all cases, to present trustworthy conclusions. At the same time, they think it desirable to make known, without unnecessary delay, certain very recent changes in the French laws on public instruction, as well as the purport of others still under consideration. These changes are affecting, and will further affect, the ordinary and higher elementary instruction, both literary and technical, of the workmen and foremen in France. With the object of showing their influence upon the former class, an account is given of the present and recent position of various branches of instruction in that country. The report proceeds to give voluminous details respecting the systems pursued in French elementary schools and training colleges. Information is also given respecting adult art schools, shelter schools, State grants for technical instruction, and the outlay of French municipalities for local technical education. These points are dwelt upon as illustrating the activity in France in all that relates to the instruction of artisans. In concluding their report, the Commissioners state that they wish it to be distinctly understood that they have not made any recommendation for the improvement of the instruction of our own artisans beyond the introduction of manual work in some of the elementary schools. They have refrained at present from further recommendations, not because they are not fully alive to the need of greatly improving general and technical training in this country, but because they are at present only at the outset of their mission.

AT the half-yearly general meeting of the Scottish Meteorological Society held in Edinburgh on Wednesday, papers were read by Mr. Clement L. Wragge, on the observations made by him on Ben Nevis last summer; by Mr. Buchan, on the results of the Ben Nevis observations, with more special reference to the Weather Forecasts; and by Dr. Arthur Mitchell, on the Smallpox Epidemic in London during 1881. A gold medal from the Council of the Society was presented to Mr. Wragge in recognition of his valuable services in connection with the Ben Nevis observations.

ON Wednesday evening, at 7 o'clock, Professors Abel and Roscoe, on behalf of the Chemical Society, the Society of Chemical Industry, and the Institute of Chemistry, received a large number of distinguished guests in the Crystal Palace, where refreshments were provided. Fifteen hundred invitations were issued and accepted, not merely within the limits of the United Kingdom, but in continental countries, and several eminent chemists from France and Germany came over expressly to join the gathering. The Crystal Palace was chosen as the meeting place because of the International Electrical Exhibition now being held there; and the party dispersed themselves about the various exhibits. It was a gala night at the Palace, and the different electric lighting systems were shown at their best. The magnificent display by Edison in the Concert Room and Entertainment Hall elicited much admiration; so also did the fine candelabrum of 96 Maxim lights, executed in cut-glass by Messrs. Defries and Sons, and exhibited by the Electric Power and Generator Company. These lights are fed by a Maxim dynamo-electric machine capable of feeding 100 Maxim lamps of 30 candle-power each. The incandescent lamps of Mr. Lane-Fox, exhibited by the Anglo-American Brush Electric Company, the Bright system exhibited by the British Electric Light Company, and the Swan system were visited in turn, as also were the suite of apartments in the northern gallery lit by Edison's lamps. The visitors scattered about the various stalls, and a special train carried most of them away to town at 9.30 p.m.

MR. CLEMENT L. WRAGGE sends us the following communication:—The observations on Ben Nevis will probably be continued during the coming summer, and with this view I hope soon to revisit Lochaber, but it is yet too early to refer to definite arrangements. The museum I have placed in Stafford, the county town of my family, is lent to the town and county for twenty-one years, thereafter to become a gift if certain conditions have been complied with. The meteorological station there will probably be started next month. I regret to say that my negotiations for a central high level observatory on the Peak have fallen through. The owner of one portion of the land, annoyed by the operations of the Ordnance surveyors, has turned a deaf ear to my appeal, being determined to prevent any further trespass; and the agent for another could only give me permission under conditions, one of which was that it would rest with me to compensate the tenants for disturbance of game. Moreover, the Meteorological Office could not see its way to provide an observer, on the ground of indirect telegraphic communication. The instruments were all in readiness; and the barometer, a fine "Board of Trade," reading to 23·6, made to my order by Messrs. Adie and Wedderburn of this city in anticipation of no difficulty, is at present hanging practically idle. A series of high level meteorological stations in direct connection with Ben Nevis, would, I consider, be of the utmost value; and until we get them we cannot hope to perfect our system of weather forecasting.

THE Congress relating to the protection of cables is not the only one which will be held in Paris next April. A circular has been sent to the different powers asking them to appoint scientific delegates to determine the exact length of the mercury column, which is to be considered as equal to an ohm; secondly, to select a new standard for comparing the photometric power of several lights; thirdly, to establish a plan of common co-operation for studying atmospheric electricity as proposed by Sir William Thomson, and adopted unanimously by the Congress of Electricians.

THE French Minister of Public Works having sent a delegate to report on the Smoke Abatement Exhibition, it is supposed that M. Cochery will be obliged to reconsider his refusal to send delegates to the Electrical Exhibition at the Crystal Palace which extraordinary step has caused great disappointment in various quarters.

IN the *État de l'Algérie*, published by the new governor-general, we see that the Algerian system of meteorological observation extends from Mogador to Tripoli. The number of stations is forty-eight, but only thirty-six send daily reports by telegrams. The warnings of the Algerian meteorological office are telegraphed to twelve commercial seaports on the coast of Algeria. The most southern station is Wargla, in the desert where Laghouat, Tuggurt, and some others have been located. This office is directed by the staff of military engineers independently of Paris.

A FEW months ago the Rev. W. S. Green, of Carrigaline, Co. Cork, started on a mountaineering expedition to New Zealand. Mr. Green was accompanied by two Swiss guides, and a telegram just received announces that the party has succeeded in making the ascent of Mount Cook.

A BILL for compelling railway companies to use continuous brakes has passed the second reading in the House of Lords.

SIR JOHN LUBBOCK, as president of the Linnean Society, will give a *soirée* on Tuesday evening at the Society's Rooms, Burlington House.

THE Berlin Society of Commercial Geography, which has been in existence for about two years, has already developed a wonderful amount of activity. It publishes two organs, one weekly, under the title of *Export*, the other *Nachrichte fur Welthandel und Volkwirthschaft* at longer intervals. In both organs, while the development of German commerce is mainly kept in view, that object is sought to be promoted by obtaining at first hand a scientific knowledge of the products and peoples of the various countries of the world. The papers and notes on these points are all of great interest, and form important contributions to various aspects of geographical science. In this respect they form a marked contrast to the publications of similar societies in other countries, and we see one more evidence of the thoroughness of education in Germany, and of the utility of science in all departments of activity. This Society, there is little doubt, will be of great service to the development of German commerce; it seems to have competent correspondents in all parts of the world.

EARTHQUAKES are reported from the following localities:—On February 27, at 9.15 a.m., a number of weak shocks were noticed at Roveredo, lasting about six to seven seconds. They were also felt at Olivone, and far more violently at Bellinzona. On March 4, at 9.5 p.m., a violent shock, accompanied by subterranean noise, was felt at St. Johann in the Wieselburg Comitat (Hungary). The shock lasted two seconds, and proceeded in the direction from south to north. An undulatory and moderately violent earthquake occurred on March 11, at 2.54 a.m. at Metkovich, on the road to Mostar (the scene of the present revolutionary disturbances). Its direction was from north to south. The volcanic phenomena which have lately alarmed the inhabitants of the Ætolian coast have not yet ceased. There is now no doubt that a submarine crater has been formed. A short time ago a tolerably violent shock of earthquake was felt, accompanied by subterranean roaring and hissing. At the same time a strong odour of sulphuretted hydrogen rose from the sea. A thick layer of a gelatine-like mineral matter covers the surface of the sea to a great distance, and floats upon it like a layer of oil. It is not disturbed by the sea being in a high state of agitation, but has, on the contrary, a tranquillising effect upon the motion of the waves.

A TELEGRAM received at Constantinople, March 21, announces that three strong shocks of earthquake were felt on that day on the island of Chios. The population had taken refuge in tents. the temperature is excessively high.

FURTHER intelligence received from Panama states that during the recent earthquake in Costa Rica there was no loss of life whatever, and the damage to property was unimportant.

