THURSDAY, MAY 23, 1895.

WERNER VON SIEMENS.

The Scientific and Technical Papers of Werner von Siemens. Translated from the second German edition. Two volumes. (London: John Murray, 1892 and 1895.)

THESE two large volumes form a complete history of the work of Werner Siemens, and give a very vivid impression of his unceasing activity. In addition to building up one of the largest commercial houses on the continent, and by his inventions and discoveries materially assisting in almost every step which, during the last fifty years, has been made in the application of electricity to the service of man, he has found time to conduct long researches on subjects unconnected with his technical work, and, particular y in his later years, has written several important papers on meteorology. It is chiefly, however, in connection with electro-technology that the name of Siemens is famous, for it is this subject that Werner Siemens in Germany, and Sir William Siemens in England, have made particularly their own.

The first of the volumes under notice contains the "scientific" papers, while the second contains the technical ones; the papers in either volume being arranged in chronological order. The distinction drawn between the scientific and technical papers is more apparent than real, for in most of the papers included under the first of these heads it is very evident that the investigations were suggested by some difficulty met with in practice, or were undertaken with a view to some practical application. Hence it is questionable whether it would not have been better to keep all the papers together, arranging them in chronological order, so as to render the relation between the experimental or theoretical investigation and its practical application more obvious.

The first paper in chronological order is a note on "an application by Second-Lieutenant Werner Siemens for a patent for a process of dissolving gold by means of the galvanic current, and for gilding by the wet method." Although no complete account is given of the method employed, this note is of interest for two reasons. In the first place, the experiments which led to the discovery of this method of electro-gilding were made in a cell at the citadel of Magdeburg, in which place, on account of his participation in a duel, young Siemens was at the time a prisoner; the chemicals and apparatus employed being procured and smuggled into the fortress by a friendly chemist of the town. In the second place, it was the sale of the patent rights in this invention in England which supplied the brothers Werner and William with the necessary funds to carry on their experiments, and so helped to lay the foundation of the important firms of Siemens and Halske in Germany, and Siemens Bros. in England.

Although still in the army, Werner Siemens continued his scientific experiments, the next discovery of importance having reference to the insulation of electric wires with gutta-percha. When the newly-discovered substance, gutta-percha, was first put upon the English market, William Siemens sent a specimen to his brother, who, being at that time engaged in an attempt to discover a practicable method of insulating underground telegraph wires, immediately proceeded to try if this substance was suitable for the purpose, and found that even a thin layer when freed from moisture possessed sufficient insulating power. In addition, the property which gutta-percha possesses of becoming plastic and sticking together when heated, appeared to remove the difficulty of making sound joints between the separate pieces of the covering. At first a hot gutta-percha strip was pressed round the wire by means of grooved rollers, and cables insulated in this way were used on a short underground telegraph line between Berlin and Gross-Beeren, as well as for the submarine mines, the first of their kind, which Siemens laid down for the defence of Kiel harbour. It was found, however, that the method of covering was defective, since the material rolled round the wire often did not stick well together. In order to overcome this difficulty, Siemens, in conjunction with his future partner, Halske, invented a machine by means of which gutta-percha could be continuously pressed round the wire without any seam. The plastic gutta-percha is in this machine forced into a metal box having a number of holes drilled through two opposite sides; the holes on the lower side being of such a size as to just allow the passage of the uncovered wire, while the holes on the upper side are the size of the finished insulated wire. The wires pass through the lower narrow holes into the space filled with hot gutta-percha, and come out through the upper holes covered with a uniform and seamless coating.

In consequence of the perfection with which wires could be insulated by this new method, Siemens was employed in designing and laying the Prussian State telegraphs, and in this connection devised a method for testing the perfection of the insulation during the manufacture of the cable, and also a system of tests for localising the position of any "faults" which might occur after the cable was buried in the ground. While superintending the laying of the Red Sea cable, these systematic tests were further elaborated by Siemens, and the success which attended the laying of this cable, as well as the numerous others laid by his firm, may be traced in a great measure to the severe and continuous testing to which the cables were subjected during the process of manufacture and the subsequent laying.

In practically all the earlier telegraph lines of the Prussian telegraphs, underground conductors were employed, since Siemens considered they were better than overhead conductors, being less liable to malicious or accidental injury. In addition, they are unaffected by the atmospheric electricity, which in a dry climate often renders the overhead lines unworkable. Although these underground lines were in after years a source of constant trouble, on account of the frequent break-downs, attributed by Siemens to careless and defective repairing, yet their use led him to two very interesting discoveries. In the first place, he found that an underground cable acted like a large Leyden jar, the copper conductor forming the inside, and the moist earth the outside coating. On this account, it was found necessary to design special apparatus to work satisfactorily through these underground lines, and the practice obtained in designing such instruments must have stood him in good stead when he came to deal with submarine cables, in which the same capacity effect is met with. The second point was the observation that very strong earth currents—that is, electric currents through the crust of the earth—were produced whenever the aurora borealis was visible.

There is one paper which, although it is included in the first volume, certainly describes a rather amusing practical application of electricity. Werner Siemens, with a party of friends, had ascended the Cheops pyramid, and after reaching the top they noticed that the wind, which had been continually increasing in strength, was raising the sand of the desert with a continuous whirling motion. "When it had arrived at the highest step we noticed a whistling noise, which I ascribed to the increasing violence of the wind. The Arabs, who were squatted around us on the nearest steps, sprang up suddenly with the cry 'Chamsin,' and held up their forefinger in the air. There was now a peculiar whistling noise to be heard, similar to that of singing water. We thought at first that the Arabs were uttering this sound, but I soon satisfied myself that it also took place when I stood upon the highest point of the pyramid and held up my own forefinger in the air. There was also a slight, hardly perceptible, prickling observable on the skin of the finger which was opposed to the wind. I could only explain this fact, observed by all of us, as an electrical phenomenon, and such it proved to be. When I held up a full bottle of wine, the top of which was covered with tinfoil, I heard the same singing sound as when the finger was held up. At the same time little sparks sprang continually from the label to my hand, and when I touched the head of the bottle with my other hand, I received a strong electric shock. It is clear that the liquid inside the bottle, brought into metallic connection with the metallic covering of the head of the bottle through the damp cork, formed the inner coating of a Leyden jar, whilst the label and hand formed the outer coating. When I had completed the outer coating of my bottle by wrapping it in damp paper, the charge was so strong that I could make use of it as a very powerful weapon of defence. After the Arabs had watched our proceedings for a time with wonder, they came to the conclusion that we were engaged in sorcery, and requested us to leave the pyramid. As their remarks, when interpreted to us, were without effect, they wanted to use the power of the strongest to remove us from the top by violence. I withdrew to the highest point, and fully charged my strengthened flask, when the Arab leader caught hold of my hand and tried to drag me away from the position I had attained; at this critical moment I approached the top of my flask to within striking distance of the tip of his nose, which might be about 10 m.m. The action of the discharge exceeded my utmost expectation. The son of the desert, whose nerves had never before received such a shock, fell on the ground as though struck by lightning, rushed away with a loud howl, and vanished with a great spring from our vicinity, followed by the whole of his comrades. We had now a full opportunity of carrying out our experiments."

Before 1860, when Siemens published his paper on a reproducible unit of resistance, there was no generally accepted unit, so that it was impossible to compare the results obtained by any one observer with those obtained by any other. The need of such a unit is very well illustrated in one of the early papers in these volumes, where the unit of resistance used in an investigation is said to be the resistance of an iron telegraph wire 2 m.m. thick and 100 Russian versts long! At the present day, with our well-defined systems of electrical units, it is almost impossible to imagine the difficulty and confusion which must have existed when resistances, to take one example, were stated in such terms as that mentioned above. It is true that Jacobi had previously proposed as unit the resistance of a certain copper wire in his possession, and had issued copies of this unit. These copies, however, varied so much one from another as to be quite useless for the more refined and accurate measurements which the previously mentioned tests for localising the faults in underground conductors rendered necessary. Weber also had proposed his "absolute" unit of resistance, but at this time no trustworthy experiments had been made so as to embody this "absolute" unit in a material resistance. Siemens was thus led to the adoption of another arbitrary unit of resistance, and for this purpose chose the resistance at 0° C. of a column of mercury 100 c.m. long and having a cross section of one square millimetre. He employed mercury, since it can be comparatively easily prepared in a practically pure state, and being a liquid its molecular condition, and hence its resistance, does not alter with time, as it was quite possible that of a solid metallic wire might do. This unit, known as the Siemens unit, came into very general use, particularly on the continent. Nevertheless, the Paris Congress in 1881 decided to use as the international unit of resistance the nearest approach possible to Weber's "absolute" unit, in order to bring the resistance unit into agreement with the other electrical units. On this subject Siemens says:—

"It was certainly somewhat hard for me, that my resistance unit, arrived at with so much trouble and labour, which had, speaking generally, made the first comparable electrical measurements possible, then was employed for more than a decennium throughout the world and adopted as the legal international standard resistance for telegraphy should have suddenly to be set aside with my own co-operation." (Siemens was the German representative at the Paris Congress.) "But the great advantage of a theoretically established system of standards consistently carried out necessitated this sacrifice offered up to science and the public interest."

One cannot help sympathising with him in this matter, for it is always hard to disown one's own offspring, particularly after they have had a comparatively long and brilliant career.

Most of the earlier papers in both volumes deal either directly or indirectly with telegraphy. In the remaining portions of either volume, however, a very prominent part is played by papers and inventions in connection with the conversion of mechanical energy into electrical energy. In connection with a form of magneto-electric machine, *i.e.* one in which the magnetic field is produced by permanent steel magnets, for use in telegraphy, Siemens invented a form of armature, which has since been known as the Siemens armature. This armature is shuttle-shaped and has an iron core, the cross section being something like an H, and has the wire wound longitudinally in the two grooves. Wilde, who may be said to have taken the first step in the direction of the evolution of the modern dynamo, combined two machines with Siemens' armatures, one a small magneto, the other a large machine with electro-magnets in place of the permanent steel magnets. The armatures of these two machines were rotated, and the current from the magneto was led round the electro-magnets of the other machine. In this way, the magnetic field in which the armature of the large machine rotated, was very much stronger than it was possible to obtain with permanent magnets.

"The technical knowledge of the production of electric currents by means of mechanical power had extended thus far," says Siemens, "when I succeeded, in the autumn of 1866, in obviating entirely the need of steel magnets. The well-known fact that the electric current driving an electro-magnetic machine (motor) is considerably weakened by the induced currents produced in the windings of the electro-magnets, made it appear probable to me that by driving a properly constructed electro-magnetic machine backwards, the slight magnetism remaining in the electro-magnets must be considerably increased since the induced currents are then produced in the same direction as those due to the existing magnetism. Experience confirmed my conjecture. I called this new kind of current-producing machine dynamo-electric, as by it mechanical force is directly changed into electric currents, whilst the magnetism only appears as an intermediate product, not as the real source of the current produced."

Siemens communicated a paper on this new dynamo-electric machine to the Royal Academy of Sciences of Berlin, on January 17, 1867. A few weeks later, William Siemens, at his brother's suggestion, communicated a paper to the Royal Society on this subject. This paper was read at a meeting at which Prof. Wheatstone, who, without knowing of Werner Siemens' discovery, had been working at this question, read a paper embodying the same idea. Some time afterwards it became generally known that a provisional patent, which had been kept secret, and which also covered this invention, had been issued to the Brothers Varley in December 1866.

It appears, therefore, that several people hit upon what may be called the dynamo principle almost simultaneously. From the fact, however, that Siemens was the first to publish the discovery, according to the usually accepted principle introduced by Arago, there seems no doubt that his claim for priority is justified.

This claim for priority with reference to the invention of the dynamo is made again and again in several addresses, &c., in the second volume. As most of these papers are mere repetitions, one of another, it is very doubtful whether any good purpose is served by printing more than one, since the reader becomes very tired of being taken over the same ground several times.

At the end of the second volume there are a number of patent claims, &c., for meters to measure electrical energy. The demand for such a meter, which should combine accuracy with a moderate cost, arose directly the supply of electric current for lighting and power purposes became at all general. Such a demand in connection with any electrical subject was always for Werner Siemens almost a mandate, and he at once devoted a good deal of time and attention to supplying this want.

The chief interest of most of the papers is, no doubt, historic; the two last of all, however, have a special interest at the present moment in this country. They form an appendix to the second volume, and have reference to the foundation by Werner von Siemens of the Physico-Technical Institution at Charlottenburg. The reasons given by Siemens for the foundation of such an institution in Germany apply to the case of our own country at the present day, for we are still without such an institution, though, through the munificence of Dr. Ludwig Mond, the region of usefulness of the Royal Institution is to be extended in this direction. Siemens, during his long and successful career, had noticed that although the general standard of scientific education was probably higher in Germany than in any other country, the result was to produce not so much scientific workers and discoverers as teachers.

"Scientific investigation," he says, "itself is nowhere a life vocation in the State organisation, it is only a permitted private business of the learned besides their vocation, teaching business. . . . It must, however, be pointed out as a waste of national strength, that highly gifted inquirers, talents such as only seldom come to light, are heavily burdened with professional (? professorial) labours, which others would perhaps perform even better, and are thereby in great measure withdrawn from science itself, to which they would bear incalculable service if they could give themselves up entirely to it. But it is a still greater pity that so many talented and highly-cultured young students find no opportunity to carry out scientific work. The unfortunate consequence in most cases is that scientific labours which would animate and fructify whole domains of life, remain undone, and that, in the struggle for existence, talents do not develop or fall to the ground unrecognised, which under more favourable circumstances would have been able to perform great things to the honour and to the material advantage of the country. It is to be feared that the advantage . . . of better scientific instruction and of more widely-spread scientific culture, will soon be lost . . . if it is not supported by State organisations. These organisations would have to fulfil a double purpose, to advance scientific inquiry generally and to aid industry by means of the solution of scientific technical problems and questions which are essential to its development. . . . In order to make clear the great importance which such an institution, well supplied and liberally endowed, would have on the development of industry, a short retrospect of the history of this development is quite sufficient. We see this everywhere associated with persons and institutions, where it was possible by specially favourable conditions that scientific researches went hand-in-hand with their technical applications. The scientific light, which in consequence led technical combinations and methods, gave such institutions such a preponderance over others that the cost of experiments was not only covered by the higher commercial results, but also whole branches of industry were radically transformed by them, and new ones of great importance created. . . . This combination is most easily realisable in chemical manufacture. . . . More unfavourable is, however, the position of the trades depending on mechanical bases. Exact physical experiments demand much more costly instruments and specially-prepared rooms. . . . If the State, therefore, confines itself as heretofore only to looking after instruction, the mechanical crafts necessarily lag behind the chemical in their develop ment."

Thus spoke Werner Siemens, a man who, by his long and eventful life, was specially qualified to speak with authority on this subject, and the results which have, during the few years of its existence, already been achieved at Charlottenburg are proving him a true prophet.

In conclusion, we may say that these volumes will be found most interesting, not only on account of the insight they give regarding the development of the electrical industry, but also on account of the interesting personality which pervades the whole. W. WATSON.

ATMOSPHERIC PRESSURE OF THE NORTH ATLANTIC OCEAN.

Répartition de la Pression Atmosphérique sur l'Océan Atlantique Septentrional, d'après les Observations de 1870 à 1889, avec la Direction Moyenne du Vent sur les Littoraux. Par le Capitaine G. Rung. (Copenhagen: 1894.)

THIS Atlas, showing the monthly and annual atmospheric pressure and prevailing winds over the North Atlantic and connected seas, is a fine example of cartography and typography. The monographs for this and the other oceans have generally dealt only with February, May, August, and November; but this work presents us with the results for each of the twelve months, and for the year, on a mean of the twenty years from 1870 to 1889.

The really heavy part of the work carried out by Captain Rung has been the calculation of the monthly means from the nine years' daily weather charts of the Danish and German meteorologists from December 1880 to November 1889, including the similar charts of the Meteorological Council for the year ending August 1883. This has been done for eighty points over the ocean between lat. 10° and 77° 30′ N. and between long. 25° E. and 80° W.

It being desirable that the discussion should cover a longer period than nine years, the twenty years ending with 1889 were adopted, these years being selected with the view of utilising the fifteen years' means (1870–84) for this part of the globe which have been published in Buchan's "*Challenger* Report on Atmospheric Circulation," thus greatly facilitating the inquiry. The means for the subsequent five years were independently worked out, and thereafter combined with Buchan's to make up the twenty years' means. The next step was to bring, by the usual method of differentiation, the nine years' means of the ocean stations to approximate means for the twenty years. Table iv. gives the means thus calculated for ninety-two coast or land stations surrounding the ocean, and Table v. for the eighty ocean stations. The mean directions of the wind have been calculated for the stations in Denmark and its colonies; but for all other stations the data have been taken *simpliciter* from the "*Challenger* Report." It might materially have aided the inquiry in the north-western part of the ocean if means for pressure and wind direction had been calculated and given for the Labrador stations at Hoffenthal, Zoar, Nain, Okak, Hebron, and Rama, the observations at which have been published from 1882 to 1889.

The monthly and annual means for the eighty ocean stations, and the charting of the results on the thirteen maps, constitute the novel part of Captain Rung's work, and must be regarded as a substantial addition to our knowledge of the meteorology of the North Atlantic. This remark holds good emphatically as regards the northern half of this ocean, and for the five months from May to September. Thus, for these months, we have now a more accurate knowledge of the distribution of atmospheric pressure and of the prevailing winds north of latitude 60° than could have been obtained from any work previously published on the subject.

But such well-merited praise cannot be extended to the working out of the results for the five winter months from November to March. An examination of the Danish and German daily weather-maps of the Atlantic of the nine years for these months shows that over the whole ocean to the north of a line drawn from St. John's, Newfoundland, to Valentia, observations from a ship at sea is an event of extremely rare occurrence. The consequence is that the monthly means for this important region, from which fresh information is so desirable, have been obtained wholly from the observations made at the land stations of this part of the ocean. Hence the results given in the Atlas cannot be regarded as a contribution to the meteorology of the ocean. In this Atlas, what strikes one at first sight as new fact is the distribution of atmospheric pressure during the winter months from the south-west of Greenland round by Iceland to north of Norway, particularly the three or four distinct areas of pressure a little lower than prevails generally over this region. But a close examination of the daily weather-maps themselves suggests the idea that these three or four low-pressure systems may be no more than the outcome of an interpretation, made in constructing these daily maps, of the amount of pressure over the ocean drawn from the pressure and winds observed at the land stations, the interpretation being made in the complete absence of observations at sea. Thus the observations made at the Greenland stations since 1840 amply show that the winds on its coast are very greatly deflected from their true direction, as that would be determined by the distribution of pressure, by the high ground and valleys near the coast. It is in this connection that a discussion of the Labrador observations would have come in so handy.

Captain Rung has raised a side issue to his report in a discussion of the distribution of atmospheric pressure in the interior of Southern Scandinavia, where the Atlas shows a singular local excess of pressure in the winter months, which excess is also plainly shown by his monthly means of the Norwegian, Swedish, and Danish stations. In looking closely at this matter, it is necessary to leave out of view the means for Dovre, Tönset, and Röros, which approach to, or exceed, 2000 feet above the sea, their positions not being suitable in discussing small sea-level differences of pressure such as are here dealt with. We have calculated afresh the January means for all other stations not exceeding 620 feet in height, for the same twenty years, and obtain a set of figures differing widely from those published in the Atlas, which give no countenance to the idea of a local excess of pressure in winter over this region. To test the matter in another way, several means for the same stations for ten years each from the observations of the last quarter of a century have been calculated, with the result that none of these series show an excess, the only variation being such as appears in the isobars of this region for December, January, and February in the maps of the "*Challenger* Report." Finally, on comparing the means for the twenty years given in the Atlas with those we have newly calculated, the strange result comes out that to the north of a line drawn from near Hernösand in Sweden, to a point fifty miles to the north of the Skaw, the pressure means of the Atlas are all in excess of the other means from 0·030 inch downwards, whereas to the south of this line, the pressure means of the newly calculated stations are all in excess of those of the Atlas from 0·030 inch downwards. For now many years, this error has appeared in nearly all maps published on the continent showing the distribution of atmospheric pressure over its surface; and it received greater currency by being adopted in 1887 in the *Meteorological Atlas*, forming part of *Berghaus' Physical Atlas*. It is probable that the error would never have appeared, if there had been established in Southern Scandinavia a true high level Meteorological Observatory, that is, an observatory situated on a peak such as we have in the Ben Nevis Observatory and the other high level observatories on the continent.