IT is related by MM. Macé de Lepinay and Nicati (*Jour. de Phys.*), that after a mountain excursion, and five hours among snow-fields, one of them found all artificial lights in town (candles and oil lamps) to appear distinctly green; the effect lasting from 7.30 to 11 p.m. This case of temporary daltonism for red is attributed to the fatigue of the retina for red persisting much longer than that for other colours. The authors describe a simple experiment by which this persistence may be verified. Three coloured glasses are taken, red, green, and blue, which, with average illumination, all bring the visual acuteness to about the same value. Having nearly shut the shutters and placed himself a few yards from a white board with printed letters of different sizes on it, the observer finds that, at the first, he can, with the blue glass, make out pretty distinctly the letters of medium size; whereas, with the red glass, the visual acuteness is so much reduced, that he cannot even distinguish the board. But if the darkness be continued, he observes that, whereas the visual acuteness does not sensibly increase with the blue glass, he is presently able, with the red, to make out, first the board, and then the largest letters. The visual acuteness in the latter case increases, at first quickly, then more slowly, for half an hour, when it becomes nearly stationary. Green glass gives results intermediate between the others. It is important to remark, that in all cases, even after an hour and a half, the visual acuteness with the red glass remains considerably less than with the blue.

A SMALL herbarium of plants, some thirty-five centuries old, must be an object of considerable interest. Such an one has recently been formed by Dr. Schweinfurth, from garlands found on the breasts of mummies discovered last year at Deir el Bahari, by MM. Brugsch and Maspero. Two garlands on the body of the King Aames I., consisted (according to a letter of the Doctor's published in *Archives des Sciences*) of leaves of Egyptian willow (*Salix safsaf*), folded twice, and sewed side by side along a branch of the date-palm, so forming clasps for separate flowers inserted in the folds. The flowers were those of *Acacia Nilotica*, of *Nymphæa cærulea*, with isolated petals, of *Alcea ficifolia*, and of a *Delphinium*, believed to be *orientale*. The garlands of the other kings contained flowers of *Carthamus tinctorius*, and the folded leaves were those of *Mimusops Kummel*. Leaves of the common water-melon (*Cucumis citrullus*) were also found on the body of Neb-Seni, a high priest of the twentieth dynasty. Dr. Schweinfurth managed to preserve many of the leaves and flowers, by moistening them, then putting in alcohol, then spreading out and drying. A remarkable thing is the preservation of colour of the chlorophyll violet in *Delphinium*, green in the water-melon leaves. All the species named are still found in the East; and they afford examples of both spontaneous and cultivated plants, continuing for many centuries without variation.

We have received part 4 of vol. ii. of "Appalachia," the organ of the "Appalachian Mountain Club." This Club is attempting to do in the United States what the Alpine Clubs are doing in Europe, and during the few years of its existence, has accomplished much in stimulating a love of science and mountain beauty in the community, also in making the mountains more accessible, and the more interesting parts better known. The part before us contains several interesting papers, mainly on the picturesque and historical aspects of the Appalachians, but includes a paper giving useful elementary instruction in geodesy. The Club includes many names well known in science in its list, and from the present and previous numbers of its journal, we judge that it is doing good work.

In some experiments with flashing signals by the electric light, conducted on the evening of the 8th, at Woolwich, the clouds were lit up at intervals as far as the zenith over Chislehurst Common, a distance of between five and six miles. The sky was everywhere overcast; but the clouds were not hanging low at the time.

At the Paris Academy of Sciences, on Monday, M. Blavier, mining engineer, called attention to the disappearance of the sardine from the coast of Brittany, where it used to bring in the fishermen 15,000,000 fr. a year He attributed this to a change in the direction of the Gulf Stream, which also accounted for the mild winter and early spring. On the suggestion of M. Faye, the question was referred to a committee composed of MM. Faye, Janssen, Daubrée, and Admiral Jurien.

The grand *soirée* given by Admiral Mouchez at the Observatory of Paris, on Monday, March 13, was very successful. Electricity formed a prominent feature of the entertainment. The illumination of the Salon du Nord by Faure accumulators lasted from 10 p.m. till 7 in the morning without the slightest interruption. Twenty-five Swan lamps were fed by a weight of 2500 kilograms in the accumulators. On the following day at 6 o'clock an experiment was made before Admiral Mouchez to show that about half of the electricity contained in the apparatus had not been used. The total force so accumulated is valued at 40 horse-power, which agrees with the determination given by Sir William Thomson and other experimenters.

The Vienna apiculturists will hold an International Exhibition of live bees, honey, wax, hives, and all other objects relating to bee-culture, on April 8-15 next. Most European as well as Trans-oceanic countries will be represented. This is the first exhibition of the kind in Vienna.

Near St. Etienne (France) a new geyser has been discovered. At a depth of 1500 metres a vein of hot water was tapped, and the result is an intermittent fountain which sends its water to a height of 26 metres. The geyser ejects carbonic acid as well as hot water.

At the monthly meeting of the Council of the Royal Historical Society, held March 17, Lord Aberdare in the chair, Mr. P. Edward Dove, of Lincoln's Inn, was unanimously elected Secretary to the Society.

The Emperor of Russia has allotted to the St. Petersburg Geographical Society a sum of 20,000 roubles as a subsidy towards the erection of a second Russian polar station in Nova Zembla. It is expected that Lieut. Andrieff will be appointed chief of this new station.

According to the *London and China Telegraph* a railway has been constructed in connection with the Kaiping collieries in North China, and permission to run a locomotive has been granted by the authorities. Six miles of line have already been laid down. The locomotive was constructed on the spot by native workmen, and is said to be very creditably done. This is the first railway ever constructed on Chinese soil for the Chinese themselves, and with the consent of the authorities. The abortive Shanghai-Woosung line was built by foreign engineers with foreign capital, against the wish of the Chinese Government.

The additions to the Zoological Society's Gardens during the past week include two Martinique Water-hens (*Porphyrio martinicus*), captured at sea, presented by Lieut. A. H. Oliver, R.N.; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mrs. Hill; a Blotched Genet (*Genetta tigrina*) from South Africa, deposited; two Ruffs (*Machetes pugnax*), two Redshanks (*Totanus calidris*), British, purchased; two Common Badgers (*Meles taxus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

The Approaching Transit of Venus.—In reply to a question in the House of Commons on Monday, the Secretary of the Treasury stated that in connection with the proposed observation of this phenomenon, 275*l.* had already been voted on a supplementary estimate for 1881-2, 14,680*l.* is provided in the Civil Service estimates for the coming financial year, and it is anticipated that about 1000*l.* will be required in the year 1883-4, for the reduction of the observations. A ship of war would convey a party to and from Madagascar. We believe it is proposed to occupy a station on the west coast of this island, the meteorological conditions being more favourable than on the eastern coast, though there is understood to be a disadvantage (any inconvenience from which the presence of a vessel of war may obviate), that the west coast is not directly under the control of the central government of the island.

It is known that the necessary arrangements are being made, with the assistance of a committee of the Royal Society, who have named Mr. E. J. Stone, the Radcliffe observer, to be directing astronomer. If success attends the British expeditions, much will be due to the energy and discrimination which Mr. Stone is exercising in that rather laborious position, as shown by his report to the International Committee on the Transit, held at Paris last October. We understand it is proposed to drill the intending observers, as far as can be done, in preparation for the special features to be noted, under the immediate direction of Mr. Stone, at the Radcliffe Observatory, Oxford.

The Topography of the Planet Mars.—Prof. Schiaparelli has published a second important memoir, entitled "Osservazioni Astronomiche e Fisiche sull' Asse di Rotazione e sulla Topografia del Pianeta Marte . . ." (*Reale Accademia dei Lincei*, anno cclxxviii. 1880–81), to which we shall refer more particularly in an early column. By combining his observations at the opposition 1879–80 with those made at the favourable opposition of 1877, he finds the position of the equator of Mars referred to the earth's equator as follows:—Ascending node (1880), in 48° 7'·8, inclination 36° 22'·9—figures differing little from those provisionally adopted by Mr. Marth.

Ceraski's Variable Star, U Cephei.—Mr. G. Knott, writing from Cuckfield on March 20, remarks that a conveniently observable series of minima of Ceraski's variable, U Cephei (DM. 81°, 25), has come round again. He obtained a good set of observations on March 18, from which the date of minimum (middle of phase) comes out March 18d. 12h. 21m. G.M.T., or about 21m. later than the time deduced from carrying on Schmidt's ephemeris (*Astron. Nach.* 2382), using his period, 2·4927703d. The magnitude of the star at minimum was 9·5, which, Mr. Knott observes, confirms an impression that had presented itself to him from an examination of the light-curve, that *at alternate minima the star touches a somewhat lower magnitude as a rule.* The difference is not great, about two or three tenths of a magnitude, but he believes it has a real existence: an interesting result, if it should be confirmed.

Prof. Pickering, in his "Photometric Measures of the Variable Stars β Persei and D.M. 81°, 25," has the remark: "The star D.M. 81°, 18 is either variable, or its light in grades is erroneously given by Glasenapp." Mr. Knott finds that the star is certainly variable to the extent of about six-tenths of a magnitude, but is not yet able to say anything as to its period. It is a decidedly ruddy-coloured star. Place for 1855·0 in R.A. 0h. 38m. 28s., Decl. + 81° 10'·5. The star is No. 113·4 of Fedorenko's Catalogue.

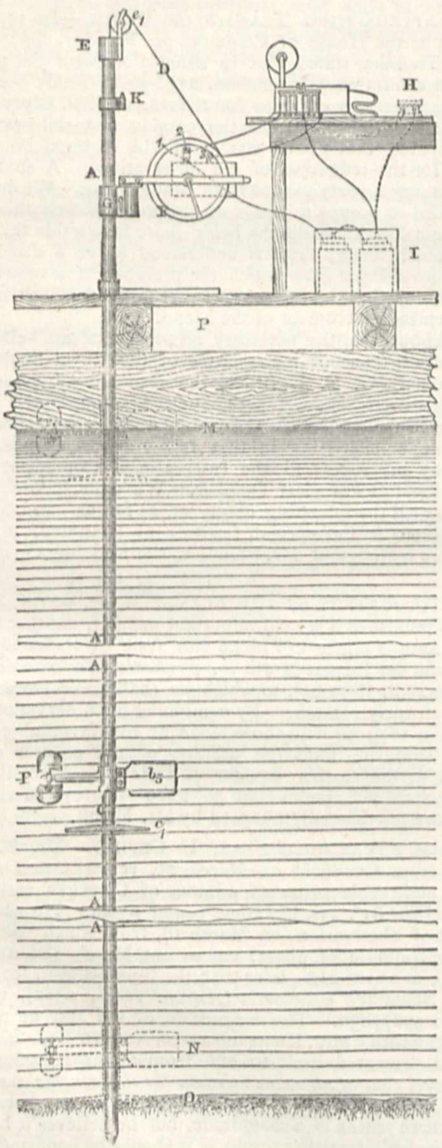
The following are times of minima of U Cephei to the end of April, inferred by Mr. Knott from his observations on March 18 :—

	h. m.			h. m.	
March 23 ...	12 0	G.M.T.	April 12 ...	10 37	G.M.T.
28 ...	11 39	,,	17 ...	10 16	,,
April 2 ...	11 18	,,	22 ...	9 55	,,
7 ...	10 58	,,	27 ...	9 34	,,

THE CURRENT METER OF PROF. A. R. HARLACHER[1]

PROF. HARLACHER, of the Technical High School at Prague, was the first to construct a current meter which obviates all the difficulties and drawbacks of the instruments

FIG. 1.



previously employed. The Harlacher current meter permits the velocity to be determined in the shortest possible time.

It is unnecessary to describe all the stages in the invention of the present form of the Harlacher meter. It is sufficient to say that Prof. Harlacher worked for several years at its improvement, and that his success was acknowledged by the award, at the Paris Exhibition, of two gold medals.

The Harlacher meter is constructed as follows :—For the movable staff, on which the Woltmann meter is fixed, an immovable staff or rod is substituted, which is planted firmly in the bed of the river, and along which the meter slides up and down during the observations on any one vertical. This rod is a cast-iron tube, with a solid point at the lower end, A, A (Fig.

FIG. 2.

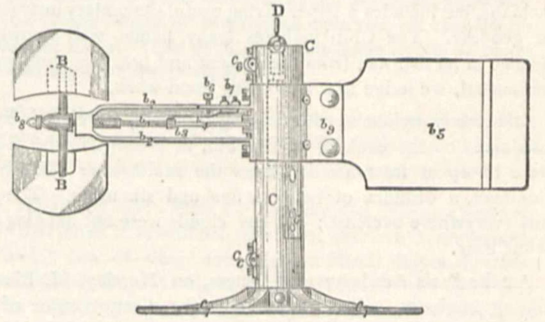
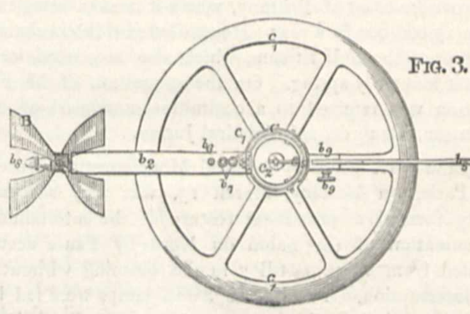


FIG. 3.



1). The other parts of the apparatus, except the electric battery and indicator, are fastened to the tube, so that the whole can be moved from one vertical to another, without having to be taken apart. The screw of the meter, B, is two-bladed. For very small velocities, it would be preferable to adopt a four-bladed screw of larger diameter. The screw is fixed on a steel shaft, b (Fig. 2), which has an eccentric enlargement at one point, b_3. This makes contact with the steel spring, b_4, at each revolution of the meter. These contacts complete the electric circuit, and the current which passes actuates the electric clock

FIG. 5.

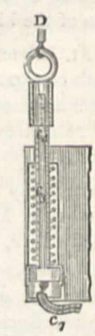
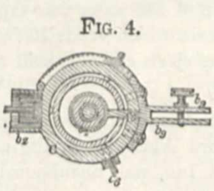


FIG. 4.



or indicator. The weight of the screw B, the shaft b, and the brass box b_2, which carries the shaft, is balanced by a counterweight b_5 (Figs. 1, 2, and 3). This keeps the axis of the instrument in a perfectly horizontal position. The screw, b_6, serves to regulate the pressure of the spring, b_4, while the two screws, b_7, fasten the spring to the brass frame which surrounds and protects the shaft. The shaft is square at the end, which receives the screw, which is put on and held fast by a nut, b_8 (Figs. 2 and 3). The brass frame, b_9, is fixed to a hollow cylinder, C. Below the hollow cylinder, C, is a plate, C_1 (Figs. 2 and 3), which pre-

[1] By Richard Blum, City Engineer, Leipzig. From advanced copy of a paper in the *Proceedings* of the Institution of Civil Engineers, by permission of the Council.

vents the instrument approaching too closely to the bed of the river, where it might be injured or retarded by obstacles. In the interior of the cylinder, C, there is a cylindric case, c_2 (Figs. 3, 4, and 5), in which a brass spring is fastened, and through which the pin, c_3, is carried. To this pin the end of the suspending rope, D, is fastened. The internal diameter of the cylinder, C, is a little larger than the outside diameter of the hollow rod, A, on which it is to slide. The part, c_2, to which the rope is attached, is connected with C by an arm which passes through a vertical slit in the hollow rod, A. Thus, the instrument is kept always, if the pipe, A, is properly placed, with its axis normal to the plane of the cross section. The cylinder, C, is also fitted with rollers, c_6 c_6, which render the motion on the fixed rod easy. After the instrument has been placed on the rod or staff, a bracket, E (Fig. 1), with a pulley, e_1, is attached at the top, and the rope is carried over this pulley. The rope, D, is wound on a barrel, F. This barrel is fixed with the frame, f_1, and the pin, f_2, on the arm, G (Figs. 1, 6, and 7), which is firmly fastened to the hollow rod, A. With the barrel is connected the apparatus, f_3, registering the depth at which the meter is at any moment. The fan, f_4, and gearing, f_5, regulate the rate of rotation of the barrel and permit the adjustment of the speed of the meter in its descent along the rod, A. By the handle, f_6, the meter is again raised. The lever, f_7, and ratchet

Fig. 6.