OUR BOOKSHELF.

Text-book of Anatomy and Physiology for Nurses. Compiled by D. C. Kimber. (London: Macmillan, 1895.)

THIS is a book of 268 pages on anatomy and physiology, written by a member of the nursing profession. The author states that the text is compiled from many well-known books, and that nearly all the illustrations are figures taken from standard works. On first taking up the book, we were surprised at the amount of detailed anatomy it is considered necessary to impart to nurses in the American training schools, and we are told that the scheme of the book has been practically worked out in class-teaching. So far as we can judge, the class-teaching is conducted in a radically wrong way. In the first place, there are no directions for practical work anywhere in the book. Anatomy and physiology cannot be taught to any one without observation; and with women entering so practical and serious a profession as nursing, actual observation and simple experiments could be insisted upon and more easily carried out than with a class of school-girls. If the work is to be considered as a text-book only, it is far too difficult to be put at once into the hands of a nurse; yet the author makes no statement about previous knowledge. The descriptions given of structure and functions must surely be in many cases very difficult, if not impossible, for beginners to understand, for such descriptions often consist of a few sentences slightly modified, apparently taken from full accounts found in well-known books. Such detached sentences alone, although correct enough in themselves, can lead to no proper understanding of the subject. The book is burdened with much detailed anatomy, such as of the bones, muscles, development of blood-vessels, which although possibly of use to nurses, would have better given place to a simple, clear, and connected description of the general structure and functions of the body. The arrangement observed in the book is not good, and some subjects are treated of in a wrong connection. For instance, the disposition and action of the muscles of the eyeball are considered in the chapter on muscles in general, as is also the action of the muscles of respiration, and these descriptions are consequently inadequate. There are instances of anticipation of topics, strange sentences thrown in, which must be unintelligible until matters treated of later have been grasped. In the chapter on the heart, the author describes almost at once the arrangement of the muscular fibres of the chambers, before even a general description of the organ is given, or the words auricle and ventricle defined; in fact, the whole description of the heart should be much clearer, and the account of its action fuller and more accurate. It would be easy enough to point out some loose and erring statements, and one or two misprints; we are told, for instance, that water is produced "when two molecules of oxygen unite with one of hydrogen." It is far the best for nurses to learn the anatomy and physiology they require from anatomists and physiologists, and nursing from nurses. The book, however, contains a full and excellent glossary.

Calcareous Cements: their Nature and Uses. By G. R. Redgrave. (London: C. Griffin and Co., Limited, 1895.)

MANY valuable contributions to the wide literature of cements have appeared from time to time in the engineering and chemical journals devoted to the industries. Several of these are of foreign origin.

The author of this work is to be congratulated on having collected, in a handy volume of 222 pages, all the most interesting and important facts dealing with the history, manufacture, testing, &c., of "Calcareous Cements."

The volume is divided into sixteen chapters and eight appendices. The first three chapters are devoted to a historical review of the subject, and then follow in systematic order chapters dealing with the various stages in the manufacture of Roman and Portland cements.

Chapter viii. contains a short but accurate account of the researches of Fremy, Le Chatelier, and Landrin on the setting of cement. The author has given to the subject of cement-testing its fullest importance; the various methods and appliances for determining the strength of cements are fully described, and the use of Unwin's formula is clearly stated. The last chapter deals with different specifications for cement. In connection with this subject, the author deplores the want of a uniform and generally accepted system of cement-testing in this country; and, in the hope no doubt of stimulating consumers and manufacturers to an agreement, he gives, in Appendix E, a full translation of the German standard tests.

It is not encouraging to find that an industry which originated in England with the work of Aspdin and Smeaton is slowly but surely passing over to the continent. The annual production of cement in Germany equals that in England; but that is not all, *starting with raw materials of an exceedingly unfavourable character*, Germany produces a finer and more reliable cement than that manufactured in England, and at no greater cost. French cement is also, as a rule, superior to the English article.

A figure of Scheibler's, or any other form of calcimeter, in the chapter on chemical analysis, due to Mr. Spackman, would help to make the work more complete in itself; and Schumann's convenient apparatus for determining the specific gravity of cement is not mentioned; the cumbrous Keates' bottle is alone described and figured.

The illustrations, thirty in number, are good, and the book is supplied with a very complete index.

E. A. 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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Origin of the Cultivated Cineraria.

IT appears to me that Mr. Bateson very imperfectly appreciates the nature of the problem of which he has hazarded what I venture to think an ill-considered solution.

In my last letter I pointed out briefly the grave objective difficulties which he had to face in substantiating his case. As Mr. Bateson is, by reputation, a serious naturalist, I think it was his duty to take up the challenge which I virtually threw down to him, and deal with the points which I brought under his consideration. This he has not chosen to do, but falls back again upon his "historical evidence" and his dialectic.

Now I must confess that I am myself as much bored as I suppose most people must be with the "modern Cineraria." And I grudge the time demanded for the discussion of a point which I brought forward as a merely incidental illustration. It may, however, be useful in saying all that I intend to say in reply to Mr. Bateson, to make a few general remarks on the whole subject.

It is apparently the fashion nowadays for the younger biologists to undertake the reconstruction of the Darwinian theory. The field is undoubtedly open, and posterity may safely be trusted to appreciate the value of their labours. But I cannot but observe that as between them and the author of the theory, there is this difference. Mr. Darwin, as he has told us, spent the best part of his life in studying patiently and sifting critically a vast mass of observation and fact. Ultimately he permitted himself to draw certain conclusions. The result is that if you take any statement which Mr. Darwin has put forward, you may feel assured that behind it is a formidable body of carefully considered evidence not likely to be upset. With the modern writers on evolution, the position is exactly the opposite. They launch their theories gaily on the world, and on demanding their substratum of facts, one is told that that is a matter for future collection. I myself am old-fashioned enough to think that, of the two methods, that of Mr. Darwin is the sounder, the more scientific, and in the long run the more convincing.

I have pointed out again and again the vast wealth of material for the scientific study of variation which is presented every day to the eyes of any one engaged in horticultural practice. The difficulty is that few persons possess either the scientific capacity, the patience, or the leisure for its profitable utilisation. We want, in fact, for the purpose a second Darwin, or at least a Herbert.

In his "Variation of Animals and Plants under Domestication," Mr. Darwin made a use which was remarkably effective of the observations made by "practical men" in horticultural literature. They served his purpose in establishing, as had never been done before, the amount and character of the variation which was possible under artificial conditions, and therefore, by analogy, under natural. But this class of evidence appears to me unsatisfactory for the investigation of the further problem which is at the moment of supreme interest, the nature and laws of variation itself. I think that Mr. Darwin squeezed out of it all that it would profitably yield. And for this reason: the evidence is not scientific—that is to say, it was never drawn up by persons having in view the requirements of scientific exactitude. Those who gave it have been pressed into court in a cause in which they never contemplated engaging. This has the merit of ensuring that their evidence is unbiassed, but it does not allow of its being pushed further than what it is capable of proving.

The defects of horticultural evidence may be illustrated in a variety of ways. One or two will suffice. In the first place, is the weakness of its nomenclature. Horticulturists are not, for the most part, skilled botanists. When they give a plant name, it is impossible to be sure that it is what a technical botanist would accept. It is as if one were reading the writings of a chemist, and when he mentioned potassium, the doubt occurred as to whether it was not lithium which was intended. I do not mean to imply any censure on the horticulturists; they use names current at the moment which are good enough for practical purposes, though they will not stand a critical test. But in after years no technical botanist would dream of accepting them as unimpeachable.

Again, it has often been found that where remarkable hybrids have been recorded, it has been ascertained later that no cross has in point of fact been effected at all. Yet the original announcement will be quoted, and often has been as an undoubted evidence of the fact.

I arrive, then, at the conviction that if any profitable use is to be made of horticultural experience in the study of variation, the so-called historical evidence will have to be discarded. Every step of the investigation must be made under the actual eye of a competent observer, and nothing taken at second-hand.

I will now return to the Cineraria. The feral form had been long lost to cultivation, but some years ago it was reintroduced to Kew from the Canaries. Mr. Rolfe, a member of my scientific staff, illustrated it in the *Gardeners' Chronicle* in 1888, and pointed out the striking changes which it had exhibited under cultivation. These have subsequently interested me because I have been endeavouring to collect facts as to the rate of variation.

Now Mr. Bateson, solely on what he calls historical evidence, still asserts, and in the face of the difficulties which I have pointed out that such a theory presents, that the modern Cineraria is of hybrid origin. Very well; let us assume that as a provisional hypothesis. How is it to be tested? It is easy to see from an analogous case. The horse and the zebra have been crossed; are we justified in asserting that the last winner of the Derby is of zebra descent? The criteria are two, and I think two only: (1) an uncontested pedigree; (2) palpable marks of parental characters.

Now, with regard to (1), practically in plants it cannot be obtained. We can only fall back upon "historical evidence." I have attempted to show above, in a general way, how little scientific value can ordinarily be attributed to this. One cannot be sure that the asserted parents were what they are stated to be. But my object was not to undermine the weight of what Mr. Bateson has brought forward. I accept it and reject it as wholly irrelevant. As my friend Prof. Rolleston was fond of saying, it would be valueless evidence even to convict a poacher.

The fact that certain shrubby Cinerarias with hoary leaves and one with yellow flowers were crossed (if they really were) early in the century, proves nothing as to the existing Cineraria, any more than the cross between the zebra and the horse does as to the parentage of any existing horse.

These shrubby Cinerarias were, as Mr. Bateson states, propagated by cuttings (they are not too easy to strike); and like many other interesting plants, they disappeared from all but botanic gardens towards the middle of the present century.

As I am quite unable, then, to attach any weight to the so-called historical evidence, because I fail to see that it establishes any filiation between the plants with which it deals, strikingly different as they are, and the plant with which I am dealing, there is nothing left but to try (2), and see what evidence of its parentage the plant itself affords.

Now, it is well known that organisms of hybrid origin preserve, in some degree, their parental characters, and this has even been shown to be true of their histological elements. Modern taxonomic botany has met with considerable success in the analysis of plants of hybrid origin into their constituents. The Floras have in consequence been cleared of a multitude of dubious plants, the real nature of which can now be accounted for. And the validity of the method has been established by the results of a corresponding synthesis. We had, then, no hesitation at Kew in applying the test to the Cineraria. Although it had often been examined before, with the assistance of some members of my staff I made a fresh examination. I took copious specimens of *Cineraria cruenta*, and of an average cultivated form, and carefully compared them point by point. Except in the multiplication of the florets in the heads, especially of the ray-florets, we could distinguish no tangible morphological difference. In fact, having accidentally mixed up leaves belonging to the two parcels, I found myself unable with any certainty to refer them back again. This is pretty conclusive evidence of the actual morphological identity of the vegetative organs of the two plants.

The next thing was to compare the cultivated Cineraria with its reputed shrubby "historical" parents. These present well marked and somewhat peculiar characteristics not readily described in non-technical language. But the cultivated Cineraria does not present the smallest trace of any one of them. As far, then, as the matter admits of investigation at all by any known methods, I regard the conclusion which is generally accepted here as a sound one. At any rate, it rests on a careful consideration of the objective facts which Mr. Bateson wholly shirks.

I now come to the other point. I put colour change entirely aside for reasons which seem valid to me, and which I may take another opportunity of explaining. Apart from these the cultivated Cineraria exhibits no variation from the feral form which may not be described as dimensional. While the foliage has remained approximately constant, the loose corymbose habit has been contracted into a tight corymb, and the heads of florets have been enormously enlarged. While the feral form stands about five feet high, the cultivated one is about eighteen inches. I am disposed to restrict the term "sporting" to a definite morphological change such as is exhibited in the flowers of the garden Chrysanthemum, and recently in the occurrence of an "ivy-leaved" form of the Chinese Primrose. But except a race of so-called double Cinerarias, which did not take the public fancy, the history of the garden Cineraria does not present, as far as I know, any trace of a real morphological change. If I might venture to use a mathematical analogy, I should say that the form of the Cineraria-function has remained unaltered.

Now the object of these dimensional changes has been to make the plant worked upon handy and convenient for decorative purposes. Those points which were unessential for this purpose have been unconsciously neglected, and their stability has not been affected. But I do not doubt that if it had been otherwise the Cineraria might have been brought by this time to any configuration which the cultivators fancied.

As far as I can make out, the transformation of the Cineraria has taken about sixty years to effect. Mr. Bateson will not complain if I quote a few words from one of his own authorities of about that date :—"One species especially merits cultivation, viz. *C. cruenta*. This may be regarded as the parent of many of those beautiful varieties which are so successfully cultivated by Messrs. Henderson." Now my memory of the cultivated Cineraria goes back some thirty years. I can remember when it was a rather lanky plant, about half the height of the feral form, with a somewhat lax inflorescence and far smaller flower-heads than are now to be seen. The present fashionable Cinerarias, with a very condensed inflorescence and very large flower-heads, only date back some ten or twelve years.

I see, therefore, no reason for abandoning my assertion that the evolution of the modern Cineraria has been slow and gradual, and not *per saltum*, and this is in accord with general horticultural experience. As soon as a new plant is introduced, every one wants to get a form with bigger flowers or floral structures than anybody else. There is only one secure path to this result, and that is by taking advantage of seminal variation and selecting the minutest trace of change in the desired direction. By patiently and continuously repeating the operation, almost any desired result can be obtained. The horticultural gambler may hope to reach it by a "sport," but he will not. *Anthurium scherzerianum* is a good illustration. Introduced in 1862, it was little more than a curiosity; now its enormous and brilliant spathes are a conspicuous object at every flower-show. This has simply been accomplished by progressive selection working on seminal variation.

Mr. Bateson has now the coolness to say that "the hybrid origin of cultivated Cinerarias is of subordinate interest." All I can say is that in that case it is a pity that he wasted three columns of NATURE with a discussion of the subject. I should have thought myself that it was a matter of very considerable importance indeed to be able to form an approximate idea of the amount of change in a given time in an unmixed species, and so obtain some measure of the possible rate of evolution, at least in regard to dimensional characters.

For my part, I think that in the study of evolution we have had enough and to spare of facile theorising. I infinitely prefer the sober method of Prof. Weldon, even if it should run counter to my own prepossessions, to the barren dialectic of Mr. Bateson. W. T. THISELTON-DYER.

Royal Gardens, Kew, May 13.

Some Bibliographical Discoveries in Terrestrial Magnetism.

I HAVE recently made some interesting discoveries pertaining to the history of Halley's famous chart of the Lines of Equal Magnetic Variation (Declination), to which renewed attention is just now being called by Prof. Hellmann's admirable facsimile reproduction of the earliest geomagnetic charts.[1]

The first reproduction in facsimile of Halley's chart was undertaken by G. B. Airy, and published in "Greenwich Observations" for 1869. Airy was led to do this by reason of the fact that he could find no geomagnetician of his time who had ever seen Halley's chart. After diligent inquiry among academies and libraries at home and abroad, it was found that the British Museum possessed a copy, and, it was believed, the *only* copy extant. Since then, Prof. Hellmann has succeeded in tracing two other copies, one at Hamburg (Stadt Bibliothek) and one at Paris (Bibliothéque Nationale), and has also, since the publication of his book (as he has just informed me), come into possession of a copy himself.

I have personally examined the Hamburg and Paris copies, and, during a brief stay in London in March, also the copy in the British Museum used by Airy. I have found, moreover, in the British Museum, three other Halley charts and two Dutch reprints. By a careful and critical study of these various copies, some new light is thrown upon the publication of Halley's chart. To make this apparent, some wearisome details with regard to the various copies will be necessary. I will begin with the British Museum copies.

Catalogue No. 974 (5).—"A new and correct Sea-chart of the Whole World, showing the Variations of the Compass as they were found in the Year 1700, by Edmund Halley." Date (according to the Catalogue), 1701.

The above is the English title of the chart referred to at times by the Latin title, "Tabula Nautica," &c. This copy appears to be the one used by Airy in his facsimile reproduction of the Halley chart published in "Greenwich Observations" for 1869, which in turn has been used for Prof. Hellmann's reproduction. There is no date on the chart, nor the name of the publishing firm. The date 1701, assigned hitherto, is probably due to Halley's defence of his chart, contained in *Phil. Trans.* vol. xxix. (Unabridged), 1714. Halley says, p. 165, "to examine the chart I published in the year 1701, for shewing at one View the Variations of the Magnetical Compass, in all those Seas with which the English Navigators are acquainted." But we find that the above number is dedicated "To his Royal Highness, Prince George of Denmark, Lord High Admiral of England, Generalissimo of all Her Majestie's Forces." As Prince George, consort of Queen Anne, did not bear this title until April 17, 1702,[2] it is evident that the above number is either not the original Halley chart published in 1701, or it is a reprint with a later dedication. If it is to be regarded as an original Halley chart (not a reprint), then a date between 1702 and 1708 must be given it, as Prince George died October 28, 1708. It was published probably not far from 1702, and is in excellent condition.

No. 973 (15). Same title as previous number. Date given in the Catalogue, 1720 (?) I found upon examination that this is identical with No. 974 (5). The Catalogue date is doubtless erroneous. This copy is cut into sections and remounted.

No. S. 112 (6). This is a large folio atlas containing a reprint of No. 974 (5), bearing now the name of the publishing firm, R. Mount and T. Page, and having in addition an extra strip, from 90° to 160° E. of London, pasted on the left-hand side, so that the chart now embraces 430° of longitude instead of 360° as before. The Hamburg and Paris copies are exact duplicates of this, the only difference being that they have pasted below a strip bearing the explanation of the chart by Halley. Prof. Hellmann, in the work cited, has given us the Hamburg text. The Paris text differs in the orthography of a few words, and in the spacing of some of the lines. It appears to be the older text, as below it we find the name of the firm as R. and W. Mount and T. Page, while the name of the firm on the Hamburg text is Thomas Page and William Mount, and the former I have ascertained to have been the earlier firm. This English text I have failed to find attached to the British Museum copies.[3]

[1] Neudrucke von Schriften und Karten Über Meteorologie und Erdmagnetismus. Herausgegeben von Prof. Dr. G. Hellmann, No. 4. . . . E. Halley, W. Whiston, J. C. Wilcke. A. von Humboldt, C. Hansteen: Die ältesten Karten der Isogonen, Isoklinen, Isodynamen; 1701, 1721, 1768, 1804, 1825, 1826. 4to. 26 pp. 7 plates. (Berlin: A. Ascher and Co., 1895.)

[2] Rapin de Thoyras's History of England, London, 1751, vol. iii. 1689-1707, p. 544.

[3] The atlas contains, besides "An Account of the Methods used to describe Lines on Dr. Halley's Chart of the Terraqueous Globe," &c., by W. Mountaine and J. Dodson, London, 1758, and copies of the Halley chart revised for epochs 1744 and 1756. It bears the title on the back: "Tabulæ Nauticul Variationes Magneticas Denotentes. E. Halley." It appears to be a compilation of charts, probably by the authors (Mountaine and Dodson) of the revision.

Nos. 974 (6) and 974 (1) are Dutch editions by R. and I. Ottens, of Amsterdam, of the Halley chart as modified and found under No. S. 112 (6). The base of the chart has been changed, but not the lines of equal variation. The dedication to Prince George has been omitted. The dates assigned by the Catalogue are respectively 1735 (?) and 1740. The chief interest in these Dutch reprints lies in the fact that they have a French text pasted on the left-hand side, and a Dutch text on the right-hand side, over Halley's name.