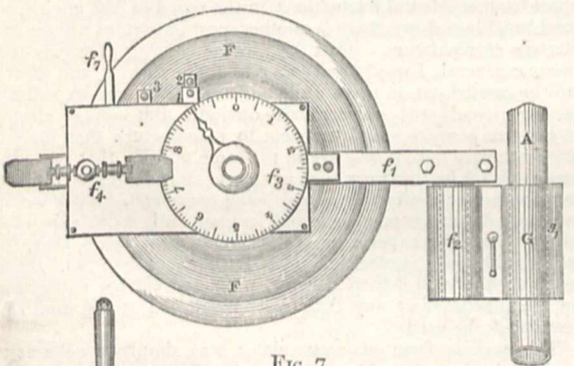
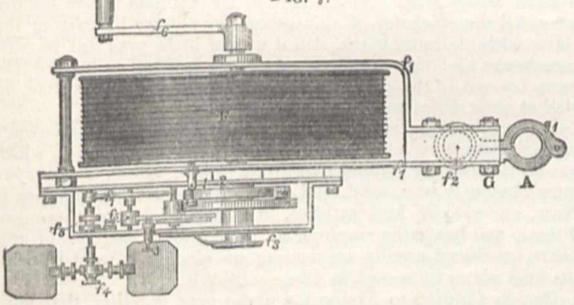


Fig. 7.



wheel, f_8 (Fig. 6), arrest the rotation of the barrel. The movement begins as soon as the ratchet is lifted by the lever. On the frame of the barrel, F, are fastened the contact screws, 1, 2, 3 (Figs. 1, 6 and 7), for attaching the wires of the electric circuit. The screw, 1, is connected with the rope, D, which is a copper-wire rope covered with insulating material. The rope is in electric contact with the shaft of the screw through the spring, c_3 (Fig. 5), because an insulated wire, c_7 (Figs. 5 and 3), connects the lower end of the pin, c_3, and the loop of one of the screws, b_7 (Figs. 2 and 3), which fasten the spring to the brass frame, b_2. The other conductor is the cast-iron pipe, A, which is in contact with the rest of the apparatus through the parts C, G, f_1, f_2 and F (Figs. 6 and 7). These parts are connected with the screw 2 (Figs. 1, 6, and 7). By putting a wire into the loop of screw 3 the depth of the meter below the water-line can be registered electrically. The registering apparatus, H (Fig. 1), has two dials, one marking single revolutions and the other hundreds of revolutions.

If desired, a recording arrangement can be added, the rotations of the meter being marked on a slip of paper in the same way as in a writing telegraph or chronograph. Prof. Harlacher used this arrangement in determining the variation of velocity at a given fixed point. The battery, I, and the clock, or indicator, H, with the rod, A, carrying the meter, are placed on a float, P. The sight vane, K, is fastened to the rod, A, so that it is parallel to the plane of the cross section, and then the axis of the screw is normal to the cross section and parallel to the current. The float is anchored in large rivers and fastened to guide ropes or poles in smaller streams. As soon as the work at one vertical of the cross section is finished, the anchor ropes on one side are slackened and on the other tightened, so as to bring the float into a new position in an easy and a speedy manner. The float must be built so as to be capable of supporting four or five persons.

The determination of the mean velocity at one vertical, by allowing the meter to slide once from the surface of the stream to the bottom, is accomplished thus. The meter, B, and all its connections, C, c_1, &c., are brought to within a few inches of the water surface, the fingers of the electric clock being set to zero. Then the barrel, F, is released by the lever, f_7, Fig. 6. As soon as the axis of the screw touches the water surface a signal is given, the electric clock is brought into the circuit by a spring lever, and begins to count the rotations of the screw. It is necessary to commence with the meter some small distance above the water surface, in order that it may acquire the proper descending velocity previous to the counting of the rotations. In a certain number of seconds the meter descends from M to N (Fig. 1), having at each point in its descent acquired the velocity of rotation corresponding to the velocity of the water at that depth. Dividing the number of revolutions by the number of seconds the rate of rotation corresponding to the mean velocity at that vertical is found. The fact that the disk, c_1 (Figs. 1, 2, and 3), prevents the meter from descending exactly to the bottom entails a small correction. This correction, however, will be more insignificant the larger the difference of the heights M N and N O, that is, the deeper the river in which the observations are made. It is a matter of course that the readings of the instrument at each vertical should be repeated, and the average of the results taken for the true mean velocity. The results of single measurements will not differ much from each other, but the repetition of the reading will give a certainty that all the variations of the velocity at the given vertical are allowed for.

Before using the meter, its constants must be determined in the same manner as with the Woltmann apparatus. A length is marked out in a still-water basin, and the meter is frequently moved through this distance at different speeds. It is essential that the movement of the boat or float on which the meter is fixed should be a uniform one.

The above description of the apparatus will prove that the advantages of this form of meter are of considerable importance.

THE STORAGE OF ENERGY[1]

THE subject of this lecture has been called by the world at large, even by well-informed *Punch*, "The Storage of Force." Why, then, have I ventured, in my title, to differ from so popular an authority? For this simple reason—that you cannot store force any more than you can store time. There is as much difference between force and work, as there is between a mile and the speed of a train or between a ship and a voyage. Work involves two distinct ideas combined, whereas force only involves one. When a weight rests on the ground, the weight pushes the ground down with a certain force, and the ground pushes the weight up with the same force. If, then, there were such a thing as a storage of force, the mere resting of a weight on the ground would be such a storage, since the force exerted between the weight and the ground never grows less. But, I need hardly say, it would be beyond the ability of the cleverest engineer to work a machine, or drive a train, by using a weight resting on the ground; the very expression, "dead weight," shows how useless it is for the practical purposes of producing motion. A weight resting on the safety-valve of a steam-engine may be a very good means of adjusting the pressure at which the valve shall open and liberate the excess steam, but this weight will never work the engine.

Work is force exerted through space; if a weight P be raised through F feet, P × F foot-pounds of work will be done, and there will be a store of P × F foot-pounds of work in the raised weight.

The continuous evaporation of the water from the seas and

[1] Abstract of a lecture delivered at the London Institution on Thursday, March 2, by Prof. W. E. Ayrton, F.R.S.

rivers by the heat of the sun, and its subsequent deposit in the form of rain on the hill-tops, supplies us with another very large raised weight store of energy, and which is practically utilised when the water falling down the hill-side works out water-wheels and turbines.

Various stores of energy arise from the separation of two bodies which desire to come together. The vast fields of coal form an enormous store of energy, owing to the tendency of carbon to combine with oxygen. Copper which is found pure and zinc, when separated from the oxygen with which it is combined in nature, are examples of the same kind. We may also have a store of energy arising from two bodies being too close together, and which desire to move apart; as, for example, in a coiled spring, in compressed gas, in two similar magnetic poles, or in two similarly electrified bodies near together.

The experiments now shown are examples of energy previously stored being utilised. This grindstone is being turned by a falling weight, the ventilating fan by falling water, this saw is worked by the gas-engine, the lathe by this galvanic battery, and the sewing machine by three Faure accumulators.

The water which is falling from the top of the building, and which is working this turbine, was really stored in the cistern for drinking and washing purposes, and, although serving us as a store of energy, it was not pumped up for this purpose. Indeed the price charged for water by the water companies would prohibit its use for the production of power. For, with water at a pressure of 100 feet, and at as low a price as *6d.* per 1000 gallons, it would cost *1s. 4d.* per horse-power per hour if the turbine had 80 per cent. efficiency.

In addition to the natural stores of water-energy on our hill-tops, there are also artificial stores of water-energy, or Armstrong's water accumulators, as they are called, although invented long before Sir William Armstrong's time, and which are employed in many large steel works, docks, &c. Water is periodically pumped into a cylinder with a heavily-weighted piston, which is therefore raised when the water is pumped in. If then at any moment, at any part of the works power is required, a tap is opened, and this large weight falling at the reservoir cylinder, drives out the water and performs the desired piece of work.

Now I want to consider how far it would be possible to drive a tramcar by one or other of these various sources of power. An ordinary tramcar for forty-six passengers weighs $2\frac{1}{4}$ tons, and when full of people about $4\frac{1}{2}$ tons. To pull such a car at the rate of six miles an hour along an ordinary line requires about $1\frac{1}{2}$ horse-power. To produce such an amount of power for one hour requires an expenditure of over 2,800,000 foot lbs. of work, or if produced by a weight falling say through 10 feet, would require the weight to be over 100 tons.

Armstrong's water accumulators are therefore clearly useless for the purpose, and coiled springs are too cumbersome.

Steam-engines are occasionally employed on tram-lines, and from the point of economy are much superior to horses; but there is the great disadvantage of the smoke, noise, and the terror of the horses of other vehicles. A detached tramway engine weighs as much as a full car, consequently nearly half the total horse-power employed is used in propelling the engine and boiler, and there is also the waste of power caused by the rapid radiation of heat from the boiler of a small engine. Gas-engines, though saving the weight of the boiler and coal, have the compensating disadvantage that per horse-power, the weight of a gas-engine is so much greater than that of a steam-engine, and cannot therefore at present be economically employed for tram-cars.

Compressed air engines have been employed with considerable success by Col. Beaumont for driving tramcars, and he has succeeded in storing in one cubic foot of air at 1000 lbs. pressure per square inch enough energy to pull three tons about half a mile along an ordinary tramway. But successful as this system is from the point of economy, there is the same objection that there is to the steam tram, viz. the comparative great weight of the locomotive. The detached compressed air engine weighs about 7 tons, while the car full of passengers is hardly 5 tons, so that seven-twelfths of the total horse-power expended is employed in pulling the compressed air engine alone. I understand it is proposed to build combined cars and compressed air engines, a change that will probably lead to a great improvement.

In order to obtain mechanical motion, we require a store of energy, and some machine for converting the energy stored into mechanical work. Now experiment shows that the weight of an electric motor is but a small fraction of the weight of a small steam-engine and boiler per horse-power developed. Electric motors, indeed, can be easily made to give out work at the rate of 1 horse-power per 50 lbs. dead weight of machine, and hence the great advantage of using them for movable machinery. [Experiment shown of drilling holes in thick wood with a hand electro-motor and raising large boxes with a small electric hoist.] The most economical store of energy we can convert into mechanical energy by the agency of electricity is evidently the energy of coal, and this is the store we shall mainly employ in driving electric motors. That is to say, coal will be burnt to produce mechanical motion, the mechanical motion will work a magneto or dynamo electric machine to produce an electric current, the electric current will be conveyed along the wires, and at the other end, by means of an electro-motor, the electric current will be reconverted into mechanical work. [Experiment shown.]

Instead of converting the electric current energy into mechanical motion I can convert it into heat, and I shall then have, as you see, the ordinary electric light.

But if the engine breaks down, the electric motor at the other end must stop, or the electric light go out; the constant occurrence of which accident has just decided the authorities at the Manchester Railway Station to discontinue the use of the electric light. To prevent this effect following such an accident, an electric accumulator is needed, that is a reservoir which has been drinking in the electric energy when the engine was going at its best, and which will now give it out when the engine has stopped. Again, apart from accidental fluctuations in the speed of the engine, or total breakings down there is another most important use for the electric accumulators. That the electric lighting of towns will become general, I need hardly stop to prove to you, and that it will be carried out in ways quite different from the expedients temporarily adopted is also equally obvious. But users of electricity in this country have at present to manufacture their electricity as they require it, and are in the same position that gas-companies would be in if they were unable to store their gas, but had to manufacture it all while it was being consumed. They would evidently require much larger and consequently more expensive plant. Now the experience of two years has shown that, for large buildings, the electric light is far cheaper than gas. How much cheaper will it then become, when the electric energy can be manufactured at any time convenient, and stored until it is required to be used.

The earliest form of accumulator was simply a voltameter worked backwards. Now although Sir William Grove greatly increased the efficiency of this secondary battery by coating the plates with platinum black, still it was of little practical importance because of the rapid escape of the greater portion of the gases formed, if the charging was continued for a long time, as well as their diffusion through the liquid.

It is clear, then, we must arrange matters so that the passage of the primary current, forms on each plate a substance which has no tendency to wander over to the other. Such a substance must obviously be a solid, and a solid not soluble in the liquid. Now, an oxide of lead satisfies, in a marked degree, these conditions, and hence the employment in secondary batteries of this oxide, produced usually by sending an electric current between the lead plates immersed in dilute sulphuric acid.

But, in addition to having the plates near together, they must have large surface, in order to store much electric energy. And the way to give the plate a large surface, without making it inconveniently large, is to make it spongy. Hence what is aimed at is two spongy lead-plates near together.

Planté's method of accomplishing this occupied some months, and even when "well formed," his cell does not store very much electric energy, so that it has hardly ever been used for any commercial purpose.

In 1880, M. Faure thought of the device of putting a thick layer of red lead on his lead plates, a substance which can easily be reduced to spongy lead by the passage of a current. The plates, after being coated with red lead, are then wrapped in flannel jackets and put side by side in a box, every alternate plate being connected together, so as to practically produce two plates with very large surface very near together. To form the cells, reverse currents are sent somewhat in the same way that they are sent in forming the Planté cell, with the exception that only days and not months are required in the formation. The red lead on the one side is reduced to a spongy material, which is probably lead very slightly oxidised; on the other side, it is reduced to lead peroxide. Charging the cell, by sending a current in the direction of the last current sent, reduces the suboxide to pure lead, and the lead peroxide, on the other side, to an even more oxidised salt. On using the cell to produce an external useful current, the pure spongy lead becomes again slightly more oxidised, and the peroxide slightly less oxidised. In fact, there is a small quantity of oxygen which travels backwards and forwards as the cell is charged and discharged.

Now, does such a cell store electricity? No! emphatically no! When charging it, just as much electricity passes out as passes in, and, when discharging it, just as much electricity passes in as passes out.

Imagine a stream of water was turning a water-wheel, and the water-wheel was employed to raise corn up into a granary, the arrangement might be called one for storing corn, but certainly not one for storing water. So a secondary battery does not store electricity, but electric energy.

The pith, then, of Faure's discovery is the mechanical placing of a salt of lead on the leaden plates the presence of which layer of lead salt enables spongy lead to be produced in a few days, instead of requiring many months, when the spongy lead is electrically formed out of the lead plates themselves by the long passage of electric currents.

The next point to consider is: (1) the storing capacity of such an accumulator; (2) its efficiency; (3) its durability. Now I am, I am glad to say, able to give you more than hearsay evidence on this point, since Prof. Perry and myself have been engaged on rather a long series of experiments on this subject. I may mention that we were both rather sceptical about the merits of the Faure accumulator before commencing this investigation, since we feared that the reports of its excellent action were almost too good to be true. Our doubts, however, gradually dispelled themselves as the investigation proceeded, and we now are able to add our tribute to its practical value.

Let us take a single example of the storing capacity. A certain cell, containing 81 lbs. of lead and red lead, was charged and then discharged, the discharge lasting eighteen hours—six hours on three successive days; and it was found that the total discharge represented an amount of electric energy exceeding 1,440,000 foot lbs. of work. This is equivalent to 1 horse-power for three-quarters of an hour, or 18,000 foot lbs. of work stored per lb. weight of lead and red lead. The large curve shows graphically the results of the discharge. Horizontal distances represent time in minutes, and vertical distances foot lbs. per minute of energy given out by the cell, and the area of the curve therefore the total work given out. On the second day we made it give out energy more rapidly than the first, and on the third more rapidly than on the second, this being done of course by diminishing the total resistance in circuit. During the last day we were discharging with a current of about 25 ampères. But in connection with the storing power, there is a very curious phenomenon to which I think not nearly sufficient attention has been directed, and that is the resuscitating power of a Faure's cell. When a cell has been apparently completely discharged, and is left for a few hours by itself, it appears to have obtained a new charge. For example after the eighteen hours discharge just referred to, although there apparently was no electric energy left in the cell at the end, it was found that after a few hours' insulation, the accumulator could give a current of over 50 ampères, and produce therefore bright flashes of fire. The phenomenon is wonderfully like the invigorating action of sleep. In one case, during our experiments of an extremely rapid and powerful discharge, we found that in subsequent discharges after rest, the cell gave out three times as much energy as it did in the first discharge. The neglect of considering this resuscitating power has doubtless misled many people who have possibly discharged a Faure's cell very rapidly, into under-estimating its storing capacity.