No. 974 (4). "A new and correct Chart showing the Variations of the Compass in the Western and Southern Oceans, as observed in y^e Year 1700 by his Ma^ties Command by Edm. Halley." Date given by the Catalogue, 1720, marked doubtful. This chart extends from 59° N. to 59° S., and from 21½° E. to 100° W. of London. It is enclosed by a border; the base of the chart is entirely different from that of 974 (5); yet the equal variation lines, as far as given, are identical with those for the same region on 974 (5). In but one respect is there a difference in the lines, viz. in no case are they drawn over the land, and in a few cases, also, they are slightly extended. It contains in addition the course of the *Paramour Pink*, the ship in which Halley made his observations, 1697–1700, with the chief aid of which he drew the equal variation lines for the Atlantic Ocean. But the matter of chief importance is that this chart is dedicated to *King William III.* This fixes its date. William III. died March 8, 1702. It is highly probable, then, that this is the chart published in 1701, referred to by Halley in the above quotation, and, in consequence, *the original Halley chart.* It is, moreover, reasonable to suppose that Halley would dedicate his first chart to King William III., who had furnished the means for the making of the observations, to which the chart was due. This chart has escaped the attention of all geomagneticians and bibliographers, and the British Museum copy may be the only one in existence.[1]

Another matter of historical interest, apparently unknown to all modern authors in terrestrial magnetism, was ascertained. I find it asserted that the Frenchman, L. I. Duperrey, was the first (1836) to construct the "Magnetic Meridians" for the whole earth, *i.e.* those lines on the earth's surface marking out the path described by following the direction pointed out by a compass needle. It seems, however, that this honour should be accorded to an Englishman, Thomas Yeates, who, in 1817, published a chart of the Lines of Equal Magnetic Variation, accompanied by a "New and Accurate Delineation of the Magnetic Meridians." A second edition of this chart was published in 1824. Copies of both editions were found in the British Museum.

Washington, April 20. L. A. BAUER.

[1] Upon furnishing Prof. Hellmann with a brief description of this chart, he has found that Le Monnier, in his "Loix du Magnétisme," Paris, 1776 and 1778, has reproduced it. Prof. Hellmann's copy of the Halley chart is a duplicate of above, No. S 112 (6), with the exception that it embraces but 360° of longitude. It also has no text.

The Unit of Heat.

DR. JOLY'S strictures on the units of heat at present in use will meet with a ready endorsement from those who have worked on calorimetry. The large calorie is too large for convenience in most cases, and the small calorie is too small, while the confusion created by different writers using different units with the same name is scarcely reduced by their writing one with a capital and the other with a small *c*. A unit of convenient magnitude would be one equivalent to about 100 small calories, and 100 calories has, indeed, been adopted as a unit by more than one writer on thermochemistry. There is, however, what may be termed a natural quantity which is nearly equivalent to such a unit, namely, the heat of fusion on one gram of water at 0° C., which is nearly eighty calories. This appears to be just as suitable from other points of view as the heat of vaporisation of one gram of water at constant temperature and 760 m.m. pressure; and if this latter can be recommended on the ground that in defining it we replace the thermometer by the barometer, the former will possess the superior claim of (for all practical purposes) not depending even on the barometer.

If I remember rightly, this unit has already been adopted in one work on thermochemistry.

No doubt the heat of fusion of water requires redetermination; but it should be determinable with quite as much accuracy as the heat of vaporisation.

Neither of these proposed units, however, possess what should be the chief characteristic of a physical unit, namely, a simple relation to other units; and before adopting either of them, it would be well to consider whether some convenient unit related to, say, the electrical units, could not be adopted. A Committee of the British Association would be a body most suited to investigate this matter.

For practical purposes, a quantity which is even of greater importance than the magnitude of the unit adopted, is the relative value of the heat capacity of water at different temperatures. In spite of the large amount of work which has been expended on this subject, great uncertainty still prevails respecting it. The heat capacity of water, and the heat of fusion of ice, are subjects which I have been for some years intending to turn my attention to, and the work is now practically in hand.

Harpenden, May 4. SPENCER PICKERING.

MY objection to the latent heat of water unit is that this is an inaccessible unit on account of the difficulties attending measurements with the Bunsen calorimeter.

Some years ago I began experiments on a gravimetric ice calorimeter. I have not had leisure to go on with them, but the results obtained were very encouraging. The substance was cooled below 0° while hanging suspended from one arm of a chemical balance. This was effected in a double-walled chamber of copper. A tube, stopped by a plug, connected this chamber with a reservoir of water and clear broken ice. The water was previously boiled to expel air. On raising the plug the water at 0° flows rapidly into the calorimeter, and a shell of clear ice forms upon the substance. The effect on the balance is noted, and by observing the change of buoyancy upon the melting of the ice, and knowing the density of ice at 0°, the mass of the latter can be estimated. The weight measurement will extend to about 0·5 of a calorie. In the steam calorimeter the weight measurement extends to 0·1 calorie, or even less.

There is, of course, much to be said for a thermo-dynamic unit. The question is certainly deserving of having the opinions and views of scientific men fully expressed upon it—as Mr. Pickering suggests. A glance at any of the recent accurate thermal work done in England will show what confusion there exists as to what is the calorie, and as to how all the pet calories of various physicists are related. To render many old measurements of value, this last question should be decided. It reminds one of the state of thermometry in De Saussure's time.

Trinity College, Dublin. J. JOLY.

Reputed Traces of Negrito Pygmies in India.

MAY I be permitted to suggest to readers of M. Quatrefages' work on the Pygmies, the English edition of which has recently been reviewed in NATURE, to pause before accepting his conclusions as to traces of Negritos being found in peninsular India.

The evidence he relies on partly consists of a description by M. Rousselet of a half-starved wanderer from Sirjuga, whom he assigns to the race Bander Lokh (or, as it is printed in the English edition, Bandra Lokh) and the tribe Djangal. Any Anglo-Indian with the slightest knowledge of the language, not to say of ethnology, would be amused at such nicknames being applied as definite racial terms. The first simply means monkey-people (equivalent to savage), as applied by dwellers in the plains to the wilder inhabitants generally; and the second, if it can be said to mean anything in the form presented, is simply "jangli," or a dweller in jungle.

The portrait of this "Djangal," from a rapid pen and ink sketch, is a caricature of a somewhat exceptional and by no means typical individual, and affords no trustworthy material for an ethnological discussion.

The "fever-stricken inaccessible" region Sirjuga,[1] from whence this specimen was a fugitive, according to M. Rousselet, is well known to me, and when travelling there I spent some days in the company of the late General Dalton; and not only then, but in connection with the production of his great work on the Ethnology of Bengal, to which I had the privilege of contributing, I had many conversations with him regarding the tribes of that region. I was, moreover, well acquainted with the true Negritos of the Andamans, of whom I had then already seen many; and I do not hesitate to say that I never met with the slightest trace of a Negrito element among the numerous tribes I became acquainted with during many years travelling in the hilly tracts of Western Bengal, the Central Provinces and the Northern Provinces of Madras. Individuals belonging to different tribes

[1] The district of Sirjuga in Chota Nagpur is not near Amerkantak, nor is it included in the Vindhyan Range as is stated by M. Quatrefages.

with curly, not really woolly, hair are occasionally to be seen; but I venture to think that such occasional freaks are casual, and wholly without significance; although they were regarded as evidence of a Negroid element in the population by the late Sir George Campbell.

As, in consequence of the statements and theories of M. Quatrefages, the idea is already spreading that traces of pygmy Negrito races are to be found in these parts of India, I contemplate on a suitable occasion, ere long, publishing some notes, made at the time, on the tribes I met with in my travels in the wild regions referred to. I shall therefore say no more at present, save that the evidence culled by M. Quatrefages out of General Dalton's lithographed groups—one of a girl with her hair *cropped* short, and another of two somewhat curly-headed Sonthals—in support of his theory, is not merely feeble, but is liable to mislead.

Sir Wm. Flower has referred to the use by M. Quatrefages of the term Mincopie for the Andamanese. As he points out, there is in reality no such term. How it originated, though long unknown, has been suggested by Mr. Man. Its derivation foiled even the acute research of Sir Henry Yule. Its first use was by Lieut. Colebrooke in the year 1795, but it has not been recognised in any Indian dialect, and does not seem to have ever been in use among Anglo-Indians, any more than is the name Zebu, which is used in some European languages for the humped cattle of India. Such names, and there are a few others, not being current in the country itself, have to be forgotten by those who visit India. I well remember being not understood when I used the term Zebu on my first arrival in Calcutta some thirty years ago. V. BALL.

Dublin, May 13.

Epping Forest: an Explanation.

SOME years ago you were good enough to publish a paper of mine on the conservation of the Forest from the naturalists' point of view (vol. xxvii. p. 447). That paper was written soon after the Forest was taken over by the Corporation of London, when some unpleasant signs of artificial treatment had become manifest, and more especially with reference to certain railway schemes which, in the interest of naturalists, we of the Essex Field Club felt it our duty to oppose. It is a matter of ancient history that our opposition was successful. My object in entering the lists again is to assure your readers, as representing the scientific public, that the controversy which is now going on concerning the management of the Forest has nothing whatever to do with the agitation about the railway scheme of 1883. This statement may appear superfluous, but I am compelled to trespass upon your space because certain unscrupulous critics are in the habit of misleading the public by quoting from that paper published twelve years ago, without giving date or context, and without a single word of explanation as to its object. Moreover, the critics in question have endeavoured, by a method which in other controversial spheres would be called by a very strong name, to make it appear that some of the views put forward in 1883 are opposed to the attitude which, it is well known, I now hold in the present controversies. So far as naturalists are concerned, they may rest assured that nothing that is now being done is in the way of injury to the Forest; far from this, there are signs of marked improvement The policy of the Conservators is to restore the Forest to a natural condition by thinning out overcrowded pollards which are now beginning to injure one another, and to kill off the varied undergrowth which is such a relief to the gloomy barrenness of an unnaturally dense growth of trees. I may point out that the overcrowding is due to two opposite causes, viz. to entire neglect in some parts, and to too much attention in others. The latter cases refer to those parts in which in past times the rights of lopping were severely exercised. Here of course, now that the Conservators have extinguished these rights, the pollards are throwing up straight and lanky branches of a most unsightly character. In those very limited parts which were not formerly pollarded, and which consist of groves of spear trees, no attempt at systematic thinning had been made before the present Conservancy, and here also there is an overcrowding necessitating woodcraft. Within the last few years all that has been done has been done with care, skill, and forethought. I rejoice to be able to bear testimony on this point, and to reassure those who may have been misled from a want of personal knowledge of the nature and history of the district, into giving credence to the intemperate correspondence in the newspapers. R. MELDOLA.

May 21.

PROFESSOR LOTHAR MEYER.

Gestern Abend 11 Uhr entschlief plötzlich sanft und schmerzlos im 65. Lebensjahre mein lieber Mann

DR. LOTHAR MEYER

ord. Professor der Chemie an der Universität Tübingen.

JOHANNA MEYER geb. Volkmann
mit ihren Kindern.

Tübingen, den 12. April 1895.

WE were thankful his "falling on sleep" was "sudden, gentle, and without pain"; but we grieved he should have left us so soon.

Julius Lothar Meyer was born at Varel in Oldenburg, on August 19, 1830. After completing his school course in the Gymnasium, he studied in the University of Zürich from 1851 to 1853, then at Würzburg from 1853 to 1854; from Würzburg he went to Heidelberg, where he remained till the autumn of 1856, and from thence he migrated to Königsberg, where he remained until Easter 1858. Meyer's original intention was to devote himself to medicine, and he graduated as Doctor in Medicine at Würzburg on February 24, 1854. At Heidelberg he came under the influence of Bunsen, and his work became more and more chemical. At Königsberg his studies were devoted mainly to mathematical physics, under the guidance of F. Neumann. In 1858 he took the degree of Ph.D. at Breslau; and on February 21, 1859, he received leave to teach chemistry and physics. From 1859 to 1866 Meyer was in charge of the chemical laboratory of the Physiological Institute at Breslau. In 1866 he was called to the Royal Prussian *Forstakademie* at Eberswalde, where he remained until 1868, when he went to the *Polyteclmikum* at Carlsruhe. In 1876 Prof. Fittig was called from Tübingen to the University of Strassburg, and Lothar Meyer was appointed to fill the vacancy at Tübingen.

He had nearly completed twenty years' work at Tübingen when the summons came. Cerebral apoplexy stopped his labours, on April 11 of this year; and, *plötzlich, sanft, und schmerzlos*, he passed.

It was while teaching chemistry and physics at Breslau that Meyer published the first edition of the work on which his reputation as a philosophical chemist chiefly rests. "Die Modernen Theorien der Chemie" appeared in 1864. A second edition was published in 1872; and since that time have appeared a third, fourth, and fifth edition. At the time of his death Meyer was engaged in the preparation of a sixth edition, which he intended to publish in three, more or less independent, parts. An English translation of the fifth edition, by Messrs. Bedson and Williams, appeared in 1888. In 1883 Profs. Meyer and Seubert recalculated the atomic weights of the elements from the original data, and laid all chemists under a debt of gratitude by publishing their results, under the title "Die Atomgewichte der Elemente aus den Originalzahlen neu berechnet."

Lothar Meyer was one of the earliest investigators of the relations between the properties and the atomic weights of the elements. In the first edition of his "Modernen Theorien" (published in 1864) he traced relations between the atomic weights and the chemical values of the elements; and in December 1869 appeared a memoir by him entitled "Die Natur der chemischen Elemente als Funktion ihrer Atomgewichte," wherein he arranged the elements in order of atomic weights, in a single table, and indicated the periodic character of the dependence of properties on atomic weights.

The clear enunciation, and the application in detail, of the most far-reaching generalisation that has been made in chemistry since the work of Dalton, must, undoubtedly, be credited to that great chemist Mendeléeff but, nevertheless, a perusal of the controversy between Mendeléeff and Meyer shows, I think, that Meyer arrived at the fundamental conception of the periodic law independently of Mendeléeff. Those who are interested in such controversies will find papers by Mendeléeff and Meyer in *Berichte* xiii. pp. 259, 1796, 2043 [1880].

In his discourse to the German Chemical Society on May 29, 1893, "Ueber den Vortrag der unorganischen Chemie nach dem natürlichen Systeme der Elemente," Meyer quotes the words which Laurent had used fifty years before concerning organic chemistry, and applies them to the teaching of inorganic chemistry at the presen time:—*que l'arbitraire y règne sans partage*. If these words can be applied to the teaching of inorganic and general chemistry to-day, how much more fully and literally were they applicable at the time when the first edition of Meyer's "Die Modernen Theorien" appeared thirty years ago! That book has probably done more than any other publication within the twenty years after 1864 to advance the study of comparative chemistry; its influence on the conception of chemistry as an accurate and orderly body of facts and principles has been very great, and has been wholly good. The labour bestowed on the preparation of the first edition of the "Modern Theories" must have been immense. The author speaks in his preface of rewriting the MS. three times. It is true that thirty years ago physical chemistry was practically non-existent, that the facts of organic chemistry could be mastered and held by a man with an ordinary memory, and that one might be a chemist without first being a mathematical physicist. But it is also true that the facts of inorganic chemistry had not been coordinated by the luminous conception of the periodic law, that there was a lack of clearness in the notions of most chemists about the structure of organic compounds—for Kekulé had not yet made his famous ride on the top of the Clapham omnibus—and that the many isolated facts regarding the influence of temperature, time, and the masses of the reacting bodies, on chemical changes had not been gathered together and illuminated by the law of mass action and the conceptions arising from the applications of this law. It was then that "Die Modernen Theorien" appeared; and at once a flood of light was thrown on the whole domain of chemical science. Old problems were made clear, and new problems were suggested. Chemistry entered on its modern phase.

As the study of comparative chemistry progressed—a study which was introduced by the enunciation of the periodic law—it became necessary to know with accuracy the analytical bases whereon rested the values accepted for the atomic weights of the elements. Hence Lothar Meyer was induced to devote a large amount of labour to the somewhat thankless task of recalculating these values; the result of this work, carried out with the help of his colleague Prof. Seubert, appeared in 1883. This work received additional value from the fact that it appeared almost at the same time as Clarke's "Recalculation of the Atomic Weights." Every worker in this department has the data of all previous workers brought to his hand, and presented in the most manageable form.

Besides these two treatises bearing on general chemistry, Lothar Meyer was an investigator in the sphere of experimental chemistry. He has published memoirs on subjects in almost every branch of the science; on the atomic weight of beryllium, on determinations of vapour densities, on the combustion of carbon monoxide, on the preparation of hydriodic acid, on the transpiration of gases, on various organic compounds, and on other matters.

A great chemist has passed away from us; his work remains, and that work will ever be held in remembrance.

M. M. PATTISON MUIR.

NOTES.

THE Institute of France has decided to solicit subscriptio for the erection of a statue to Lavoisier at Paris. It is intende to make the appeal an international one, so that all admirers of Lavoisier may do honour to the memory of one of the creators of modern science. Subscriptions may be sent to the Treasurer of the Committee for the Lavoisier Memorial, 55 quai des Grands-Augustins, Paris.

THE centenary of the Institute of France is to be celebrated next October. The *Times* states that on the 24th of that month the foreign representatives invited to the celebration will be received, and the Minister of Education will hold a reception. On the following day M. Faure will attend a ceremony at the Sorbonne, and a banquet will be held. There will also be a dramatic entertainment and a reception at the Élysée. Chantilly, the future property of the Institute, will be visited on the 27th, by permission of the Duc d'Aumale.

LIVERPOOL, determined that the visit of the British Association in 1896 shall be a success, has taken time by the forelock. At an influential meeting held in the Town Hall last week, it was announced that an executive working committee had been appointed thoroughly representative of the inhabitants of Liverpool and the neighbonrhood. The Chairman is the Right Hon. the Lord Mayor of Liverpool, the Vice-Chairmen are Sir W. B. Forwood and Mr. E. K. Muspratt; the Hon. Treasurer, Reginald Bushell, and the Hon. Secretaries, Prof. W. A. Herdman, F.R.S., Mr. J. C. Thompson, and Mr. W. E. Willink. The meeting was very enthusiastic, and the key-note running through the various speeches was to the effect that the welcome extended to the members of the British Association should not in any direction be allowed to compare unfavourably with that at the meeting at Manchester in 1887, which in the matter of subscriptions at present holds the record. From the short statement made by the Hon. Treasurer, this hope seems likely to be realised. Without making any public appeal for funds, but simply putting the matter before a few of his more influential friends, the Hon. Treasurer was able to make the gratifying statement that no less than £1350 had been subscribed. The Committee preferred a subscription list to a guarantee fund, and in this they are no doubt well advised. A donor, however, is not entitled to any privileges as a member of the British Association, by reason of his subscription, but to every subscriber of £10 a member's ticket or two associate tickets will be given, and one associate ticket to subscribers of £5. With this early start, Liverpool ought to have no difficulty in raising the £5000 which Sir W. Forwood regards as the minimum sum required for a successful and record meeting.

THIS year's conversazione of the Society of Arts will be held in the South Kensington Museum on Wednesday evening, June 19.

DR. THORNE THORNE, C.B., F.R.S., has been appointed a member of the General Medical Council for five years, in place of Sir John Simon, resigned.

MR. GEORGE MURRAY has been appointed Keeper of Botany in the British Museum, in succession to Mr. Carruthers, who retires on superannuation.

THE death is announced of Dr. H. F. C. Cleghorn, well known for his work in connection with the organisation and development of the Forest Department of India. He was for some years president of the Royal Scottish Arboricultural Society, and examiner in forestry to the Highland Society. He also took a leading part in the founding of the forestry lectureship in the University of Edinburgh.