Secondly, as regards efficiency. The efficiency of an electric accumulator—that is, the ratio of the work put into it to the work given out—depends on the speed with which it is charged, and the speed with which it is discharged. If charged or discharged too quickly, a certain amount of energy will be wasted, heating the cell itself; since, whenever a current passes through a body, some heat is developed, and the greater the current, the greater the heat, the latter, indeed, increasing much more rapidly than the current. Now, it is possible, in a way I will not at the moment trouble you by explaining, to distinguish between the work given to the cell to produce chemical decomposition and the work wasted by too hurried charging. Similarly, in discharging, it is also possible to find out how much of the electric energy stored up in the cell is wasted in heating it by too hurried discharging. Allowing for such unnecessary waste, experiment shows that, for a million foot-pounds of stored energy discharged with a mean current of 17 ampères, the loss in charging and dischaiging combined need not exceed 18 per cent.; indeed, in some cases, for very slow discharges, we have found it not to exceed 10 per cent. I do not, of course, mean by this, as some people have mistakenly imagined from the published numbers of Prof. Perry and myself, that a current of only 17 ampères can be obtained by discharging a single cell; since, of course, far greater discharge-currents can be produced if the external resistance be low; indeed, I shall show you a constant discharge of about 70 ampères presently. In speaking of the number 17, I merely mean to say that was the average current when the experiments on the efficiency above referred to were made.

As to deterioration, two months constant charging and discharging of the two test-cells showed no signs of deterioration.

I have said that a cell containing 81 lbs. of lead and red lead stored 1,440,000 foot-pounds of work. Now, consider what that means. It represents all the energy required to be expended to pull a tramcar containing forty-six passengers over two miles, after allowing for considerable waste of power in the electrical arrangements. The electromotor and gearing need not weigh, as I told you, more than about 200 lbs., to produce about two horse-power. We have, therefore, this wonderful conclusion, that about 300 lbs. dead weight contains all the energy and all the machinery necessary for over two miles' run of a tramcar with forty-six passengers. Now, is this result actually obtained at present in the tramcar running at Leytonstone, and which is propelled by Faure's accumulators? No, and why? Partly because the electro-motor has not been made to suit the accumulators, nor the accumulators the electro-motor, nor is the gearing adapted to either.

The cells, as at present made, would not give off their energy quickly enough; hence a greater number are employed, but which, consequently, require to be charged much less frequently than would otherwise be necessary. Indeed, in a ton of the cells as at present constructed, there is about fifty miles' run of a tramcar containing forty-six passengers.

But, in spite of the temporary character of this arrangement, the total weight of the Faure cells, dynamo and gearing combined, used at Leytonstone, is only 1½ tons, or one-third of the weight of a detached steam or compressed air-engine commonly used for tramcars.

Spacious as is the Lecture Theatre of the London Institution, it is unfortunately not large enough to admit a tramcar. I have therefore done the next best thing to prove to you that the Faure accumulators really contain a vast store of available energy. We have here a circular saw which is now cutting wood over an inch in thickness. As you see, the circular saw is driven by that Gramme electro-motor, and the electro-motor itself is fed by the energy stored up in these accumulators, and which was put into them by a dynamo machine yesterday, on the other side of London.

When the Faure's accumulator was first invented, there were various suggestions of electricity being delivered at houses every morning like milk in cans, and the exaggeration of this idea no doubt did something to prejudice the cells in the eyes of the public. The reason why milk is delivered in cans and brought by carts is simply because the total quantity required is so extremely small. If milk were required to be consumed in large quantities like water is, we should have it sent through pipes, and not by cans. The main use of the accumulators will be as stationary reservoirs corresponding with cisterns for water or gasometers for gas. But in certain cases where the accumulators can be used to propel a cart, as in the case of tramcars, not the cart employed solely to carry the accumulators, then there is not the same objection to their being moved about, seeing that the total weight necessary is small compared with that necessary for a steam-engine or a compressed-air engine for tram lines to develop the same horse-power.

Again, just as ordinary electro-motors are not made to discharge a Faure's cell rapidly, so ordinary electric lamps are unsuited for this purpose; and, therefore, although there is enough energy, in a 100 lbs. dead weight of Faure accumulator, to give a light of 1500 candles for thirty minutes, an ordinary electric lamp cannot be illuminated at all by a single cell. Mr. Edison, however, has been turning his attention to this subject, and here is the result of his handiwork, which arrived last night from America, and which is, therefore, shown for the first time in England this evening. This incandescent lamp, as you see, only requires four Faure accumulators to illuminate it, this one eight, and this other one twelve. But must the accumulators be even as large as those I am using on the table? The answer is, No; if you do not require them to give out the light for a very long time. Four much smaller boxes would give just as much light as you see at the present moment; but, of course, would not keep the light burning so long. It is, therefore, now possible to have a box of accumulators and an incandescent lamp, and the whole thing quite easily carried by one man.

Last year Prof. Perry drew attention, in his lecture at the Society of Arts on the "Future of Electrical Appliances," to the great waste of energy that is produced by the coal being carried to the steam-engine, instead of steam-engines being brought to the coal, and the power given out by the engines conveyed electrically to the place where it was commercially required. Why, said he, should not the coal be burnt at the pit's mouth, or in the pit, or even in that part of the mine where the seams were thickest, and the engines driven by burning it used to work large dynamo-machines on the spot, and the power transmitted electrically to any towns where it was required? Again, it has been often asked, why should not the wasted power in streams be utilised? At present it is more economical to use steam-engines in a town than to do work in the country by means of the streams, and convey the manufactured articles over the hills into the towns; and for that reason one sees the old water-wheels, in the neighbourhood of a place like Sheffield, being gradually deserted, and the men preferring to pay a higher rent for steam-driven grindstones in the town, to a smaller rent for water-driven grindstones in the suburbs. The question then arises would it be possible to convey economically the power from the coal-pits or from the streams into the towns by means of electricity; and this obviously turns on, how much power can be got out of one end of a wire compared with the amount that is put in at the other. I have, during this evening, been talking of the measurements of electric energy put into or taken out of an accumulator in foot-pounds, and you may have wondered how it was possible to measure electric energy in the engineer's unit of foot-pounds. In reality it is very simple. The maximum amount of work a waterfall can do, depends on two things, the current of water and the height of the fall. In the same way, the work a galvanic cell or accumulator can do, depends on two things, the current it is producing, and what is called its electromotive force, the latter being analogous with the difference of pressure or head of water. Again, when electric energy is being turned into mechanical work by means of an electro-motor, the energy which is being put into the motor can be measured by the product of the current sent through the motor, and the electromotive force maintained between the terminals of the motor. Now, here are two instruments, devised by Prof. Perry and myself, an Am-meter and a Volt-meter, the one for measuring a strong current, and the other a large electromotive force. With these we will now make simultaneous measurements when we allow this motor, which is driving the lathe, and which is itself driven by an electric current, to run at different speeds. First, we will start with the motor, which has one ohm resistance absolutely at rest, by putting a break on it, and ending by allowing it to run as fast as possible.

Experiment performed and the following results were obtained:—

Speed of motor.	Current in Ampères.	Electro-motive force between terminals of the motor in volts.	Electric power put into the motor in foot pounds per minute.	Power wasted by the current heating the wires of the motor in foot pounds per minute.
0	15	15	$15 \times 15 \times 44.25$ *i.e.* 9956.25	$15^2 \times 1 \times 44.25$ *i.e.* 9956.25
Slow	10	21	$10 \times 21 \times 44.25$ *i.e.* 9292.5	$10^2 \times 1 \times 44.25$ *i.e.* 4425
Fast	4	28	$4 \times 28 \times 44.25$ *i.e.* 4956	$4^2 \times 1 \times 44.25$ *i.e.* 708

We see in the last case, when the load was light and the speed of the motor very great, there was less than one-tenth of the waste of power arising from the current heating the wires when the speed was very slow. On the other hand, we observe that the electro-motive force between the terminals of the motor has been practically doubled.

This simple experiment really points to the solution of economic transmission of power by electricity, and to which Prof. Perry and myself have on numerous occasions directed attention. It is, to allow only a very small current to pass through the wires connecting the electro-motor with the generator, and to maintain a very great electro-motive force between them; since, in this way, the amount of power transmitted can be made as large as we like, and the waste from the heating of the wires from the passage of the current as small as we like.