THE munificent gift made by Mr. Seth Low, ex-Mayor of Brooklyn, and now President of Columbia College, to that college, at a meeting of the trustees a few days ago, places him in the front rank of the world's benefactors. One million dollars for a library building, twelve scholarships for Columbia College for Brooklyn boys, and twelve to Barnard College for Brooklyn girls, eight university scholarships and a university fellowship, make a list of gifts rarely equalled. At the same meeting, Mr. C. Schermerhorn presented 300,000 dollars for a new building. Then the Townsend library, a complete compilation of all the printed matter relating to the American Civil War, in eighty-nine volumes of 600 pages each, larger than an ordinary ledger, which was begun six months before the war, and is the result of thirty-three years of unceasing labour by Thomas S. Townsend, was formally presented to the college, together with an encyclopædia of reference to it, and 4000 dollars to complete the encyclopædia.

THE trustees at the same meeting, following the recommendation made by the National Academy of Sciences at their recent meeting, awarded the Barnard medal to Lord Rayleigh for the discovery of argon. This gold medal, which has a value of 200 dollars, is awarded every five years to the investigator who makes within the preceding five years the most valuable discovery in physics or astronomy, in accordance with the will of President F. A. P. Barnard, who died in 1889, and was the immediate predecessor of Mr. Seth Low.

THE Brooklyn Institute has just sustained a great loss in the retirement of General John B. Woodward, who has been president for eighteen years, covering the entire epoch of the great development and expansion of the Institute. He will be succeeded by Mr. A. Augustus Healy.

SIR WILLIAM DAWSON has sent us a printed statement, in which he traverses the arguments against the organic nature of *Eozoon Canadense*, brought forward by Dr. J. W. Gregory and Prof. Johnston-Lavis, in a recent paper entitled "Eozoonal Structure of the Ejected Blocks of Monte Somma," noted in our issue of January 10 (p. 251). He states a number of facts which indicate "that the specimens of Eozoon found in the Laurentian limestone of Canada in no respect resemble in their associations and mode of occurrence the banded forms from Mount Somma described in the paper in question."

A STRONG earthquake disturbance of about five seconds' duration occurred at Florence at nine o'clock on the evening of Saturday last, and was felt at Bologna four minutes earlier. Two hours later another shock was felt. Many of the houses in Florence were injured by the movements, but the damage appears to have been greater in the surrounding villages—Grassina, Lapaggi, and San Martino, where the church was destroyed. At Orezzo the earthquake is said to have lasted ten seconds, and there were two distinct shocks at Siena. The movement was strongly marked at Parma, and to a less degree at Pisa and Placentia. Reuter's correspondent at Spoleto reports that severe shocks were also felt there on Monday evening.

A GENERAL meeting of the Federated Institution of Mining Engineers will be held in London on Thursday, May 30, and on Friday, May 31. The presidential address will be given by Mr. W. N. Atkinson on the Thursday. The papers to be read on the same day are:—Notes on bauxite in County Antrim, &c., and its uses, by Mr. George G. Blackwell; sampling, by Mr. T. Clarkson; blasting explosives, by Prof. Vivian B. Lewes; and the gold-milling process at Pestarena, by Mr. A. G. Charleton. At the meeting on May 31 the following papers will be read, or taken as read:—Remarks on the banket formation of Johannesburg, Transvaal, by Mr. A. R. Sawyer; the composition of the extinctive atmospheres produced by various flames and by respiration, by Prof. Frank Clowes; the composition of the limiting explosive mixtures of various gases with air, by Prof. Frank Clowes; the mineral oils of Lower Elsass, by Dr. L. van Werveke; copper-mining in India, by Mr. Robert Oates; the recent magnetic survey of the United Kingdom, by Prof. A. W. Rücker; the MacArthur-Forrest process, by Mr. John McConnell.

IN consequence of the renewed attacks upon the Conservators of Epping Forest, another large and influential meeting of the Essex Field Club was held on Saturday last, under the conductorship of Mr. Edward North Buxton, Prof. Boulger, Prof. Meldola, and the hon. secretaries. More than 100 members and visitors were present, among them being many residents in the district and lovers of the Forest, as well as such well-known experts as Prof. W. R. Fisher, of Cooper's Hill, and Mr. Angus D. Webster. The districts visited were those about which complaints had been made by a certain class of newspaper correspondents, viz. Bury Wood, the so-called Clay Ride, and Monk Wood. Beyond a few personal discussions between the conductors and experts and one or two of those who had been criticising the action of the Conservators, no public ventilation of views was permitted, as the conductors were of opinion that a mere inspection of the places named would enable the members and their friends to form their own conclusions. The party assembled at the King's Oak at High Beach for tea, after which an ordinary meeting of the Club was held, the President, Mr. David Howard, taking the chair. Mr. E. N. Buxton explained a scheme which he had been carrying out for the purpose of affording protection to the birds of the Forest district. By enlisting the sympathies and securing the co-operation of the surrounding landowners, he had succeeded in obtaining a promise that a total area of some 20,000 acres, including the 6000 acres of Forest, should constitute a sanctuary within which no rare or interesting birds should be destroyed. The President indicated that such an organisation as the Essex Field Club was well calculated to enforce by example and precept the desirability of protecting both animals and plants. Mr. F. C. Gould, in reply to those correspondents who had stated that the birds were becoming rarer in Epping Forest, said that this was quite contrary to the facts. Birds were never so plentiful in the Forest as they had been during the past few years, and Mr. Gould gave a list of species which had been observed by his son in the course of one day. After tea the party proceeded to the more northern part of the Forest, and inspected Epping Thicks. Although no formal division on the question of the management of the Forest was taken, the majority could not help expressing their admiration at the skill and judgment with which this year's thinnings had been effected. Many of those present also expressed some anxiety that the Conservators might be influenced by the newspaper correspondence, and allow the Forest to degenerate by acceding to the request recently made by a deputation to the Committee that no further thinning should be allowed for a period of five years.

A SPELL of very cold weather for the time of year was experienced last week over the entire area of the British Isles, owing to a depression which, at the time of our last issue, lay over Denmark, and caused strong gales from north and north-west over the North Sea. The temperature fell about 30° over the inland parts of England, while snow and hail were reported from many places. On several nights the sheltered thermometer fell to within a few degrees of the freezing-point, and actually reached it in the east and west of Scotland, on the morning of the 17th instant; while the highest day readings have in many parts failed to reach 50°, a temperature which is fully 10° below the average. During the first part of the present week a depression which had spread westwards from Germany, caused a continuation of cold, gloomy weather over our islands.

SOME years ago the desirability of publishing the observations made by the late J. Allan Broun at Trevandrum, in Southern India, for over twelve years, was brought before the Royal Society of London by the Royal Society of Edinburgh, and the records were deposited at the Meteorological Office for safe keeping. The Meteorological Council subsequently drew the attention of the Royal Society to the subject, and that body induced the Indian authorities to render this valuable material accessible to scientific men, the result being that the Meteorological Department of India has just published the barometrical and thermometrical observations in vol. vii. of their *Memoirs*. The publication contains the hourly observations and means from January 1853 to December 1864, with the exception of Sundays, on which no observations were taken. The whole of the original entries have been carefully examined for clerical errors, under the superintendence of Mr. J. Eliot, the Government Meteorological Reporter, and we gather from the preface that a discussion of the results will eventually be carried out.

A MOST eloquent appeal for the wider diffusion of a knowledge of sanitary matters has been recently made by Dr. Carlo Ruata, Professor at the University of Perugia, in his introductory address to a course of lectures on the duties of sanitation. Efficient sanitation, urges Dr. Ruata, may justly be demanded as a right by the individual from the State; but, at the same time, each individual must be adequately impressed with his duties and responsibilities to other members of society in the proper conduct of sanitary matters. It is pointed out how much may be, and has been, done by judicious legislation and enlightened public opinion in recent years; but Dr. Ruata would insist upon more vigorous measures, and upon a knowledge of the principles of hygiene being rendered compulsory in systems of education. Ignorance and lack of all sense of responsibility is only too frequently to blame for the generation and spread of disease, and Dr. Ruata's appeal, that proper hygienic conduct should be insisted upon as the serious duty which one member of society owes to another is fully justified. Dr. Ruata is confident that with improved hygienic conditions society will benefit not only physically but morally; but whether it will bring about the utopian state sketched by the lecturer in his sanguine peroration, remains yet to be seen.

A REPORT, by Mr. P. G. Craigie, on the agricultural experiment stations and agricultural colleges in the United States, just published as a Parliamentary Paper, should be seen by every one interested in agricultural education and research. It appears that, at the present day, upwards of three-score collegiate institutions are engaged in the United States wholly or partly in agricultural teaching, and, according to the statistics collected and published for 1892, they enjoy an aggregate revenue of £689,000, practically one half of which was granted by the Federal Government, while £223,000 is added by the several States, minor aid being rendered by £40,000 which came from fees, and by the benevolence of local committees or private individuals, while the remainder was raised by the sale of farm produce or miscellaneous receipts. The number of separate experiment stations is fifty-four, of which forty-eight receive subventions from the Federal Government out of national funds, the uniform grant being roughly £3000 to each station. According to the returns published of the revenue of these stations in 1892, upwards of a million dollars, or roughly £200,000, is available as annual revenue, the Federal Government finding £140,000, and the grants of the States reaching rather more than £30,000.

MR. CRAIGIE'S report bears out his conclusion that "great and practical energy is being directed to the discovery of the best means of extending the field of agricultural and horticultural knowledge. It should not be overlooked that side by side with the growth of local stations a very extensive development of the scientific staff engaged on the special inquiries of the Federal Department at Washington has taken place in the last ten years. The American Government seems willing to face any cost to the community that promises the better to equip the farmer with a knowledge of his business. The authorities seem assured that in indicating methods of profitable production, and still more by the careful perfecting of the produce of the vast lands of the Republic, in whatever directions of extensive or of intensive culture the economic circumstances of the moment may prescribe, they are providing a solid means of advancing the well-being of the nation as a whole."

A FRESH addition to periodical literature is the *Journal of the South-Eastern Agricultural College*, Wye, Kent, which is to be published three times a year, and is intended to be a brief record of the history of the college from term to term, and to announce the results of investigations and experiments conducted by the college or members of its staff, together with other observations that may seem of interest to the agriculture of the counties of Kent and Surrey. The first number contains a description, with a plan, of the farm attached to the college, together with an account of the dairy school, of the water supply of the college, and of the field experiments which are being instituted. Mr. F. V. Theobald's notes on poultry parasites would appear to open out an instructive field of inquiry. Mr. J. Percival gives an abstract of a paper, already published, relating to eelworms in hop plants, their ravages resulting in the condition of the plants known as "nettle-headed." The nematode *Heterodera Schachtii* attacks so many kinds of plants, that its presence in hops was quite to be looked for. No reference seems to be made to the value which hop-growers set upon rape as a "trap-plant" for enticing the eelworms away from the infested crop. If future numbers are as attractive as this one, the publication is likely to prove acceptable to those in whose interest it is issued.

IN a recent number of the *Bulletin Geol. Soc. America*, Messrs. G. K. Gilbert and F. P. Gulliver give an interesting account of the remarkable "tepee buttes" that occur abundantly in the neighbourhood of Pueblo, Colorado. Using the term "butte" to denote steep-sided hills with narrow summits, which may be of very various origin, the authors mention the various types of buttes (volcanic necks, geyser deposits, &c.), and discuss this particular form. They are low hills, less than twenty feet in height, that owe their origin to the resistance to denudation of peculiar vertical masses of limestone occurring in the shales of the Pierre series (Upper Cretaceous). The limestone is composed of shells, chiefly of *Lucina* and *Inoceramus*, united by a matrix of shell-fragments, foraminifera and clay. This structure of the limestone, in comparison with that of the calcareous concretions that occur normally throughout the shale, negatives its concretionary origin, nor does it resemble the spring-deposited masses of limestone known elsewhere. It is concluded that particular local conditions determined the establishment of colonies of Mollusca that continued for generations at these spots, though what these conditions may have been it is not easy to explain. Attention is called to the description, by Dr. Bell, of similiar limestone masses in Devonian shales in Canada.

THE motion of a pianoforte wire when struck has been investigated by Herr W. Kaufmann, whose paper on the subject in *Wiedemann's Annalen* is accompanied by a set of very interesting photographic records, obtained by a modification of the method invented by Raps and Krigar-Menzel. By vibrating the wire in front of a luminous slit, and throwing the image of it upon sensitive paper rotating upon a cylinder, a white line is traced upon a black ground. This line, which is due to the interruption of the luminous slit by the opaque wire, exhibits all the motions of the particular point in the wire which is crossed by the slit. In order to bring the plane of the slit into exact coincidence with the wire, an image of the slit, produced by a lens with the aid of the electric arc, was thrown upon the wire itself. Since the hammer struck the wire at the point photographed, the motion of the wire was traced from the very first, the commencement of the vibration being the most interesting stage. Hard and soft hammers were tried, the latter corresponding to those actually used in the piano. It was found that the duration of contact is longer with feeble than with hard striking. As the force increases, the duration of contact rapidly approaches a limiting value equal to that of a hard hammer of equal weight. But the practically most important resultant was the proof that when a wire is struck at a point between one-seventh and one-ninth of its length, the fundamental tone has a maximum, and the harmonics—especially the third—are very feeble. Hence a wire thus struck gives its strongest and richest tone. This fact is acted upon by piano-builders, but is not explained by supposing that the nodes of the higher harmonics are struck, thus preventing their being heard. They are heard, but are outweighed by the more harmonious ones.

AN interesting paper on the magnetisation of iron in very weak fields, by W. Schmidt, appears in the current number of *Wiedemann's Annalen*. The author uses the magnetometer method slightly modified, a compensating coil being placed on the opposite side of the magnetometer to the magnetising coil. The effect of the iron under investigation on the magnetometer needle is compensated by passing a known current through an independent coil of large radius, so that the method is a "zero" one. A Duprez-d'Arsonval galvanometer was used to measure the current, its constant being determined by means of standard Clark cells. The samples of iron and steel under investigation had the form of ellipsoids, the semi-minor axis being 3 m.m. and the semi-major axis 200 m.m. for one set of experiments, and 150 m.m. for the other. The curves obtained for iron and steel show that for fields up to 0·06 C.G.S. units the susceptibility is constant, thus confirming Lord Rayleigh's results. As the magnetising field increases between 0·06 and 0·4 units, the curve giving the relation between the magnetising force and the susceptibility is a straight line. The author sums up the results of his experiments as follows:—Steel follows weak magnetising forces more quickly than iron. The susceptibility of soft steel is for small magnetising forces greater than that of iron. Thus for fields less than 0·06 C.G.S. unit the susceptibility of soft steel is to that of iron in the ratio of 4 to 3. For magnetising fields of about 1 unit the susceptibility of the two is about the same, while for greater field strengths the susceptibility of iron is greater than that of steel. The limits within which the susceptibility remains constant vary considerably for different samples, but the author considers that 0·06 C.G.S. unit may be taken as the upper limit with sufficient accuracy for most purposes.

THE May number of the *Irish Naturalist* well sustains the reputation for utility and general interest which has been obtained by this periodical. Mr. R. M. Barrington gives an interesting sketch of the career and writings of the late Mr. A. G. More, a naturalist of unusual versatility, who has contributed greatly to our knowledge of Irish Natural History. Mr. Robert Warren writes on the Breeding Birds of Loughs Conn, Carra, and Mask. Dr. R. Hanitsch gives a brief but valuable account of the Fresh-water Sponges of Ireland. The distribution of these forms presents certain features of peculiar interest. The eastern part of the island possesses only common European forms, whereas three out of the four species found along the west coast prove to be American. It is pointed out that the formation of gemmules gives to the Spongillidæ more favourable chances of dispersal than are enjoyed by most other animals. Mr. Clement Reid has examined a sample of marl from which skeletons of the Irish elk had been obtained, and finds that it consists largely of *Chara* and *Potamogeton*. He offers an ingenious suggestion to explain the occurrence of skeletons of *Cervus megoceros* in deposits of *Chara*-marl. Those familiar with pools containing *Chara* will be well aware of the appearance of shallowness, and of a solid floor, which is so deceptive. The plants grow to a depth of several feet, but appear to form a carpet of turf just below the surface of the pools; any animal treading on this turf would immediately plunge head-foremost into the water, and in the case of the elk the antlers would almost inevitably become entangled among the stems of *Chara* and other still tougher pondweeds. This entanglement theory accounts very well for the fact that the remains of stags are far more abundant than those of hinds.

A PHOTOGRAPH of the late Prof. J. D. Dana, taken about six weeks before his death, is reproduced in the current number of the *American Journal of Science*, together with a full biographical notice, and a list of his works.

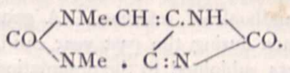
THE "Year-Book of Scientific and Learned Societies" in Great Britain and Ireland, the twelfth annual issue of which has just been published by Messrs. C. Griffin and Co., is undoubtedly a very useful handbook of reference. A general idea of the progress of science during the past year can be obtained from the lists of papers subjoined to the designations of the various societies.

IN the Michigan Mining School theoretical knowledge seems to be well combined with practical training. We notice in the Calendar, just received, that the elements of astronomy is one of the subjects in which all students are examined for entrance into the School. The course of instruction is arranged so that a good foundation is given in the principles of science, and experience and practice are obtained in every subject taught.

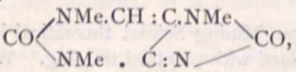
THE new editions received during the past week include the first volume of the British Museum "Catalogue of Fishes," containing the Centrarchidæ, Percidæ, and Serranidæ (part), by Mr. G. A. Boulenger, F.R.S. When the first edition of the work was published, in 1859, the Museum collection of fishes comprised 29,275 specimens. The additions since that date have brought the collection up to twice its dimensions at the time when the original catalogue was compiled. The need for revision will, therefore, be fully understood. In the volume which begins the publication of the new edition of the catalogue, Mr. Boulenger confers a benefit upon ichthyologists by omitting seventy-six imperfectly or incorrectly characterised species. The result of this is that, though many new species are included, the present volume contains only fifty-eight more recognised species than the original one. Mr. Boulenger's list gives 375 species, of which 261 are now represented in the British Museum collection, by 2353 specimens.

NEW editions of two technical manuals have reached us from Messrs. Whittaker and Co. One of these is "Electricity in our Houses and Workshops," an admirable handbook by Mr. S. F. Walker, in which the every-day working of common forms of electrical apparatus is simply described. "The Practical Telephone Handbook," by Mr. Joseph Poole, which is now issued in an enlarged form, should prove of increased value to all interested in the methods of telephone working. A new and enlarged edition (the fourth) of Balfour Stewart's "Lessons in Elementary Physics" has been published by Messrs. Macmillan and Co. In this volume we have a treatise in which the whole domain of physics is covered, and which is so arranged that the connections between the various branches of the subject are clearly brought before the student. Though a quarter of a century old, if the book is brought into line with modern physics from time to time, it will command success for many years to come. "Wild Nature won by Kindness" (Fisher Unwin), has attained the eminence of a sixth edition. Mrs. Brightwen's pleasantly written papers on natural history subjects are evidently appreciated by a large public. Three books by Prof. S. Cooke, of the College of Science, Poona, published by Messrs. George Bell and Sons, have also been received. They are "First Principles of Astronomy" (fifth edition), "First Principles of Chemistry" (sixth edition), and "Test Tables for Qualitative Analysis" (third edition.)

THE synthesis of caffeine is the subject of a short communication to the Berlin Academy by Emil Fischer and Lorenz Ach (*Sitzb. König. Preuss. Akad. Wiss. Berlin*, xiv. p. 261). By the condensation of dimethyl urea and malonic acid the substance $CO : (NMe.CO)_2 : CH_2$, is obtained. The nitroso-derivative, $CO : (NMe.CO)_2 : CH.NO$, is reduced to dimethyluramil, $CO : (NMe.CO)_2 : CH.NH_2$, whence dimethylpseudouric acid, $CO : (NMe.CO)_2 : CH.NH.CO.NH_2$, is produced. By abstraction of the elements of water with oxalic acid, dimethyluric acid is formed. This substance is converted into theophylline, an isomeride of theobromine, of the formula



Thence caffeine,



is obtained by the ordinary methyl iodide reaction. As this is the first synthesis of caffeine, details of the methods used will be looked forward to with considerable interest.