Reasoning in this way, Sir W. Thomson showed, in his inaugural address last year to the British Association, that, if we desire to transmit 26,250 horse-power by a copper wire half an inch in diameter, from Niagara to New York, which is about 300 miles distance, and if we desire not to lose more than one-fifth of the whole amount of work—that is, to deliver up in New York 21,000 horse-power—the electromotive force between the two wires must be 80,000 volts. Now, what are we to do with this enormous electromotive force at the New York end of the wires? Fancy a servant dusting a wire having this enormous electromotive force. You might as well, as far as her peace of mind is concerned, ask her to put a lightning flash tidy.

The solution of this problem was also given by Sir W. Thomson on the same occasion, and it consists in using large numbers of accumulators. All that is necessary to do in order to subdivide this enormous electromotive into what may be called small commercial electromotive forces is to keep a Faure battery of 40,000 cells always charged direct from the main current, and apply a methodical system of removing sets of 50 and placing them on the town supply circuits, while other sets of 50 are being regularly introduced into the main circuit that is being charged. Of course this removal does not mean bodily removal of the cells, but merely disconnecting the wires. It is probable that this employment of secondary batteries will be of great importance since it overcomes the last difficulty in the economical electrical transmission of power over long distances.

I will conclude my lecture by illustrating one of the other important uses to which the accumulator can be applied, and that is the practical lighting of railway trains, which may be seen in daily operation in the Pullman cars on the Brighton line. The most natural method of lighting a railway train would be to attach a dynamo-machine to the axle of one of the carriages—the guard's van, for example—and the rotation of which, necessarily very rapid when the train is going fast, would, without the use of any gearing, produce the necessary current. But the difficulty that immediately meets us is that as soon as the train slows, or stops at a station, or in consequence of the signal being against it, the speed of the dynamo-machine will diminish and the lights will go out. If, however, while the train is going fast, the dynamo performs two operations, the one to keep the lights burning, the other to charge a battery of Faure's accumulators on the train, then the electric energy so stored can be applied to maintain the lights while the train is going slowly or stopping. With such an arrangement there would be, of course, an automatic contrivance for disconnecting the dynamo-machine from the circuit when the speed becomes too low; otherwise the Faure's accumulators would simply discharge themselves back through the dynamo-machine.

Imagine, now, we are in a train which is going slowly, or which has actually stopped, and that the Faure accumulators lying here on the floor is the Faure battery in the train, and which have been charged when the train was going fast; then that it has sufficient store of energy to continue lighting is proved, because, on connecting these two wires, those fifty Maxim lamps, kindly lent me by the Electric Light and Power Company, and eight Edison lamps before you, are instantly brilliantly illuminated, each of the former possessing about forty candle-power, and each of the latter about seventeen, and giving, therefore, far more light than is, at present, ever supplied to a whole train of twelve carriages. The light, you observe, is perfectly steady, and is turned on and off at will. Imagine, now, we are in a tunnel in the daytime, and the lights, therefore, burning. We now emerge from the tunnel into daylight. I disconnect the wires, and the lights are instantly extinguished. Again, it may be we are entering a second tunnel. The wires are again connected by the guard, and we have the whole of this lecture-theatre, which represents the train, brilliantly illuminated.

There has been an erroneous impression existing lately, that the Faure accumulator could not produce a constant current of more than 17 Ampères; but, that this is a mistake, is clearly seen from the fact that, at the present moment, each of the cells in this room is producing a current of about 75 Ampères.

Electric storage of energy, therefore, makes us nearly independent of accidents to the engine or dynamo machine, or irregularities in their working, enables us to receive our supply of electric energy from the central supply station in our proper turn, and independently of the particular time we require to utilise it, and lastly it enables large amounts of power to be transmitted over very long distances with but little waste.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The following notices have been issued with regard to lectures and classes in Natural Science for the summer term, beginning April 11:—

Prof. Clifton will give a course of demonstrations on instruments and methods of observation employed in optical measurements. The course is intended as an introduction to the study of practical physics in the Clarendon Laboratory. Mr. Stocker will deliver a course of lectures on Elementary Hydro-mechanics, and Mr. Heaton will form a class for the study of problems in Elementary Statics and Hydrostatics, these two courses being designed to meet the requirements of candidates for the Preliminary Honour examination in Natural Science.

Prof. Moseley, the new Linacre Professor of Physiology, proposes to commence a course of Comparative Anatomy, to extend over one year. The Professor's course is open to all students who have attended a course on Practical Biology, or Mr. Robertson's course on Histology. The Professor will attend after his lecture each day until 1 p.m. to superintend the practical work, which will be continued in the afternoon of that day and on the following day, by all students able to attend. Mr. Charles Robertson will give a course on the use of the Microscope and Histology to a junior class. The Professor will give an inaugural lecture on "The Zoological Results of the *Challenger* Expedition," in the large lecture-room at the University Museum, at 8.30 p.m. on Thursday, April 20. The lecture will be illustrated by means of photographs exhibited with the lime-light.

In the Department of Geology Prof. Prestwich proposes to have excursions to visit the several geological sections in the neighbourhood of Oxford on several Saturdays during the Term; and will lecture or give informal instruction on the subject of the excursion on each preceding Friday. Notice will be given from time to time in the *Gazette* and in the Museum of the places to be visited, hours of meeting, &c.

The Biological Fellowship at University College has been awarded, after examination, to Mr. J. T. Cunningham, B.A., late Brakenbury Natural Science Scholar at Balliol College.

Mr. Cunningham obtained a 1st Class in Mathematical Moderations, and a 1st Class in the Final Honour School of Natural Science.

The Delegates for licensing lodging-houses have appointed Mr. E. F. G. Griffith, C.E., to be Sanitary officer of the Delegacy for a period of two years.

Examinations for the Degree of Batchelor of Medicine, both First and Second, will be held in Trinity Term, on days to be hereafter notified.

Candidates for either of these examinations are requested to send their names, on or before May 1, to the Regius Professor of Medicine, Medical Department, Museum.

CAMBRIDGE.—Under the action of the new Statute A, which comes into force from the end of the present term, the entire Cambridge year is compulsorily lengthened a fortnight, and may be further lengthened at the pleasure of the Senate. Three terms are to be kept between October 1 aud June 24 of the succeeding year, to include 227 days. Residence must be for not less than three-fourths of each term, instead of two-thirds as heretofore.

Section 12 of the Second Chapter of the Statute is important in the interests of science and reads thus:—"Students in science who, having already taken a degree in Arts, Law, Medicine, or Surgery, have given proofs of distinction in science by some original contribution to the advancement of science, and have done all that is required by the statutes and ordinances of the University, may be admitted to the title of Doctor Designate in Science, and shall afterwards be created doctors at the time prescribed by the University." In this Statute the claims of original work are fully recognised, and there is only necessary the formulation and promulgation of adequate regulations to place science in a sufficient position of honour in the University. It is provided in a subsequent chapter that honorary degrees in science may be conferred on foreigners or British subjects of conspicuous merit.

Section 19 of the same chapter is important, for it sanctions the adoption of affiliated colleges in any part of the British dominion, the recognition of their lectures and arrangements, and the allowing of periods of study at them not less than two years, as counting three terms towards a Cambridge degree.

The last report of the Board of Examinations (Ordinary Degrees) shows that in the year 1881 there were forty-eight candidates in chemistry, of whom nine attained a first class, and their papers were very favourably reported on, while fourteen failed; the examiner in geology (in which there were only three candidates) recommends that the examination should include one paper devoted to practical work, and that the subject should be divided into two branches, petrology and palæontology, of which one only need be taken. This seems an undesirable separation, seeing that the degree is given for geology only. In botany there were six candidates, of whom three passed in the first class, and three failed. Zoology attracted only two candidates. These results do not show that these latter subjects are neglected, but that a considerable proportion of the candidates who do not take honours, including many medical students, find chemistry the most advantageous subject for the B.A. degree.

The Examiners in Mechanism and Applied Sciences report favourably of the work done; there were five candidates in mechanism, two in electricity, and none in theory of structures. The papers were well done, and showed real interest in the subject, as well as a thorough appreciation of the principles.