THE observation by Martin Freund and Ernst Göbel, that thebaïne is a derivative of phenanthrene (*Ber.* 28, 7, 941) brings this alkaloid into line with morphine and codeïne as instances of the few natural phenanthrene derivatives yet known. Thebaïne is related to dihydrophenanthrene in the same manner as morphine and codeïne are connected with tetrahydrophenanthrene.

THE additions to the Zoological Society's Gardens during the past week include a Vervet Monkey (*Cercopithecus lalandii*, ♀) from Natal, presented by Mr. Alfred James; a Common Jackal (*Canis aureus*), two Bengal Foxes (*Canis bengalensis*), a Jungle Cat (*Felis chaus*) from India, presented by Dr. John Anderson, F.R.S.; two Short-tailed Capromys (*Capromys brachyurus*) from Jamaica, presented by Mr. Frank Cundall; a Dorsal Squirrel (*Sciurus hypopyrrhus*) from Central America, presented by Mrs. Brett; five Squirrel-like Phalangers (*Belideus sciureus*) from Australia, presented by the Right Hon. Earl Cadogan, K.G., a Cambayan Turtle Dove (*Turtur senegalensis*) from West Africa, presented by Mr. C. L. Sutherland; a Salle's Amazon (*Chrysotis sallæ*) from St. Domingo, presented by Mr. W. Windsor Spriggs; a Spotted Salamander (*Salamandra maculosa*), European, presented by Mr. E. Layton Bennett; two Great Wallaroos (*Macropus robustus*, ♂ ♀), two Agile Wallabies (*Halmaturus agilis*) from Australia, a Blue and Yellow Macaw (*Ara ararauna*) from South America, deposited; two Canada Geese (*Bernicla canadensis*) from North America, two Yellowish Weaver Birds (*Sitagra luteola*) from Tropical Regions, thirteen Green Lizards (*Lacerta viridis*) from Jersey, purchased; a Malaccan Parrakeet (*Palæornis malaccensis*) from Malacca, a Nicobar Pigeon (*Calænas nicobarica*) from the Indian Archipelago, received in exchange; a Bennett's Wallaby (*Halmaturus bennetti*, ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

STARS WITH REMARKABLE SPECTRA.—At those stages of celestial evolution in which the temperature is low, it is probable that the average condensing body will not be very bright, so that the study of their spectra at these phases of their development presents some difficulties. Thus, the great majority of the stars with bright-line spectra, and stars showing intense carbon absorption, are of low magnitudes, and because comparatively few were identified in the earlier surveys of stellar spectra, they were looked upon as "peculiar." Nevertheless, a study of these spectra in relation to those of the brighter stars indicated that they probably represented stages in the history of all condensing bodies, so that their discovery in greater numbers was only to be expected. At Harvard College especially has the photographic investigation of these faint spectra been carried on, and the discovery of eleven more objects with peculiar spectra has been recently announced by Mrs. Fleming; two of these are simply stated to be "peculiar," three are nebulæ with bright lines, two have the F line of hydrogen bright, two appear to be bright-line stars of the Wolf-Rayet type, and two show carbon absorption. In addition, the spectra of eleven stars of the α Herculis type were found to show the F line of hydrogen bright, and this unfailing sign of variability in this group of stars is fully substantiated by an examination of chart plates of the same regions. (*Astrophysical Journal*, May.) In his observations of the visual spectra of faint stars, Rev. T. E. Espin has discovered numerous objects which he describes as "remarkable." (*Ast. Nach.* 3286.) Most of these, however, appear to be of the α Herculis type; but some are spectra in which carbon absorption is predominant (Group VI.). The most interesting observations by Mr. Espin are those of variable stars, and they tend to show that, in the stars of Group VI., the variability is accompanied by spectroscopic variations which render it difficult to distinguish them from stars of Group II., in which the carbon flutings are bright.

THE PARIS OBSERVATORY.—The annual report for 1894 indicates a high state of activity in this institution, in many directions besides those with which occasional published papers have made us familiar. Great progress has been made with the star catalogue initiated by Admiral Mouchez in 1882, comprising 350,000 observations made between 1837 and 1881; two additional volumes will be published during the present year, and the last two in 1899; a supplementary couple of volumes, dealing with observations made since 1881, will also be issued very shortly. The number of meridian observations during last year amounted to over 18,000, while observations of sun, moon, and planets total 455. Comets, minor planets, and double stars have also received a vast amount of attention. For the great photographic chart, 278 negatives were taken during the year, and the positions of nearly 33,000 stars on various plates have been measured under the direction of Mdlle. Klumpek.

The reduction of these measures was seriously commenced in November, and up to the end of the year the measures of 11 plates, showing 1760 stars, were completely reduced. Meteorological observations on the usual plan were continued regularly.

The magnificent work on lunar photography with the equatorial coudé, as well as the spectroscopic researches of M. Deslandres, have already been referred to in our columns.

THE ACTION OF LIGHT ON ANIMAL LIFE.

ALTHOUGH a number of investigations have been made on the action of light on bacteria, very few experiments have been carried out to ascertain how direct insolation affects animals inoculated with particular disease microbes. Does exposure to sunshine increase or diminish an animal's susceptibility to disease? De Renzi was, we believe, the first to study this question experimentally, and he endeavoured to answer it as regards tuberculosis by inoculating guinea-pigs with tuberculous material. Some of the animals he kept in glass boxes exposed to the direct rays of the sun for five or six hours daily, whilst others were placed in the sunshine, but instead of glass, wooden boxes were used. De Renzi found that, whilst the guinea-pigs in glass boxes—to which, therefore, the maximum amount of sunshine had access—died after 24, 39, 52, and 89 days, those in the opaque wooden boxes died after 20, 25, 26, and 41 days. Thus it would appear that sunshine materially assisted these animals in combating with tuberculous disease, for those individuals deprived of sunshine succumbed far more rapidly.

More recently, Dr. Masella has carried out a series of similar experiments with guinea-pigs inoculated, however, with cholera and typhoid bacilli respectively. Various points were investigated as to whether insolation *previous* to inoculation increased the animal's susceptibility to these diseases, also what was the effect of insolation on the animal after infection, and whether the same results were obtained when the temperature of the surrounding air during insolation was not permitted to rise. The toxic properties of the cholera and typhoid broth cultures employed were carefully tested, and it was ascertained that the lethal dose in the case of cholera, procuring death in twenty-four hours, was secured by employing cultures in the proportion of 0·20 per cent of the weight of the animal operated upon, whilst to obtain similar results with typhoid cultures, 0·40 per cent. of the weight of the animal was the proportion in which they had to be used.

In the case of both cholera and typhoid it was found that previous exposure to sunshine increased the animals' susceptibility to these diseases, for not only did they die more rapidly when subsequently inoculated with these cultures than the guinea-pigs similarly treated, exposed, however, only to diffused light, but they succumbed to smaller doses, and doses which did not prove fatal to the guinea-pigs which had been previously protected from sunshine. When the exposure to sunshine took place *after* infection fatal results were greatly accelerated, for instead of dying in from 15 to 24 hours they succumbed in from 3 to 5 hours. These experiments were, however, open to the objection that the accelerated lethal action through subsequent insolation might be due to the higher temperature which necessarily prevailed in boxes exposed to sunshine over those to which diffused light only was admitted. To dispose of this difficulty, boxes were constructed with double cases through which a current of water was kept circulating; in the "sunshine" boxes, as before, only glass was used, whilst in the "diffused light" boxes the outer case was made of zinc. In spite, however, of these precautions as regards temperature the results confirmed those previously obtained, the insolated animals still exhibiting the same increased susceptibility to infection from these diseases over the non-isolated animals.

Dr. Masella does not attempt to give any explanation of the remarkable results he has obtained, but we would suggest that the action of sunshine should be tried on anti-toxines. It would be of great interest to ascertain how the potency of these protective fluids outside the body was affected by exposure to sunshine, and also what result, if any, isolation had on their generation within the animal system.

We know that the toxic properties of, for example, tetanus cultures may be entirely destroyed in from 15 to 18 hours in direct sunshine at a temperature of from 35° to 43° C., and Roux and Yersin state that five hours' direct insolation greatly modifies the toxic properties of diphtheria cultures; again, Calmette has found that after two weeks' insolation the poison of the *Naya tripudians* is completely destroyed, whilst a similar exposure has a damaging effect on the poison of the rattlesnake. So far as we are aware, the action of sunshine on the immunising properties of serum has not been investigated, and its study should prove of immense interest and importance.

The results obtained by De Renzi with tuberculous infection have a practical confirmation in the acknowledged benefit which patients suffering from tuberculosis derive from residence in places such as Davos, where the maximum amount of sunshine may be secured. On the other hand, Dr. Masella's experiments leave us with an uncomfortable uncertainty as to the wisdom of basking in the sunshine. He would have us believe that his investigations explain the greater prevalence and virulence of typhoid and cholera (which he states as an accepted fact) in hot countries where the sun shines with greater power and more continuously. After all, our smoke-laden atmosphere and dreary yellow fogs may be turned to account seemingly, and the London water companies may congratulate themselves that these two water-borne diseases, *par excellence*, may be made to yield not only to efficient purifying processes at their hands, but that such an unexpected ally, according to Dr. Masella, is to be found in the limited amount of sunshine which Londoners can enjoy!

G. C. FRANKLAND.

THE CONSTRUCTION OF STANDARD THERMOMETERS.

A SERIES of important articles on the preparation and testing of standard thermometers have been communicated to the *Zeitschrift für Instrumentenkunde* by Drs. Pernet, Jaeger, and Gumlich, of the Physikalisch-Technische Reichsanstalt. The selection of the best glass, the calibration of the thermometers, the determination of the coefficients of external and internal pressure, and the verification of the principal points are fully dealt with. One source of error in thermometers as usually constructed, lies in the fact of the bulbs being blown from the tubes. The vaporisation of certain constituents of the glass during this operation leads to a difference of chemical constitution between the stem and the bulb. This may be obviated by making the bulbs out of thin walled tubes of the same kind of glass, and welding them on to the stems. As regards the depression of the freezing point, it was found by Wiebe and Schott, of Jena, that glasses containing either sodium or potassium, but not both, showed this after-effect to the least extent. In order to render the reading of temperatures accurate to within 0°·002, the length of a degree should not be less than 6 mm., and since the length of the stem cannot conveniently exceed 60 cm., the range of measurable temperature is practically limited to 100°. Stem thermometers without enamel backs or enclosing tubes were the only ones found suitable for first-class standards. When certain fixed points outside the scale were to be brought in, this was accomplished by widening out the tube above them. An equal linear division of the scale was adopted, this having great advantages over the more or less untrustworthy division by equal volumes. For calibration, threads of mercury of different lengths were cut off from the main portion and measured with micrometer microscopes, viewing them both through the face and the back of the stem. But the threads were not cut off by local heating, since that is apt to produce a permanent change of capacity. The small and almost microscopic bubble which remains in every thermometer was made use of. It was brought to the entrance of the bulb when the desired portion of the thread had been driven into the stem, and then a slight jerk sufficed to cut off the required length. To facilitate this operation, the bulb was narrowed to a neck at the entrance to the stem. As regards pressure, two factors had to be considered. The external atmospheric pressure, and the pressure of the liquid in which it is immersed, tend to compress the glass vessel and to produce an apparent elevation of temperature. The capillary pressure of the mercury, and its hydrostatic pressure, on the other hand, tend to widen the bulb and produce an apparent cooling. The first of these elements was investigated by exposing the thermometer to various high and low pressures in a glycerine bath, and the second by observing the readings when the thermometer stood horizontally and vertically respectively, at its highest measurable temperature. The capillary pressure was found to be too capricious to be accurately measured, but it is a negligible quantity. The coefficient of apparent expansion of mercury in the new Jena glass thermometer 16III was found to be 0·0001571 between 0° and 100°.

THE INFLUENCE OF MAGNETIC FIELDS UPON ELECTRICAL RESISTANCE.

IT is well known that the resistance (R) of a wire of bismuth, as measured with a constant current, increases under the influence of a magnetic field, and that this increase depends on the strength of the field and its direction with reference to the current in the wire. If the current traversing the bismuth is oscillatory, the resistance has a value O outside the magnetic field, or in a field in which the lines of force are parallel to the wire which is less than R. If, however, the wire is perpendicular to the lines of force of a field greater than 6000 C.G.S. units, the resistance O is greater than R; the difference O – R increases from this point pretty rapidly as the strength of the field increases. These changes are not due to alterations in the self-inductor, since they are independent of the form of the bismuth spiral. This curious phenomenon has lately been examined by M. I. Sadovsky (*Journal de la Société Physico-Chemique de Russe*, xxvi. 1894, and *Journal de Physique*, April 1895), who sums up the results of his experiments as follows: (1) The difference in the resistance of bismuth observed with constant or alternating currents is measurable outside a magnetic field with 300 alternations per second, and can be detected in magnetic fields with only three or four alternations per second; (2) this difference depends on the number of oscillations per second, and without the magnetic field increases with the increase in the frequency of the alternations; (3) the resistance which bismuth, in a strong magnetic field, offers to an increasing current is greater, and that to a decreasing current less than the resistance for steady currents. The difference between the resistances to an increasing and decreasing current increases with the rate of change in the strength of the current $\left(\frac{dC}{dt}\right)$, and this difference is more marked with strong currents than with weak. Thus M. Sadovsky has discovered the remarkable fact that for variable electric currents the resistance of bismuth changes with any change in $\frac{1}{C}$ or $\frac{dC}{dt}$ where C is the current. The author mentions that the effects observed cannot be due to self-induction, or they would occur when the bismuth is not in a magnetic field. In a note on the above paper in the *Journal de Physique*, M. Sagnac considers what would happen if the same series of experiments were repeated with an iron wire. A straight cylindrical iron wire becomes, when traversed by a current C, circularly magnetised; the energy due to this magnetisation being, according to Kirchhoff, $\pi\kappa l C^2$, where κ is the susceptibility and l the length of the wire. This energy may possibly increase the coefficient of self-induction by $2\pi\kappa l$. From Klemenčič's data the order of the change in the apparent resistance can be calculated. For weak magnetic fields in which κ has a large value, the difference between the value of the apparent resistance for steady currents and for increasing currents may amount to several hundredths of the value of the resistance for steady currents.

TONBRIDGE SCHOOL LABORATORIES.

I HAVE often been asked to give some account of the laboratories at Tonbridge School; and as they represent some ten years of pleasant labour on my own part, and a considerable expenditure, joined with much sympathy and help from the Governors of the School (the Company of Skinners), I feel it a privilege to do so.

It is difficult to render the subject interesting to those who are not concerned in teaching, although as an instance of an ancient foundation lending itself to the most modern of claims, it may appeal to a wider circle. I must ask to be excused from entering upon any treatment of the well-worn subject, scientific education. I am not quite sure that it is any business of mine. In course of time, no doubt, a condition of stable balance will be reached, as regards the relative weight and value of the various school subjects. Those who are in the thick of the fight cannot always tell which side is winning.

So far we have little at Tonbridge beyond the training-ground itself, consisting of laboratories and workshops, which may be mentioned in sequence as follows:—

Wood Workshops.
Metal Workshops.
Mechanical Laboratory.
Physical Laboratories.
Chemical Laboratories.
Engine-rooms with electric light plant.
Biological Laboratory and Museum.

A description of these in detail is given herewith.

Wood Workshops.—These shops are well lighted and airy, occupying a ground space of 48 feet by 30 feet. Work-benches to the number of sixteen, with appropriate fittings, allow about sixty boys to work at the same time. A skilled carpenter is always in attendance for teaching his craft, and a course of graduated tasks are exacted before a pupil is allowed to construct the shelves, boxes, coal-boxes, tables, and other articles which form the staple produce of school shops.

Metal Workshops.—The wood workshops lead on to the metal shops, in use as well as in fact. They are under the care of a practical instrument-maker, and the physical laboratory owes much to his skill. It may be mentioned here that no physical laboratory can be considered complete unless it is in connection with suitable workshops wherein instruments may be constructed and repaired. These shops are devised to accommodate about twenty boys working together. They are fitted with all the necessary appliances, including planing and drilling machines and six lathes (from 4 in. centre up to 7 in.). The ground space devoted to metal work is 40 feet by 20 feet. After a course of woodwork, boys are taught to make their own tools, forging and tempering them themselves, to use the file properly, to turn, and afterwards to construct such instruments as they may fancy, it being always required that a working drawing should be made beforehand. The favourite occupation is the construction of electric bells, small dynamos, microscopes, and levels.

Mechanical Laboratory.—This room, which measures 40 feet by 21 feet, is fitted for those important lessons in accuracy of observation to which I give the name of Elementary Physical Measurements, *i.e.* the measurements of length, mass, and time, and for Practical Mechanics, *i.e.* the simpler measurements of forces and the conditions of equilibrium, the measurement of gravitation, and observations of the general properties of matter and the behaviour of matter under stress. All the work-tables are movable, and the walls are fitted with brackets and boards for the support of models and apparatus.

Physical Laboratory.—This laboratory opens out from the Mechanical Laboratory, and like it is well-lit and lofty. It is 42 feet long and 30 feet broad. The centre of the room is fitted with five solid benches attached to the floor and provided with gas. These benches are arranged to enable elementary classes to work together at the same experiment. With this object, drawers in the benches are stocked with a large quantity of apparatus which enables a class of twenty-four boys to work together through a long series of experiments in practical physics. Each experiment has to be represented by at least twelve sets of apparatus for this purpose, and some years have been occupied in organising this branch of work. The work-benches along the walls of the room lend themselves to the more advanced work in practical physics. It is needless to say that here the apparatus is not twelve-fold. Beyond the physical laboratory is the science master's private room, which has a tendency to shape itself as an advanced physical laboratory.

Chemical Laboratory.—This is a fine room, with both skylight and side windows. It is 45 feet long, 30 feet broad, and 30 feet high. Eight benches are fixed, two abreast, across the room, allowing the greatest possible freedom of movement. The benches are arranged to admit forty-eight students working together. They are fitted with shelves for reagents, fixed across the bench, and not lengthways, whereby reaching over one's work is avoided, and also a more complete view and control of the whole room is possible for the master. Each student is provided with a most efficient draught-box, serving also as a support for the vessels he is using. This arrangement keeps the laboratory thoroughly free from fumes, in spite of all well-meant efforts to the contrary on the part of pupils. The shelves and draught-boxes are removable from the benches, so that a clear space can be obtained when required for setting up apparatus on an extensive scale. The wall space is occupied by shelves for reagents, and by lead troughs for washing-up purposes. By this arrangement of confining the water-supply to the walls of the room, most of the ordinary splashing and untidiness of laboratories is avoided. The transverse arrangement of the benches reduces to a minimum the walking about occasioned by this plan. The cupboards and drawers of these benches recede, so that it is possible to sit close up to one's work. A balance-room, 30 by 15 feet, leads out from the laboratory, and beyond this is a large theatre or lecture-room capable of seating about 150 boys. The balance-room is provided with chemical balances and books of reference. The lecture-room has a suitably furnished lecture-table, blackboards, screen for lantern, and cases of minerals and chemical specimens.

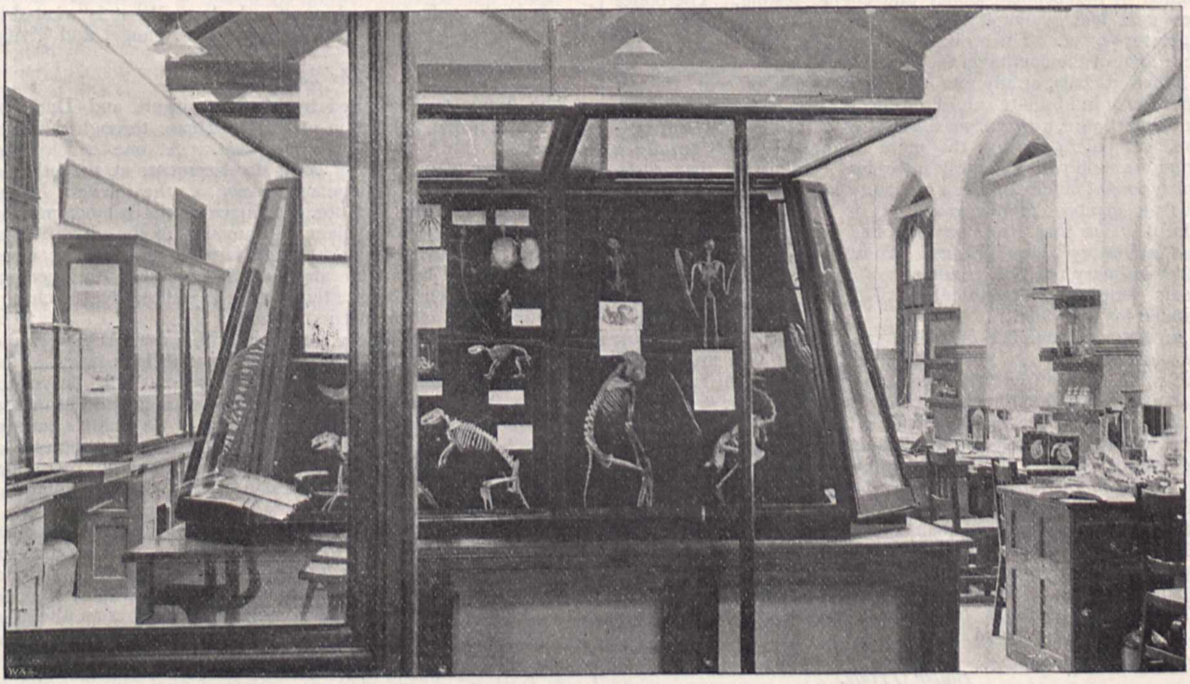
Engine and Electric Light Rooms.—The electric light, being used for the main portion of the school, puts the Science Department in possession of valuable plant. A gas-engine of 12 indicated horse-power, and a reserve steam-engine of 6 indicated horse-power, fitted with a Crosby indicator, together with dynamos and accumulators, give plenty of opportunity for gaining a practical knowledge of electric engineering. In addition to this, the current obtained is most useful in providing means for practical work and testing in the physical laboratory. The electric light is also used with the mirror galvanometer, to the great advantage of cleanliness and convenience.

Biological Laboratory and Museum.—It is appropriate that the description of this laboratory should come last. It is one of the most recent additions to the school, and it should undoubtedly be the last laboratory for the schoolboy to enter. Biology, unless it is approached through a training in physics and chemistry, is not to be considered as a suitable subject for preparatory education. The roots of biological sciences must always be in physical and chemical ground.

The room devoted to this work is carefully planned to ensure the most perfect light. The work-benches face windows which come down to the level of the benches, and in the roof is fixed a good skylight. The work-benches are formed of plate glass, gently sloping at the back into a white glazed gutter running into large white-ware troughs or sinks. Water-supply is at the hand of each worker, and the benches can be kept continually flushed and clean. Standing away from the work-bench is the small writing-table and cupboard, &c., of each student. The arrangements are peculiar, but—I may be allowed to say—most successful. The greater part of the room, which is 40 feet long by 21 feet wide, is occupied by cases which contain preparations and specimens under the headings of (*a*) Form and Locomotion, (*b*) Alimentation, (*c*) Circulation and Respiration, (*d*) Nervous System and Sense Organs, and lastly, objects displaying the main lines of classification. In fact, a collection modelled, both as to cases and modes of display, on the same lines as the admirable Introductory Collection of Sir William Flower at the Natural History Museum. It is, of course, on a miniature scale, and it is not yet complete. Beyond the main laboratory is a smaller room temporarily occupied for another purpose.

It now remains for me to add some attempt at a description of the general appearance of these laboratories. In the main, one may say, there is an air of dignity about the lofty and well-proportioned rooms, with their substantial and costly fittings. The woodwork is pitch-pine topped everywhere by thick teak. In the biological museums the cases are of mahogany, and perfectly constructed. Most of the teak tops of benches and tables are thinly coated with paraffin as a preservative. It is still important that rooms devoted to scientific work in schools should be exceptionally neat and bright in appearance. Indeed, it may even be said that appearances are at present more important than anything else as regards the value attached to the subject. Manners must grow to match the clothes. We have to bear in mind that we labour in the cause not of science alone, but of science as an instrument of school training. The laboratories are all *en suite*, whereby control is more easy, and a feeling of organic unity gained. Moreover, the workshops are within touch of the laboratories, as is also the large drawing-school.

Finally, I may mention that all the water and waste system has been laid down in direct contravention of all the best traditions of plumbing, with the happy result that we never need the services of a plumber for repairing. ALFRED EARL.



A Case of Specimens in Biological Laboratory, illustrating Form and Locomotion.

THE DEVELOPMENT OF THE EXPERIMENTAL STUDY OF HEAT ENGINES.[1]

IT was Mr. Forrest's intention that the annual lecture bearing his name should illustrate the dependence of the engineer in his practical professional work on the mathematical and physical sciences. It therefore naturally takes the form of a review of some branch of engineering with special reference to the scientific principles which have been factors in its advancement.

Twenty years ago, Lord Armstrong stated that of all the coal raised in this country about one-third was used for household purposes, one-third for generating steam, and one-third for iron-making and manufacturing processes. He remarked that in the two first divisions the waste of fuel was shameful, and that without carrying economy to extreme limits, all the effects now realised from the use of coal could be obtained by an expenditure of half the quantity. The improvement of the steam engine is mainly due to an incessant attempt to diminish the waste of fuel.

Tests of Steam Engines in Cornwall.

Steam engineers have been face to face with the problem of economy for more than a century. Coal was excessively dear in Cornwall, and as the mines were deepened and more power was required, the cost of working increased ruinously. By reducing fuel cost, Watt saved the mining industry from extinction, and he adopted the plan of taking in payment for his engines a sum reckoned equivalent to one-third of the fuel saved. By agreement with the miners, tests were made, and the standard duty of a Newcomen engine was fixed at 7,037,000 foot lbs. per bushel. Regular duty determinations were made for all Watt's engines. Generally they gave a duty of 20,000,000. When Watt's connection with Cornwall ceased in 1800; the duty determinations were neglected, and the engines deteriorated.

Then Captain Joel Lean, who had reorganised the machinery at one of the mines, and effected great economies, started again the system of duty trials. He and his sons carried on the work for many years, and published reports of the results of the trials. Of these reports Dr. Pole says: "The publication produced an extraordinary effect in improving the duty of the engines. Engineers were stimulated to emulation amongst themselves. The practice of reporting is thought to have been attended with more benefit to the county than any other single event excepting only the invention of the steam engine itself."

I shall show later that the creation of a new and more scientific system of testing by Hirn and his colleagues in Alsace, in 1855, was the starting-point of a similar process of improvement. Quite lately there has been a revival of careful and independent engine testing and of the publication of the results, and records have been established which would have been thought impossible ten years ago.

The peculiar character of the load against which the Cornish

[1] Abstract of the "James Forrest" Lecture, delivered at the Institution of Civil Engineers, May 2, by Prof. W. C. Unwin, F.R.S.

engine worked, the lifting of a heavy mass of pump-rods, contributed to force the use of expansive working. To work without shock, a large initial and gradually diminishing effort was necessary. The extraordinary economy obtained was due probably in part to the special action of the steam, the Cornish engine being virtually a compound engine, and the admission surface being protected from cooling to the condenser; partly to the great effectiveness of a steam-jacket in an engine which worked slowly and with pauses at the end of the stroke, partly to the small clearance and separate admission and exhaust valves. The lesson engineers should have learned from Cornish experience was that in restricted conditions high ratios of expansion were economical. In this case, as in others, later engineers generalised too crudely, and concluded that expansive working was always economical. A new scientific investigation was required to correct the error.

Experiments on Boilers.

To generate steam power economically the boiler must be efficient, and the engine must be efficient, and the conditions of efficiency of boiler and engine are completely independent. Hence the early method of Watt, in which attention was paid only to fuel used and water pumped has been replaced by a method of independent boiler and engine testing. The boiler uses coal and generates steam; the engine uses steam and generates power. The economy of the boiler is reckoned on the fuel; that of the engine on the steam.

Different coals, at any rate the better kinds of coal, do not differ much in absolute calorific value. Used in boiler furnaces, they differ more, partly from differences of mechanical aggregation, but chiefly because the more bituminous coals require a larger air supply for tolerably smokeless combustion than those which consist chiefly of fixed carbon. The greater excess of air involves greater chimney waste. It is to test the commercial calorific value that Prof. Schröter has established in Munich a heat laboratory where fuels can be tested on a large scale and under ordinary practical conditions of combustion. The arrangements permit the determination of the exact conditions most suitable for each fuel.

An enormous number of boiler trials have been carried out, but most of them are mere individual tests of very little scientific value. Engineers have been too much under the impression that the evaporation depended chiefly on the type or proportions of the boiler, or the arrangement of the heating surface. But there are no obscure or complicated actions concerned in generating steam. Boilers of all types give nearly the same results, provided only proper conditions of combustion are secured. They may differ in cost, in durability, in convenience, but in efficiency they differ less than I think is commonly assumed. The following table shows that boilers of extremely different types, with very different proportions of heating surface and very different rates of combustion, and even with different coals, have all reached evaporations of from 11 to 13 lbs. of water from and at 212° per pound of coal:—

Boiler Trials.

Type.	Ratio of grate to heating surface.	Coal per sq. ft. of grate per hour.	Evaporation from and at 212° per lb. of coal.	Coal.
Cornish ...	—	7·2	11·9	Welsh
Lancashire ...	1 : 36	22·9	11·2	Lancashire
Galloway ...	1 : 24	8·5	11·6	Anthracite
Portable ...	1 : 69	12·8	11·8	Welsh
Tubular ...	1 : 46	10·8	11·9	Anthracite
Babcock ...	1 : 38	8·9	11·8	,,
Marine ...	1 : 34	22·4	12·9	Welsh
,, ...	1 : 50	25·5	12·5	Lancashire
Thorneycroft	1 : 70	7·7	13·4	Welsh
,,	1 : 61	18·6	12·5	,,

Mulhouse Trials of 1859.

The earliest boiler trials carried out in a completely satisfactory way were those made by the Société Industrielle of Mulhouse in 1859. The Society offered a prize to the maker of any boiler which would evaporate 1800 lbs. per hour, at 75 lbs. per square inch pressure, and which would evaporate 9·1 lbs. of water, from and at 212°, per pound of Alsatian coal of not very good quality. With the coal used in these trials, 130 cubic feet of air per pound of coal are chemically necessary for complete combustion. It was found that the reduction of the air supply almost to this limit, and to a point at which there was definitely incomplete combustion, reduced the chimney waste and increased the efficiency of the boiler. In two special trials, each of a week's duration, the evaporation was 9 lbs. with 331 cubic feet of air, per pound, and 9·53 or 6 per cent. more with 247 cubic feet.

The determination of the air supply to a boiler is not altogether an easy operation. An anemometer was used in Alsace, and in suitable conditions it will give approximately accurate results. In recent trials chemical analyses of samples of the furnace gases have been made, and the amount of air supplied calculated from the percentage of CO_2. This method is accurate in principle, but the samples analysed are a very minute fraction of the total chimney discharge, and the samples may not be average samples. In some trials in which this method has been used, there are discrepancies in the ratio of the chimney loss and the undetermined loss, chiefly due to radiation, difficult to understand. Neither anemometer nor chemical analysis is suited to serve as a means of regulating the air supply in the ordinary working of a boiler. What is wanted is an instrument as easily read as a pressure gauge, and giving continuous indications.

The Dasymeter.

The dasymeter, invented by Messrs. Siegert and Durr, of Munich, is a fine balance in an enclosed case through which a current of the furnace gases is drawn. At one end of the balance is a glass globe of large displacement, at the other a brass weight. Any change of density of the medium in the chamber disturbs the balance. A finger on the balance moving over a graduated scale gives the amount of the alteration of density. An air injector draws the furnace gas from the flues, and it is filtered before entering the balance case. An ingenious mercurial compensator counterbalances any effect due to change of temperature or barometric pressure.

The dasymeter is usually combined with a draught gauge, and an air thermometer or pyrometer in the flue is required if the amount of waste heat is to be calculated. The dasymeter requires, initially, exceedingly delicate adjustment, and its indications must be checked from time to time by a Bunte's burette. It is set to read zero with pure air, and then any increase of density due to CO_2 is read as a percentage on the scale. When in adjustment, it is as easy to read the percentage of CO_2 in the furnace gases as to read the pressure on a pressure gauge. When the dasymeter is fitted to a boiler, the stoker has directions to adjust the supply of air so that the furnaces gases have about 12 per cent. of CO_2. With practice he learns what alterations of the damper or fire-door, or thickness of fuel on the grate, are necessary, or whether an alteration of grate area is desirable. After a little time the percentage of CO_2 can be kept very constant.

Isherwood's Experiments on Marine Engines.

About the year 1860, Mr. Isherwood, Chief Engineer of the United States Navy, began a series of systematic tests of engines and boilers on a very large scale, and with resources only available in a Government establishment. The trials were made with skill and determination, and the substantial accuracy of the results, startling as they were, has never been questioned.

All Isherwood's trials of large marine engines showed that when expansion was extended beyond exceedingly small limits, it caused not an economy, but a waste. In his second volume he sums up his results as proving that when cut-off is earlier than 0·6, or perhaps even 0·7 of the stroke, the consumption of steam reckoned on the work done is increased. Curiously enough, this led him to attack the compound engine. From the quantities in the table of experiments, he says, "it will be seen how useless in point of economic gain is the preposterous arrangement of steam engine known as the double cylinder, Woolf, or Hornblower engine. . . . Opposed to these facts, the declarations of interested patentees and engine builders must be classed in value with those set forth by quacks in advertisements of their nostrums." This is from a paper dated 1865, and it is curious, because Isherwood generally saw clearly enough the danger of drawing sweeping conclusions from narrow experimental premises.

The proper lesson from Isherwood's results was merely that certain conditions must be observed to secure economy in expansive working. Unfortunately, more generally the conclusion was drawn that the Cornish results were not to be trusted, and that expansion was not economical, and Isherwood's own language lent authority to the least accurate view of his results. To obtain greater insight into the true action in the cylinder, and to find a reconciliation of the Cornish and American tests, experiments of a much more refined character were wanted and insight due to wider scientific knowledge.

The Physical Properties of Steam.—Regnault.

No useful progress could be made with a theory of the steam engine, no accurate reduction even could be made of the results of engine tests without exact determinations of the relations of pressure, temperature, volume, latent heat and liquid heat of steam. It was fortunate, therefore, that about 1840 M. Regnault obtained the means from the French Government to make a series of researches on the physical properties of steam with splendid instrumental appliances. He wisely carried out his determinations over a very wide range of conditions, and spared no labour or trouble in attaining accuracy. Regnault's results were of the greatest importance as a foundation for accurate study of the steam engine.

The Foundation of Thermodynamics.—Carnot and Joule.

The next important step was the discovery of the equivalence of heat and work. Joule's investigations began with an attempt to improve Sturgeon's magnetic engine. He was so led to consider motive power problems from the engineer's standpoint, as a question of duty, or of something obtained for something expended. He ascertained the amount of electric current produced by the chemical combustion of a given amount of zinc, and comparing his results with those obtained in good steam engines, he concluded that, making the largest allowance for possible imperfections of his magneto engine, it was never likely to be a rival in economy to the steam engine. That was a negative but a useful result. It closed one direction of useless endeavour only too likely to attract the inventor.

One of the effects of electric action which Joule noticed was the heating of his conductors, and it was to the measurement of this heating effect he next addressed himself. The heat developed in the conductor by the electric action due to elements combining in the galvanic cell was found to be identical with that which would be generated by the direct combustion of the same elements. Finally, he came to consider the relation between the mechanical work expended in driving a magneto electric machine, and the heat developed in the external circuit of the machine. He concluded that for 838 foot lbs. expended a pound degree of heat was generated. Later experiments corrected this value, but the discovery of the equivalence of heat and work was made.

As early as 1824, twenty years before Joule's discovery, Sadi Carnot, in a remarkable pamphlet on the "Motive Power of Heat," demonstrated the fundamental principle that the amount of work obtainable from any given quantity of heat cannot exceed a quantity proportional to the fall of temperature. Unfortunately adopting, though with hesitation, the view held in his time that heat is material and indestructible as heat, he coupled with his true principle the false corollary that all the heat entering an engine is discharged in the condenser. Joule, in 1845, found this principle of Carnot, and looking to the corollary as essential, supposed the principle itself to be false. He failed to perceive that Carnot's principle was the essential supplement to his own discovery, and that it showed why the apparent efficiency of the steam engine is so low. It took six years before Joule's and Carnot's principles were reconciled, and for three of them even Lord Kelvin refused to accept Joule's discovery, because it apparently conflicted with the principle of Carnot.

The Founders of the Rational Theory.—Rankine, Clausius, Zeuner.

The impetus given to the study of thermodynamics by the discovery of Joule, and the perception of the fundamental importance of Carnot's theorem, was enormous. Heat problems could now be brought out of the region of mere empirical solutions, and treated from the rational standpoint of an exact science, and the steam engine, as the most important example of heat transformation, attracted at once the attention of scientific men of commanding intellectual ability. In a very few years Rankine and Clausius had built up a strictly rational mathematical theory of the steam engine, and, a little later, Zeuner carried further the analysis of some of the more subordinate details. The theory with one exception, to be referred to presently, took account of all the actual conditions under which steam is used, and furnished exact rules for the relation of steam expended and work done for all arrangements of the actual steam engine practically adopted.

It was just at this time that the experiments of Isherwood were published, and a comparison of experimental results and theoretical calculations showed directly a very large discrepancy. The steam consumption in some trials was 30, 40 or 50 per cent. more than it should have been in the assigned conditions of working according to the rational theory. Some action of quite governing importance had obviously been neglected in the theoretical analysis.

The Experimental Theory.—Hirn and the Alsatian School.

A year or two before Isherwood began his experiments, an Alsatian engineer, M. Hirn, had discovered and measured cylinder condensation.

Joule's discovery attracted Hirn's attention, and he set to work in 1854 to verify, by an exact engine test, whether the difference between the heat received by an engine and discarded in the condenser was the equivalent of the work done. His two most important memoirs relating to the steam engine, are a memoir on the utility of steam-jackets in 1855–6, and another on the use of super-heated steam in 1857. In these researches he devised a method of accurate engine tests, involving the measurement of all the quantities of heat received by or rejected from the engine, which, with hardly any change at all, is the method of accurate engine testing adopted ever since. Under his influence and direction, engine tests were carried out in Alsace for many years, and the results exactly analysed. It may be recalled that the admirable series of engine tests, the first tests in which the heat quantities were accurately measured in this country, which were made by Mr. Mair Rumley, and described in three papers on "Independent Engine Tests" in the *Proceedings* of this Society in 1882, 1885, and 1886, were trials carried out strictly in accordance with Hirn's methods.

As with Lord Kelvin, so with Hirn. It was the recognition of an apparent conflict of Joule's discovery with Carnot's law which first attracted his attention. It was the attempt to determine whether part of the heat supplied to an engine disappeared as work which determined the form of his trials. His experiments of 1854 showed that "heat in a steam motor is not only dispersed, but actually disappears, and the power obtained is exactly proportional to the heat which disappears as heat to reappear as motive power." Some rather later and more careful experiments enabled him to verify Joule's equivalent by the actual results of a large engine test to an accuracy of about one per cent.