SOCIETIES AND ACADEMIES

LONDON

Zoological Society, March 7.—Prof. W. H. Flower, F.R.S., president, in the chair.—The Secretary exhibited and made remarks on some living examples of *Helix hæmastoma* from Ceylon, which had been forwarded to the S ciety by Mr. J. Wood-Mason, F.Z.S.—Mr. W. A. Forbes read a paper on certain points in the anatomy of the Great Anteater (*Myrmecophaga jubata*), as observed in two adult female specimens that had lately died in the Society's Gardens. The arrangement of the ducts of the submaxillary glands and their relations to the stylo-hyoid muscle, the composition of the anterior cornu of the hyoid bone, the presence of clavicles, and the structure of the brain and of the female reproductive organs, were amongst the chief features touched upon.—Capt. G. E. Shelley read an account of the birds collected by Mr. Joseph Thomson while engaged on an exploration of the river Rovuma, East Africa. The collection contained examples of forty-three species of birds, among them being two new species, proposed to be ca' d *Merops dresseri* and *Erythrocercus Thomsoni*.—A second paper by Capt. Shelley gave an account of a series of birds recently collected by Sir John Kirk, in Eastern Africa. This collection was made chiefly in the neighbourhood of Mambois, on the eastern slopes of the mountain-range which separates Ugogo from the Zanzibar province.

Anthropological Institute, March 7.—Major-General Pitt-Rivers, F.R.S., president, in the chair.—Mr. E. F. Newton, F.G.S., exhibited a Romano-British burial urn containing human bones that was found in Cheapside, about 18 feet below the footpath, in 1879. Two of the bones have green glass melted around them.—Mr. E. H. Man read the first part of a monograph on the Aboriginal inhabitants of the Andaman Islands. The paper contained an exhaustive description of the natives, based upon the lines laid down in the "Anthropological Notes and Queries of the British Association." Many points regarding the physical characteristics of these savages, on which misapprehensions have hitherto existed, were noticed. The latter portion of the paper was devoted to a description of the tribal communities and the peculiarities connected with the sub-division of the same among inland and coast-men; and reference was made to the system of rule and the power of the chiefs, and various details connected with their customs and mode of life were dealt with. Their social and marital relations, superstitions, traditions, beliefs, &c., were reserved for discussion on another occasion. The author also exhibited an album containing a number of his photographs of the Andamanese, their huts, weapons, &c., and he further illustrated these subjects on a screen by means of a sciopticon and limelight.—Dr. J. G. Garson exhibited an Andamanese skeleton recently presented to the Royal College of Surgeons by Mr. W. Beatson, of the Bengal Medical Service.

Vienna

Imperial Academy of Sciences, March 2.—L. I. Fitzinger in the chair.—The following papers were read:—W. Biedermann, contribution to general nerve and muscle physiology (Part 8), on the apparent "Oeffnung-zuckung" of injured muscles.—W. Becker, on the knowledge of the mouth-parts of the Diptera.—Fr. Wæchter, on the material particles in the electric spark.—Josef Boehm, on the formation of sulphuretted hydrogen from sulphur and water.—M. Holl, on the correct explanation of the transvere processes of the lumbar vertebræ and the development of the spinal column of man.—E. Lecher, on radiation and absorption.—L. Burgerstein, a geological study on the thermal springs of Deutsch-Altenburg.—A Koch, on the Meteoric fall of Mocs in Transylvania.—E. Hann, on the Foehn at Bludenz.—V. Mises, on the nerves of the eyelids.

Paris

Academy of Sciences, March 13.—M. Blanchard in the chair.—The following papers were read:—Double decompositions of haloid salts of mercury, by M. Berthelot.—On the reproduction, by photography, of different phases of the flight of birds, by M. Marey. An instrument, like a rifle in shape, gives twelve successive images per second, each image taking 1-700th of a second. In bright sunlight, the time of exposure may be reduced 1-1500 sec. (a chronograph regulates the time). With Plateau's phenakistscope, the motion indicated by those images may be easily analysed. M. Janssen (whose "photographic revolver' for observing Venus' transit seems to have been suggestive to M. Marey) made some remarks.—On photography of the spectrum of the great nebula in Orion, by Dr. Huggins.—On an application of the theorem of Abel.—Considerations on the kinetic theory of gases, and on the vibratory state of matter, by M. Ledieu. This is meant to show that the present theory of gases presents a *secondary* kinetic hypothesis, which is quite gratuitous, and three errors of principle. Hence, a certain amount of experimental invalidation of it recently. But the *general* kinetic hypothesis remains intact.—Crystallised oxychloride of gallium, by M. Lecoq de Boisbaudran.—On a case of preservation against anthracic disease observed in man, by M. Cosson. A farmer had a slight anthracic affection, in 1854, and, in February last, another attack of very threatening character, from which, however, he quickly recovered. The earlier attack, M. Cosson conceives to have acted like vaccination.—New facts, proving the extreme frequency of transmission, by heredity, of morbid organic states, produced accidentally in ascendants, by M. Brown-Séquard. He has now, at the College of France, more than 150 animals showing this kind of heredity, all of them guinea-pigs (a species in which the nervous system seems to have a specially strong influence on nutrition and secretions). The new facts here given relate chiefly to alteration of nutrition of the eyeball; also to muscular atrophy through section of the sciatic nerve.—On uniform functions of an analytic point (x, y), by M. Appell.—Tempering by compression, by M. Clémandot. This new process consists in heating (say) steel to a cherry red, compressing strongly, and keeping it compressed till quite cool. The metal becomes very hard, and, like tempered steel, can be permanently magnetised. In either process of tempering *amorphism* is probably produced. There is advantage in the power of graduating the compression.—On the use of bitumen of Judæa in antiquity, as a preservative of the vine, by M. Leclerc. He quotes from an Arabian physician and naturalist of the tenth century, Temimi.—The death of M. Poitevin was announced and commented upon.—The Minister of Agriculture communicated a letter from M. Balbiani urging the importance of methodical experiments (of a nature indicated) both in the laboratory, and in cultivation on a large scale, with a view to destroying the winter egg of phylloxera.—On the theory of uniform functions of a variable, by M. Mittag-Leffler.—On uniform functions presenting lacunæ, by M. Goursot.—On the compressibility of gases, by M. Sarran. He gives some results with a method previously indicated.—Boiling temperature of zinc, by M. Ville. He obtains 930°, closely agreeing with M. Edm. Becquerel (932°). MM. Deville and Terrost's figure was 1040°. The great difficulty of the determination lies in the small calorific capacity of the vapour.—Hydrodynamic experiments (fourth note); imitations with liquid currents, of Nobili's rings, obtained with electric currents, by M. Decharme. A thin liquid stream falls from a vertical glass tube, on a horizontal glass plate covered with a fine layer of minium suspended in water. Concentric rings, &c., are formed.—Apparatus for regulating the flow of a gas at any pressure, by M. Ville. This is placed between an *enceinte* and a reservoir of compressed gas, and, as the pressure in the former diminishes, admits a compensating quantity from the reservoir. Mercury, in a special manometer, closes a circuit, actuating an electromagnet, and thereby a valve.—On the heat of formation of ferrocyanhydric acid and of some ferrocyanides, by M. Joannis.—On the products of distillation of colophony, by M. Renard.—On chlorination of camphor; formation of bichlorinated camphor, by M. Cazeneuve.—On the essence of *Licari kanali*, by M. Morin.—On determination of tannin and œnogallic acid in wines, by M. Jean.—On gastric digestion, by M. Duclaux. *Inter alia*, raw albumen resists gastric juice much, and often leaves the stomach without suffering more than superficial action. Cooked albumen is more quickly attacked; then comes gluten, then blood-fibrin. The question why the stomach does not digest itself is not perplexing, when we know that gastric juice does not act indifferently on all albuminoid matter, but "respects" some.—Influence of the nervous system on the lymphatic vessels, by M. Bert. Stimulation of the mesenteric nerves caused constriction of chyliferous vessels; of the splanchnic, dilatation; but, with a curarised animal, there was dilatation in either case.—Chemical action of different metals on the frog's heart, by M. Richet. The toxicity of metals is not, apparently, related to atomic weight. These experiments give considerably different results from those with fishes. Some metallic chlorides stop the heart in systole, some in diastole.—On the passages by which the seminal liquid and the eggs are evacuated in the common Asteria, by M. Jourdain.—Geographical distribution of Coleoptera in Abyssinia, by M. Raffray.—Mode of formation of the coal-basin of the Loire; causes which modify, at various points, the nature of the coals, by M. Gruner.

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Erratum.—P. 463, column 1, line 17 from bottom, for 6¼ Ampères read "6¼ Milli-Ampères"