The discovery of initial condensation, and the proof of the powerful action of a small amount of heat transmitted from the jacket, both pointed to the conductivity of the cylinder wall as the cause of the large waste of steam which the constructors of the rational theory had neglected. The cylinder is cooled during expansion, and still more during exhaust by an action analogous to internal radiation to the condenser. Before any work can be done in the next stroke, the wall has to be reheated by condensing fresh steam. The extreme facility with which steam yields or abstracts steam by condensing and evaporating, accounted for the rapidity of the action. The magnitude of the condensation increases with the range of temperature to which the cylinder wall is subjected. It is larger in condensing than in non-condensing engines, and larger with high ratios of expansion.

Some time ago I ventured to say that there was no trustworthy engine test which showed that the consumption of steam with a jacket is greater than without the jacket. I believe that is still true, but undoubtedly the economy due to the jacket varies in different cases from 30 per cent. to very nearly zero. Roughly, the jacket is more useful with small engines than with large; with slow engines than with fast engines; but all this amounts to little more than saying that the jacket is most useful in those cases where the initial condensation is largest. Just in proportion as the engine, whatever its type, is of the highest class and most scientific design, the jacket is less useful.

The jacket reduces, but it does not prevent initial condensation. Hirn looked for some more powerful way of heating the cylinder wall without causing condensation; he found it in super-heating. He constructed, in 1855, a super-heating apparatus in the flues of the boiler at Logelbach, which still exists. The experiments with super-heated steam were carried out between 1855 and 1856, and showed clearly the effectiveness of the method in reducing condensation. Super-heating came largely into use in the years 1860-70 in this country in marine engineering, the practice having been introduced here by John Penn. In every case in which it was used an economy of coal was realised. Generally the economy amounted to from 15 per cent. to 20 per cent. It was ascertained that this was due strictly to economy of steam, and not to the utilisation in the boiler of heat previously wasted. But the use of super-heated steam in this country was gradually abandoned, partly no doubt from some practical difficulties, but chiefly, I believe, because practical engineers had no clear idea why super-heating should produce so large an economy, and they were not indisposed to abandon a complication, the action of which they could not satisfactorily explain to themselves.

No possible improvement of the steam engine, of which we have any knowledge at this moment, offers anything like so great a chance of important economy as the reintroduction of super-heating, and especially of super-heating to at least 100° or more above the saturation temperature of the steam. I obtained in Alsace on a very good 500 H.P. compound mill engine with jackets, and every appliance for economical working, an economy of 15 per cent. Mr. Mair Rumley has fitted a super-heater to a Babcock boiler supplying a triple engine, and has obtained an economy of 10 per cent. In both cases the economy is economy of steam, and therefore is not due to any increase of boiler surface or increase of efficiency in generating the steam. Lately Prof. Schröter, of Munich, has been experimenting with a small special compound condensing engine of only 60 I.H.P., running at the moderate piston speed of 380 feet per minute, and with the not excessive boiler pressure of 165 pounds per square inch. The H.P. cylinder is not jacketed. The L.P. is jacketed with receiver steam. In this case in a tube super-heater of a rather special construction in the uptake of the boiler, the steam is superheated to 670° F., or nearly 300° above the saturation temperature corresponding to the pressure. In two trials of six and eight hours' duration, periods quite long enough for accurate determination of results with so accomplished an observer as Prof. Schröter, the consumption of steam was only 10·2 pounds per I.H.P. hour, and the consumption of German coal of moderate quality only 1¼ pounds per I.H.P. hour. The steam consumption is the lowest on record for any engine of any type or size, and is very remarkable for so small an engine.

Conflict of the Rational and Experimental Theories.—Zeuner, Hirn, and Hallauer.

On the appearance of Isherwood's researches in 1863, the discrepancy between the rational theory and the results of experiments was recognised by Rankine and others. But the conditions of cylinder condensation are so complex, that for a long time the more theoretical writers practically ignored both Hirn's and Isherwood's results. Zeuner, perhaps, had pushed the rational theory to the furthest limit of detail, and with the greatest insight into practical conditions. But it was not till 1881 that he began to explicity admit the largeness and importance of the condensing action in the cylinder. Zeuner then was disposed to attribute initial condensation to the presence of a permanent and not inconsiderable mass of water in the clearance space of the engine. No doubt it is simpler analytically to deal with the thermal changes of the steam plus a given mass of water than with the thermal changes of steam, water, and a varying area of solid cylinder wall. In opening a discussion with Hirn in 1881, Zeuner wrote that if the presence of water in the clearance space was conceded, the Alsatian calculations would be greatly shaken, and "the enormous influence which they attributed to the cylinder wall would in future be attributed in part, perhaps chiefly, to the water in the clearance space." He thought it conceivable that in certain cases the whole of the initial condensation was due to water in the clearance space. There thus arose a rather angry controversy, which has been summed up in the question, "Is it water or iron?" I do not know that this controversy has been as yet completely decided, or that perhaps an absolute decision is possible. I cannot help thinking that Hirn, with the clearness of view due to his experimental work, had on the whole the best of the controversy, and I do not know that anything better or more instructive can be said than the words in which he finally summed up his position. "We recognise," he said, "that the interpretation of the Alsatians differs from that of M. Zeuner, not at all in that it denies the possible presence of water in the cylinder (we are not so hydrophobic), but in that it admits that the water, varying in quantity, is presented only temporarily, is carried away and renewed stroke by stroke, and acts chiefly as the medium between the steam and the cylinder wall. In the Alsatian explanation the action of the water raises the thermal action of the sides. In Prof. Zeuner's view, the water is permanently present and acts independently of the cylinder sides."

Recent Systematic Experiments.—Willans.

It has been quite impossible in this lecture to do more than select one or two of the most important of the experimental investigations during the last fifty years. But I should not like to omit all reference to the two series of experiments of the late Mr. P. W. Willans. Mr. Willans' work is no doubt well known to all steam engineers, and needs no detailed description. However purely practical the object Mr. Willans had in view, his experiments were made in the true spirit of scientific research. No trouble was too much to secure accuracy to the last decimal, no possible cause of error was so trivial that its investigation was reckoned unnecessary. A few experimenters, Isherwood, Gately and Kletsch and others had made experiments on a methodical system, varying a single factor at a time. Willans carried out the method of experiments in series on a scale which, till he proved that it could be done, no one would have supposed possible. There is a series of non-condensing and a series of condensing trials; in each there are trials of simple, compound, and triple engines; and for each of these, again, trials with initial pressure varied, with expansion varied, and with speed varied. The results, tabulated in the clearest way, form a quarry of scientific data, but at present, in the main, an unworked quarry. Perhaps that statement will seem surprising, and of course I am expressing only my own view, for which I claim no infallibility. What Mr. Willans might have done had he been spared, it is impossible to say. He had the most active mind and the widest experience devoted, perhaps, at any time to the study of steam problems.

Let me protest as strongly as possible, again with the reservation that I am stating my personal view, against the tendency to suppose that the great work of Willans can be summed up in a so-called Willans' law, or that that law, handy as it may be for practical steam engineers, is more than a quite subordinate part of Willans' work. The Willans' law is nothing more than the empirical descriptive statement that the relation of total steam consumption and indicated or effective horse-power can be very approximately expressed by a linear equation, for the case of an unjacketed engine working with a fixed cut-off. Further, nothing is done in Willans' papers to fix what is the linear equation for any given engine. So far as those papers go, and until some kind of theory taking account of initial condensation is discovered, we can only find the relation of steam consumption and horse-power for any given engine by making two accurate trials of the engine itself. Willans' law leaves us in regard to any given engine in the same position as an astronomer with a new comet. When the comet has been observed for a sufficient period, and some of its positions fixed, a probable orbit can be calculated. The straight-line law leaves the steam consumption of a new engine as unknown as the elliptic law the orbit of a new comet.

Willans himself says nothing whatever as to any possible rational basis for the Willans' law. He put it forward purely as the result of plotting his experiments. Later, Captain Sankey showed that the total steam consumption of an engine working adiabatically with fixed ratio of expansion would also follow nearly but not exactly a straight-line law if all clearance losses, radiation, and exhaust waste and back pressure loss were neglected.

If we assume isothermal expansion (and really so far as the area of the diagram is concerned, it matters little what law of expansion is assumed), it is easy to find a formula for the total steam consumption of an engine working without clearance loss or exhaust waste. I have found such a formula, and plotted the results both for a condensing and a non-condensing engine in the diagram. It is found that the lines plotted are not exactly, but very nearly, straight lines. That carries us a certain way, but it is an enormous jump to assume without examination that the steam wastes in the engine, amounting to from 20 to 50 per cent. of the steam used, and arising from causes of the most complex kind, depending on the volume of the clearance, the action of the cylinder wall, the loss of the toe of the diagram, the waste expansion between the cylinders, and other causes of loss, that these also can be expressed as a simple linear function of the horse-power.

Now, in the first edition of his treatise on the steam engine, which appeared in 1878, Prof. Cotterill had seriously attacked the problem of cylinder condensation from the theoretical side. Prof. Cotterill found it possible to give a partly rational, partly empirical, formula for cylinder condensation.

But, according to his formula for unjacketed simple engines, the initial condensation has a fixed ratio to the steam present at cut-off. In the diagram, lines for steam present at cut-off are given, calculated in the manner already described. Above these has been set up the condensation by Cotterill's law, and the total steam consumption at various loads is then given by a line very nearly straight and closely agreeing with a Willans' line.

The curves on the two diagrams agree well with Willans' results, and they differ from Willans' lines in being obtained entirely by calculation without experimenting on the engine. It would not be right to make too much of the coincidence, but I thought it would be interesting to show that theory and experiment converge. A good deal has yet to be explained, but the discussion in Prof. Cotterill's treatise has done more than anything else to throw light on the conditions which promote or hinder cylinder condensation, and on the means useful in securing economy of working.

Since 1845, purely scientific men, scientific experimenters, and practical engineers have all been engaged in the study of the steam engine. I do not believe that any one of the three can claim all the credit for the improvement of the steam engine to the exclusion of either of the others.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—At a Congregation of the University held on Tuesday, 21st inst., the proposed Statute on Degrees for Research was passed in its final form, *nemine contradicente*. It only remains for the Statute to be passed by Convocation, and it will come into force.

At the same meeting, the addition to Statute conferring the title of Professor of Anthropology on Dr. E. B. Tylor, so long as he shall hold the office of Reader in Anthropology, received the final sanction of Congregation. A proposal recognising Anthropology as a subject for the Final-Honour School of Natural Science was then brought forward. After some debate the preamble was passed. Placets 24, non-placets 16.

CAMBRIDGE.—The following is the Speech delivered by the Public Orator, Dr. Sandys, in presenting for the honorary degree of Doctor in Science, Mr. Francis Galton, F.R.S.

Sedes olim sibi notas hodie revisit alumnus noster, qui flumine Nilo quondam explorato, et Africa Australi postea perlustrata, velut alter Mercurius omnium qui inter loca deserta et inhospita peregrinantur adiutor et patronus egregius exstitit. Idem, velut alter Aeolus, etiam ipsos ventos caelique tempestates suae provincae audacter adiunxit. Hodie vero Academiae nemora nuper procellis nimium vexata non sine misericordia contemplatus, e frondibus nostris caducis capiti tam venerabili coronam diu debitam imponi patitur. Tempestatum certe in scientia iamdudum versatus, ventorum cursus tabulis fidelibus olim mandavit, gentesque varium caeli morem praediscere docuit, laudem philosopho cuidam antiquo a Nubium choro Aristophanico quondam tributam uno saltem verbo mutato meritus:—*οὐ γὰρ ἂν ἄλλῳ γ' ὑπακούσαιμεν τῶν νῦν μετεωρολογούντων.* Longum est avorum et proavorum ingenia magna in ipsorum progenie continuata ab hoc viro, Caroli Darwinii cognato, virorum insignium exemplis illustrata percensere. Longum est tot honores titulosque ab ipso per tot annos cumulatos commemorare. Hoc autem in loco, eloquentiae eius undecim abhinc annos conscio, instituti anthropologici praesidem non corporis tantum sed etiam mentis humanae mensorem appellaverim. Inter antiquos quidem celebratum erat illud Protagorae, omnium rerum mensuram esse hominem. Inter recentiores autem notum est hunc praesertim virum hominum omnium, imprimis pessimorum, mensuram ad amussim velle exigere. Ceterum plura hodie dicere supervacaneum est; constat enim ne optimorum quidem virorum a laudibus abesse debere mensuram.

Duco ad vos virum de scientia anthropologica et meteorologica praeclare meritum, caeli et terrae indagatorem indefessum, studiorum denique geographicorum etiam inter nosmet ipsos fautorem insignem, FRANCISCUM GALTON.

Lord Acton will deliver his inaugural lecture as Regius Professor of Modern History on June 11, at noon.

Prof. Lewis announces courses of lectures and demonstrations in Crystallography during the Long Vacation, beginning on July 9.

Prof. Roy announces a practical course in Bacteriology, to be given by Dr. Wesbrook and Dr. Lazarus-Barlow, in the Long Vacation, beginning on July 8. There will also be a course of lectures with practical work in Elementary Pathology, beginning on July 9.

Mr. H. F. Baker, Fellow and Lecturer of St. John's College, has been appointed University Lecturer in Pure Mathematics, in the place of Dr. Forsyth, now Sadlerian Professor.

A YEAR ago a committee, representing various educational bodies, was formed, at the instance of the Association of Headmasters, to formulate an examination syllabus on which to award major scholarships, offered by County Councils and similar bodies, and tenable at places of higher education. All who know how very different are the scholarship schemes of the County Councils, agree that a larger degree of uniformity should prevail in the examinations held for the purpose of selecting candidates for the scholarships. The Association's scheme for major scholarships has been so drawn up that only candidates showing ability and intelligence distinctly above the average can be elected. Candidates must not be more than eighteen years of age in case of boys, and nineteen years in case of girls, and must have passed a preliminary examination to test their general education before they can compete for the scholarships. The scheme provides that the final scholarship examination shall consist of three groups—science, commercial, and literary—each containing obligatory and optional subjects. The subjects of examination for science scholarships have been carefully selected, and with due consideration to the claims of practical work.

THE second annual report of the Technical Education Board of the London County Council appears in the *Technical Education Gazette*. A sum of nearly £15,000 was granted, during the year covered by the report, to intermediate and secondary schools. The fact that the Board has now over six hundred scholars attending these schools indicates how seriously it is concerned with technical education. While the question of the Teaching University for London has been in abeyance, the Board has not been able to act upon the proposal in Mr. Llewellyn Smith's report to contribute £10,000 a year towards the technical departments of the University. It was thought undesirable, however, to wholly abstain from helping institutions of university rank until the establishment of the Gresham University, so a grant of £1000 was made to University College, and £500 to Bedford College. The polytechnic institutes are rapidly increasing in extent and advancing in efficiency. The total annual expenditure of the eight institutions open last year amounted to about £90,000, their total number of students to over 27,000, and their aggregate number of separate classes or courses of lectures to over 1250. It is believed that the polytechnics now give probably nine-tenths of all the evening instruction in technological subjects in London, and three-fourths of the evening science instruction. All this represents an immense advance on the state of things five years ago, and indicates that the Board has remarkably extended the facilities for technical education during the two years it has been at work.

THE fourth annual report (2 vols.) of the U.S. Commissioner of Education has been received. The volumes provide a mine of information on educational methods in France, Austria, Germany, Sweden, Switzerland, Alaska, the United States, and our own country. A full account is given of the character and development of German Universities, by Prof. Paulsen, of Berlin, supplemented by a statistical review of the subject by Prof. Conrad, of Halle. School museums in various parts of the world form the subject of a separate chapter. There is also an elaborate paper in which methods of physical training are very fully treated.

SCIENTIFIC SERIALS.

Bulletin of the American Mathematical Society, vol. i. No. 7, (April 1895).—"Riemann and his significance for the development of modern mathematics," is the translation, by A. Ziwet, of an address delivered by Prof. F. Klein at the general session of the Versammlung Deutscher Naturforscher und Aerzte in Vienna, September 27, 1894. In it the author attempts to give an idea of the life-work of Bernhard Riemann, "a man who more than any other has exerted a determining influence on the development of modern mathematics."—Prof. Cajori contributes a note on the multiplication of semi-convergent series, in which, following up his work in a recent number of the *Bulletin*, he further extends results arrived at by Pringsheim (*Math. Ann.* vol. xxi. pp. 327-378) and by A. Voss (*Math. Ann.* vol. xxiv. pp. 42-47).—Mr. L. E. Dickson discusses Gergonne's Pile Problem (*cf.* Ball's "Recreations," pp. 101-6), and points out one or two slight inaccuracies in a proof given by Dr. C. T. Hudson in *Educational Times* Reprints, vol. ix. pp. 89-91.—Prof. Ziwet gives an account of the Repertoire bibliographique des Sciences Mathematiques, *i.e.* a card catalogne of mathematical literature which has been widely circulated amongst mathematicians. Notes, and new publications, as usual, close the number.

Bulletin de l'Académie Royale de Belgique, No. 3.—On Chandler's formulæ, by F. Folie. The author criticises the latest formula enunciated by Chandler for the variation of latitude. Even when compared with the Strassburg observations, which most closely accord with the formula, it is evident that the periods are not correctly rendered. The constants in the formula require further empirical research.—On the equations of the physical field, by Ch. Lagrange. The form, *i.e.* the law of distribution of a quantity of matter round its centre of inertia, constitutes in physics a principle as important as the quantity of matter itself, or its mass. Besides the principle of concentration, there is a principle of direction, and the latter is as important as the former. The author investigates the equations of motion in a medium consisting of rigid points, and introduces the conception of axial matter (*matière axée*), in which account is taken not only of the mass of a point, but also of all the qualities depending upon the shape of the mass. The density of a point is simply the intensity of one of the parameters determining its action, but a large number of other parameters of known form remain to be considered. The consideration of axial matter leads in a manner analogous to that which obtains in Kelvin's theory of the intensity of magnetisation, to theorems upon wires, plates, and leaves of similar substance, and then upon bodies made up of these structures.—On the colour, density, and surface tension of hydrogen peroxide, by W. Spring. This substance, which is highly explosive in the anhydrous state, has a blue colour when seen in a thickness of 100 cm. The colour resembles that of water, but is 1·83 times as intense. The density of the anhydrous substance is 1·4996. When 60·0445 gr. of it are contained in 100 cc. of an aqueous solution, the density is 1·2540. The surface tension is 0·456, that of water being 1. The addition of 6·4 per cent. water raises the surface tension by 102·5 per cent.—Action of certain hot gases upon red phosphorus, by A. J. J. Vandevelde. Retger's supposition that phosphamine is produced by passing hot hydrogen over red phosphorus is not correct. Vapour of phosphorus is formed and carried off by the hot gas, exhibiting the phenomenon of spontaneous combustion on emerging into the air. Other hot gases, such as nitrogen, CO_2, CO, SH_2 and dry HCl gave rise to the same phenomenon.

Wiedemann's Annalen der Physik und Chemie, No. 4.—On luminescence, by Eilhard Wiedemann and G. C. Schmidt. An important distinction must be drawn between luminescence due to physical and that due to chemical causes. A prolonged afterglow makes the presence of chemical luminescence very probable. Thermoluminescence occurs after the body has been exposed to a temperature far below incandescence. A phenomenon now called "lyoluminescence" occurs with some substances during solution, when they have been previously exposed to strong light. The authors show that luminescence under cathode rays is always accompanied by chemical action. Mixtures of calcium and manganese salts show luminescence phenomena of great brilliance under cathode rays, and when subsequently heated.—On normal and anomalous dispersion of electric waves, by L. Graetz and L. Fomm. The dielectric constant and the conductivity of a body are not perfectly independent quantities, but are connected by the constitution of the body in a manner similar to that in which refraction and absorption are connected in optics.—Magnetisation of iron by very small forces, by Werner Schmidt. Steel obeys very small magnetising forces more rapidly than iron. The limit of proportionality between magnetising forces and magnetic moment may with practically sufficient accuracy be placed at a field intensity of 0·06.—Otto von Guericke's original air pump, by G. Berthold. The pump in the Royal Library at Berlin cannot be considered as Guericke's original air pump, since the latter was bought by the Archduke of Saxony, and taken to Sweden by Dr. Heraeus, where it was used as late as 1726 as a lecture instrument. When last heard of, in 1734, it was in charge of the Professor of Mathematics at Lund.—Remarks upon Mack's paper on the double refraction of electric rays, by Wilhelm von Bezold. The different behaviour of wood towards electric radiation along and across the fibre may be shown in a variety of ways. Lichtenberg's figures on wooden plates cut along the fibre show an elliptical shape, like doubly refracting crystals. A similar phenomenon is exhibited by a plate of ebonite rendered anisotropic by sticking strips of tinfoil parallel to each other on the other side. The production of a doubly refracting or even a circularly polarising body for electric rays by embedding conducting rods in a suitable dielectric does not appear to be hopeless.

THE only article of general interest in the *Nuovo Giornale Botanico Italiano* for April is one by Dr. U. Brizi, on the disease of the vine known as *brunissure* or blackening. The plasmode found in the diseased cells of the leaves cannot, he considers, be properly referred to *Plasmodiophora*, as has been done by most authorities hitherto. It belongs to an organism which appears rather to present characters intermediate between the Myxomycetes and the Amœbæ.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, April 25.—Mr. A. G. Vernon Harcourt, President, in the chair.—The following papers were read:—Action of nitroxyl on amides, by W. A. Tilden and M. O. Forster. The interaction of nitroxyl chloride and amides usually results in the exchange of the amidogen group for an atom of chlorine.—Action of nitrosyl chloride on asparagine and aspartic acid; formation of lævorotatory chlorosuccinic acid, by W. A. Tilden and B. M. C. Marshall. Asparagine and nitroxyl chloride interact with formation of lævo-chlorosuccinic acid.—A property of the non-luminous atmospheric coal-gas flame, by L. T. Wright.—Diortho-substituted benzoic acids. (1) Substituted benzoyl chlorides, by J. J. Sudborough.—Diortho-substituted benzoic acids. (2) Hydrolysis of aromatic nitriles and acid amides, by J. J. Sudborough. In these two papers the author describes a number of new nitro- and bromo-benzoyl chlorides and benzoic acids.—Note on the action of sodium ethylate on deoxybenzoïn, by J. J. Sudborough. When deoxybenzoïn and sodium ethoxide are heated together, stilbene and hydroxydi-benzyl are produced.—A constituent of Persian berries, by A. G. Perkin and J. Geldard. In addition to the substances previously isolated from Persian berries, the authors have obtained aquercitin dimethyl ether which they term rhamnazin.—Potassium nitrososulphate, by E. Divers and T. Haga. The potassium nitrososulphates, by described Hantzsch, and by Raschig, seem to be identical with that first prepared by Pelouze.—The milk of the gamoose, II., by H. D. Richmond.

May 2.—Studies on the constitutions of the tri-derivatives of naphthalene. No. 10, the dichloro-α-naphthols and trichloronaphthalenes from 3 : 4-dichlorophenyl-1-isocrotonic acid. No. 11, the trichloronaphthalene derivable from Cleve's 1 : 2 : 2-α-nitrochloronaphthalenesulphonic chloride. No. 12, the trichloronaphthalene, derivable from Alén's α-nitronaphthalene-2 : 2'-disulphonic chloride. No. 13, the α-naphthylamine-2 : 2'-disulphonic acid of Freund's German Patent, 27346. No. 14, the fourteen isomeric trichloronaphthalenes. The non-existence of a trichloronaphthalene melting at 75·5°: the formation of chloro-derivatives from sulphonic chlorides, by H. E. Armstrong and W. P. Wynne. In these six papers the authors describe thirteen out of the fourteen possible isomeric trichloronaphthalenes, together with a large number of compounds obtained during the preparation of these halogen derivatives.—The solubilities of gases in water under varying pressure, by E. P. Perman. Henry's law holds for chlorine, bromine, carbon dioxide, and hydrogen sulphide, but large deviations are observed with ammonia, hydrogen chloride, and sulphur dioxide.—The existence of hydrates and of double compounds in solution.

Part I, by E. P. Perman. From experiments on the pressure of gases dissolved in various solutions the author concludes that sodium sulphate exists in aqueous solution as the hydrate $Na_2SO_4,10H_2O$, and that silver chloride exists in ammoniacal aqueous solutions as the compound $AgCl,3NH_3$.—Derivatives of π-bromocamphoric acid, by F. S. Kipping.—Paraheptyltoluene and its derivatives, by F. S. Kipping and O. F. Russell.—Note on the formation of a phosphate of platinum, by R. E. Barnett. On passing phosphorus pentoxide vapour and oxygen over red-hot platinum, a yellow phosphate PtP_2O_7, insoluble in aqua regia, is obtained.

Linnean Society, May 2.—Mr. C. B. Clarke, President, in the chair.—Dr. O. Nordstedt of Lund, Dr. Rudolph Philipppi of Santiago, and Dr. M. Woronin of St. Petersburg, were elected foreign members.—Mr. H. M. Bernard showed under the microscope the circumscribed patches of setæ above and below the stigmatæ on the pupa of the vapourer moth (*Orygia antiqua*). The arrangement suggested a vanished notopodium just where, in the Hexapods, a dorsal branch of a parapodium ought to have vanished, according to the exhibitor's method of deducing the different groups of the Arthropoda from their Annelidan ancestors, as sketched in his recent paper on the *Galeodidæ*.—Mr. E. M. Holmes exhibited some new British Algæ from Dorsetshire and Surrey; amongst others, *Uluella confluens* and *Ectocarpus Reinholdi*, both discovered last month at Weymouth, and the latter previously known only from the Baltic.—Mr. J. E. Harting exhibited and made remarks on a specimen of *Cuculus canorus* in the rare hepatic plumage (*Cuculus hepaticus*, Sparrman), recently obtained at Bishop's Waltham, Essex.—Mr. W. T. Thiselton-Dyer, C.M.G., then gave an abstract of a paper by the late Mr. John Ball, F.R.S., on the distribution of plants on the southern side of the Alps, prefaced by some account of the author's life, and special work in relation to the Alpine flora.

Mathematical Society, Thursday, May 9.—Major P. A. Macmahon, R.A., F.R.S., President, in the chair.—Dr. Hobson, F.R.S., made a communication on the most general solution of given degree of Laplace's equation.—Prof. M. J. M. Hill, F.R.S., read two short notes: (1) a property of a skew determinant; (2) on the geometrical meaning of a form of the orthogonal transformation.—Prof. Greenhill, F.R.S., and Mr. T. I. Dewar gave an account of results relating to the spherical catenary. The investigations given in NATURE, p. 262, January 10, 1895, when the parameter of the associated elliptic integral of the third kind is of the form $4\omega_3/\mu$, where ω_3 is the imaginary period and μ is an integer, worked out in detail for $\mu = 3, 4, 5$, and 8, have been extended by Mr. Dewar to the cases of $\mu = 6, 7, 9, 10$, and 12.

In particular, when $\mu = 10$, the catenary is given by an equation of the same form as for $\mu = 5$,

$$(1 - z^2)^{\frac{5}{2}} e^{5\chi i} = Hz^5 + H_1z^4 + H_2z^3 + H_3z^2 + H_4z + H_5 + i(Lz^3 + L_1z^2 + L_2z + L_3)\sqrt{Z},$$

where

$$Z = (1 - z^2)(z - h)^2 - A^2,$$

and

$$\chi = \psi - p\int Z^{-\frac{1}{2}}dz;$$

and it was found that p could be made to vanish, so that the catenary becomes a closed algebraical curve on the sphere, by taking

$$h = \frac{1}{2}\sqrt{\frac{17}{3}},\ A = -\frac{1}{10}\sqrt{\frac{5}{3}},\ L = -\frac{5}{6}\sqrt{\frac{5}{3}},$$

$$L_1 = -\frac{5}{36}\sqrt{85},\ L_2 = \frac{35}{72}\sqrt{\frac{5}{3}},\ L_3 = \frac{13}{144}\sqrt{85};$$

$$H = \frac{1}{6}\sqrt{\frac{17}{3}},\ H_1 = 0,\ H_2 = -\frac{5}{12}\sqrt{\frac{17}{3}},\ H_3 = -\frac{25}{108},$$

$$H_4 = \frac{65}{288}\sqrt{\frac{17}{3}},\ H_5 = \frac{25}{144}.$$

A model was exhibited of this spherical catenary, formed by a chain wrapped on a terrestrial globe; and so far this appears to be the only real algebraical case, for which it is possible for p to vanish.—Mr. G. Heppel exhibited a set of Napier's Bones, of date 1746, and explained how they were used in calculations, referring for a further description of them to the *English Cyclopædia*.—The following papers, in the absence of their authors, were taken as read:—On those orthogonal substitutions that can be generated by the repetition of an infinitesimal orthogonal substitution, by Dr. H. Taber.—Notes on the theory of groups of finite order (continuation), by Prof. W. Burnside, F.R.S.—Applications of trigraphy, by Mr. J. W. Russell; and the reciprocators of two conics, by Messrs. J. W. Russell and A. E. Jolliffe.

Zoological Society, May 7.—Sir W. H. Flower, K.C.B., F.R.S., President, in the chair.—A letter was read from Dr. F. A. Jentink, concerning a monkey lately described as *Cercocebus aterrimus*, of which the type had lately been acquired by the Leyden Museum. Dr. Jentink considered this monkey to be the same as *Cercocebus albigena*, Gray.—Mr. J. H. Gurney exhibited and made remarks on a rare kingfisher (*Alcedo beavani*) obtained in Ceylon by Mr. A. L. Butler.—Mr. G. F. Scott Elliot made some remarks on the fauna of Mount Ruwenzori, in British Central Africa. Mr. Scott Elliot stated that elephants occur in great numbers on the east side of Ruwenzori. There were also many still living and vast stores of ivory in the Congo Free-State, just beyond the south-west corner of the English sphere of influence. He pointed out the presence of the hippopotamus in the Albert-Edward Nyanza, and its extraordinary abundance in the Kagera River. The rhinoceros was found frequently in the country of Karagwe, usually near the marshy lakes leading to the Kagera.—On the alluvial plains about the east of Ruwenzori, Jackson's hartebeest (*Bubalis jacksoni*), the kob (*Cobus kob*), and another waterbuck (perhaps of a new species) were common. No buffaloes were seen. A bushbuck also occurred on Ruwenzori from 7000 to 8000 feet. Of monkeys, Mr. Scott Elliot had noticed the presence of a black and white *Colobus*, which he could not identify, and of at least two other species, probably a *Cercopithecus* and a baboon. Some small mice brought home had not yet been identified. Leopards were numerous, and lions were also common on the lower grounds. Two species of sunbird were brought back, one of which ascends to 11,000 feet on Ruwenzori. Mr. Scott Elliot concluded by remarking that the general idea of distribution gathered from the flora seemed to confirm such data as he could gather from the fauna of the country which he traversed during his journey.—Mr. F. E. Beddard, F.R.S., and Mr. P. Chalmers Mitchell made a communication on the structure of the heart in the alligator, as observed in specimens that had died in the Society's menagerie.—Mr. Chalmers Mitchell described the anatomy of the crested screamer (*Chauna chavaria*), pointing out some resemblances between the alimentary canal of that bird and the ostrich, and giving a detailed comparison of the structures of *Chauna chavaria* and *Palamedea cornuta*.—A communication was read from Dr. Percy Rendall, containing field-notes on the antelopes of the Transvaal.—Dr. Mivart, F.R.S., read a paper on the skeleton of *Lorius flavopalliatus* as compared with that of *Psittacus erithacus*.

Geological Society, May 8.—Dr. Henry Woodward, F.R.S., President, in the chair.—The Stirling dolerite, by Horace W. Monckton. The rock described in the paper forms a mass of about eight miles in length, with an average width of about a mile; it is intruded into the lower part of the carboniferous limestone series. There is little doubt that the Abbey Craig rock, north of the Forth, is connected with the Stirling rock; and there is reason to think that the igneous rocks of Cowden Hill and of the hills around Kilsyth are outlying portions of the Stirling rock, being connected with it underground. All these patches, as well as the main mass, are for the most part composed of a more or less coarse-grained dolerite, the marginal part always becoming finer-grained, whilst the actual edge has apparently been a tachylyte now devitrified. The author gave the results of his macroscopic and microscopic examination of the rocks from various parts of the mass.—Notes on some railway cuttings near Keswick, by J. Postlethwaite. Several cuttings have recently been made on the Cockermouth, Keswick, and Penrith Railway, chiefly through drift, though some occur in the Skiddaw slates, and in one case a diabase dyke (much decomposed) was met with. The author described the drifts as blue clay beneath, and brown clay above, and considered that these two clays were produced during two separate periods of glaciation, with no long interval between. In some places near Keswick water-borne gravel may be seen surmounted by blue clay; this gravel was considered by the author to be of fluviatile origin.—The shelly clays and gravels of Aberdeenshire considered in relation to the question of submergence, by Dugald Bell. The drifts of this region have been described by Mr. Jamieson, and also in the publications of the Geological Survey. The two authorities agree that the lower (grey) boulder clay of the district was produced by a local glaciation. The geological surveyors, however, maintain that the intervening sands and gravels with marine shells were produced during a submergence of 500 feet or upwards, whilst the upper (red) boulder clay was formed by an ice-sheet from the south. Mr. Jamieson, on the other hand, assigns a purely glacial origin to the middle sands and gravels, and considers that the red clay (which contains a few fragments of marine shells) indicates a submergence. The author discussed these views, and maintained that submergence is not proved in the case of either middle gravels or red clay, but that the former are, as Mr. Jamieson maintained, truly glacial, whilst he advocated the existence of extra-morainic lakes to explain the latter.

PARIS.

Academy of Sciences, May 13.—M. Marey in the chair.—On the cœlostat, a mirror apparatus giving an image of the sky which remains fixed with regard to the earth, by M. G. Lippmann. A plane mirror is mounted on an axis resting on fixed bearings. The mirror and its axis are parallel to the polar axis. A motor turns the system at a uniform speed once round in forty-eight sidereal hours, in the same direction as that of the celestial sphere. The author gives a proof that this mirror fulfils the necessary conditions, and points out wherein it differs from the ordinary siderostat. He shows how the siderostat can be used to demonstrate the principle of the cœlostat, and how the latter instrument can be employed in place of an equatorial.—Thermochemical relations between the isomeric forms of ordinary glucose, by M. Berthelot. Three forms of glucose are distinguished: α, the ordinary form, for which $\alpha_D = +106°$; β, produced by transformation of α at 100°, giving $\alpha_D = +52°.5°$; and γ, formed from α at 110°, having $\alpha_D = +22°.5$. These rotations are observed immediately on solution; left for some time all are converted into the β form in solution. The change of α into β glucose absorbs 1·55 Cal., the corresponding change of γ into β glucose absorbs 0·67 Cal., in the anhydrous state.—On an automatic registering measuring machine for the comparison of end measures, by M. L. Hartmann.—Researches on the hatching of "l'œuf des sexués" of the vine Phylloxera, by M. L. J. Leroux.—The works printed in the correspondence are: A necrological notice on Ernest Mallard, by M. A. de Lapparent. Petroleum, asphalt, and bitumen, from the geological point of view, by M. A. Jaccard. Invasions of locusts in Algeria, by M. J. Künckel d'Herculais.—Demonstration of Tchébychef s theorem, by M. André Markoff.—On the equivalence of six different forms of expression of the quadratures of algebraical differentials reducible to elliptic integrals, by M. F. de Salvert.—On the integration of the system of differential equations, by M. A. J. Stodolkievitz.—On a new method for the production of fringes with great differences of phase, by M. Gouy. A theoretical paper.—On the electromagnetic theory of the absorption of light in crystals, by M. Bernard Brunhes.—Anomalous rotatory dispersion of absorbent bodies, by M. A. Cotton.—General solution of Maxwell's equations for a homogeneous and isotropic absorbent medium, by M. Birkeland.—On argon and helium. An extract from a letter by Prof. Ramsay to M. Berthelot. An account is given of a sample of gas obtained from a meteoric iron from Augusta County, Virginia, U.S.A. After sparking with oxygen and over caustic soda, the residual gas gave spectroscopic evidence of the presence of argon and helium. Only the lines of argon and helium were observed. This evidence is taken as proof that argon exists in extra-terrestrial bodies, though it has not been noticed in the sun. Helium is found in most of the rare earth minerals examined by Prof. Ramsay.—On the definite combination in copper-aluminium alloys, by M. H. Le Chatelier. The author corrects his previous announcement of the alloy AlCu. The substance had been more profoundly altered by the reagents used than was at the time suspected.—Estimation of sulphur in cast-irons, steels, and irons, by M. Louis Campredon.—Researches on mercurous chloride, bromide, iodide, and oxide, by M. Raoul Varet. A thermochemical paper giving details concerning the heats of formation of these salts.—On the molecular origin of the absorption bands of cobalt and chromium salts, by M. A. Étard. The conclusions are drawn:—(1) That chromium salts and the red cobalt salts have fine spectroscopic bands, just as is the case with the rare earths and uranium salts. (2) That these are spectra of molecules like the spectra given by organic substances of the chlorophyll type. (3) The hypothesis that each band of the spectrum of a rare earth corresponds to an element is not necessarily true, according to the evidence of cobalt. (4) The bands may be displaced or disappear for one and the same element according to the nature of the molecules in solution or of the compound observed.—On the molecular modifications of glucose, by M. C. Tanret.—On the use of carbon tetrachloride as a means of separating methylene from ethyl alcohol, by M. Maxime Cari-Mantrand.—On a brown pigment in the elytra of *Curculio cupreus*, by M. A. B. Griffiths.—On the aeration of the soil in the Paris promenades and plantations, by M. Louis Mangin.—On the existence of numerous crystals of orthoclase felspar in the chalk of the Paris basin; proofs of their genesis *in situ*, by M. L. Cayeux.—On gypsum from the neighbourhood of Serres (Hautes-Alpes) and Nyons (Drôme), by M. Victor Paquier.—On the miocene near Bourgoin and Tour-du-Pin, by M. Henri Douxami.—On the presence of *Ostrea (Exogyra) virgula* in the upper Jurassic of the Alpes Maritimes, by M. Adrien Guébhard.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—The Study of "Primitive" Man: E. Clodd (Newnes).—Catalogue of the Fishes in the British Museum, 2nd edition, Vol. 1 (London).—Year-Book of the Scientific and Learned Societies of Great Britain and Ireland, 12th annual issue (Griffin).—L'Industrie Chimique: A. Haller (Paris, Baillière).—My Climbs in the Alps and Caucasus: A. F. Mummery (Unwin).—Transmissions par Cables Métalliques: H. Léauté and A. Bérard (Paris, Gauthier-Villars).—Lessons in Elementary Physics: Prof. J. B. Stewart, new edition (Macmillan).—Agriculture, Practical and Scientific: Prof. J. Muir (Macmillan).—A Monograph of the Order of Oligochæta: F. E. Beddard (Oxford, Clarendon Press).—Fingerprint Directories: Dr. F. Galton (Macmillan).—First Principles of Astronomy: Prof. S. Cooke, 5th edition (Bell).—First Principles of Chemistry: Prof. S. Cooke, 6th edition (Bell).

PAMPHLETS.—Rapport Annuel sur l'État de l'Observatoire de Paris, 1894 (Paris).—Jamaica in 1895 (Kingston, Jamaica).—The Rise and Development of the Bicameral System in America: T. A. Moran (Baltimore).—The Pocket Gophers of the United States: V. Bailey (Washington).—The Student's Practical Chemistry: Test Tables for Qualitative Analysis: Prof. S. Cooke, 3rd edition (Bell).—Report of the Departmental Committee upon Merionethshire State Mines (Eyre and Spottiswoode).

SERIALS.—Journal of the Franklin Institute, May (Philadelphia).—Royal Natural History, Part 19 (Warne).—Aus dem Archiv der Deutschen Seewarte, xvii. Jahrg. 1894 (Hamburg).

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